Econ. Theory 7, 381–405 (1996)

Economic Theory

Research articles

The preference reversal phenomenon: Response mode, markets and incentives*

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Received: September 15, 1994; revised version March 2, 1995

Summary. This paper addresses the apparent conflict between the results of experiments on individual choice and judgement and the results of market experiments. Data are reported for experiments designed to analyze the effects of (a) economic incentives, repetition, feedback and information and (b) choice and valuation response modes on (c) subjects' decisions in paired market and nonmarket environments. Causes of divergent market and nonmarket behavior are identified in the context of the preference reversal phenomenon (PRP). Study of the PRP is extended to two types of market environments. The PRP is observed on the first repetition in a market setting (second price auction) with immediate feedback, both with and without financial incentives. However, after five repetitions of the auction, the subjects' bids are generally consistent with their choices and the asymmetry between the rates of predicted and unpredicted reversals disappears. An individual pricing task using the BDM mechanism yields similar results on the first repetition but results which differ from the second price auction on the fifth repetition. Choice tasks produce lower rates of reversals than do pricing tasks in both market and individual decision making settings.

1. Introduction

The preference reversal phenomenon has been a subject of research for over two decades. First discovered by pschologists (Slovic and Lichtenstein [60]; Lichtenstein and Slovic [42, 43]; Lindman [44]), it has been studied by economists

* We are grateful for financial support from the National Science Foundation (grant no. SES-8820552) and the California Institute of Technology. Research facilities were provided by the Economic Science Laboratory at the University of Arizona. Valuable computer programming was provided by Sean Coates and Shawn LaMaster. We thank Professor Jeffrey Dubin for his help in setting up the software for the generalized Tobit model, and Professors Joyce Berg, Graham Loomes, and Charles R. Plott for helpful comments.

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beginning with Grether and Plott [31] (see Cox and Epstein [16]) and references there). In addition to replication of the phenomenon, there have been several attempts to explain it (Holt [33]; Loomes and Sugden [48]; Loomes, Starmer and Sugden [45, 46]; Goldstein and Einhorn [29]; Karni and Safra [41]; Segal [58]; Schkade and Johnson [56]).

In preference reversal experiments, subjects are asked to choose between two lotteries in each of several pairs of binary lotteries. One lottery (or "gamble" or "bet") in a pair typically has a high probability of winning a small amount of money; this is the probability bet or "P bet." The other, riskier lottery in the pair has a smaller chance of winning a larger amount of money; this is the dollar bet or "$ bet." In addition to choosing between the gambles, subjects are asked to place monetary values on them. The valuation (or judgement) question has been asked in many ways; the most common procedure has been to elicit selling prices using the procedure introduced by Becker, De Groot, and Marshak [5].[1]

A preference reversal occurs when the preference revealed by choice is the reverse of the preference revealed by valuation, i.e. when the chosen bet is given a lower valuation than the other bet. In most previous experiments, observed preference reversals have been asymmetric: subjects have frequently chosen the P bet and assigned the higher price to the $ bet, but rarely have they chosen the $ bet and placed a higher value on the P bet (however, see Casey [14], for a notable exception).

a. Markets vs. individual experiments

The experimental literature seems to us to establish the following: individual behavior is in some situations inconsistent with expected utility theory in ways that are systematic and replicable. In addition, individual beliefs about probabilities are often poorly calibrated and do not obey the rules of the probability calculus (Allais [1]; Beach and Wise [3]; Beach, et al. [4]; Ellsberg [23]; Fischhoff [24]; Kahneman, Slovic and Tversky [40]; Grether [30]). On the other hand, the results from market experiments generally are reported to be consistent with economic theory. The predictions verified are often from models which include the assumption that agents are expected utility maximizers whose subjective probabilities are objectively correct and internally consistent (Plott, Miller and Smith [53]; Forsythe, Palfrey and Plott [26]; Plott and Sunder [54]; Cox, Smith and Walker [20]; Forsythe et al. [25]; Camerer [13]). Like all generalizations, the one we have just stated has exceptions (Kagel and Levin [38]; Isaac and Plott [37]; Plott and Sunder [55]; Gigerenzer [27]). One of the few researchers to study the biases observed in individual experiments in market settings is Camerer ([10, 11]). He found biases in markets which were in the direction predicted from individual

[1] The Becker-DeGroot-Marshak (BDM) procedure works as follows. A subject states the minimum price at which he would sell his right to play a lottery. A buying price is then drawn from some probability distribution. If the buying price exceeds the stated selling price, the subject sells his right to play the lottery at the buying price. If the buying price is less than the selling price, the subject plays the lottery.

experiments but of small magnitudes. The effects of arbitrage on preference reversals in marketlike experiments have been studied by Berg et al. [6] and by Chu and Chu [15].

There are several reasons why phenomena such as preference reversals that are robust observations in individual choice experiments may not be robust in market experiments.

Feedback: Individual decision experiments differ greatly in the feedback subjects receive during the experiments. In some experiments, subjects learn the results of their decisions immediately, while in others they are only informed about a subset of their decisions at the end of the experiment. In contrast, in market experiments subjects are almost always informed at once of the consequences of their actions.

Repetition: Market experiments usually involve repetition; often there are multiple periods, each with identical parameters. Experiments which do not literally repeat the same environments still require subjects to perform a series of similar tasks (submitting bids, making offers, etc). Individual choice and decision experiments are more varied in this regard. In some cases, subjects are made to repeat the same task several times and in other cases each task is done once. These comments are not necessarily intended as criticism of some experiments focusing on individual behavior; in many cases repetition could be inappropriate, possibly leading subjects to view their task as a consistency test.

Psychologically different tasks: It may be that behavior in market environments is different from behavior in individual decision making settings. It is possible that putting people into market environments causes them to act differently. The presence of other active participants whose behavior influences their rewards may cause people to behave in a more strategic manner. Also, speculation about what actions others may take may lead to a different analysis of the situation and affect the actions taken.

Institutions: Market institutions may be robust. We know that the standard textbook conditions of perfect competition are not necessary to attain competitive outcomes in market experiments (Smith [61]). It may be that some market institutions can achieve efficient allocations even with traders that commit preference reversals and other anomalies in individual choice experiments. After all, double auction market allocations are highly efficient even with *random* bids and offers, given the imposition of minimal rationality by "budget constraints" (Gode and Sunder [28]).

Information: Markets generate information of many types that are not available from individual decision making environments. Individual parameters and actions are aggregated to produce market prices, sales volumes, etc. Depending upon the institution and the trading rules, participants may also see the bids, offers, and transactions of other agents.

Economic incentives: Most economists use financial incentives in their experiments while psychologists do so some of the time. There does not appear to be a consensus in psychology on the usefulness of monetary incentives (see, for example, Wright and Abdoul-Ezz [68]; Scott, Jr. et al. [57]; Irwin et al. [36]). Furthermore, when psychologists do use monetary incentives, their payoffs are often much lower than those typically used by economists. For example, the expected salient payoffs in the Tversky et al. [67] nonhypothetical preference reversal experiments were a small fraction of those in the Cox-Epstein [16] experiments. One can argue about the appropriate level of payoffs, and about the costs to the subjects of deviating from "optimal" behavior in particular experiments, but the fact is that the effect of economic incentives is an empirical question that can only be addressed with empirical methods. Therefore, in our experiments we vary the level of individual subjects' salient rewards from zero to full dollar value.

b. Response mode

Psychologists have theories about how the response mode affects subjects' decisions, and some of these theories have been used to explain the preference reversal phenomenon. For example, Slovic and Lichtenstein [59] used the "anchoring and adjustment" theory to explain preference reversals. According to this theory, a subject when asked to choose between two lotteries first anchors on the relative probabilities of winning and then makes an insufficient adjustment for differences in win state payoffs. Furthermore, a subject when asked to choose selling prices, is said to first anchor on the relative win state payoffs and then make an insufficient adjustment for differences in the probability of winning. This theory explained the asymmetric pattern of inconsistencies between choices and prices that was observed in many preference reversal experiments: subjects more commonly (a) placed a higher price on a $ bet and chose the paired P bet (committed a "predicted reversal") than they (b) placed a higher price on the P bet and chose the paired $ bet (committed an "unpredicted reversal").

A recent response mode explanation of preference reversals has been provided by Bostic, Herrnstein, and Luce [7]. They present evidence that the difference between the choice task and the judgement (i.e., pricing) task is a key factor in explaining the results of preference reversal experiments. In their experiments, certainty equivalents to the gambles are elicited in two different ways. One procedure, a variant of that used in Grether and Plott [31], required subjects to state the amount of money such that they were indifferent between it and the gamble. In the other procedure, subjects were asked to give their preference between the gamble and a fixed sum of money. If the subject preferred the gamble (money), the amount of money was increased (decreased) by $0.04 and the question repeated.The procedure was iterated until the preference changed. Bostic et al. reported that the frequency of preference reversals dropped substantially when the second procedure was used and that the asymmetry between the number of predicted and unpredicted reversals was eliminated.

We have given several reasons why anomalous results of individual choice experiments may not be robust to markets. Thus if we are to understand the implications of the preference reversal phenomenon for markets we need to test for it in market settings. Furthermore, to test the implications of the response mode explanation of preference reversals we need to identify economic institutions with different response modes that can produce preference reversals. The experiments reported in the following sections include both market and nonmarket decisions and market decisions with both pricing and choice response modes. In addition, they also include repetition and feedback (subjects are informed immediately of the results of their decisions) to establish if these are significant determinants of subjects' responses.

2. Experimental design

In the present paper, we vary the response mode in both nonmarket and market contexts. The nonmarket pricing task is implemented with the BDM mechanism. The market pricing task is the second price sealed bid auction. The nonmarket choice task is choosing the most preferred item from each of three pairs: {P bet, $ bet}, {P bet, $X}, and {$ bet, $X}, where $X is between the subjects' sales prices in a preceding pricing task. (In addition to eliciting choices between P bets and $ bets for comparison with selling prices, these choice questions can directly reveal intransitivities.) The market choice task is implemented with the English clock auction. In this auction, the price clock starts at the amount of the win state payoff in a bet and then decreases by five cents every second. Each subject must decide whether to choose to play the bet by exiting from the auction at the price showing on the clock or to remain in the auction. The last subject remaining in the auction receives the amount of money on the price clock when the next-to-the-last subject chose the bet. All of the other subjects play the bet.

We adopted the method of pairwise choice used by Tversky et al. [67], but in our experiments the amount $X was determined separately for each subject in order to ensure that it was between the stated certainty equivalents and therefore that all the data would be usable. We were able to do this because all subjects made their decisions at computer terminals and $X was set equal to midpoint between the certainty equivalents (rounded to the nearest multiple of five cents). The subjects were not informed of the procedure for calculating $X. Certainty equivalents were obtained in three different ways: the BDM mechanism, a second price sealed bid auction, and an English clock auction. We implemented the BDM procedure in the usual way. Subjects were given the right to play a gamble and asked to state their minimum selling prices. A random offer price was generated and those subjects with reservation prices below the offer price sold the gamble and the others retained their rights to play the gamble. In the sealed bid auction, subjects were given the right to play a gamble and were asked to submit bids giving the lowest price they would accept to give up the right to the gamble. The experimenter would buy the gamble from the lowest bidder at the second lowest price. During the clock auction, a box on the subject's screen displayed an amount of money which decreased by five cents every second. Subjects could choose to leave the auction if they preferred the right to play the gamble to the amount of money on the clock or they could choose to remain in the auction. The last person to leave the auction sold the right to play the gamble at the amount of money on the clock when the next to last person opted out of the auction by choosing to play the gamble. Whenever a subject chose to leave the clock auction, the other subjects were informed with both auditory ("beep beep") and visual signals and the number remaining was displayed. Note that, while both auctions are market mechanisms, the response mode in the clock auction consists of choices whereas the response mode in the sealed bid auction consists of stating prices.

Common practice in auction experiments is to run several periods with the same parameters to allow the prices to converge to equilibrium. We wished to conform to practice, but also wanted the conditions to be as nearly as possible the same across experimental sessions, so we repeated each auction five times. The BDM mechanism was not repeated in our basic design, though we did run four sessions in which it also was repeated five times. Our reason for treating the market and BDM mechanisms differently was that we wished to implement each of them in a way similar to that commonly found in the literature.

In each experimental session, subjects were presented with two pairs of gambles, each consisting of one P bet and one $ bet. These gambles were used by Lichtenstein and Slovic [42] and by most researchers since. Certainty equivalents for one of the pairs were obtained using the BDM mechanism and for the other pair an auction mechanism was used. Varying the order of the auction and BDM mechanism, the two bet pairs and the auctions provides a basic $2 \times 2 \times 2$ design. Applying the three payment schedules yields 24 cells. After completing the 24 cell design we added four sessions (switching the bet pairs and the order of presentation) using repeated BDM and sealed bid auctions.

The bet pairs used in this study were the following:

P bet 1: 35 chances to win \$4.00;	1 chance to lose \$1.00;	expectation \$3.86
\$ bet 1: 11 chances to win \$16.00	25 chances to lose \$1.50;	expectation \$3.85
P bet 2: 29 chances to win \$2.00;	7 chances to lose \$1.00;	expectation \$1.42
\$ bet 2: 7 chances to win \$9.00;	29 chances to lose \$0.50;	expectation \$1.35

Notice that for bet pair 1 the bad outcome from the $ bet is worse than that from the P bet while the opposite is true for bet pair 2. Thus one pair satisfies the conditions of Loomes, Starmer and Sugden [46] while the other does not.

3. Procedures

All experiments were conducted in the Economic Science Laboratory at the University of Arizona. The subjects, all of whom were students at the University of Arizona, participated in groups of five. Each subject was seated at a separate computer terminal on which the instructions were displayed. Subjects could page through the instructions at their own paces. Decision problems, outcomes, and subjects' accumulated earnings were all displayed on computer screens.

Random outcomes were determined by drawing balls from bingo cages. Subjects were asked to inspect the balls. Subjects could observe the balls being placed in the cages, the draws, and the outcomes. Random prices for the BDM mechanism were generated by three draws (with replacement) from a cage with ten balls numbered 0 through 9. Outcomes of gambles, all of which had probabilities stated in 36ths, were determined by drawing from another bingo cage loaded with 36 balls numbered 1 through 36.

After each decision, the random outcomes were observed, the amounts of money won or lost were determined, and the results displayed on the subjects' computer screens. Three payment schedules were employed: full payment; payment equal to one-half of the total earnings; and payment of $10 independent of decisions and outcomes of the gambles. No money was actually paid until the end of the sessions, but subjects knew the results of their decisions, including accumulated earnings, as the experiment proceeded.

Subjects were paid in cash at the end of the experiment. Those subjects in sessions with full payment were simply paid their total earnings. Before subjects entered the laboratory for sessions with fifty percent payment or with the fixed $10 payment, written notices were placed on the tables beside their keyboards. For those receiving one-half the amount won, the notice stated: "The actual amount of money you will receive from today's experiment will be one half of the amount of money displayed on your computer screen." Subjects whose payment did not depend upon their decisions were given the notice: "Your total payment for participating in this experiment will be $10. You will not be paid the amounts that appear on your computer screen. However, you are asked to make the same decisions that you would make if you were going to win or lose the amounts of money that appear on your computer screen." After the subjects had finished the instructions on their computer screens, they were asked whether they had read the payment notice. When they all indicated they had read the notice, the experiment began. The experimenter did *not* read the notice aloud to the subjects, nor make any reference to it other than asking if the subjects had read it. This procedure was intended to minimize the possibility of unintended communication of the economist experimenter's expectation that the payoff rate might affect behavior.

4. Results

a. *Preference reversals*

Table 1 reports the number of predicted reversals (PR) and the number of unpredicted reversals (UR) for 28 experimental sessions. Proportionate rates of occurrence (Rate) are reported for both PR and UR. Table 1 also reports means and standard deviations (in parentheses) of the reversals in nominal values for each payment schedule. Summary statistics for the prices are given in Table 2.

Consider the first repetition results for the BDM mechanism (BDM 1) in Table 1. Observe that the basic preference reversal phenomenon has been replicated. Subjects with full financial incentives (CR1) and with 50 percent financial incentives (CR.5) together made 100 choices between P bets and $ bets which resulted in 35 predicted and 4 unpredicted reversals. More subjects chose the P bets (57 to 43), but even allowing for this the rate of predicted reversals is much higher than the rate of unpredicted reversals (61 percent to 9 percent). The results are substantially the same for the subjects who were paid a fixed fee (CR0). Of their 40 choices, 17 resulted in preference reversals of which 15 were of the predicted type. Overall, the reversal rate with the BDM mechanism was just under 61 percent for those choosing the P bets and about ten percent for choices of $ bets. As stated earlier, in four sessions the

Table 1: Frequencies of outcomes of choices; means and standard deviations of reversals

	PR	Rate	Mean	UR	Rate	Mean	PR	Rate	Mean	UR	Rate	Mean
	BDM1						BDM5					
CR1	11	0.458	2.24 (1.59)		0.0625	0.50						
CR.5	24	0.727	2.51 (1.81)	3	0.111	0.50 (0.45)	4	0.400	3.49 (2.37)	0	0.000	
Total	35	0.614	2.43 (1.73)	4	0.093	0.50 (0.37)	4	0.400	3.49 (2.37)	0	0.000	
CR0	15	0.600	2.20 (1.67)	2	0.133	1.23 (0.39)						
Total	50	0.610	2.36 (1.70)	6	0.103	0.74 (0.50)	4	0.400	3.49 (2.37)	0	0.000	
	SPA1						SPA5					
CR1	11	0.733	2.84 (2.29)	0	0.000		4	0.267	3.25 (2.86)	1	0.200	2.40
CR.5	16	0.762	3.72 (3.16)	2	0.105	0.60 (0.42)	6	0.286	2.27 (1.43)	7	0.368	1.69 (0.15)
Total	27	0.750	3.36 (2.82)	2	0.083	0.60 (0.42)	10	0.278	2.66 (2.03)	8	0.333	1.78 (1.27)
CR0	4	0.364	2.54 (1.72)	4	0.444	1.35 (0.47)	0	0.000		3	0.333	1.47 (1.08)
Total	31	0.660	3.26 (2.70)	6	0.182	1.10 (0.57)	10	0.213	2.66 (2.03)	11	0.333	1.70 (1.17)
	ECA1						ECA5					
CR1	7	0.467	6.01 (4.25)	4	0.800	0.34 (0.38)	6	0.400	3.50 (2.20)	3	0.600	0.78 (0.47)
CR.5	11	0.786	3.45 (4.67)	4	0.667	0.70 (0.35)	3	0.214	4.85 (5.82)	4	0.667	0.92 (1.23)
Total	18	0.621	4.44 (4.56)	8	0.727	0.51 (0.39)	9	0.310	3.95 (3.46)	7	0.636	0.86 (0.92)
CR0	13	0.929	6.73 (3.53)		0.167	2.20	10	0.714	5.67 (3.81)	1	0.167	0.95
Total	31	0.721	5.40 (4.25)	9	0.529	0.71 (0.67)	19	0.442	4.86 (3.65)	8	0.471	0.88 (0.85)

Key: CR1 conversion rate = 1.0; CR.5 conversion rate = 0.5; CR0 conversion rate = 0.0; PR = predicted reversal; UR = unpredicted reversal; BDM_i = Becker-DeGroot-Marshak repetition i = 1,5; SPA_i = second price auction repetition i = 1,5; ECA_i = English Clock Auction repetition i = 1,5; (Figures in parentheses are [illegible] standard deviations).

Table 2: Means and standard deviations of bids by institution and payoff

	7 chances \$9.00 29 chances \$−0.50			29 chances \$2.00 7 chances \$−1.00			11 chances \$16.00 25 chances \$−1.50			35 chances \$4.00 1 chances \$−1.00		
	N	Mean	Std. deviation	N	Mean	Std. deviation	N	Mean	Std. deviation	N	Mean	Std. deviation
						Second price auction						
CR1	50	3.38	2.57	50	1.51	0.44	50	4.18	4.33	50	3.02	1.27
CR.5	100	3.66	2.81	100	1.72	0.28	100	4.16	2.95	100	3.48	0.95
CR0	50	1.48	1.72	50	1.02	0.72	50	2.02	1.50	50	3.06	1.28
CR1 & .5	150	3.57	2.73	150	1.65	0.35	150	4.16	3.46	150	3.33	1.08
All	200	3.04	2.67	200	1.49	0.54	200	3.62	3.22	200	3.25	1.14
						English clock auction						
CR1	50	2.00	2.57	50	1.56	0.29	50	6.25	4.44	50	3.97	0.09
CR.5	50	2.76	2.34	50	1.76	0.20	50	4.64	3.96	50	3.79	0.32
CR0	50	5.65	2.18	50	1.67	0.32	50	8.12	5.68	50	3.82	0.29
CR1 & .5	100	2.38	2.48	100	1.66	0.26	100	5.44	4.26	100	3.88	0.26
All	150	3.47	2.84	150	1.67	0.28	150	6.34	4.93	150	3.86	0.27
						Becker-DeGroot-Marshak						
CR1	20	3.43	2.40	20	1.45	0.47	20	3.94	1.61	20	3.52	0.65
CR.5	70	3.60	2.37	70	1.37	0.69	70	6.44	4.22	70	3.53	1.01
CR0	20	2.42	2.09	20	1.49	0.45	20	5.55	4.02	20	3.23	1.11
CR1 & .5	90	3.56	2.38	90	1.39	0.65	90	5.88	3.93	90	3.53	0.94
All	110	3.36	2.35	110	1.40	0.61	110	5.82	3.93	110	3.47	0.97

Key: CR1 conversion rate = 1.00; CR.5 conversion rate = 0.50; CR0 conversion rate = 0.0

BDM mechanism was repeated five times for both gambles in the pair. The results are reported under BDM 5 in Table 1. If we compare the preference (which was always obtained after the last BDM repetition) with the ordering of the fifth BDM prices, we find a substantially lower rate of reversals (4 predicted and no unpredicted reversals out of 20 choices). The proportion of subjects that chose the P bet, and the rates of predicted and unpredicted reversals, were roughly the same for the two pairs of bets. This suggests that the argument presented by Loomes, Starmer and Sugden [46] is not the source of the reversals in these experiments because only one of the pairs satisfies the conditions for their argument to apply.

Not only are predicted reversals frequent, but they involve significant amounts of money. Consider the Mean column for BDM1 in Table 1. Reversals for subjects being paid full value averaged $2.24 and the corresponding amounts for subjects receiving half earnings or a flat fee were $2.51 and $2.20 respectively. Unpredicted reversals were not only less frequent but of smaller magnitude as well. The overall mean unpredicted reversal was $0.74 with the mean being smaller than the mean of the predicted reversals for each group, though the sample sizes are small.

Mean bids with the BDM mechanism were above the expected values for both of the $ bets and generally quite close to the expected values for the P bets. The overall mean for P bet 2 was within two cents of the expected value. Not surprisingly, the distributions of the bids for the P bets were more concentrated than those for the $ bets.

Turning to the second price auction, we see a similar pattern of results for the first repetition. Consider the SPA1 results in Table 1. Comparing the subjects' choices with their first stated prices (the first and sixth of the ten prices obtained) we find that, for the subjects paid salient rewards, 27 of the 36 choices of the P bets resulted in preference reversals while only 2 of the 24 choices of the $ bets did so. Subjects paid a fixed fee made 4 reversals of each type (of 11 choices of the P bets and 9 of the $ bets). Now consider the SPA5 results in Table 1. Comparing the choices with the fifth prices, the number of reversals drops and the rates of the two types of reversals are nearly the same for the subjects with financial incentives (10 out of 36 P bet choices and 8 of the 24 $ bet choices). The subjects without financial incentives committed only three reversals, all of the unpredicted type.

The distributions of the bids in the second price auction show results which are similar to those obtained with the BDM mechanism. Bids for the $ bets tend to be above the expected values while the bids for P bet 1 are on average below the expected value, and bids for P bet 2 are on average quite close to the expected value. One difference that does emerge is that in the second price auctions the subjects without financial incentives had the lowest average bids on all four gambles.

The third institution studied, the English clock auction, yielded results which strikingly illustrate the effects of financial incentives. The results are also suggestive of institutional differences between this mechanism and the other two but at this time we cannot make a definitive statement on this issue as there simply are not enough data. Consider the ECA1 results in Table 1. Looking at first prices for subjects with monetary incentives, we find 18 reversals from the 29 choices of the P bets and 8 reversals from 11 choices of the $ bets. The number of predicted reversals drops to 9 if we use the fifth set of prices while the number of unpredicted reversals is nearly constant (dropping from 8 to 7). Subjects without financial incentives behaved differently. Of the 14 choices of the P bets, the number of preference reversals based upon the first prices is 13 and this number drops only to 10 if we consider the final prices. There was one unpredicted reversal of the 6 choices of the \$ bet. Note that, for the \$ bets, the bids are substantially higher (and significantly so) for the group without incentives (\$5.65 snd \$8.12 compared with \$2.38 and \$5.44 respectively). Reversals tell the same story, the mean predicted reversal being \$6.73 for the no monetary incentive group compared with \$4.44 for the other group (first prices). The corresponding figures for fifth prices are \$5.67 and \$3.95.

If one considers how the clock auction proceeds, the effects of financial incentives seem intuitive. In the clock auction, the higher the bid the sooner the subject drops out and the less time and effort spent watching the computer screen. This is especially the case for the \$ bets with their high win state payoffs and thus high starting clock prices. In all cases the clock is started at the maximum payoff; subjects can spend less time watching the computer screen by pressing the key for choosing the bet and dropping out early. When no money was at stake it appears that this is what some of them did.

Restricting attention to the subjects with financial incentives reported in Table 1, note that subjects in the clock auction had higher rates of unpredicted reversals than subjects in the other institutions. With both sets of prices, the reversal rates were higher with the clock auction and the asymmetry between the predicted and unpredicted rates does not appear. Indeed, for the clock auction the reversal rate is higher for subjects choosing the \$ bet. Given the small sample sizes we do not wish to push this point too far but it appears that, as hypothesized, the task in the clock auction is to the subjects more of a choice task than a pricing task.

The discussion of the clock auctions shows the danger of comparing institutions without the use of monetary incentives. The conclusions that one would be tempted to draw comparing the sealed bid and clock auctions are opposites depending upon whether one uses data from experiments with or without monetary payoffs. The incentives in these experiments were not trivial. Payments to subjects paid the full amount averaged \$58.82, with the lowest being \$19.50 and the highest being \$109. Those on the fifty percent schedule in experiments without repetition of BDM earned an averate of \$35.72, with individual subject payments ranging from \$18.50 to \$63.75. Subjects in the 50 percent payoff rate experiments *with* repetition of BDM earned, on average, \$56.42, with a range from \$38.25 to \$96.50. The experiments lasted between 1 and $1\frac{1}{2}$ hours, including the instruction period.

From the data shown in Tables 1 and 2 we conclude that the preference reversal phenomenon has been replicated. We also conclude that preference reversals occur in non-repetitive market environments. In *repetitive* market environments, the rate of preference reversals is much lower and the asymmetry disappears. There is some preliminary indication that choice-based institutions may not exhibit the phenomenon.

Turning to the reversal or intransitivity rate from pair wise choice, we basically replicate the results of Tversky et al. [67]. Of the 200 sets of choices made by subjects with financial incentives, 21 of them resulted in intransitivities. The rate of intransitivity was somewhat higher (13 out of 80) for subjects without financial incentives but the difference is not statistically signfiicant. We conclude that, in our experiments with individually chosen $X amounts, we have replicated the Tversky et al. result of approximately ten percent intransitivity and that the result is independent of the monetary payment schedule.

In addition to the 28 cells in our design, we ran five sessions the data from which are not included in the tables. The first and second attempts to run a clock auction were terminated by software failure. The third clock auction was completed, but the clock always started at $4.10, substantially below the maximum payoffs for the $ bets. Two other sessions with BDM and the sealed bid auction were also discarded. These were the first and second sessions without monetary incentives. At the end of the second session, two of the five subjects seemed surprised that they did not receive the amounts shown on their computer screens and one of them seemed quite angry about it. In both sessions, subjects were given written notices stating: "You will be paid $10 for participation in this experiment. However, in making your decisions you are asked to pretend that you will win or lose the amounts of money that appear on your computer screen." They had been asked whether they had read the notice before the experiments began. Apparently, two of the subjects interpreted the notice to mean that they would receive $10 extra. We discarded the data from these two sessions, and reworded the notice to the one stated in section 3.

b. Determinants of choices and prices

Table 3 gives the results of logit estimation of two equations explaining the choice of the safer versus the riskier alternative. The set of gambles used in the experiments is not very rich so one should be careful not to over interpret the results. All choices were either between a P bet and a $ bet or between one gamble and a fixed amount of money. We have estimated separate equations for each payment schedule, for the subjects with financial incentives and for all subjects. The reader can thus judge from the log likelihood statistics whether the equations are the same for the different groups. Both equations include constant terms (generally insignificant except for the group without incentives, for which it is negative, implying a preference for risk), cumulative winnings (never significant) and a dummy for when the safer bet is a P bet (insignificant). One equation includes the expected values of the two gambles and the other contains their difference. In the unconstrained equations, both variables are statistically significant with coefficients that are of approximately the same magnitude and of opposite sign. The sum of the two coefficients is not significantly different from zero for the group with financial incentives but the hypothesis is rejected at the five percent level (though not at the one percent level) for those without monetary incentives.

The results of fitting statistical models to the bids are shown in Table 4. The models are estimated separately for each payment schedule, for the positive conversion rate groups and for all the data combined. The two basic models estimated are a static model and a dynamic adjustment model. For the static model, it is assumed that subjects determine their bids based upon the characteristics of the lottery being sold, their cumulative earnings in the experiment, and the institutions. The variables included are a constant term, the expected value of the lottery,

Table 3: Maximum likelihood logit estimates

Conversion rate *Variable*	1.0		0.5		0.0		1.0 & 0.5		All data	
constant	−0.28	0.13	0.19	0.10	−1.13	−0.49	−0.03	0.08	−0.33	−0.08
	(0.7)	(0.6)	(0.6)	(0.6)	(2.9)	(2.1)	(0.1)	(0.6)	(1.7)	(0.7)
expects	0.89		0.72		0.68		0.79		0.75	
	(5.4)		(5.6)		(5.1)		(7.7)		(9.2)	
expectr	−0.74		−0.76		−0.44		−0.75		−0.65	
	(4.2)		(5.2)		(2.8)		(6.7)		(7.2)	
sumwin	−0.01	−0.01	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
	(1.3)	(1.4)	(0.5)	(0.5)	(1.1)	(1.0)	(0.2)	(0.2)	(0.5)	(0.4)
pbets	0.32	0.31	0.21	0.21	0.14	0.15	0.00	0.00	0.04	0.04
	(1.5)	(1.5)	(1.2)	(1.2)	(0.6)	(0.7)	(0.1)	(0.0)	(0.3)	(0.3)
diff		0.84		0.73		0.63		0.78		0.73
		(5.3)		(5.8)		(5.0)		(7.8)		(9.2)
n	400	400	600	600	400	400	1000	1000	1400	1400
−*ln L*	254.2	255.1	384.5	384.5	253.4	255.6	642.5	642.7	901.5	902.8
% correct	63.3	62.3	59.8	60.0	68.0	62.8	59.9	58.6	64.9	62.9
Variable = 1 % Dependent	52.8	52.8	54.8	54.8	46.3	46.3	54.0	54.0	51.8	51.8

Dependent variable = 1 if chose gamble with greatest chance of winning, = 0 otherwise; expects = expected value of less risky gamble; expectr = expected value of more risky gamble; pbets = 1 if safer gamble is a pbet; diff = expects − expectr; sumwim = cumulative winnings through previous round. (Figures in parentheses are *t*-ratios)

Table 4: Maximum likelihood estimates tobit model with varying cutoffs dependent variable is bid

Conversion rate	1.0							
Repetition	1st	1st	All	All	2nd–5th	2nd–5th	2nd–5th	2nd–5th
Variable								
constant	1.73	1.59	1.71	1.64	1.38	1.36	1.03	1.23
	(2.9)	(2.1)	(4.1)	(3.3)	(3.3)	(2.5)	(3.0)	(2.8)
w		0.86		0.81		0.79		−0.01
		(4.6)		(7.1)		(5.5)		(0.1)
		0.56		0.68		0.75		0.43
		(0.5)		(1.0)		(0.9)		(0.7)
w+1	0.89		0.82		0.79		−0.05	
	(5.5)		(8.4)		(6.5)		(0.4)	
pbet	−2.03	−1.84	−1.46	−1.38	−1.15	−1.13	−0.57	−0.83
	(5.2)	(2.6)	(6.0)	(3.1)	(3.9)	(2.1)	(2.3)	(2.0)
sumwin	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
	(0.2)	(0.2)	(0.4)	(0.4)	(0.5)	(0.5)	(0.0)	(0.1)
eca	0.47	0.47	0.04	0.05	0.22	0.22	−0.42	−0.42
	(1.0)	(1.0)	(0.1)	(0.1)	(0.7)	(0.7)	(1.7)	(1.7)
spa	0.74	0.74	−0.07	−0.07				
	(1.6)	(1.6)	(0.2)	(0.2)				
lag bid							0.40	0.40
							(7.0)	(7.0)
lag price							0.40	0.40
							(3.8)	(3.9)
lag win							0.15	0.14
							(3.6)	(3.5)
n	160	160	480	480	320	320	320	320
$\hat{\sigma}^2$	6.09	6.09	6.76	6.76	6.90	6.90	4.02	4.01
−ln L	360.53	360.48	1080.65	1080.63	714.89	714.89	635.15	634.86

Conversion rate					0.5					
Repetition	1st	1st	All	All	2nd–5th	2nd–5th	2nd–5th	2nd–5th	2nd–5th auctions only	2nd–5th
Variable										
constant	2.86	2.65	2.71	2.98	2.63	3.07	0.52	0.79	0.42	0.80
	(6.1)	(4.5)	(10.3)	(9.1)	(8.0)	(7.8)	(1.8)	(2.4)	(1.8)	(2.6)
w		0.77		0.77		0.76		0.23		0.23
		(5.3)		(10.0)		(8.7)		(3.6)		(2.8)
		0.34		1.33		1.69		0.74		0.94
		(0.4)		(3.0)		(3.3)		(2.1)		(2.2)
w+1	0.82		0.71		0.67		0.18		0.15	
	(6.5)		(10.9)		(8.9)		(3.2)		(2.1)	
pbet	−2.55	−2.28	−1.70	−2.04	−1.38	−1.95	−0.07	−0.39	−0.05	−0.48
	(8.3)	(4.0)	(10.5)	(6.9)	(7.4)	(5.7)	(0.5)	(1.6)	(0.3)	(1.7)
sumwin	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
	(0.2)	(0.2)	(0.1)	(0.2)	(0.2)	(0.1)	(0.6)	(0.6)	(0.9)	(0.8)
eca	−0.20	−0.21	−0.75	−0.75	−0.88	−0.88	−0.41	−0.42	−0.11	−0.11
	(0.5)	(0.5)	(3.3)	(3.3)	(3.1)	(3.2)	(1.9)	(1.9)	(0.7)	(0.7)
spa	0.12	0.12	−0.46	−0.46	−0.61	−0.61	−0.30	−0.31		
	(0.4)	(0.4)	(2.5)	(2.5)	(2.6)	(2.6)	(1.5)	(1.6)		
lag bid							0.73	0.73	0.59	0.59
							(26.7)	(26.7)	(16.2)	(16.3)
lag price							−0.02	−0.03	0.10	0.07
							(0.5)	(0.6)	(1.4)	(0.8)
lag win							0.01	0.01	0.01	0.01
							(0.4)	(0.5)	(0.4)	(0.6)
n	240	240	880	880	640	640	640	640	480	480
$\hat{\sigma}^2$	5.65	5.65	5.67	5.65	5.49	5.46	2.47	2.46	2.54	2.52
−ln L	538.03	537.87	1953.04	1952.08	1405.19	1403.21	1160.65	1159.35	872.37	870.66

Table 4 (*continued*)

Conversion rate	1.0 and 0.5									
Repetition	1st	1st	All	All	2nd–5th	2nd–5th	2nd–5th	2nd–5th	2nd–5th auctions only	2nd–5th
Variable										
constant	2.38	2.19	2.39	2.54	2.49	2.78	0.53	0.79	0.51	0.78
	(6.4)	(4.7)	(10.7)	(9.2)	(8.2)	(7.7)	(2.0)	(2.6)	(2.6)	(3.1)
w		0.81		0.78		0.77		0.22		0.14
		(7.0)		(12.2)		(10.2)		(3.7)		(2.1)
		0.42		1.09		1.37		0.76		0.70
		(0.6)		(2.9)		(3.1)		(2.4)		(1.9)
w+1	0.85		0.75		0.71		0.17		0.09	
	(8.5)		(13.8)		(11.0)		(3.2)		(1.4)	
pbet	−2.35	−2.11	−1.62	−1.81	−1.31	−1.68	−0.17	−0.51	−0.20	−0.54
	(9.6)	(4.7)	(12.0)	(7.3)	(8.2)	(5.7)	(1.4)	(2.3)	(1.4)	(2.2)
sumwin	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
	(0.2)	(0.2)	(0.3)	(0.3)	(0.2)	(0.2)	(0.4)	(0.3)	(0.7)	(0.6)
eca	0.08	0.08	−0.52	−0.52	−0.79	0.79	−0.27	−0.27	−0.15	−0.15
	(0.2)	(0.3)	(2.8)	(2.8)	(3.1)	(3.2)	(1.3)	(1.3)	(1.1)	(1.1)
spa	0.37	0.37	−0.40	−0.40	−0.71	−0.71	−0.18	−0.18		
	(1.3)	(1.3)	(2.4)	(2.4)	(3.1)	(3.1)	(0.9)	(0.9)		
lag bid							0.65	0.65	0.51	0.51
							(25.8)	(25.9)	(16.2)	(16.2)
lag price							0.02	0.02	0.24	0.23
							(0.5)	(0.5)	(4.1)	(3.9)
lag win							0.04	0.04	0.04	0.04
							(2.2)	(2.3)	(2.5)	(2.5)
n	400	400	1360	1360	960	960	960	960	800	800
$\hat{\sigma}^2$	5.89	5.89	6.08	6.08	5.97	5.95	3.17	3.16	3.23	3.21
ln L	900.81	900.60	3039.75	3039.32	2124.43	2123.28	1836.51	1834.79	1529.49	1528.01

(*continued*)

Conversion rate				0.0				
Repetition	1st	1st	All	All	2nd–5th	2nd–5th	2nd–5th	2nd–5th
Variable								
constant	2.22	1.66	2.11	2.23	0.48	0.97	−0.15	−0.06
	(3.4)	(2.1)	(4.7)	(4.2)	(1.0)	(1.7)	(0.4)	(0.1)
w		0.75		0.80		0.81		0.29
		(3.8)		(6.7)		(5.6)		(2.0)
l		−0.48		1.07		1.91		0.48
		(0.4)		(1.5)		(2.2)		(0.6)
w+l	0.87		0.77		0.71		0.27	
	(5.1)		(7.6)		(5.6)		(2.2)	
pbet	−2.38	−1.62	−1.88	−2.04	−1.60	−2.27	−0.47	−0.60
	(5.8)	(2.1)	(7.5)	(4.4)	(5.1)	(4.0)	(1.7)	(1.2)
sumwin	−0.01	−0.01	0.00	0.00	0.01	0.01	0.00	0.00
	(0.7)	(0.8)	(0.3)	(0.3)	(0.7)	(0.8)	(0.6)	(0.7)
eca	1.93	1.93	1.38	1.38	2.70	2.71	0.85	0.87
	(3.8)	(3.8)	(3.8)	(3.8)	(8.6)	(8.7)	(2.6)	(2.6)
spa	−0.71	−0.71	−1.30	−1.30				
	(1.4)	(1.4)	(3.6)	(3.6)				
lag bid							0.51	0.51
							(8.7)	(8.6)
lag price							0.21	0.19
							(1.9)	(1.7)
lag win							0.00	0.00
							(0.0)	(0.0)
n	160	160	480	480	320	320	320	320
$\hat{\sigma}^2$	6.77	6.71	7.39	7.39	7.53	7.47	5.10	5.10
−ln L	369.96	369.24	1095.72	1095.63	721.29	720.31	664.99	664.95

(continued)

Table 4 (*continued*)

	All Data									
Repetition	1st	1st	All	All	2nd–5th	2nd–5th	2nd–5th	2nd–5th	2nd–5th auctions only	2nd–5th
Variable										
constant	2.29	2.00	2.31	2.45	2.44	2.77	0.32	0.57	0.28	0.47
	(7.0)	(4.9)	(11.2)	(9.7)	(8.1)	(8.0)	(1.2)	(1.9)	(1.6)	(2.1)
w		0.80		0.79		0.78		0.23		0.15
		(7.8)		(13.6)		(11.3)		(4.0)		(2.4)
		0.18		1.09		1.48		0.72		0.55
		(0.3)		(3.2)		(3.7)		(2.4)		(1.6)
w+1	0.86		0.76		0.71		0.18		0.11	
	(9.7)		(15.3)		(12.0)		(3.6)		(2.0)	
pbet	−2.35	−1.98	−1.68	−1.87	−1.38	−1.82	−0.22	−0.52	−0.23	−0.47
	(11.0)	(5.0)	(13.7)	(8.3)	(9.4)	(6.8)	(1.9)	(2.5)	(1.8)	(2.1)
sumwin	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
	(0.0)	(0.0)	(0.6)	(0.6)	(0.8)	(0.8)	(0.8)	(0.8)	(1.0)	(1.0)
eca	0.67	0.69	0.05	0.05	−0.27	−0.27	0.02	0.01	0.09	(0.10)
	(2.4)	(2.4)	(0.3)	(0.3)	(1.1)	(1.1)	(0.1)	(0.0)	(0.7)	(0.8)
spa	0.12	0.12	−0.65	−0.65	−1.00	−1.00	−0.12	−0.14		
	(0.5)	(0.5)	(4.2)	(4.2)	(4.2)	(4.2)	(0.6)	(0.7)		
lag bid							0.63	0.63	0.52	0.53
							(26.8)	(26.9)	(18.6)	(18.7)
lag price							0.8	0.07	0.26	0.25
							(2.1)	(1.9)	(5.1)	(4.8)
lag win							0.02	0.02	0.03	0.03
							(1.5)	(1.5)	(1.9)	(1.9)
n	560	560	1840	1840	1280	1280	1280	1280	1120	
$\hat{\sigma}^2$	6.37	6.36	6.74	6.74	6.71	6.69	3.70	3.69	3.81	
−ln L	1281.45	1280.79	4181.86	4181.37	2882.36	2880.51	2526.04	2524.42	2214.68	

Key: al = amount if lose (in dollars); aw = amount if win (in dollars); pw = the number of chances to win; w = aw·pw/36; l = al(1 − pw)/36; sum win = cumulative winnings through previous round; lag win = amount won on previous repetition of this task; lag price = market price on previous repetition of this task; pbet = 1 if gamble is a pbet, 0 otherwise; spa = 1 if second price auction, 0 otherwise; eca = 1 if English clock auction, 0 otherwise; lag bid = amount bid on previous repetition of this task (Figures in parentheses are t-ratios) cumulative earnings, a binary variable for the clock auction, and a binary indicator for the second price auction. For an alternative specification, the expected value was split into its positive and negative terms and both parts entered as explanatory variables.

For the dynamic adjustment model we add three new explanatory variables: the previous value of the bid, the previous market price, and the amount won on the prior repetition of the task. The idea is that, to the extent that the variables introduced into the static model are incorporated in the previous bid, the new information available consists of the market price and the subjects' experience with this task. We expect all three variables to be significant, with positive coefficients.

In implementing the clock auction, subjects were informed when a participant dropped out and told how many remained. This means, as noted before, that the last person to drop out knows that he or she will sell the gamble and knows the price. Thus the last (lowest) price may not be a meaningful number. In least squares regressions, we simply used those prices assuming them to be correct. To delete them could cause biases as we would be deleting the smallest prices in certain groups of five, thus sampling on the dependent variable. Strictly speaking, all we know about the prices in question is that they are less than the next-to-the-lowest prices.

In the Tobit model, some observations are completely observed, and for some observations the explanatory variables are observed while the dependent variable is below some generally unknown threshold. The situation we have here is similar to that for which the Tobit model is appropriate but in our case the thresholds are known and vary across observations. The equations in Table 4 were estimated by ordinary least squares and by maximum likelihood using a generalization of the Tobit model which allows the thresholds to vary. The results of the maximum likelihood estimation are shown in Table 4. The estimated coefficients for the clock auction dummy variables shown in Table 4 are all lower than the corresponding estimates from the least squares regressions. The estimated coefficients and t-ratios for the other variables are nearly identical for the two estimation procedures, hence we present only one set of estimates. The similarity is not surprising as the number of observations affected (one fifth of the clock auction observations) is a small fraction of the total.

As only the static model is available on the first repetition, we fit it to data for repetition one, repetitions two through five, and to all repetitions to allow for tests of stability. The results are sensible and consistent with the estimates on choices discussed previously. The coefficient of the P bet variable is significant and negative. The coefficient of the expected value is highly significant, positive, and takes on sensible values. The cumulative winnings variable is never significant. The institutional dummies are generally insignificant for the first repetition, and significantly negative for subsequent repetitions, suggesting that after the first repetition the bids from both auctions are lower, *ceteris paribus*, than those from the BDM mechanism. The striking exception is that the clock auction coefficient is significantly positive and large (on the order of \$2 to \$3) for all repetitions for subjects without financial incentives. The hypothesis that it is the expected value of the gamble, rather than its positive and negative parts, that influences bids is not rejected.

The dynamic adjustment model is estimated using all data from the second through the fifth repetition and for subjects on the fifty percent payment schedule we reestimated the model using data on the two auctions only. Auction market prices provide information about other bids, but BDM "prices" are simply the result of random draws from a bingo cage. For the BDM mechanism, the price may provide information about the randomizing device, but it provides no information about the value of the gamble, thus we would expect the lagged price variable to be more significant when only the auction data are used.

The results for the dynamic adjustment model shown in the last two to four columns of each panel for Table 4 are encouraging and in agreement with our expectations. The coefficients of the three lagged variables are positive and generally significant (the exception is for the fifty percent group). Furthermore, dropping the BDM observations (for the fifty percent payment and combined fifty percent and one hundred percent groups) increases the magnitude of the coefficients and t-ratios for the lagged market price. Including the lagged dependent variable reduces the magnitudes and t-ratios for the variables included in the static model, which makes sense as their influence should be largely incorporated in the previous bid. The significance of the lagged market price variable is especially noteworthy as this indicates that subjects do alter their bids based upon the market price which carries information about the bids of other market participants. This suggests that the extra information available in market (as distinct from individual choice) experiments is used by the subjects, which could account for some of the typical differences between results from the two types of experiments. In no case was the cumulative earnings variable close to being statistically or economically significant, indicating that the immediate crediting of winnings to subjects does not yield significant wealth effects, but the significance of the lagged win variable (the amount won or lost on the previous repetition of the task) suggests that subjects do respond to feedback (and possibly repetition).

In summary, the story from the regressions is that subjects in our experiments based their bids upon the expected values, taking into account previous market prices (in auctions), and adjusted their bids down for P bets. On average, subjects bid more with the BDM mechanism, except those without monetary incentives who bid most when participating in the clock auction. We note that the explanatory variables account for about 20 to 40 percent of the variance, so the individual characteristics, learning patterns and other unmeasured factors account for the majority of the variation in the bids. Since our experiments included only four (two-outcome) gambles, one should be careful in interpreting our results or in extending them to other contexts.

5. Summary and conclusions

The preference reversal phenomenon violates the consistency properties of economic theories of decision making under uncertainty. Asymmetry of observed reversals is even more problematic for economics than is symmetric inconsistency. The reason is that symmetric inconsistency could be interpreted as resulting from mistakes or could be accommodated by introducing an unbiased random element into choices. In contrast, asymmetric preference reversals provide support for psychological theories that the choice response mode can elicit different preferences than the valuation response mode. Dependence of revealed preferences on the response mode could pose a serious challenge to the usefulness of accepted economic models as a *positive* theory of market behavior if the phenomenon is robust to market choices and valuations.

Preference reversals and other anomalies observed in individual choice experiments clearly have implications for economics (Slovic and Lichtenstein [59]). Preference reversals are one of several types of systematic violations of expected utility theory that are commonly observed in individual choice experiments (see Machina [49] and Camerer [12], for surveys). Observations from individual choice experiments imply that people making nonrepetitive choices and judgements in nonmarket contexts frequently violate expected utility theory (Grether and Plott [31]) and its generalizations (Cox and Epstein [16]). Preference reversals and other anomalies *may* imply that accepted economic models are fundamentally flawed as a *positive* theory of market behavior. There is, however, a large literature on market experiments that has found results that are generally consistent with the market allocation implications of rational choice theory (Plott [52]; Smith [62, 63]; Cox, Smith and Walker [20]).[2]

In our experiments we observed the preference reversal phenomenon on the first repetition in a market setting (second price auction) with immediate feedback, both with and without financial incentives. However, after five repetitions of the auction, the subjects' bids were generally consistent with their choices and the asymmetry between the rates of predicted and unpredicted reversals had disappeared.

The pairs of gambles used were a subset of those original used by Lichtenstein and Slovic [42]. The same subjects also provided valuations elicited by the traditional BDM mechanism (without repetition). Their responses replicated the usual findings: those who chose the P bets committed preference reversals about sixty percent of the time while those who chose the $ bets committed reversals at a ten percent rate. A subset (20) of the subjects participated in five repetitions of the BDM mechanism; the rates of predicted and unpredicted reversals after the fifth repetition were 0.4 and 0.0 respectively, which suggests that the phenomenon may persist at a lower rate but the sample size is so small that we reserve judgement on this. Our subjects replicate the preference reversal phenomenon on the first repetition of the BDM procedure and the first repetition of the second price auction. With repetition, however, the preference reversal rate was substantially lower.

Previous researchers (e.g. Bostic, Herrnstein, and Luce [7]; Tversky, Slovic, and Kahneman [67]) have focused on the response mode. In addition to the BDM and second price auctions in which subjects provide numerical valuations of gambles, we employed two choice-based methods, one in the individual decision making setting and one in a market environment. Taking X to be the midpoint between subjects valuations for the $ bet and P bet (rounded to the nearest multiple of five cents)

[2] In addition, some types of individual choice experiments have generally produced results that are consistent with theory. For example, finite horizon sequential search models have predicted subject decisions reasonably well (Braunstein and Schotter, [8, 9]; Cox and Oaxaca [17, 18, 19]).

subjects were asked their choices from {$ bet, $X}, {P bet, $X} and {P bet, $ bet}. Our results replicate those of Tversky et al. and Bostic et al., as we observe rates of intransitive choice (approximately ten percent) much lower than the observed rate of preference reversals. Values obtained using the clock auction, a choice-based institution, produced fewer total reversals and roughly equal numbers of predicted and unpredicted reversals.

We have observed preference reversals with market institutions in experiments with every decision being acted on immediately using one of three different payment schedules (full payment, half payment, and a fixed payment independent of the outcomes of subjects' decisions). Thus we are inclined to rule out feedback and incentives alone as causes of discrepancies between the results of market and individual choice experiments. With repetition, the rate of preference reversals fell substantially with all methods and the asymmetry between rates of predicted and unpredicted reversals generally disappeared. This suggests that the repetitive nature of the tasks in market experiments in conjunction with feedback is an important factor.

We cannot rule out the possibility that market and individual decision making environments present psychologically different tasks which could lead to qualitatively different behavior. However, merely being in a market was not sufficient to lower the rate of reversals. Subjects in our experiments seemed to be influenced by the past values of market prices. Thus we conclude that the extra information generated in markets is used by market participants and could provide a partial explanation for the difference between the results from market experiments and individual choice experiments.

Our experience with the notice informing subjects that their rewards would not be based upon their decisions points out the difficulty in studying incentive effects. Subjects in our experiments were undergraduates at the University of Arizona and all sessions took place in Economic Science Laboratory there. It is possible that the expectation that their earnings would depend upon their performance was so strong that some subjects actually believed this to be the case in spite of receiving notice to the contrary. We did not observe any evidence of this with the second (revised) notice and from the results with the clock auction do not believe it was a problem. However, we did not anticipate a problem with the first notice though there clearly was one.

The effect of financial incentives was dramatically illustrated by the results of the clock auction. Our conclusions about preference reversals and repetition in market institutions are reversed with and without monetary incentives. In other respects, we do not see striking results of varying the payment schedule. The negligible effects, except for the clock auction, of varying the level of salient rewards is a noteworthy finding because the level of salient rewards in our full payment experiments was much higher than is typical for economics experiments. For example, at full payoff the win state payoff for a *single play* of $ bet 1 was $16. In comparison, the win state payoff from P bet 1 was $4. As a consequence the low, average, and high subject rewards in the full payoff experiments were $19.50, $58.82, and $109 for experiments that only took 1 to $1\frac{1}{2}$ hours to complete. Of course, the *salient* rewards in our low payoff experiments were all zero. We followed the method of Cox and Epstein [16] in playing out each decision as it was made and updating subjects' earnings during the experiments and found no effect of cumulative earnings on subjects' behavior.

Although the win state payoffs of the \$ bet and P bet in our two lottery pairs differ by \$12 and \$7, their *expected* payoffs differ by only 1¢ and 7¢ (see the last paragraph in section 2). Thus one could be tempted to try to attribute the preference reversals that we observed to random choices made by risk neutral subjects. But that would not be credible because the mean *difference* between BDM prices for paired lotteries in our full payoff experiments was \$2.24 (see Table 1). Grether and Plott ([31], pgs. 632–633) had previously reported preference reversals involving significant amounts of money. Finally, Cox and Epstein [16] reported that reversals persisted even after they introduced a 50 percent difference between the expected payoffs in paired \$ bets and P bets.

The results presented in this paper support the view that the nature of market institutions and the information generated by the markets, together with feedback and the repetitive nature of market tasks, account for the generally positive results of market experiments. The BDM method and the first repetition of the second price auction produce comparable preference reversal results, but by the fifth repetition of the auction mechanism we no longer observe the preference reversal phenomenon.

In our first attempt at understanding the apparent discrepancies between results from market and individual experiments, we have performed both types of experiments as they are traditionally presented in the literature. Thus we have presented subjects with the same tasks in both market and non-market environments and observed the outcomes. While we have identified several factors that often differ between the two types of experimental settings (information, repetition, feedback, psychological setting, incentives, and institutions) we leave the identification of the separate effects of these factors for future work. Indeed, our results suggest that several of the factors combined rather than any one of them are required to account for the differences between the two classes of experiments.

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