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Preference Reversals Without the Independence Axiom

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The preference reversal phenomenon was believed to be inconsistent with the transitivity axiom of decision theory. However, recent papers have demonstrated that previously observed preference reversals could be explained by subject violations of the independence axiom or the compound lottery axiom. The present paper reports the results of experiments in which a substantial proportion of subject responses violate the asymmetry axiom. These results are inconsistent with expected utility theory and its generalizations.

Economic theories of decision making under uncertainty imply consistency of choice and valuation that seems to be violated by a substantial percentage of subject responses in many experimental studies. This puzzling behavior has become known as the preference reversal phenomenon. A preference reversal can be explained with the following example. A subject is offered a direct choice between two lotteries. One of the lotteries offers a high probability of a relatively small monetary payoff (hereafter referred to as the P or probability bet), and the other lottery offers a low probability of a relatively large monetary payoff (hereafter referred to as the $ or money bet). In addition, the subject's minimum selling price is elicited for each lottery. A preference reversal occurs when a subject places a lower selling price on the directly chosen lottery. Given that preferences are monotone (more wealth or income is preferred to less), preference reversals were alleged to be inconsistent with the transitivity axiom of decision theory.[1]

David Grether and Charles Plott (1979) noted some possible problems in earlier experimental designs for preference reversal experiments. Three of these were absence of salient monetary payoffs, lack of control for wealth effects, and absence of a possibility for subjects to record indifference.[2] Grether and Plott designed some experiments that did not have the problems of earlier work. However, it was subsequently demonstrated by Charles Holt (1986) and Edi Karni and Zvi Safra (1987) that the Grether-Plott experimental design cannot discriminate between subject responses that are inconsistent with the independence axiom of expected utility theory and responses that are inconsistent with more fundamental postulates of rationality such as transitivity. Furthermore, Uzi Segal (1988) showed that the Grether-Plott experimental design cannot discriminate between violations of the transitivity and compound lottery axioms. Therefore, the Grether-Plott experimental results do not support their conclusion that subjects frequently violate transitivity.

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[1]Grether and Plott (1979, p. 623) provide a detailed explanation of the "intransitivity" interpretation of the traditional preference reversal.

[2]"Saliency" is one of the sufficient conditions ("precepts") for conducting a valid controlled microeconomic experiment; these precepts are explained in Smith (1982).

In this paper, we present experimental designs that do not contain any compound lotteries and do not require the independence axiom to interpret the results. Therefore, any reversals of revealed preferences for lotteries that are observed in our experiments cannot be attributed to subject violations of the compound lottery axiom or the independence axiom. In fact, the results of our experiments are that a substantial proportion of subject responses violate the asymmetry axiom. This type of reversal is inconsistent with expected utility theory *and* more general decision theories that relax the independence axiom.

The design of our experiments, and the pattern of reversals that they produce, both differ in essential ways from previous preference reversal experiments. Thus we will refer to the reversals that we observe as "choice reversals," as distinct from the traditional preference reversal phenomenon.[3]

I. Economic Theory and Experimental Design

Economic theories of decision making under risk are concerned with choices among actions that yield outcomes that matter. This body of theory is not intended to explain how an agent might choose from among actions that yield consequences of no importance to the agent. Therefore, the relevance to economics of experiments involving hypothetical choices can be questioned. In preference reversal experiments, the critical distinction is between those experiments involving hypothetical choices among lotteries and those experiments involving real choices among lotteries such that the chosen lotteries are played and the resulting prizes (usually cash payoffs) are delivered to the subjects. Of course, it is an empirical question whether *economically* unmotivated choices are, in fact significantly different from economically motivated choices.[4] Nevertheless, the results of preference reversal experiments involving *only* hypothetical choices cannot provide a convincing challenge to economic theory.

Economic theories of decision making under risk explain how variations in wealth can affect choices. Thus an agent with wealth w may prefer lottery A to lottery B but that same agent with wealth $\hat{w} \neq w$ may prefer lottery B to lottery A. Therefore, the results of preference reversal experiments that allow a subject's wealth to change between choices cannot provide a convincing challenge to economic theory unless it can be shown that wealth effects cannot account for the results.

Economic theories of decision making under risk provide explanations of optimal portfolio choice. Such theories explain why an agent might prefer lottery A to lottery B but prefer the portfolio (A, B) to the portfolio (A, A). If the portfolio is accumulated by sequential choice of A over B and then B over A, an apparent preference reversal could consist of choices that construct an agent's optimal portfolio. Therefore, the results of preference reversal experiments that allow a subject to accumulate a portfolio of lotteries cannot provide a convincing challenge to economic theory unless it can be shown that the resulting *portfolio* is dominated by an alternative feasible *portfolio* (by, say, first-order stochastic dominance).

Expected utility theory is the most familiar economic theory of decision making under risk. An expected utility functional obtains its simple form of linearity in the probabilities as a consequence of the independence axiom. An experimental design that requires the independence axiom to interpret subject responses as preference rever-

[3] One referee commented that the preference reversal phenomenon occurs when a subject chooses the P bet and places a higher selling price on the \$ bet, and that it does *not* occur when a subject chooses the \$ bet and places a higher selling price on the P bet. Another referee commented that the preference reversal phenomenon can only occur in experiments that use a (selling) price elicitation mechanism.

[4] Grether and Plott in preference reversal experiments and David Grether (1980, 1981) in experiments on Bayes' rule have made important contributions in testing the effect of financial motivation on subject responses.

sals can provide a convincing challenge to expected utility theory, but it cannot provide a challenge to more general decision theories that do not include the axiom. This point is important to evaluating the implications of earlier preference reversal experiments. In an attempt to get the subjects to reveal their certainty equivalents for the lotteries, researchers have commonly used the selling price elicitation procedure introduced in G. Becker, Morris DeGroot, and Jacob Marshak (BDM, 1964). In this BDM procedure, a subject states the minimum price at which he would sell a lottery. An offer to purchase is then selected randomly from some distribution (usually, a uniform distribution). If this offer exceeds the stated price, the subject sells the lottery for the amount of the offer; if the offer is below the stated price, the subject retains the lottery. The BDM procedure is designed to be a dominant strategy revelation mechanism. However, in preference reversal experiments the selling prices that are elicited are for lotteries. Since the BDM procedure is itself a lottery, its use in preference reversal experiments creates compound lotteries. The compound lottery and independence axioms allow one to "reduce" the compound lotteries and to interpret the elicited prices as certainty equivalents. But Karni and Safra (1987) have shown that the BDM procedure can elicit prices that are different from, and may be in reverse order to, the certainty equivalents of the respective lotteries if a subject's behavior does not satisfy the independence axiom. Furthermore, Segal has demonstrated that a similar reversal can occur if a subject's behavior does not satisfy the compound lottery axiom. Therefore, any preference reversal experiment that uses the BDM procedure cannot discriminate between violations of the independence and compound lottery axioms and other, more problematic inconsistencies with decision theory such as intransitivities.

II. Earlier Experimental Work

Preference reversal experiments were first reported in papers by Sarah Lichtenstein and Paul Slovic (1971, 1973) and Harold Lindman (1971). These experiments are part of a much larger group of experiments whose results have been interpreted as calling into question economic theories of rational choice (Slovic and Lichtenstein, 1983). Subsequent preference reversal experiments were reported by Grether and Plott, Werner Pommerehne, Friedreich Schneider, and Peter Zweifel (1982), Robert Reilly (1982), and Joyce Berg, John Dickhaut, and John O'Brien (1985). All of these papers report interesting experiments. However, the generality of the implications for economics of all of their results can be questioned.

Grether and Plott discussed preference reversal experiments that preceded theirs; hence, we will begin our discussion with their paper. The Grether-Plott experimental design had the following properties. The subjects were divided into two groups. One group made hypothetical choices. The other group made real choices with salient monetary rewards. One of each subject's choices in the financially motivated group was randomly selected for cash payoff at the end of the experiment. This random selection procedure was used to control for possible wealth and portfolio effects. In addition, the BDM procedure was used for both groups. Grether and Plott found a high proportion of preference reversals for both groups. Their experimental design can support a challenge to expected utility theory. But it cannot support a challenge to more general decision theories that do not involve the independence axiom (Soo Hong Chew, 1981; Peter Fishburn, 1983; Mark Machina, 1982; Menahem Yaari, 1987). All of the Grether-Plott experiments used the BDM procedure. Therefore, their experimental design cannot discriminate between violations of the independence and compound lottery axioms and violations of transitivity for the reasons explained by Karni and Safra (1987) and Segal (1988). Furthermore, as explained by Holt (1986), the Grether-Plott random selection procedure used in the experiments with cash payoffs also creates a compound lottery and, hence, an inability to discriminate between violations of the independence axiom and of transitivity.

Pommerehne et al. addressed three potential problems in Grether and Plott's experimental design. First, they questioned whether the subjects were sufficiently motivated. This led them to increase the point ("play money") payoff by a factor of 100; however, the conversion rate of play money into cash was not known in advance by the subjects.[5] Second, Pommerehne et al. contended that nearly equal expected payoffs in Grether and Plott's P and \$ bets could have resulted in subject boredom, effectively raising decision costs above expected return. This led them to run a set of experiments in which each lottery in a pair had a different expected value than the other. Finally, they questioned whether inexperienced subjects understood the random decision selection procedure. This led them to provide their subjects with an opportunity to learn the mechanics of this procedure prior to making their final decisions. Their results indicated that preference reversal was robust. They failed to eliminate reversals, and only the first treatment, increased incentives, resulted in a significant reduction in their frequency. The description of the experimental design in Pommerehne et al. is inadequate for ascertaining their actual procedures. They did write that "The design of our experiments is basically the same as that used by G-P" (Pommerehne et al., 1982, p. 571). We presume that this statement means that they used the BDM and random selection procedures. Therefore, the implications of their results are limited to expected utility theory for the reasons explained by Holt, Karni and Safra, and Segal.

Reilly also addressed the questions about possible insufficient incentives and subject confusion in the Grether-Plott (1979) experiments. He altered the design by increasing the dollar amounts at risk in the lotteries and by increasing the amount of instruction given to the subjects. Although their frequency was lowered, he found that preference reversals were still common. Reilly's experiments used the BDM procedure and the random decision selection procedure; therefore, the implications of his experimental results are confined to expected utility theory.

Berg et al. (1985) attempted to generate consistent choices from their subjects by an arbitrage procedure which forced them to engage in unprofitable trades if they made inconsistent decisions. This procedure succeeded only in reducing the dollar amount of reversals, not their frequency. These experiments used the BDM procedure or a modification of it (the O'Brien procedure) and the random decision selection procedure; hence the implications of the results are confined to expected utility theory.

III. Experimental Designs

Our experiments are designed to eliminate violations of the independence and compound lottery axioms as potential explanations for observed reversals of revealed preferences for lotteries. To do this, it is necessary to address the issues raised by Holt, Karni and Safra, and Segal. First, we simply abandon the random selection procedure and pay subjects for each decision. This, however, depending upon its implementation, creates one of two other potential problems. If subjects are paid as each decision occurs, wealth effects might exist, as money is accumulated between decisions. If no payment is made until after all decisions are made, portfolio effects might be a factor. That is, a consistent subject might prefer lottery A to lottery B and the portfolio (A, B) to the portfolio (A, A). This type of portfolio choice could appear to be a choice reversal. We decided to pay subjects after each choice and contend with possible wealth effects as the lesser of two evils, recognizing that proceeding in this way was risky: if we found that wealth effects could explain most of the choice reversals then the experiments would not have produced any useful information.

[5] This feature of the experimental design has some potential problems. Since a subject does not know the conversion rate of play money into cash but, presumably, has some expectations about it, yet another compound lottery has been created. In addition, the conversion rate depends on the choices of *all* of the subjects; hence there exists an uncontrolled externality among the subjects in cash rewards.

TABLE 1—THE SIX LOTTERY PAIRS FOR THE DIRECT CHOICE QUESTIONS OF EXPERIMENT 1

Pair	Type	Probability of Winning	Amount If Win (Fr)	Amount If Lose (Fr)	Expected Value (Fr)
1	*P*	35/36	4,000	−1,000	3,861
	$	11/36	16,000	−1,500	3,847
2	*P*	29/36	2,000	−1,000	1,417
	$	7/36	9,000	− 500	1,347
3	*P*	34/36	3,000	−2,000	2,722
	$	18/36	6,500	−1,000	2,750
4	*P*	32/36	4,000	− 500	3,500
	$	4/36	40,000	−1,000	3,556
5	*P*	34/36	2,500	− 500	2,333
	$	14/36	8,500	−1,500	2,389
6	*P*	33/36	2,000	−2,000	1,667
	$	18/36	5,000	−1,500	1,750

Note: The lotteries used in the selling price questions are constructed by reducing every win and lose payoff amount in the table by 1,000 francs.

Second, it was necessary that we not use the BDM price elicitation procedure. Furthermore, we concluded that Karni and Safra's Theorem 2 makes it highly unlikely that anyone will be able to design a price elicitation mechanism for choices in a lottery space that does not require the independence axiom. Therefore, we concluded that it would be impossible for us to elicit true selling prices in an experiment that is designed in such a way that behavioral inconsistencies with the independence axiom are not confounded with more fundamental inconsistencies with decision theory. But preference reversals are inherently properties of inconsistent orderings. The absolute magnitude of prices is basically irrelevant; it is the fact that the less preferred lottery is given a higher price that represents an inconsistency with decision theory. We utilized this fact by asking subjects to state selling prices for both lotteries in every pair. They were informed that we would pay them an announced price of 1,000 francs for the lottery to which they had given the lower selling price, and they would play the lottery to which they had given the higher selling price. The payoffs for both lotteries in every pair were reduced by the amount of the announced price so that the subjects were choosing between the same probability distributions of returns in a selling price question as in the paired direct choice question.[6] In this manner we should preserve the ordering property of the stated selling prices despite the fact that we cannot elicit true selling prices. That is, a subject should still place a higher selling price on the lottery for which a direct preference is stated. If not, this represents a choice reversal. Since no compound lottery is created, the independence and compound lottery axioms are not involved.

A. *Experiment 1*

In experiment 1 we presented subjects with essentially the same six pairs of lotteries used by Grether and Plott and by Lichtenstein and Slovic (1971, experiment 3). Table 1 reports the lotteries that we used in the direct choice questions. These are the same as the Lichtenstein and Slovic lotteries except that our "franc" payoffs are ten times their "point" payoffs. Our exchange rate between francs and U.S. dollars was 4,000 to 1. This exchange rate implies dollar payoffs

[6] Note that the consequences of placing a higher price on the $ bet (*P* bet) in a selling price question are exactly the same as the consequences of choosing the $ bet (*P* bet) in the paired direct choice question. This would not be the case if we had failed to reduce the lottery payoffs by 1,000 francs in the selling price questions.

that are 1/4 the dollar payoffs in the Grether-Plott lotteries. This reduction in dollar payoffs was necessitated by budgetary considerations since we paid subjects for each decision whereas Grether and Plott selected only one of eighteen decisions for cash payoff.

The lotteries were presented to our subjects in the form of figures like those in the Grether and Plott experiments. Some representative figures from our experiments are contained in the Appendix. A bingo cage containing balls numbered 1–36 was used to generate random numbers. Subjects were informed that for each lottery, if the number drawn was less than or equal to x, they would win a set amount of francs, and if the number drawn exceeded x, they would lose a specified number of francs. The exchange rate between francs and dollars was clearly specified in the instructions.

Inspection of Table 1 reveals that there are only small differences between the expected payoffs for the P and \$ bets in every pair; in fact, using the 4,000 francs to \$1 conversion rate, these differences only vary from one cent to two cents. The differences between the expected payoffs in the paired P and \$ bets used by Grether and Plott (1979, p. 629) varied from one cent to eight cents. However, the probability that any one decision would be selected for cash payoff in the Grether-Plott experiment was 1/18. Hence, in order to calculate the actual expected payoffs to their subjects, one needs to divide their expected lottery payoff figures by 18. If one does that, and then rounds to the nearest cent, one finds that the P and \$ bets in four of their lottery pairs had the same expected payoff and there was a one-cent difference in the other two pairs. Therefore, in both our experiment 1 and the Grether-Plott experiments, a risk-neutral subject would incur a very small reduction in expected utility from reversals. In order to increase the expected opportunity cost of choice reversals for any subjects who might be risk neutral, we designed a second experiment with larger separations of expected lottery payoffs.

B. *Experiment 2*

Experiment 2 was designed in the same way as experiment 1 except that the lotteries were different. Table 2 presents the lotteries that we used in the direct choice questions of experiment 2. The Table 2 lotteries were constructed from those in Table 1 by increasing the win state payoff for one lottery in each pair by an amount that yielded about a 50 percent difference between the expected payoffs for the paired lotteries. We alternated between \$ and P bets in increasing the expected payoffs. Thus, in pair 1 (Table

TABLE 2—THE SIX LOTTERY PAIRS FOR THE DIRECT CHOICE QUESTIONS OF EXPERIMENT 2

Pair	Type	Probability of Winning	Amount If Win (Fr)	Amount If Lose (Fr)	Expected Value (Fr)
1	*P*	35/36	4,000	−1,000	3,861
	\$	11/36	22,400	−1,500	5,803
2	*P*	29/36	2,800	−1,000	2,061
	\$	7/36	9,000	− 500	1,347
3	*P*	34/36	3,000	−2,000	2,722
	\$	18/36	9,200	−1,000	4,100
4	*P*	32/36	6,100	− 500	5,367
	\$	4/36	40,000	−1,000	3,556
5	*P*	34/36	2,500	− 500	2,333
	\$	14/36	11,400	−1,500	3,517
6	*P*	33/36	3,100	−2,000	2,675
	\$	18/36	5,000	−1,500	1,750

Note: The lotteries used in the selling price questions are constructed by reducing every win and lose payoff amount in the table by 1,000 francs.

2) the $ bet has an expected value that is about 50 percent higher than the P bet, whereas in pair 2 the P bet's expected value is about 50 percent higher than the $ bet's expected value, and so on in an alternating pattern. These differences between expected values for paired P and $ bets vary from 714 francs (18 cents) to 1,942 francs (49 cents).

IV. Experimental Procedures

Subjects were recruited from undergraduate economics classes at the University of Arizona. Only one subject at a time participated in an experiment to avoid the possibility of an agent's decisions being influenced by others' wins, losses, or decisions. Each subject was permitted to ask questions for clarification. As each person entered the room, he or she was presented with a set of written instructions which explained the nature of the lotteries and the decisions to be made. All 36 balls were outside the bingo cage for inspection and the subject would place them into the cage for himself or herself. Twelve figures accompanied the instructions, each depicting a pair of lotteries. The instructions and some of the figures for experiment 2 are contained in the Appendix. Each subject made decisions over the six pairs of lotteries twice. One of the times a direct preference was stated, and the subject immediately played the chosen lottery; the other time selling prices were given, and the subject played the lottery for which a higher price was stated.[7] The lower-priced lottery was sold to the experimenter for a set fee of 1,000 francs (25 cents), regardless of the actual quoted price. Once again, this permitted us to elicit selling prices which would be ordered the same as the true prices without the necessity of eliciting the true prices.

Sequential decisions over any pair of lotteries were always separated by six responses. That is, if a subject stated a direct preference for lottery A or B in decision one, he would state selling prices for the same pair of lotteries in decision seven. Two and eight were similarly related, as were three and nine, and so on. The questions were posed in an alternating fashion; if preferences were given in period t, then prices were elicited in period $t+1$. Half the subjects (Group I) began by stating preferences and the other half (Group II) began by stating prices. Therefore, for each pair of lotteries, half the subjects stated preferences prior to selling prices and half responded in the reverse order.

V. Results

Thirty subjects participated in each of the two experiments. All 60 subjects were distinct individuals. Each subject was given $5.00 (or 20,000 francs) in working capital to cover possible losses in the lotteries. In experiment 1, individual subject payoffs from playing the chosen lotteries varied from $3.25 to $13.75. The average subject lottery payoff was $7.28; hence the average total subject payoff in experiment 1 was $12.28. Individual subject payoffs from the lotteries chosen in experiment 2 varied from $4.25 to $25.75. The average subject payoff from the lotteries in experiment 2 was $10.02 and, hence, the average total subject payment in this experiment was $15.02. On average, a subject took 21 minutes to complete experiment 1 and 23 minutes to complete experiment 2.

A. *Frequency of Choice Reversals*

Table 3 reports a summary of the results from both experiments. There were three choices of "indifferent" in experiment 1. In the other 177 decision pairs of experiment 1, 62 (or 35 percent) were reversals. In experiment 2, 258 of 360 (or 72 percent) of the subject's responses implied selection of the bet with higher expected payoff in each pair. Even so, 52 of 180 (or 29 percent) of the decision pairs in experiment 2 were reversals. In experiment 1, 26 of the 30 subjects, or 87 percent, reversed at least one time. Of the 30 subjects in experiment 2, 24 (or 80 percent) reversed at least one time.

[7] If a subject reported indifference between two bets or set equal selling prices on two bets, then the one he or she would play was determined by a coin toss.

TABLE 3—SUMMARY OF THE RESULTS

Group	Total Decisions	Consistent Decisions	Inconsistent Decisions	Indifference
		Experiment 1		
I	90	52	35	3
II	90	63	27	0
I+II	180	115	62	3
		Experiment 2		
I	90	69	21	0
II	90	59	31	0
I+II	180	128	52	0

B. *Analysis of Possible Wealth Effects*

Subjects in our study were accumulating money from lottery payoffs during our experiments. This could produce wealth effects on subject choices over lotteries. We could not control for possible wealth effects without using the Grether-Plott random decision selection procedure that confounds violations of the independence and compound lottery axioms with other sources of choice reversals. Therefore, we have examined the data in several ways to determine whether wealth effects can explain the results.

Define the following variables:

$$D_t = \begin{cases} 1 \text{ if subject selected the \$ bet in} \\ \quad \text{period } t, \\ 0 \text{ if subject selected the } P \text{ bet in} \\ \quad \text{period } t; \end{cases}$$

$$TW_{t-1} = \text{total wealth at the end of period } t-1.$$

Logit analyses, using various subsamples of the data, were done to relate the probability that $D_t = 1$ (rather than 0) to TW_{t-1} and D_{t-6} (the binary variable for the previous selection from the same lottery pair). If no reversals occurred there would be perfect correlation between D_t and D_{t-6} because subjects faced the same (although slightly "disguised") lotteries in periods t and $t-6$. A significant coefficient on TW_{t-1} would indicate a statistical relationship between subject choice and wealth, and indicate a confounding of wealth effects with inconsistent revealed preferences. Table 4 reports the logit coefficients and t-ratios (in parentheses) for the first set of estimations.

First consider the results for the experiment 1 data. The second column of Table 4 reports the pooled sample estimation.[8] There is clearly a significantly positive relation between decisions in period t and $t-6$ (at any reasonable confidence level) despite the observance of 35 percent reversals. The wealth coefficient is barely significant at the 10 percent confidence level (actually, at $\alpha = 0.091$). The third and fourth columns report the results for the subject Group I and Group II subsamples. Group I data are for those subjects who began by stating a direct preference whereas Group II subjects began by stating prices. Both groups show highly significant relations between decisions over the same pair of lotteries, yet neither group exhibits significant wealth effects at the 10 percent confidence level.

[8]All 180 observations, including those three in which subjects did not indicate a strict preference between the two lotteries are used. Two of the three are statements of "Do not Care" when direct preference was given, and one is a statement of identical selling prices. None of the three is consistent with the theory; the decision over the same pairs of lotteries when asked in the opposite fashion is a specific choice of one lottery over the other in each of the three cases. For this reason, the dummy variable $D_t(D_{t-6})$ takes the value of 1 when $D_{t-6}(D_t)$ has the value 0, and $D_t(D_{t-6})$ takes the value 0 when $D_{t-6}(D_t)$ has the value 1.

TABLE 4—TESTS FOR WEALTH EFFECTS

Design Determinants	Experiment 1			Experiment 2		
	Pooled Sample	Group I	Group II	Pooled Sample	Group I	Group II
TW_{t-1}	0.0000272	0.0000232	0.0000272	0.0000138	0.0000163	0.0000141
	(1.689)	(0.959)	(1.18)	(1.363)	(0.842)	(1.223)
D_{t-6}	1.33	0.975	1.692	1.768	2.422	1.238
	(4.113)	(2.11)	(3.603)	(5.333)	(4.708)	(2.752)
$\ln L$	−114.61	−59.970	−53.973	−107.15	−48.492	−56.948
$-2\ln\lambda$	20.119	4.782	16.643	34.430	27.383	10.471
$\chi_2^{2,0.95}$	5.99	5.99	5.99	5.99	5.99	5.99
N	180	90	90	180	90	90

Note: Notation in the table is defined as follows: L is the value of the likelihood function; λ is the likelihood ratio under the null hypothesis that all of the coefficients are jointly zero; $-2\ln\lambda$ is distributed as a chi square with two degrees of freedom; $\chi_2^{2,0.95}$ is the critical value of the chi square variate at the 95 percent confidence level with two degrees of freedom; and N is the number of observations. The figures in parentheses are t-ratios.

TABLE 5—TESTS FOR WEALTH EFFECTS IN INDIVIDUAL DECISION PAIRS

Design Determinants	Subsamples					
	Pair (1,7)	Pair (2,8)	Pair (3,9)	Pair (4,10)	Pair (5,11)	Pair (6,12)
	Experiment 1					
TW_{t-1}	0.0000359	−0.00000172	−0.0000510	0.0000991	0.00000706	0.0000906
	(0.844)	(−0.039)	(−1.129)	(1.756)	(0.173)	(1.442)
D_{t-6}	0.5010	1.413	1.029	2.713	0.4387	2.772
	(0.603)	(1.785)	(1.214)	(2.147)	(0.530)	(2.270)
$\ln L$	−17.786	−18.793	−17.569	−13.151	−19.554	−14.777
$-2\ln\lambda$	1.0793	3.4676	3.0528	6.2949	0.32200	7.0972
$\chi_2^{2,0.95}$	5.99	5.99	5.99	5.99	5.99	5.99
N	30	30	30	30	30	30
	Experiment 2					
TW_{t-1}	0.0000236	0.0000264	0.000157	−0.0000468	0.0000413	0.0000321
	(0.760)	(1.007)	(1.343)	(−0.773)	(1.122)	(1.298)
D_{t-6}	1.209	0.300	2.901	3.398	0.992	0.457
	(1.501)	(0.318)	(1.759)	(1.770)	(1.089)	(0.549)
$\ln L$	−18.794	−18.382	−10.071	−5.342	−14.985	−17.135
$-2\ln\lambda$	4.001	1.427	6.8908	4.011	2.625	2.382
$\chi_2^{2,0.95}$	5.99	5.99	5.99	5.99	5.99	5.99
N	30	30	30	30	30	30

Note: Notation in this table is defined in Table 4.

The last three columns of Table 4 report logit estimations for experiment 2. All of the D_{t-6} coefficients are highly significant whereas none of the wealth coefficients is significant at the 10 percent confidence level.

The preceding estimates are based on data that are aggregated over decision pairs. The results of logit estimations of the data from individual decision pairs are reported in Table 5. For experiment 1, pairs (2,8), (4,10), and (6,12) show significantly positive relationships between decisions in period t and decisions in period $t-6$ (at 10 percent confidence level for (2,8) and 5 percent confidence level for (4,10) and (6,12)). As one would expect, these represent the three decision pairs for which subjects made the fewest reversals. In the (2,8) pair, 10 of 30 subjects reversed choices. In the (4,10) pair, this proportion dropped to 7 of 30, and in the (6,12) pair it was 6 of 30. Only one wealth coefficient, that for the (4,10) pair, is significant at 10 percent. This is noteworthy because this one indication of significant wealth effects occurs in the decision pair where observed reversals are at their next-to-lowest proportion of any of the six decision pairs. No other decision pair shows any significant relationship between choice and wealth.

The experiment 2 results in Table 5 reveal the following. Only pairs (3,9) and (4,10) have significantly positive relationships between decisions in period t and $t-6$ at the 10 percent confidence level. These are the decision pairs for which subjects made the fewest reversals: 5 of 30 subjects reversed choices in (3,9) and 2 of 30 subjects reversed in (4,10). There is no significant wealth coefficient at 10 percent confidence level for any decision pair in experiment 2.

The logit analyses reported in Tables 4 and 5 detect little in the way of significant wealth effects on subject decisions. But these estimations aggregate subject responses. Perhaps there are significant wealth effects that are not homogeneous across subjects. If one looks at the order of decision and ignores the type of question (selling price or direct choice), there are two categories of choice reversals. One category consists of those instances in which a subject initially selects the P bet and then (six decisions later) selects the \$ bet in the paired decision. This category of inconsistent decisions will be referred to as $P\$$ reversals. The other category, $\$P$ reversals, consists of those instances in which a subject first selects the \$ bet and, subsequently, selects the P bet in the paired decision. Since in almost all cases a subject had earned several dollars between the first decision in a pair and the second, perhaps the $P\$$ ($\$P$) reversals can be explained by *consistent* subject preferences that exhibit decreasing (increasing) risk aversion. Tests such as those reported in Tables 4 and 5, that aggregate across $P\$$ and $\$P$ reversals, may fail to detect wealth effects because in the aggregate they are offsetting.

One cannot credibly argue that the same subject exhibits both decreasing and increasing risk-averse preferences for the wealth changes in our experiments. Therefore, one cannot simply count the total number of reversals of each type, but must examine the pattern of reversals for each subject. If a subject makes *both* $P\$$ and $\$P$ reversals, then the decisions are inconsistent with both increasing and decreasing risk aversion and such wealth effects cannot immunize the theory to the falsifying evidence. First consider the individual subject results for experiment 1. If we consider only those subjects that make $P\$$ or $\$P$ reversals, but not both types, we find 19 $P\$$ reversals and 6 $\$P$ reversals. These are accounted for by 9 subjects who make only $P\$$ reversals and 4 subjects who make only $\$P$ reversals. There are 13 subjects who make both types of reversals and 4 subjects who do not reverse. Table 6 reports logit analyses of the subsamples of the data for the 9 subjects who make only $P\$$ reversals and the 4 subjects who make only $\$P$ reversals in experiment 1. The t-ratios in parentheses do not indicate that wealth effects are significant for either group of subjects.

Next, consider the individual subject results for experiment 2. If we consider only those subjects that make $P\$$ or $\$P$ reversals, but not both types, we find 9 $P\$$ reversals and 6 $\$P$ reversals. These are accounted for by 7 subjects who make only $P\$$ reversals and 5 subjects who make only $\$P$ reversals. There are 12 subjects who make both types of reversals and 6 subjects who do not reverse in experiment 2. The experiment 2 results in Table 6 report logit analyses of the subsamples of the data for the 7 subjects who make only $P\$$ reversals and the 5 subjects who make only $\$P$ reversals. The t-ratios in parentheses do not indicate that wealth effects are significant for either group.

We interpret the results of all of these logit analyses as follows. Changes in subject wealth during our experiments may have affected subject decisions. However, any such wealth effects cannot account for either the frequency or the pattern of subject choice

TABLE 6—TESTS OF WHETHER INCREASING OR DECREASING RISK AVERSION CAN EXPLAIN CHOICE REVERSALS

Design Determinants	Experiment 1		Experiment 2	
	$P\$$ Reversers	$\$P$ Reversers	$P\$$ Reversers	$\$P$ Reversers
TW_{t-1}	0.00000628	−0.0000398	0.0000443	0.00000218
	(0.150)	(−0.342)	(1.032)	(0.057)
D_{t-6}	14.292	15.157	3.796	17.595
	(0.045)	(0.026)	(3.207)	(0.013)
$\ln L$	−23.320	−8.2584	−18.221	−10.094
$-2\ln\lambda$	17.171	10.475	19.378	16.465
$\chi_2^{2,0.95}$	5.99	5.99	5.99	5.99
N	54	24	42	30

Note: Notation in this table is defined in Table 4.

reversals. True reversals of revealed lottery preferences are an important feature of our subjects' decisions.

If wealth effects cannot explain the reversals then, perhaps, subject inexperience can. Perhaps our subjects were learning about the lotteries during the experiment and this learning process can account for the reversals. We will next examine this question.

C. *Analysis of Possible Outcome Effects*

It is possible that some subjects did not fully understand the lotteries at the beginning of the experiment. For example, an initial selection of the \$ (long shot) bet, followed by an unlucky outcome (a loss), might cause a subject to avoid the \$ bet in the subsequent paired decision. In experiment 1 we observed 24 instances of subjects that exhibited $\$P$ reversals, and 53 instances where selection of the \$ bet was consistently followed by the same selection six periods later. Of the 24 reversals, 20 (or 83 percent) involved a loss when the \$ bet was played. In contrast, subjects lost only 29 out of 53 (or 55 percent) of the first plays of the \$ bet in those cases where they did not reverse in experiment 1. Any effects of lottery outcomes on subsequent decisions, unless they are wealth effects, are incompatible with decision theories that assume well-defined risk preferences. However, they could also indicate that the reversals are attributable to subject inexperience with the lotteries and therefore that the frequency of reversals might decrease significantly with more subject experience.

We performed the logit estimations that are reported in Table 7 to test for possible paired lottery outcome effects on subsequent decisions. These estimations used the decision (D_{t-6}) and wealth (TW_{t-1}) variables defined above and the outcome variable defined as follows.

$$OUTCM_{t-6} = \begin{cases} 1 \text{ if subject received the high payoff in period } t-6, \\ 0 \text{ if subject received the low payoff in period } t-6. \end{cases}$$

Table 7 reports the pooled sample and group results of the estimations for both experiments. In all columns, the coefficient on the paired decision variable (D_{t-6}) is highly significant. However, none of the coefficients on the paired outcome ($OUTCM_{t-6}$) and wealth (TW_{t-1}) variables is significant at the 10 percent confidence level. Similar estimations for individual decision pairs of both experiments are reported in Table 8. None of these coefficients on paired outcome is significant at the 10 percent confidence level. We conclude that paired lottery outcome did not significantly affect the subjects' lottery choices.

It might not be outcome in the paired decision that could affect lottery selection but, instead, outcome in the immediately

TABLE 7—TESTS FOR PAIRED PERIOD OUTCOME EFFECTS

Design Determinants	Experiment 1 Pooled Sample	Experiment 1 Group I	Experiment 1 Group II	Experiment 2 Pooled Sample	Experiment 2 Group I	Experiment 2 Group II
TW_{t-1}	0.0000197	0.0000192	0.0000169	0.0000131	0.0000177	0.0000105
	(1.131)	(0.769)	(0.658)	(1.239)	(0.900)	(0.864)
$OUTCM_{t-6}$	0.5719	0.4269	0.7163	0.1105	−0.3305	0.5451
	(1.144)	(0.633)	(0.952)	(0.220)	(−0.425)	(0.818)
D_{t-6}	1.659	1.244	2.083	1.840	2.197	1.583
	(3.735)	(1.948)	(3.229)	(3.917)	(3.011)	(2.524)
$\ln L$	−113.93	−59.764	−53.499	−107.13	−48.404	−56.604
$-2\ln\lambda$	21.481	5.1949	17.590	34.479	27.559	11.158
$\chi_3^{2,0.95}$	7.81	7.81	7.81	7.81	7.81	7.81
N	180	90	90	180	90	90

Note: L, λ, and N are defined in Table 4. Other notation in this table is defined as follows. $-2\ln\lambda$ is distributed as a chi square with three degrees of freedom; $\chi_3^{2,095}$ is the critical value of the chi square variate at the 95 percent confidence level with three degrees of freedom.

TABLE 8—TESTS FOR PAIRED PERIOD OUTCOME EFFECTS IN INDIVIDUAL DECISION PAIRS

Design Determinants	Subsamples Pair (1,7)	Pair (2,8)	Pair (3,9)	Pair (4,10)	Pair (5,11)	Pair (6,12)
			Experiment 1			
TW_{t-1}	0.0000287	−0.00000210	−0.0000606	0.000129	−0.0000255	0.0000807
	(0.545)	(−0.043)	(−1.228)	(1.941)	(−0.520)	(1.247)
$OUTCM_{t-6}$	0.3802	0.0194	0.8197	−2.364	13.863	0.7792
	(0.233)	(0.018)	(0.517)	(−1.451)	(0.033)	(0.711)
D_{t-6}	0.7245	1.424	1.5641	1.089	13.338	3.049
	(0.573)	(1.430)	(1.123)	(0.701)	(0.031)	(2.331)
$\ln L$	−17.759	−18.793	−17.427	−12.182	−17.482	−14.522
$-2\ln\lambda$	1.1336	3.4679	3.3377	8.2322	4.4656	7.6069
$\chi_3^{2,0.95}$	7.81	7.81	7.81	7.81	7.81	7.81
N	30	30	30	30	30	30
			Experiment 2			
TW_{t-1}	0.0000733	0.0000321	0.000138	−0.0000324	0.0000458	0.0000347
	(1.318)	(1.192)	(1.166)	(−0.474)	(1.037)	(1.313)
$OUTCM_{t-6}$	−2.500	−1.400	12.429	−0.9394	13.219	−0.9048
	(−1.231)	(−0.924)	(0.019)	(−0.418)	(0.024)	(−0.659)
D_{t-6}	−0.7505	−0.6913	14.968	3.022	13.774	−0.1803
	(−0.429)	(−0.471)	(0.023)	(1.442)	(0.025)	(−0.138)
$\ln L$	−17.897	−17.898	−9.612	−5.266	−13.770	−16.903
$-2\ln\lambda$	5.794	2.394	7.809	4.164	5.057	2.846
$\chi_3^{2,0.95}$	7.81	7.81	7.81	7.81	7.81	7.81
N	30	30	30	30	30	30

Note: L, λ, and N are defined in Table 4. Other notation in this table is defined in Table 7.

TABLE 9—TESTS FOR PREVIOUS PERIOD OUTCOME EFFECTS

Design Determinants	Experiment 1			Experiment 2		
	Pooled Sample	Group I	Group II	Pooled Sample	Group I	Group II
TW_{t-1}	0.00003185	0.00003518	0.00002525	0.0000121	0.0000137	0.0000129
	(1.883)	(1.370)	(1.064)	(1.178)	(0.697)	(1.103)
$OUTCM_{t-1}$	−0.359755	−0.802385	0.169015	0.4085	0.3784	0.3768
	(−1.050)	(−1.675)	(0.333)	(1.164)	(0.705)	(0.797)
D_{t-6}	1.30221	0.960542	1.71865	1.817	2.449	1.296
	(4.007)	(2.054)	(3.596)	(5.384)	(4.711)	(2.821)
$\ln L$	−114.05	−58.527	53.917	−106.47	−48.241	−56.627
$-2\ln\lambda$	21.227	7.6672	16.754	35.802	27.885	11.113
$\chi_3^{2,0.95}$	7.81	7.81	7.81	7.81	7.81	7.81
N	180	90	90	180	90	90

Note: L, λ, and N are defined in Table 4. Other notation in this table is defined in Table 7.

preceding period. To test for this possibility, we performed the logit estimations reported in Table 9. These estimations used the outcome variable defined as follows.

$$OUTCM_{t-1} = \begin{cases} 1 \text{ if subject received the high payoff in period } t-1, \\ 0 \text{ if subject received the low payoff in period } t-1. \end{cases}$$

The coefficient on $OUTCM_{t-1}$ for Group I in experiment 1 is just significant at the 10 percent confidence level. All of the other columns of Table 9 report insignificant coefficients on the previous period outcome variable. However, the one significant coefficient on $OUTCM_{t-1}$ is negative, indicating that a lucky subject is *less* likely to choose the long shot ($) bet on the next choice. Table 10 reports similar estimations for individual decision pairs from both experiments. None of these coefficients on previous period outcome is significant at the 10 percent level. We conclude that previous period lottery outcome cannot account for the observed reversals.

D. *Analysis of Possible Framing Effects*

In the direct choice questions, the subjects were simply asked to indicate which one of two lotteries in a pair they would prefer to play. In the selling price questions, the subjects were asked to report selling prices for both lotteries in a pair. They would then play the lottery on which they had placed the higher price and sell the other to the experimenter for 1,000 francs. In addition, the payoffs for the lotteries in the selling price questions were uniformly lower by 1,000 francs than the payoffs for the corresponding lotteries in the direct choice questions. Thus the decision in a selling price question is exactly equivalent in economic terms to a choice between the lotteries in the corresponding direct choice question. The only difference is in the way the questions are "framed." Psychologists have extensively studied "framing effects" and concluded that they significantly affect subject responses in a variety of contexts (Slovic and Lichtenstein, 1983; Amos Tversky and Daniel Kahneman, 1981). This leads us to ask whether the choice reversals in our experiments can be explained by systematic framing effects. For example, does framing the choice as a selling price question cause the subjects to more or less frequently choose the $ bet than does direct choice framing? Table 11 reports the $ and *P* bet choices for both experiments disaggregated by type of question. The nearly identical choices in the selling price and direct choice questions in experiment 1 do not reveal any framing effect. Subject responses to the direct choice ques-

TABLE 10—TESTS FOR PREVIOUS PERIOD OUTCOME EFFECTS IN INDIVIDUAL DECISION PAIRS

Design Determinants	Subsamples Pair (1,7)	Pair (2,8)	Pair (3,9)	Pair (4,10)	Pair (5,11)	Pair (6,12)
	Experiment 1					
TW_{t-1}	0.00004986	0.000003469	−0.00004830	0.0001057	0.000007282	0.00009371
	(1.018)	(0.073)	(−1.044)	(1.785)	(0.176)	(1.360)
$OUTCM_{t-1}$	−0.629345	−0.523611	−0.247884	−0.545925	−0.02418	−0.12346
	(−0.608)	(−0.583)	(−0.294)	(−0.510)	(−0.030)	(−0.116)
D_{t-6}	0.362484	1.30338	0.987279	2.67356	0.437548	2.80971
	(0.418)	(1.605)	(1.147)	(2.099)	(0.528)	(2.205)
$\ln L$	−17.602	−18.621	−17.526	−13.021	−19.553	−14.771
$-2\ln\lambda$	1.4475	3.8113	3.1386	6.5534	0.32290	7.1107
$\chi_3^{2,0.95}$	7.81	7.81	7.81	7.81	7.81	7.81
N	30	30	30	30	30	30
	Experiment 2					
TW_{t-1}	0.0000195	0.0000261	0.000126	−0.0000429	0.0000424	0.0000370
	(0.565)	(0.986)	(1.201)	(−0.712)	(1.102)	(1.459)
$OUTCM_{t-1}$	0.2454	0.5322	0.9245	−13.640	−0.1464	−0.7698
	(0.247)	(0.615)	(0.693)	(−0.017)	(−0.107)	(−0.869)
D_{t-6}	1.312	0.1447	2.986	2.550	0.9821	0.3312
	(1.436)	(0.149)	(1.856)	(1.331)	(1.073)	(0.388)
$\ln L$	−18.763	−18.189	−9.822	−4.546	−14.980	−16.747
$-2\ln\lambda$	4.063	1.812	7.390	5.605	2.637	3.159
$\chi_3^{2,0.95}$	7.81	7.81	7.81	7.81	7.81	7.81
N	30	30	30	30	30	30

Note: L, λ, and N are defined in Table 4. Other notation in this table is defined in Table 7.

TABLE 11—TEST FOR FRAMING EFFECTS

Type of Question	Number of \$ Bet Choices	Number of P Bet Choices
	Experiment 1	
Direct Choice	87	91
Selling Price	86	93
	Experiment 2	
Direct Choice	89	91
Selling Price	77	103

tions in experiment 2 are essentially the same as in experiment 1. The only evidence of a possible framing effect in Table 11 is in the subject responses to the selling price questions in experiment 2. Compared to the other rows of the table, the last row reports a greater proportion of P bet choices. But the experiment 1 figures in Table 11 show no effect of framing and this experiment has a higher percentage of reversals than does experiment 2. We conclude that systematic framing effects cannot explain the choice reversals in our experiments.

VI. Interpretation of the Results

We have found that neither wealth nor outcome nor framing effects can account for the choice reversals in our experiments. Hence the results of our experiments have implications for both the psychologists' anchoring and adjustment theory and for economists' rational decision theory.

A. *Implications for the Anchoring and Adjustment Theory*

In earlier preference reversal experiments, most reversals were of one type: subjects were much more likely to state a preference for the P bet, and place a higher selling price on the \$ bet, than to make the other type of reversal. In those experiments, very few of the subjects who chose the \$ bet failed to order their prices consistently. Psychologists have used the anchoring and adjustment theory to explain why the one type of preference reversal that was commonly observed in earlier experiments was "expected" (that is, predicted by the theory) and the other type was not expected (Slovic and Lichtenstein, 1983, p. 597). According to this theory, in choosing between two lotteries a subject first anchors on probabilities of winning and then makes an insufficient adjustment for differences in payoffs. Furthermore, in deciding on selling prices a subject first anchors on payoffs and then makes an insufficient adjustment for differences in probabilities. Thus the theory postulates that the "message space" or "response mode" substantially affects subject decisions.

In our experiment 1, 45 percent of the 62 reversals were of the previously common type while 55 percent were of the other variety. Similarly, in experiment 2 only 54 percent of the 52 reversals were the expected type. Thus we do not find the predicted asymmetry in the reversals despite the fact that we ask our subjects to make choices in one type of question and to state selling prices in the other. Furthermore, the pattern of the responses reported in Table 11 does not support anchoring and adjustment. The first two rows reveal no effect of the type of question on subjects' responses in experiment 1. The experiment 2 data reveal a greater tendency for the subjects to place higher selling prices on P bets than to choose them in response to direct choice questions. This is the opposite of the pattern of results that is consistent with anchoring on dollar payoffs in selling price questions and anchoring on probabilities in preference reporting questions.

If the anchoring and adjustment theory is *not* to be contradicted by our results then there must be something that is essentially different about the way that we formulated our selling price questions *that can be explained by the theory*. The explanation might be based on our conjecture that most of our subjects realized that the particular numbers they stated for prices were irrelevant except for their relative magnitudes. This was evidenced by their comments and by their propensity to state prices such as 1,000 francs for lottery A and 999 francs for lottery B in any given (A, B) pair. However, if the anchoring and adjustment theory is to be immunized to the apparent falsifying evidence of our experiments, it will have to be extended to incorporate more than a message space explanation of choice reversals.

B. *Implications for Decision Theory*

As explained above, the results of the Grether-Plott (1979) experiments provide an empirical challenge to expected utility theory. However, the theoretical results of Holt, Karni and Safra, and Segal immunized more general decision theories to the results from the Grether-Plott experimental design by showing that it does not discriminate between subject violations of the independence and compound lottery axioms and much more problematic violations of axioms such as transitivity.

Our experiments were designed so that the independence and compound lottery axioms are not required to interpret the results. The results of our experiments differ dramatically from those reported by Grether and Plott and all of the other cited authors in that we do not find the pronounced asymmetry of the type of reversal that they reported. Taken together, the Grether and Plott results and our results are consistent with the hypothesis that subject violations of the independence axiom and/or the compound lottery axiom can account for the asymmetry in preference reversals that was observed in previous experiments. But that provides scarce comfort for decision theory because we observe about the same overall frequency of reversals (30–35 percent) as in previous research. Furthermore, the choice reversals in our experiments are violations of the asymmetry axiom, which is an axiom of decision theory that is even more fundamental than transitivity.[9]

[9]Asymmetry is more fundamental than transitivity in that it is possible to develop a choice model that does not include the transitivity axiom (Sonnenschein, 1971).

VII. Perspective on the Results

Grether and Plott (1979, p. 623) offered the following interpretation of the preference reversal phenomenon:

> *Taken at face value the data are simply inconsistent with preference theory and have broad implications about research priorities within economics. The inconsistency is deeper than the mere lack of transitivity or even stochastic transitivity. It suggests that no optimization principles of any sort lie behind the simplest of human choices and that the uniformities in human choice behavior which lie behind market behavior may result from principles which are of a completely different sort from those generally accepted.*

Subsequently, Holt, Karni and Safra, and Segal effectively immunized various generalizations of expected utility theory to falsification by the Grether-Plott and other preference reversal experiments previous to the ones reported in our paper. But our experimental design does not require the independence and compound lottery axioms to interpret the results and we observe a high frequency of choice reversals that are known to be violations of the asymmetry axiom. Must one now accept the Grether-Plott interpretation of the preference reversal phenomenon?

Results of individual choice experiments such as ours clearly have implications for the generality and applicability of economic theory. Experiments on preference reversal and Bayes' rule (David Grether, 1980, 1981) have consistently produced results that are incompatible with accepted models of rational choice.[10] But what are the implications for theory? In contrast to the individual choice experiments, there is a large literature that has obtained results in *market* experiments that are generally consistent with the market allocation implications of individual choice theory (Vernon Smith, 1982, 1986). This pattern of results could have either of two, quite different implications. On the one hand, it may turn out that we do, indeed, need to develop fundamentally different approaches to decision theory in order to understand market behavior. On the other hand, it may be that our traditional models are incomplete but not fundamentally flawed. Recalling the traditional "as if" interpretation of economic theory, it may be the case that the informational and disciplining properties of economic institutions cause real economic agents to learn to behave as if they are like our theoretical agents. Two related lines of research could provide the answer. One would consist of theoretical research on the process by which economic institutions mold the characteristics of agents. The other would consist of an experimental economics research program combining suitably paired nonmarket and market choice experiments. James Cox and Mark Isaac (1986) analyzed these questions at length and provided an outline of an experimental research program that could eventually provide empirical support for either the "replace it" view of economic theory or the alternative "complete it" view.

[10]A notable exception to this pattern is provided by the experiments by Cox and Ronald Oaxaca (1988) on search models. They found that a finite horizon (dynamic-programming) search model generally predicted subject behavior quite accurately.

APPENDIX

This Appendix contains the complete instructions (and the referenced Figure 0) given to the subjects in experiment 2. The instructions for experiment 1 were the same except for a reversal of the positions of the "win 8,000" and "lose 1,000" areas in Figure 0. The Appendix also contains two sample paired questions (Items 1 and 7) and the corresponding Figures 1 and 7. Each subject was asked 12 questions (Items 1–12) that alternated between selling price and direct choice questions. The selling price and direct choice questions in each pair were separated by five intervening questions. Thus the sample Item 1 is the selling price question for lottery pair 1 in Table 2. The sample Item 7 is the direct choice question that is paired with Item 1. Complete copies of the instructions and all questions (Items 1–12) and figures used in both experiments are available upon request to the authors.

INSTRUCTIONS

The experimenters are trying to determine how people make decisions. We have designed a simple choice experiment, and we shall ask you to make decisions in each of several items. Each decision you shall make will involve two bets. When a bet is actually played, one ball

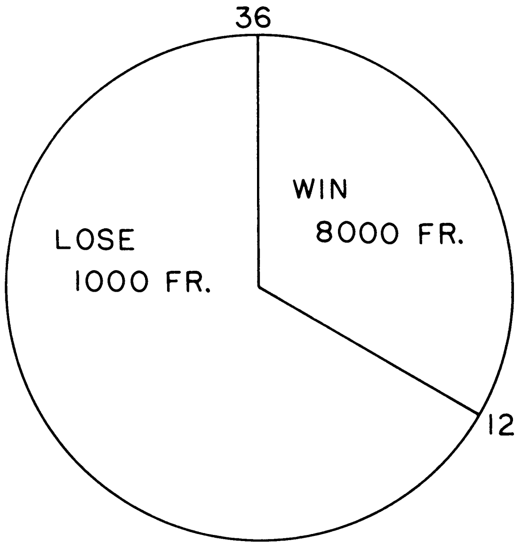

FIGURE 0

will be drawn from a bingo cage that contains 36 balls numbered 1–36. Depending upon the nature of the bet, the number drawn will determine whether you lose an amount of money or win an amount of money. Consider, as an example, the bet represented by Figure 0. If you play this bet, you will win 8,000 francs if the number drawn is less than or equal to 12, and you will lose 1,000 francs if the number drawn is greater than 12. For the purpose of this experiment, one U.S. dollar is equal to 4,000 francs.

We are going to ask you to make two basic types of decisions regarding the bets which are presented.

Decision Type I. When making this type of decision, you will be presented with two bets. Then you will be asked which bet you would prefer to play, and you will simply play the bet which you have selected immediately following your choice. If you do not care which bet you play, merely indicate this in the space provided, and the bet you play will be determined by a coin toss.

Decision Type II. When making this type of decision, you also will be presented with two bets. Then you will be asked to state the smallest price for which you would sell each of the bets. Once you have stated the two selling prices (one for each bet), we will pay you 1,000 francs for the bet to which you have given the lower selling price. Then you will be allowed to play the bet to which you have given the higher selling price. If you state the same selling price for the two bets, the bet you sell to us will be determined by a coin toss.

Further Explanation. To begin, we are giving you an endowment of 20,000 francs, or $5.00. If you were to lose each and every bet you play, you could lose $4.50. Therefore, if you should lose every bet, you will still have at least $.50. This amount is your minimum possible payment for participating in this experiment. Your maximum possible payment for participation is $53.50.

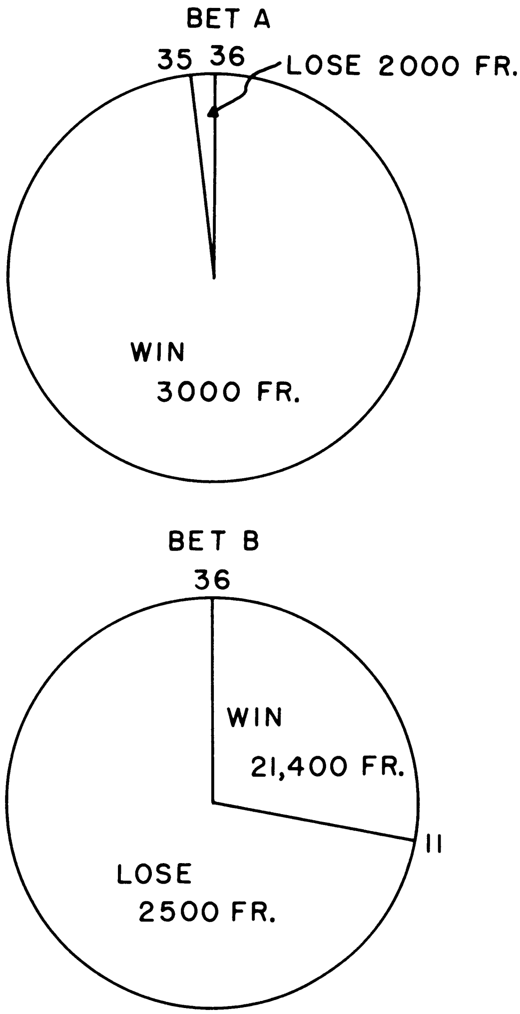

FIGURE 1

Do You Have Any Questions?

Item 1: Consider carefully the two bets shown in Figure 1.

What is the smallest price for which you would sell the opportunity to play each of these bets? We will then pay you 1,000 francs for the bet to which you have given the lower selling price, and you will play the bet to which you have given the higher selling price. If your two prices are the same, the bet you sell to us will be determined by a coin toss.

Smallest Price for Bet *A*:____ Win/Loss____
Smallest Price for Bet *B*:____ +1000 Fr. =____

Item 7: Consider carefully the two bets shown in Figure 7.

You will have the opportunity to play one of these bets. Make one check below to indicate which bet you

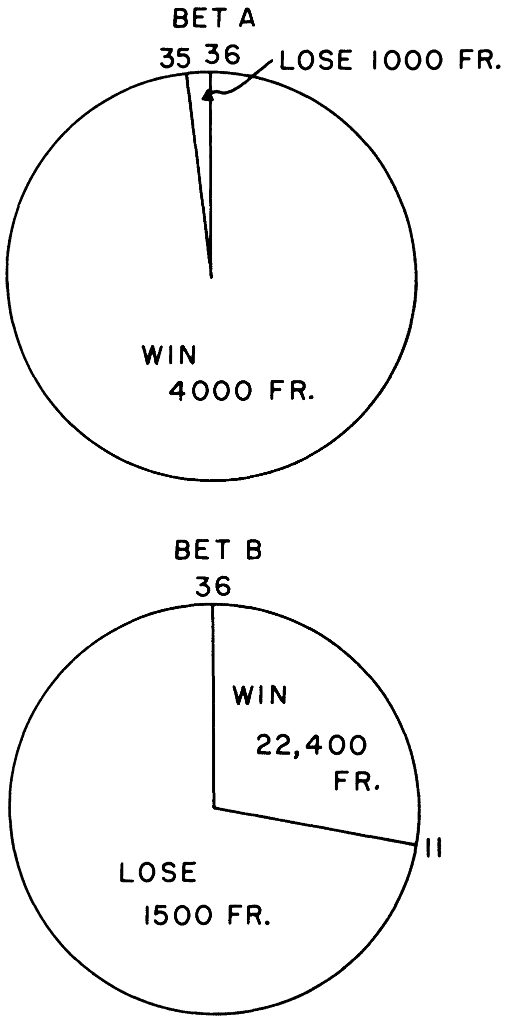

FIGURE 7

would prefer to play. Then you will play the bet you have selected. If you do not care which bet you play, the one you play will be determined by a coin toss.

Bet *A*:____
Bet *B*:____
Do not Care:____ Win/Loss____

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