Sunk and Opportunity Costs in Valuation and Bidding*

OWEN R. PHILLIPS
University of Wyoming
Laramie, Wyoming

RAYMOND C. BATTALIO
Texas A&M University
College Station, Texas

CARL A. KOGUT
University of South Florida
Tampa, Florida

I. Introduction

Studies have shown that when individuals make decisions they often do not ignore sunk costs.[1] Richard Thaler [19] and Arkes and Blumer [1] show in their experiments that commitment to an endeavor is strengthened after sunk costs are paid. Generally, such behavior is explained by Kahneman and Tversky's [11] prospect theory, in which individuals edit and evaluate choices using different mental rules. They model individual behavior with a value function that acts as a filter, weighing potential losses differently than potential gains. In this filter utility is defined on gains and losses, not the final wealth outcome. People dislike a loss so much that in an uncertain environment the prospect (perceived value) of a loss is not offset by the prospect of an equivalent gain. But after a loss is incurred, for example through the payment of a sunk cost, further investment is easier to make in order to obtain a gain. The value function is convex for losses and concave for gains.

As presented in McKean [16], Kahneman and Tversky also argue that the way a problem is presented to an individual affects a person's decision. The presentation is called a *frame*.[2] Frames

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1. For our purposes a sunk cost is a payment made or committed as a result of an earlier decision. Once committed or actually paid, subsequent decisions should no longer be influenced by this cost, but would instead be based on a comparison of marginal costs and marginal benefits. We will often refer to sunk costs as by-gone expenses.

2. See Tversky and Kahneman [21] and Machina [15] for a thorough list of citations to framing, filtering, and prospect theory.

can vary in their level of *transparency* which measures how obvious costs and benefits appear to an individual. If a problem is transparent, an individual is more likely to make the "correct" decision. In particular, if a sunk cost has a transparent frame, it is likely to be ignored. Direct, out-of-pocket expenses are generally more transparent as losses than indirect, opportunity costs.[3] As a result, opportunity costs, which may be sunk, are often valued as smaller losses relative to equivalent direct costs.[4] Evidence generated by Kahneman and Tversky [11] and others shows that framing and the resulting transparency is an important determinant of how costs enter a decision.

Economic theory teaches that a rational decision-maker should ignore sunk costs at the margin, and that opportunity costs should be equivalent to out-of-pocket expenses. Despite the inconsistencies in behavior suggested by the above literature, this evidence has not been embraced by economists. Cox and Isaac [7], for example, are skeptical because much of this evidence exists outside a market environment. Many times the empirical evidence from the behavioral literature is the tallied responses of subjects to hypothetical situations with no economic incentive to make the best decision. Better tests would place individuals in a market that compensated them for making decisions over several choice periods. It is widely believed that repetition affords learning, and market competition disciplines those who make incorrect choices. Irrational behavior cannot persist under these conditions.

In rebuttal, several economists and psychologists have used markets to replicate some of the experimental results in the behavioral literature. For example, Grether [9] has found that when choices have uncertain outcomes individuals often violate the Bayes rule assumption. Knetsch and Sinden [13] and then Knetsch, Thaler, and Kahneman [14], using market settings, have demonstrated a continued discrepancy between an individual's maximum willingness to pay for a good and the amount he or she demands as minimum compensation for the same good. Grether and Plott [10] have examined the preference reversal phenomenon and found that it exists even after the introduction of a monetary reward for decisions. Thus, economists cannot safely conclude that irrational behavior observed in some experiments will disappear when experimental conditions more closely resemble markets.

In this paper we present the results of three economic experiments in which subjects make payments and are paid for the decisions they make. These experiments are designed to examine empirically whether decision makers ignore sunk costs in selected contexts, and whether or not an implicit opportunity cost is perceived as equivalent to an explicit direct cost. The experiments place subjects in two distinct choice environments with different manifestations of by-gone expenses. In the first set of experiments, two lottery designs ask subjects to value identical lottery tickets. In a second series of experiments subjects are placed in a competitive auction environment.

3. See Becker, Ronen, and Sorter [4] and Neumann and Friedman [17]. Their results found that MBA students evaluated income statements more favorably when some costs were indirect as opposed to direct outlays.

4. In studies done by Knetsch and Sinden [13], Knetsch, Thaler, and Kahneman [14], Coursey, Hovis, and Schulze [6], and Brookshire and Coursey [5], subjects put different values on objects depending on whether they were asked to buy an object they did not possess or sell an object they already possessed. When they bought, the question was "How much are you willing to pay?" When they sold, the question was "How much are you willing to accept?" Paying is a direct expense while deciding not to sell presents the subject with an opportunity cost. Knetsch, Thaler, and Kahneman [14] found that when individuals were endowed with a commodity, willingness to accept (WTA) was consistently greater than willingness to pay (WTP). This difference in valuation has been termed the endowment effect. In general under a variety of market conditions, WTA is generally higher than WTP. In one-shot valuation trials the ratio WTA/WTP on the mean valuations among subjects ranges from 4.0 to higher values. With repetition the ratio falls, but remains greater than 1. In the three experimental designs described by Coursey, Hovis, and Schulze [6], all with repetition, this ratio was significantly greater than 1 except when valuations were made in an auction environment. This is evidence that opportunity costs are undervalued relative to direct expenses. For more discussion see Kahneman, Knetsch, and Thaler [12].

In the two lottery experiments tickets with identical expected payoffs are valued as ticket prices change. Since the lottery tickets have equal expected values, the subjective value of each should not change with its price; the ticket price should be treated as a sunk cost. In the first lottery experiment subjects are given several tickets to evaluate. The tickets are presented simultaneously, so the experiment does not have repetitive choice periods. It is a "one-shot" decision environment. In this setting one-half of the subjects do not ignore sunk costs. A second lottery experiment presents the subjects with a *sequence* of lottery tickets to value at different prices. This experiment allows the opportunity for learning, as well as maintaining monetary incentives. This design results in about one-fifth of the subjects not ignoring sunk costs.

In the auction experiments subjects enter bids for a fictitious commodity with the option of paying an admission fee to bid in a second market. The admission fee is presented as an explicit fee part of the time and as an implicit fee the rest of the time. If sunk costs do not matter—in this case the admission fee to the second auction market—there should be no difference in bidding behavior across the two markets, and if explicit direct costs are perceived the same as implicit opportunity costs there should be no differences across the two representations (or frames) of the admission fee. In the auction market only 5 percent of the subjects do not ignore sunk costs, but a significant number of subjects view opportunity costs and direct costs differently.

We chose these two designs because they capture the contextual aspects that economists have contended are necessary for a credible test of behavior, namely a monetary reward structure, the opportunity for learning, and the discipline of an economic market institution. The first lottery experiment was designed to observe choice behavior when there was just an explicit monetary reward. There was no repetition for learning and no market interaction between subjects. When we discovered that half of the subjects were not ignoring sunk costs, we then went to a sequential lottery design that allowed for learning. The auction design was decided upon in advance because it satisfied all three criticisms of earlier tests. In the auctions, subjects were paid; they had repeated bidding opportunities; and were placed in a market environment. The next two sections discuss our experimental results. The two lottery designs are presented first. Behavior in the auction experiments is analyzed separately in section III.

II. Lottery Experiments

The two lottery designs are presented and the data are analyzed in this section. We discuss the one-shot or one-time design in part A and then the sequential lottery in part B.

One-Time Lottery

Two one-time lottery sessions (L1 and L2) were conducted at Texas A&M University using students from beginning economic classes. There were 34 subjects in the first experiment and 41 subjects in the second. Each subject was given an initial balance of $6.00, set high enough so that the lowest possible amount earned during a session was $5.50. The sessions lasted about an hour and a half, and average earnings were slightly more than $10.00.

Each session followed precisely the same procedures. To begin the experiment subjects were seated in a reserved classroom where talking between students was not permitted. Written instructions, which were a description of how the lottery would be organized, were provided to everyone and read aloud. At the end of the instructions, students were presented with three hypothetical cases on the valuation process and then completed a questionnaire to check understanding. Correct answers to the questions were discussed thoroughly by the experimenters. (These instructions, as well as those for the sequential lottery and auction experiments, are available upon request from the authors.)

In these experiments each subject was allowed to hold as many as six identical lottery tickets. The tickets, labeled A1 through A6, were presented in sequence and increased in price from $0.00 to $2.50. Only one of the six lotteries however, was randomly played at the end of the experiment, and it was only in this lottery that the ticket price was actually collected. However it was made clear to all subjects that they were obligated to pay the price of the ticket chosen for payment. Hence accepting a ticket committed them to paying the price. The final payoff to any ticket was determined by rolling a six-sided die. If a 1 was rolled payment was $2.00, a 2 paid $3.00, and so on, with a 6 paying $7.00. The expected payoff for all tickets was constant at $4.50.

Since subjects knew in advance they would only pay the fee attached to the ticket chosen at the end, most realized that it was in their best interest to take all of the tickets. One ticket did present a possibility of losing money. Its price was $2.50 and there was a 1/6 probability that $2.00 could be paid in the lottery. Apparently because of this potential loss three subjects refused this ticket.

To get an accurate reading of how subjects valued the six tickets, attached to each ticket was a dollar and cent scale calibrated in five-cent increments going from $0.00 to $9.99. Once all the tickets were distributed, subjects were asked to indicate the place on the scales at which they would be indifferent to a cash payment or keeping the ticket. Incentive control during this phase of the experiment was maintained by following the Becker, Degroot, and Marshack [3] procedure for compatible value revelations, hereafter BDM, employed also by Grether and Plott [10].[5]

When all of the scales were marked, a six-sided die was thrown to decide which lottery ticket would be played. The price of this ticket and the value chosen by the subject were recorded on a record sheet. The BDM procedure then went as follows: all value scales were collected and three different 10-sided die were thrown to determine a random dollar and cent figure, which we shall call the BDM amount. One die was for the penny amount, one for the dime, and the third for the dollar value. Subjects who placed a value on the scale below the BDM amount were paid the greater BDM amount for the ticket. Subjects who marked a greater value on the scale were paid a value decided by the roll of a six-sided die. Several advantages of this experimental design are recognized. Importantly, paying only one ticket at the end minimized the possibility that subjects would build a portfolio across the lotteries to reduce the variance of expected earnings. Our purpose was to make subjects consider the cost of each ticket separately rather than viewing the tickets as a bundle over which they maximized expected earnings. Also, since the subjects held all the tickets they wanted before any were valued, and because they could go back and change their valuations before the tickets were collected, this design eliminated the possibility of (i) a balance effect,[6] (ii) variation in the method of valuing tickets, and (iii) any impact on valuation

5. This procedure was described as part of the lottery played for the ticket selected for payment. Subjects were told that once they decided on a value from the scale, the experimenter would randomly choose the penny, dime, and dollar amount on the scale. If this random amount was larger than a subject's value, they were paid this amount and gave the ticket back to the experimenter. If it was less, subjects kept the ticket and a die was thrown to determine its payoff according to the provided schedule. See Becker, Degroot, and Marshack [3] and Grether and Plott [10] for more discussion of the procedure.

6. Since all tickets were valued simultaneously, initial wealth levels were held constant. Conceptually we could have controlled for wealth effects by holding final wealth levels constant or by controlling the changes in wealth. Neither of the two alternatives, however could be implemented and still allow subjects to make non-trivial choices. We therefore could only control the initial wealth levels.

possibly caused by the ordering of tickets. Two disadvantages of this design, however, are that there was no opportunity for learning, and that by randomly choosing a ticket and then deciding the ticket's outcome confronted subjects with a compound probability. Carefully considering their expected earnings may have burdened and confused some subjects.

We draw attention to the way the sunk cost was collected. The price of the ticket was not collected at the time subjects decided to hold a ticket, even though they were informed that they were committed to paying the price if they took the ticket and it was selected for payment. We argue this "contractual" commitment represented a sunk cost even though the price was not literally collected until the end of the experiment. We believe that the physical act of paying the price at the time of valuation would have made it more transparent that the price was truly a bygone expense, but collecting and then refunding the money on the tickets not chosen for payment would have burdened the procedure and possibly led to confusion.[7] Regardless of how the price was collected, *to maximize expected earnings subjects should have placed identical values on all of the tickets they held* and treated the ticket price as sunk.[8]

We found that in the one-shot design most subjects did not ignore the individual ticket price when they decided upon a ticket value. Combining the data from both experiments, 17 subjects (22.7%) placed exactly the same value on all tickets, relative to the ticket whose price was zero; 21 subjects (28%) placed a higher value on all of the tickets with a non-zero price, and 24 subjects (32%) placed a lower value on all of the tickets that had a non-zero price. The remaining 13 subjects showed no consistent relationship to the zero price. The probability of observing all five positive priced tickets valued above (or below) the value of the zero priced ticket is .03 if values were randomly selected.

To help determine whether the recorded values followed a systematic pattern across tickets, the following regression was run for each subject in the two experiments:

$$Z_i = \alpha + \beta P_i + \epsilon_i \quad i = 1, \ldots, 6 = \text{number of tickets selected},$$

where Z_i is the value recorded by the subject for the ith ticket, and P_i is the price of the ith ticket. It is assumed that the random disturbance term ϵ_i has zero mean and finite variance. A coefficient estimate of β significantly different from zero would indicate that a subject systematically changed the value assigned to each ticket depending on the price of the ticket, while an estimate of zero would indicate the value assigned to each ticket either did not change or did not systematically change relative to the price of each ticket.

7. Richard Thaler has pointed out to us that subjects might feel compelled to value tickets differently since they are priced differently. He noted that subjects may find it hard to believe that the experimenters would expect them to place the same value on each ticket. However, we were very careful not to encourage any kind of behavior. Additionally, changing values would clearly be non-optimizing behavior and represents to us an avenue by which sunk costs affect behavior.

8. Assuming no wealth effects, the value of the lottery ticket will be independent of its purchase price, and depend only on the parameters of the lottery. The reasoning is outlined as follows. Let $L_i = (p_i, A_i, B_i)$ denote a two outcome lottery with probability p_i of winning A_i and $(1 - p_i)$ of winning B_i. If ticket L_1 is preferred to L_2 then $E[U(L_1)] > E[U(L_2)]$, where $E[\,]$ is expected utility. We assume constant absolute risk aversion such that $r(x) = -U''(x)/U'(x) = k$. This implies the utility function is $U(x) = -e^{-kx}$, $k > 0$. Given this utility function the value of the lottery (v_i) is

$$v_i = (1/k)\ln\{E[U(L_i)]\} \quad i = 1, 2, \ldots n.$$

If the lottery tickets are individually priced, the value of the ith ticket should be independent of price.

Table I. Signs of the Estimated Coefficient, Individual Subject Regressions

Experiment	−*	−	0**	+	+*
L1	7	5	11	2	9
L2	10	7	6	7	11
Total	17	12	17	9	20

*Significant at the .05 level.
**These subjects placed the same value on each ticket.

Table II. Risk Preference and Coefficient Estimates

Panel 1: Significant Estimates

Classification	Negative	Positive	Total
Risk Loving	10	3	13
Risk Neutral	5	3	8
Risk Averse	2	14	17
Total	17	20	37

$\chi^2 = 13.10$

Panel 2: All Estimates

Classification	Negative	Zero	Positive	Total
Risk Loving	19	9	8	36
Risk Neutral	6	3	3	12
Risk Averse	4	5	18	27
Total	29	17	29	75

$\chi^2 = 15.11$

The results of these regressions are presented in Table I.[9] It is interesting to note that the 50% of subjects with significant coefficients were about evenly split between lowering their value relative to the price of the ticket or raising their value. These results allow us to identify three types of responses to a sunk cost. As the sunk cost of acquiring an asset increased, 23% valued a ticket less, 27% valued it more, and 50% did not systematically change their valuation.

Panel 1 of Table II presents the relationship between the significant coefficient estimates of Table I and each individual's risk taking behavior. We define an individual as being "risk loving" if they place a value greater than $4.50 on the ticket whose price is zero. We make the interpretation that if the selling price of a gamble is greater than its expected value, the individual behaves as a risk lover. Similarly, if the value determined by the individual is less than $4.50, the subject is "risk averse", and a value equal to $4.50 would indicate the individual was "risk neutral".

9. We also tested the pooled model:

$$Z_{ij} = \alpha_j + \beta_j P_{ij} + \epsilon_{ij} \quad i = 1, \ldots, 6 = \text{number of tickets}$$
$$j = 1, \ldots, n \quad n = 34, 41.$$

The error terms are assumed to be identically distributed with zero mean and finite variance across subjects. In this model, β_j was significant (at the .05 level) for 44% of the subjects. As the price of the ticket increased, 24% valued it less and 20% valued it more. The remaining 56% of the subjects did not significantly change their valuation.

After organizing the data this way some striking results emerge. Of the seventeen individuals who had significantly negative coefficient estimates, 10 of them (59%) are risk loving; and of the twenty individuals who had significantly positive estimated coefficients, 14 (70%) are risk averse. More generally, if we consider all the negative coefficients (those significant as well as those not significant), 66% of the subjects are risk loving, and 62% of the positive coefficient estimates can be attributed to individuals who are risk averse. Panel 2 of Table II shows the complete breakdown. The chi-squared test statistic for independence between risk preference and the sign of the estimated coefficient is, for the significant coefficients, 13.10 and for all the coefficients, 15.11. Both test statistics are significant at less than the .01 level.[10]

These results indicate that there is a correspondence between individual responses to sunk costs and risk preference. If an individual is risk loving, he or she is likely to think of sunk costs in such a way that they "net out" the amount of the expense from the expected value of the ticket. An individual who is risk averse is likely to value something more highly because the cost is paid. In the lottery they believed that since the price was higher the ticket must be worth more.

Those who valued the tickets less as the price increased showed *less* commitment to the endeavor as sunk costs rose. This behavior has not been observed in the earlier experimental literature. Casual interviews with these subjects exhibit the belief that since they could win less, net of the ticket price, the ticket was worth less. There were also those who valued the tickets more as the price went up. From talking to these subjects, a higher priced ticket had a higher value because if it were chosen for payment, they would need more to "sell it back" to the experimenter, even though they knew that it had the same potential payoff as the lower priced tickets. This behavior is consistent with earlier observations that commitment increases in the face of sunk costs. These two forms of behavior, distinct from that which ignores sunk costs, are not random observations. Indeed, nearly half of the subjects changed their valuation of the ticket as the price of the ticket changed.

It is possible that by choosing only one ticket for payment we caused some subjects to treat the ticket prices as if they were sunk even though they did not view them this way. Because only one ticket price was actually paid, ex ante subjects may have thought of this price as a random variable. Subjects knew the probability distribution of ticket prices, and from this they could calculate an expected ticket price of $1.25 over all six tickets.[11] A subject could be adding or subtracting a constant from their BDM values and it would appear that they were ignoring ticket prices in their valuations. Hence in this one-shot design more than half the subjects could be treating the ticket price as something other than sunk.

Sequential Lottery

We found the above results surprising. Apparently a monetary reward for decisions was not suffi-

10. A linear regression of the individual slope coefficients for all subjects on the measure of risk-loving or risk-aversion (value of the zero priced ticket—$4.50) results in an insignificant intercept and a significant slope ($\alpha < .01$). These results give the same qualitative results as those reported in Panel 2 of Table II.

11. We thank an anonymous referee for pointing this out to us. Taking the argument one step further, if the expected ticket price is $1.25, then the expected payoff is the difference between this price and the expected ticket value of $4.50, or $3.25. In Table I, of the 75 subjects in the one-shot lotteries 17 subjects put identical values on their tickets. None of these subjects valued all their tickets at $3.25. However in the whole group of 75, 25 (33%) of the subjects did not have a mean valuation significantly different from $3.25 at the .05 level. Even though these subjects did not necessarily ignore the ticket price, their valuations may have fluctuated around the $3.25 level. Table I also shows 38 subjects without a significant price coefficient, only 5 (13%) of these people had a mean valuation not significantly different from $3.25. Thus, $3.25 does not appear to be a focal point in subject valuations.

cient in the one-shot lottery design to make subjects ignore sunk costs. It occurred to us that along with payments and rewards, subjects may need a sequence of periods to learn optimal behavior.

A sequential design that repeated payment and the valuation several times, was constructed for two reasons. First, the one-time lottery design made it easy for subjects to use the "net out" rule discussed earlier. It was tempting to judge a ticket's value by the difference between its expected value and its price, especially when the price had not been physically collected yet. Second, a sequential design would let subjects learn. An unavoidable side effect however, is that the decision to buy and value one ticket may not be independent of the other tickets in the sequence. The sequential design we describe below is not directly comparable to the one-shot lottery experiment because sunk costs are framed differently. As we explain below, subjects in the sequential lotteries literally pay for a ticket before they value it. The physical act of handing over the money makes the payment certain and reinforces its loss. Collecting the ticket price at the end of the one-shot lotteries may have caused subjects to not see the ticket price as a sunk cost.

The specific steps in this experiment were ordered so that subjects, if they wanted the first lottery ticket, paid the experimenter on the spot from a beginning balance of \$2.00. Subjects were under no compulsion to buy this ticket or any ticket, and some did not from time to time. For those who made a purchase, when the money was collected, a ticket along with a value scale, ranging from \$0 to \$2.00, was provided. At this point the procedure described in the one-time lottery was followed. Buyers were asked to find a value on the scale for which they were indifferent between keeping and selling the ticket. This amount was recorded on their record sheet. A dollar and cent amount was then randomly picked by the experimenter. Those subjects with values below this amount were immediately paid the experimenter's choice, and those above were immediately paid the random value determined by the roll of a six-sided die.

Of the twenty times this sequence of events was repeated, two different payment schedules for lottery tickets were used. The six-sided die determined the first schedule. It paid \$.50 for a one on the die, .70 for a two, .90 for a three, and so on. This schedule was used for 14 tickets, with the first two tickets acting as practice trials. During the two practice sessions subjects were given a \$1.00 balance in *play money*. Subjects paid, and were paid, in a plastic currency with no other change in the instructions. Real money was used for the remaining 12 tickets. Ticket prices ranged from \$0.00 to \$.55 in \$.05 increments, the order in which the tickets were offered was randomly chosen by the experimenters beforehand.[12]

Care was taken to emphasize that each ticket represented a set of decisions independent of previous tickets. Subjects, in particular, were not told how many tickets would be offered. Nor did they keep a running balance of their earnings. They could mentally subtotal their earnings, but it was never recorded during the experiment, which moved quickly after the practice sessions.

The last 6 of the 20 tickets in the experiment had a certain value of \$1.00. This was because regardless of what came up on the six-sided die, the ticket paid a dollar. In essence, the ticket amounted to buying a one dollar bill. Subjects were asked to place a value on the certain lottery ticket as the price varied randomly from \$.00 to \$.50 in ten cent increments. Once again, no one was told how many tickets would be offered for sale. At the end of the experiment, earnings were added and a receipt written to subjects for their cash balance.

This sequential design was conducted twice, first with beginning economic students from the University of Wyoming and then with similar students at Texas A&M University. Each ex-

12. The order of ticket prices was: \$0.15, .35, .30, .55, .20, .00, .45, .25, .10, .50, .05 and .40. Thus, ticket prices and balance were not related.

Table III. Signs of the Estimated Coefficient β_1, Individual Subject Regressions

Periods	−*	−	0**	+	+*
RANDOM	1	12	3	19	7
FIXED	0	13	18	11	0

*Significant at the .05 level.
**These subjects placed the same value on each ticket.

periment lasted a little longer than 2 hours and subjects earned an average of $17.75. There were 16 subjects in the experiment at Wyoming and 26 at Texas A&M.

To analyze the results of these experiments we estimated the following regression for each subject:

$$Z_i = \alpha + \beta_1 P_i + \beta_2 PER + \beta_3 BAL + \epsilon_i, \quad i = 1, \ldots, T, \ T = 12, 6.$$

The value of the ith ticket as recorded on the scale for the subject is Z_i, the ticket price is P_i, the period in the sequence is *PER*, and the subject's balance (unrecorded) is *BAL*. The above regression was also estimated using only the data from periods 4 through 12. We looked at this subset of periods to see if behavior was different after some learning time. We found no qualitative difference in the estimates for the two data sets with or without the first four periods. In both estimations *PER* was significant for only a handful of subjects. Thus, if any learning occurred, it must have occurred during the practice periods.

Of the estimated coefficients for balance and period, a total of 4 out of 42, or approximately 10%, were significantly different than zero at the .05 level, and a total of 11 out of 42, or approximately 26%, were significantly different than zero at the .10 level. Thus, we chose to leave the variables balance and period in the model.[13] Table III reports the results for the estimated coefficient on price, β_1. Separate regressions were estimated for those periods the ticket payoff was random and for those periods the ticket payoff was certain at $1.00.[14] The table reports the results as RANDOM and FIXED.

Looking first at the results for the random periods, we see that there is only 1 (2.5%) significantly negative coefficient and 7 (17%) significantly positive coefficients, for a total of about 19% of the subjects. These results can be contrasted with the one-time lottery experiment. In the previously discussed lottery experiment nearly 50% of the estimated coefficients were significantly different than zero, with about an equal number of negative and positive coefficients. The sequential design clearly caused a decrease in the number of significant negative coefficients. Repetition and the collection of the price before the valuation of the ticket appears to have eliminated any strong tendency to net-out the price and thereby value the lottery ticket less as the price increases. The percentage of subjects that are classified as risk loving has also decreased from 48% in the one-shot lottery to 24% in the sequential design. Although we do not have any explanation for why the differences in design should give this result, the decrease in risk-loving behavior is consistent with the decrease in significantly negative coefficients.

13. We also conducted the analysis that follows without the balance and period variables and found only very slight differences. The totals for the two experiments do not differ, although we lose a positive significant estimate for the Wyoming experiments and gain a positive significant estimate for the A&M experiments.

14. We also ran a pooled regression similar to the one noted above and found no significant differences from these results. We emphasize that the data are being fit to the slopes of the regression equation, making it unlikely that these results are caused by subjects misunderstanding the instructions.

Table IV. Risk Preference and Coefficient Estimates

Panel 1: Significant Estimates

Classification	Negative	Positive	Total
Risk Loving	1	0	1
Risk Neutral	0	0	0
Risk Averse	0	7	7
Total	1	7	8

$\chi^2 = 8.00$

Panel 2: All Coefficient Estimates

Classification	Negative	Zero	Positive	Total
Risk Loving	9	0	1	10
Risk Neutral	4	2	6	12
Risk Averse	4	1	15	20
Total	17	3	22	42

$\chi^2 = 16.02$

We note that the overall correspondence between the sign of the coefficient and risk preference is still present. The significantly negative coefficient is attributable to a subject who is risk loving, and the significantly positive coefficients can be attributed to subjects who are risk averse. Furthermore, the correspondence between all the coefficients (both significant and not significant) and risk preference is again present. Panels 1 and 2 of Table IV present the results. The χ^2 statistics are 8.00 and 16.02, again significant at the .01 level.[15]

Returning to Table III, during the six periods the lottery ticket had a fixed payoff there appears to be no sunk cost effect. For these data 43% of the subjects placed exactly the same value on each ticket;[16] there were no significant coefficients, and the number of positive and negative estimates were approximately equal. During these periods subjects seem to have realized that the value placed on the ticket and its price should be independent. Since the sunk cost effect disappears during the fixed payoff periods, we believe that the uncertainty of the ticket value during the random periods makes the decision process less transparent.[17]

In general, we believe the separation of the payment and valuation decisions in this sequential design made the problem more transparent to the subjects, and hence they were more likely to ignore a sunk cost. Furthermore, when ticket prices were fixed it was even more obvious to the subjects that the value of the ticket should be unaffected by its price. The more transparent the problem became, the more likely it was that subjects ignored sunk costs. Nevertheless, when ticket prices did change we found that approximately 19% of the subjects did not ignore by-gone costs when they make decisions at the margin. These subjects were of a certain type. As prices increased, so did their valuation of a ticket. This behavior is closely tied to risk aversion.

15. Regression results similar to those reported in footnote 10 were also obtained for the sequential lottery.

16. The subjects who did not value the tickets at exactly $1.00 randomly assigned values between .95 and 1.05. Although this pattern of choices did not yield any significant coefficient estimates, it may indicate that subjects do not have a complete understanding of the BDM procedure when they are close to their valuation of an object.

17. See Tversky and Kahneman [20; 21] for a discussion of transparency and opaqueness. In short, the more obvious a problem is, the more likely it is that the subject will choose the "correct" decision.

III. Auction

In the auction experiments we look at behavior with sunk costs in a competitive market environment. Valuation may be affected by sunk costs because there is uncertainty in these experiments. However, it is not the same kind of uncertainty that was present in the lottery experiments. In the lotteries, uncertainty literally arose from the roll of a die. When setting a value subjects knew the distribution of ticket payoffs, but not the ticket value. In our auction experiments the value of the auctioned object was always known. The uncertainty came from the random assignment of resale values. The value of the object was fixed, but individuals did not know if they could obtain the auctioned object since they were bidding against people with different resale values.

Four auction experiments proceeded in the following manner. Subjects entered one sealed bid for a fictitious commodity. The highest bidder received the object after paying his or her bid, and then resold the object to the experimenters at a pre-assigned resale value.[18] These auctions were conducted with the aid of a PRIME computer at Texas A&M University using undergraduate students from beginning economics classes. Ten subjects participated in each experiment. During a session, the ten subjects were divided into two markets of five, where members of each group would submit sealed bids for the purchase of their group's fictitious commodity. (We will refer to this market as market A.)

The experiment's instructions were distributed to each individual at the start of a session and read aloud. Subjects were told that if they were the winning bidder in their market they would be paid the difference between their resale value and their bid. Subjects were also told that bids were accepted only if they were greater than or equal to zero, and less than or equal to their resale value for that period. The instructions included two examples of the procedure, as well as a sample record sheet which was completed using the information from the examples.

A second market, which we call market B, was open to the subjects for bidding. This market was limited to five of the ten bidders. If more than five subjects wished to participate, five would be randomly chosen by the computer. If less than five asked to participate, the market did not form.[19] A fee was imposed if a subject actually participated in this market. The fee varied across experimental sessions and periods. Table V provides a summary of the fees charged in these markets. The "Fee Type" column shows that the charge was imposed in one of two ways. A direct charge made those entering the optional B market pay an out-of-pocket amount, shown in the fourth column of Table V, from their balances. An opportunity charge paid subjects the same amount to *not enter* the market. At the start of a market period each person was randomly assigned one resale value to be used in both markets during that period. The resale values were drawn from the uniform interval [$0.10, $27.10]. This range of values was chosen to keep the expected risk neutral profit at $0.40 per period.

A sunk cost is borne by subjects who enter the optional B market, but optimization theory shows that individuals should ignore the entry fee when calculating their bid.[20] If the subjects did

18. See Vickrey [22] for an analytical solution of an optimal bid function to first price, sealed bid auctions.

19. A similar type of dual market experiment was examined by Battalio, Kogut, and Meyer [2]. They found that the technique did not influence subjects' bidding behavior. Very few subjects (10–15%) opted to stay out of the B market for the duration of the experiment. For some it may have been a perception of always having low resale values. It may be that the entry fee—direct or indirect—kept them from entering. The decision not to enter is not a sunk cost effect. We did not explore the reasons for never entering the B market, since our focus was primarily on behavior once the fee was paid.

20. It can be shown that in the absence of wealth effects, in the standard Vickrey model [22] the optimal bid should depend only on the resale value and the number of other bidders in the market. See also Cox, Roberson, and Smith [8] for a discussion of bidding behavior for individuals who are not risk neutral.

Table V. Auction Market Experiments: Summary of Treatments

Experiment	Fee Type	Condition*	Fee Amount in the B Market	Periods
V1	Direct	A	$0.00	1–13
	Direct	B	0.30	14–25
	Direct	A′	0.00	26–36
V2	Opportunity	A	$0.00	1–13
	Opportunity	B	0.30	14–30
V3	Direct	A	$0.00	1–13
	Direct	B	0.30	14–25
	Opportunity	C	0.30	26–39
V4	Opportunity	A	$0.00	1–13
	Opportunity	B	0.30	14–25
	Direct	C	0.30	26–38

*Conditions represent treatment changes in the data analysis. If experiments did not switch between a direct and opportunity charge in the optional market (V1 and V2) there were at most two conditions. Condition C in V3 and V4 represents a change in the way the fee was presented.

not ignore the sunk cost, there are two ways in which their behavior might be affected. First, individuals may feel that it is more important to be the high bidder and increase their bid in order to increase the probability of winning. Second, they may lower their bid in order to "recover" the entry fee in the event they were the high bidder.

By making the sunk cost direct in experiments V1, V3, and V4 and indirect in experiments V2, V3, and V4, the auction design also allows us to examine the issue of how individuals value explicit, out-of-pocket expenses, relative to implicit, opportunity costs. Differences in subject's responses to the two alternative presentations of the fee can appear in two forms. First, subjects can use different bidding rules under the different implementations of the fee. Second, the subject's decision to enter the alternative market could be related to the way the fee was presented.

Experiment V1 always utilized a direct cost approach to impose the fee; here subjects were told that if they participated in the second market they would pay an up-front charge from their balance. A crossover design was employed during this series of tests. The first treatment condition set the fee at $0.00 to provide a baseline from which to measure the effect of a positive fee (set at $0.30) during the second condition. The third condition was a return to the zero fee condition to test whether bidding behavior during the first condition was recoverable.

This A-B-A′ design yielded a shortage of data for the positive fee condition in the B markets, since on average the subject would only participate in half of these markets. The bidding behavior in condition A′ ($0.00 fee) was compared to that in condition A ($0.00 fee) to determine if the A′ part of the design could be dropped in favor of a longer B condition. We found only one subject whose behavior was statistically different ($\alpha = 0.03$) during the A′ condition relative to the A condition, so the next three experiments were conducted without returning to the baseline condition as shown in Table V.

Experiment V2 used the opportunity cost method of imposing the fixed fee in the alternative market. This opportunity cost was set at $0.00 for the first condition to again give us a baseline for comparison against the second condition when the opportunity cost was set to $0.30. Experiment V3 returned to the direct cost method of imposing a fixed fee during the first two conditions and then switched to the opportunity cost method for the last thirteen periods. Finally, experiment V4 used the opportunity cost method first and then the direct cost method. If sunk costs do not matter, bidding behavior should not be influenced by either kind of fee in the optional market.

To empirically detect any change in bidding behavior from sunk costs we use in our model the dependent variable *DIFBID* which is defined as the bid in market A less the bid in market B. The following regression was estimated for each individual:

$$DIFBID = \alpha + \beta_1 VSTAR + \beta_2 DUM1 + \beta_3 DUM2 + \beta_4 PER + \beta_5 BEGBAL + \epsilon_i .$$

The variable *VSTAR* is the subject's resale value, *PER* is the period number, *BEGBAL* is the subject's balance at the beginning of the period, and *DUM1* and *DUM2* are dummy variables defined as:

$$DUM1 = \begin{cases} 1 & \text{if condition is market B} \\ 0 & \text{otherwise, and} \end{cases}$$

$$DUM2 = \begin{cases} 1 & \text{if condition is market C} \\ 0 & \text{otherwise.} \end{cases}$$

The variable *DUM2* is only relevant during experiments V3 and V4 since these experiments used a crossover from a direct fee (opportunity cost) condition into an opportunity cost (direct fee) condition.

An F-test was performed to test the joint hypothesis that the coefficients associated with period and beginning balance were both insignificant. We found that for 4 of the 40 subjects, or 10%, we were able to reject the hypothesis at the .05 level. For 8 of the 40 subjects, or 20%, we were able to reject the hypothesis at the .10 level. Thus, we decided to keep these two variables in the rest of our analysis. We did, however, run the regression without these variables and found no qualitative difference in the results that follow.[21]

If a sunk cost effect exists, we should find the coefficients on *DUM1* and *DUM2* significantly different from zero. The results reported in Table VI show that there was one subject who had a significant coefficient on *DUM1* in experiment V1, none in experiment V2, two in experiment V3, and one in experiment V4. For the variable *DUM2*, there were two subjects who had significant estimated coefficients in experiment V3 and one in experiment V4. Furthermore, for all but one of these subjects the coefficients were significantly *positive*, indicating that the overwhelming tendency for subjects was to attempt to increase the probability of winning the auction by increasing their bid, rather than attempt to recover the sunk cost when (and if) they won the auction by reducing their bid. Included in parentheses in Table VI are the subject numbers for the individuals whose estimated coefficients were significantly different than zero.

Interestingly, the significant estimates on *DUM2* were for the same individuals who had significant estimates on *DUM1*; namely subjects 1 and 7 in experiment V3 and subject 7 in experiment V4. Thus, there was a total of only 4 individuals who significantly adjusted their bidding behavior when a sunk cost was imposed. Given that the three significant estimates in V3 and V4 were from the same individuals, we also tested whether *DUM1* was equal to *DUM2* and then tested whether the coefficient on a single dummy variable was significantly different than zero.

21. Additionally, we fitted a time trend across the data and found no significant effect. The first 13 periods, which did not impose any fee, provide time to learn how the auction market functioned. If there was any additional learning after the fee was imposed it occurred very rapidly.

Table VI. Signs of the Estimated Coefficients β_2 and β_3, Subject Number in Parentheses

	β_2				β_3			
Experiment	−*	−	+	+*	−*	−	+	+*
V1	0	3	6	1 (8)				
V2	0	4	6	0				
V3**	0	3	4	2 (1,7)	0	4	3	2 (1,7)
V4	1 (7)	3	6	0	1 (7)	6	3	0

*Significant at the .05 level.
**One subject did not have a sufficient number of observations for an estimate to be made.

Table VII. Percent of Periods an Auction Market with a Fee Forms

Experiment	No Fee*	Direct	Opportunity
V1	100 (D)	42	—
V2	100 (O)	—	100
V3	100 (D)	58	71
V4	100 (O)	62	100
Average	100	54	90

*D = Direct fee set to \$0.00, O = Opportunity fee set to \$0.00.

When using a single dummy variable for these three subjects only subject 1 in experiment V3 continued to have a significant sunk cost coefficient. With this one individual and subject eight from experiment V1, we have a total of 2 (5%) subjects showing a significant change in bidding behavior due to the sunk cost. We conclude that there is very little effect on bidding behavior from the imposition of an entry fee. In these experiments, sunk costs are ignored.[22]

There is, however, evidence that the subjects did respond differently to the direct and indirect fee. For the \$0.30 cost of entering the optional market, there were two distinct presentations in the instructions. For a direct charge the instructions read as:

> You can choose to also participate in a 'B' market. If you do so, you will be charged a fee that will be greater than or equal to zero.

When subjects were presented with an opportunity cost, on the other hand, the instructions read:

> You can choose to also participate in a 'B' market. If you do not enter market B, you will be paid a fee that will be greater than or equal to zero.

The cost to enter the B market is identical in both cases, but we find the payment offer to not enter the optional market created more interest in the market than a direct charge.

Table VII presents the evidence. It shows the percent of auction periods that had five or more

22. We examined the relationship between the coefficients β_1 and β_2 and a measure of risk aversion that was based on the coefficient value during the no fee condition. There was no significant relationship between the coefficient values in the no fee condition and the coefficient values in the fee condition.

requests to enter the alternative market. When there was no fee to enter the B market, column 1 shows the markets operated 100% of the periods in all of the experiments. With a direct fee in experiments V1, V3, and V4, the B market formed on average 54% of the time. The frequency a market formed was different, and more frequent, when subjects were paid to stay out of the B market. In these periods the market operated on average 90% of the time. The difference in the mean percentage of times the market formed is significant at the .01 level. Except for the first experiment we also know the percentage of the subject pool that asked to enter the optional market each period.[23] For experiments V2–V4, 47% of the subjects on average requested to make a bid in market B when there was a direct charge, while 73% made such a request when subjects were paid to not enter the market. The difference in means is again significant at the .01 level.

If the subjects view their accumulated balances as part of their endowment, the above results are consistent with what Thaler [19] has identified as an endowment effect. The endowment effect refers to the tendency to value the goods in one's possession more highly than the opportunity to obtain the same good. In this context the endowment effect reflects the increased subjective weight on losses relative to gains. The endowment effect has recently been demonstrated in an experimental market setting by Knetsch, Thaler and Kahneman [14].

Independent of why there is a difference in willingness to enter the market dependent on the type of fee imposed, what is clear is that the subjects were paying attention to the fee before entering the market. Therefore, the fact that there is hardly any effect on bidding behavior from the fee is a much stronger result. Although subjects may treat opportunity costs and direct expenses differently, once either one is borne, at least in these auction markets, they become irrelevant to the subsequent decision.

It is interesting to note that these auction market experiments contained all the aspects that economists have contended should eliminate inconsistencies in individual behavior. A monetary reward structure, along with the opportunity for learning while optimizing within a market, are present in this set of experiments. As an economist would predict, the incidence of a sunk cost effect is practically non-existent. What an economist would not predict, yet nevertheless did occur, was the difference in the way subjects entered a market when different fee types were imposed.

IV. Conclusions

This research has discovered that when individuals are making economic choices, sunk costs, and the way they are presented, may influence their decisions. In the one-time lottery experiment we identified two distinct types of behavior by those subjects that did not ignore sunk costs. As the sunk price increased, one quarter of the subjects valued the ticket more and one quarter of the subjects valued the ticket less. Altogether, marginal gains and losses were dominated by other considerations for about half of the subjects in this experiment.

Given these results, to provide an opportunity for subjects to learn that a ticket's price was sunk, we developed the sequential lottery design. Subjects paid for, valued, and then collected their earnings on tickets one by one with 20 repetitions. The physical act of paying for a ticket before the ticket was valued framed the sunk cost differently than in the one-shot design. When ticket values were random about 19% of the individuals still did not ignore the sunk cost. Their tendency was to value a ticket less as the price increased. For the certain value lottery tickets, there

23. The computer program for experiment V1 did not record requests to enter the B market.

was no evidence of a sunk cost effect. The sequential design made sunk costs more transparent. The increased transparency resulted from the explicit collection of the fee, repeated trials, and the reduction of uncertainty in the object's value. We conclude that as cost and valuation become better separated, it is more likely that individual decision makers will ignore the sunk cost.

In both lottery designs there was a correspondence between risk preferences and valuation as the ticket price changed. Those showing risk aversion valued tickets less as price increased; those who were risk loving valued tickets more as price increased. This evidence indicates that sunk costs do not necessarily induce stronger commitment to an endeavor. Those who value a ticket more show behavior consistent with greater commitment to continued expenditure after sunk costs are paid. Those who value a ticket less show signs of being less committed. Sunk costs can influence decision makers to abandon a project too early.

The results in the auction markets showed that only about 5% of the subjects allowed the sunk cost to influence their bidding decision in the market. It appears that the market has a strong influence on the behavior of the individuals.[24] It was the case, however, that the subjects in these experiments faced a different type of uncertainty than those in the lottery experiments. Therefore, it is possible that it was not just the market that influenced decisions, but also the source of the uncertainty. This is an issue that deserves more study.

An unexpected result in the auction market data, was that the type of cost imposed had a significant impact on the interest expressed by the subjects to participate in the optional market. It was clear in this case that the framing of the cost resulted in different behavior. Individuals were much more likely to ask to participate in the optional market when the cost was an implicit, opportunity cost rather than an explicit direct payment. That is, the opportunity costs were undervalued relative to direct costs by the participants.

When we have discussed the results of these experiments with other economists, they seem satisfied (relieved?) that 95% of the bidders ignore sunk costs in the optional auction market with an entry fee. This is, after all, strong support for the economist's contention that markets work. Eventually those few bidders who do not behave correctly will continue to lose money until they exit from the market. In this sense the market rewards those who use correct decision rules and punishes those who do not. The market thus forces agents to correctly deal with cost.

We add to this view a note of caution that was expressed in our introduction. Not all choices are made in a marketplace. There are numerous contexts in which individuals must decide without the market's guidance, many of these are one-shot decisions like our first lottery experiment or even repeated decisions like the sequential lottery. We find that in such instances the way in which a sunk cost arises, that is, its frame, determines its direction and degree of influence on the person's decision at the margin. In certain economic decision-making environments it is not a foregone conclusion that economic theory will be a good predictor of individual behavior.

24. Plott and Uhl [18] studied the behavior of middlemen who bought in one market and then sold in another. The cost of inventory became sunk when these individuals sold in the secondary market. During the first half of the experiment middlemen tended to be conservative buyers, rarely selling at a loss. In two periods there were fewer sales than purchasers. Also, mean buying and selling prices were correlated, which suggests cost-based pricing. This behavior may all be attributed to the influence of the middlemen's sunk costs. In later market periods, however, this behavior disappears. Market agents appear to learn to ignore sunk costs.

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