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Are decisions under risk malleable?

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ABSTRACT Human decision making under risk and uncertainty may depend on individual involvement in the outcome-generating process. Expected utility theory is silent on this issue. Prospect theory in its current form offers little, if any, prediction of how or why involvement in a process should matter, although it may offer *ex post* interpretations of empirical findings. Well-known findings in psychology demonstrate that when subjects exercise more involvement or choice in lottery procedures, they value their lottery tickets more highly. This often is interpreted as an "illusion of control," meaning that when subjects are more involved in a lottery, they may believe they are more likely to win, perhaps because they perceive that they have more control over the outcome. Our experimental design eliminates several possible alternative explanations for the results of previous studies in an experiment that varies the degree and type of involvement in lottery procedures. We find that in treatments with more involvement subjects on average place less rather than more value on their lottery tickets. One possible explanation for this is that involvement interacts with loss aversion by causing subjects to weigh losses more heavily than they would otherwise. One implication of our study is that involvement, either independently or in interaction with myopic loss aversion, may help explain the extreme risk aversion of bond investors.

Human decision making under risk and uncertainty may depend on individual involvement in the outcome-generating process. This dependency is beyond what utility theory predicts. We show that experimental subjects make different decisions based on whether or not they are allowed to participate in the outcome generating process in a way that, according to utility theory, cannot affect the utilities or objective probabilities of outcomes.

Many psychologists believe that minor manipulations in experiments can cause subjects to increase their estimates of the probability of success, relative to control treatments. Social psychologists refer to this as an illusion of control, and Ellen Langer (1) defined it as "an expectancy of a personal success probability inappropriately higher than the objective probability would warrant." She argued that it can be caused in situations involving only chance by giving subjects cues to situations involving skill, even though skill plays no role in determining the outcome. Articles after Langer (1) showed that illusion of control affects behavior in a wide variety of settings. Camerer (2) summarizes a few articles related to illusion of control in experimental economics. Presson and Benassi (3) provide a meta-analysis of illusion of control research in psychology.

Others believe that minor manipulations intended to improve the moods of subjects will make them more risk seeking when the stakes or probability of losing are low, and more risk averse when the stakes or probability of losing are high (4, 5). Isen and Geva (6) argue that manipulations designed to improve the mood of subjects may make them more loss averse in gambles with relatively high stakes, providing evidence that in these gambles, subjects with an induced good mood are not only more risk averse, but also record more thoughts about loss in a thought listing exercise. Dunn and Wilson (7) link the two literatures, showing that illusion of control improves moods and increases risk taking in a gamble with small stakes but not in a gamble with larger stakes.

In many of the studies on illusion of control and on emotional moods and risk, it is possible that uncontrolled variables explain the results. For example, Langer (1) conducted a lottery in a real firm in which subjects could buy a lottery ticket for $1. Once purchased, the ticket had an expected value of $1, so that overall, the gamble had an expected value of zero. Langer allowed subjects in one condition to choose their own lottery ticket from among a set of football player cards and had the experimenter assign a card from the same set to subjects in the other condition. She then had the experimenter elicit the price each subject was willing to accept to sell his or her lottery ticket. Those in the choice condition stated an average selling price of $8.67 and those in the no-choice condition stated an average selling price of $1.96. Because subjects in Langer's choice condition were allowed to pick their cards, it is possible that they were simply registering a preference for betting on their favorite football player. Many studies allow subjects to choose a number to bet on and thus are subject to the same criticism. Another feature of Langer's study is the self-selection of risk-seeking subjects in her design: her data set consists of subjects who voluntarily participated in a lottery with an expected value of zero.

A third problem in many studies is the nature of the incentives. Many did not use cash incentives. Many of them that do use incentives use course credit [see for instance, Dunn and Wilson (7), and Isen and Geva (6)]. Finally, some studies used cash incentives and provided a measure of risky behavior by eliciting the prices at which subjects were willing to part with their tickets, but did not use mechanisms in which truthful revelation of private values of the lottery was a dominant strategy. In the study by Langer (1) described above, subjects were simply asked for the price at which they would be willing to sell their lottery tickets. No indication was given that the probability of a sale depended on the price. In fact, the sale was not actually implemented.

Koehler, Gibbs, and Hogarth (8) addressed some of these problems with a very minimal treatment in which subjects in the illusion of control treatment rolled their own dice to determine their lottery numbers and outcomes, whereas subjects in the other treatment had the dice rolled for them by an experimenter. They found an illusion of control when subjects rolled one die, but not in lotteries where subjects rolled 30 dice. Their motivation for the 30-dice treatment was Gigerenzer's argument (9) that cognitive illusions can be undermined by increasing the salience of the frequencies of winning and losing in lotteries. However, the level of involvement also changes with the 30-dice manipulation. When 30 dice are rolled and the

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outcome is to be determined by a single die that lands closest to a predetermined spot, involvement in the roll of the die that determines the outcome is diluted be rolling of 29 other dice. We thus use the Koehler *et al.* (8) single-die versus 30-dice manipulation as a second way of varying the degree of involvement. Unfortunately for our purposes, the Koehler *et al.* study is not designed for drawing inferences on the effect of involvement on risk preferences. We replicate the single-die and 30-dice procedure from Koehler *et al.* but change the actual lottery and elicitation mechanisms to increase the level of risk and allow comparison of risk attitudes.

Design

We designed an experiment to bias our design against finding any effect of involvement at all by removing several alternative explanations that exist in previous studies. We reduce the possibility for effects of preferences for lucky numbers by not allowing subjects in the involved treatment to choose their lottery numbers. Furthermore, unlike previous studies, we collect our dependent variable, the value of the lottery ticket to the subject, by having subjects sell their tickets in an English seller's auction, a mechanism well known for providing individuals with no incentive to misrepresent the value placed on the item being sold. The English seller's auction begins with a selling price that falls at a certain rate over time. By default, all sellers are in the auction with their objects for sale when the auction starts. To keep an object rather than sell it, the seller must exit the auction. Subjects are informed about how many people remain in the auction at all times. A participant sells his or her object simply by remaining in the auction until it ends. In our experiment, two out of 10 participants sold a lottery ticket in each auction at the third highest price. That is, when the eighth person exits, the auction ends and the remaining two sellers sell their lottery tickets for the price at which the eighth person exited. The key feature of this mechanism is that a subject has no incentive to exit at any point except that which represents the true value he places on his ticket. A subject who exits early forfeits the chance to sell the lottery ticket at a price higher than its value. A subject who exits late risks selling the lottery ticket for a price that is lower than its value. Without these incentives to truthfully reveal the value of one's object, involvement may interact with subjects' attempts to profit by overstating the values they place on their lottery tickets. Second, in the English auction we used cash incentives that were higher than those used in most psychology experiments on this topic, which offer either course credit or expected cash payoffs that are either zero or nominal. This consideration is important because previous findings have been sensitive to stake size. Just before each English lottery ticket auction, we elicited the values subjects placed on their lottery tickets with a hypothetical sealed bid mechanism. In this mechanism, subjects were prompted by a computer screen to privately enter the price at which they would be willing to sell their lottery ticket given that the two lowest-priced tickets out of 10 would sell at the third-highest price. The purpose of eliciting a hypothetical value and a real English auction selling price for each lottery ticket was to test the joint hypothesis that selling prices are sensitive to cash incentives and the auction mechanism.

Involvement. We vary the level of involvement the subjects have in determining lottery numbers for two independent lotteries. Our minimal-involvement manipulation had subjects rolling for themselves by using six-sided dice and one of the authors (C.F.) rolling the same dice for the subjects in the uninvolved treatment. Subjects participated in two lotteries. In each lottery, subjects received a lottery ticket with two lottery numbers. These numbers were obtained either by rolling their own dice or having the experimenter roll for them. Every subject participated in one lottery in which a single die was rolled, and in another lottery in which 30 dice were simultaneously rolled with the outcome of each roll determined by the single die that landed closest to a predetermined spot. At the end of the experiment, subjects who did not sell their tickets rolled or had the experimenter roll to determine the outcome of the lottery. Subjects who rolled their own dice did so throughout the experiment and subjects in the experimenter-rolls condition never handled the dice.

By using the die roll instead of allowing subjects to pick their own numbers we excluded the possibility of measuring preferences for lucky numbers. We also allowed subjects to state before they rolled whether or not the upcoming roll would count as a lottery number or be excluded from the set of possible lottery numbers. We did not allow them to make this choice after they rolled because it would allow them to pick a preferred or lucky number. This procedure of allowing subjects to make a decision about the upcoming roll, in addition to the fact that subjects were rolling their own dice, might generate an involvement effect, while not allowing subjects to actually pick their own numbers. However, this procedure also opens the possibility of a feedback effect because those who chose for a roll not to count are likely to roll more often before obtaining two different lottery numbers. As a control, we calculated the summary statistics in Table 1 by using only those subjects who rolled, or for whom we rolled, exactly twice to obtain the two lottery numbers. We found no noteworthy changes in the summary statistics, suggesting that our findings are not driven by feedback effects. The number of independent observations in this reduced data set is too small for further analysis.

The 30-dice lottery served as a second manipulation on the level of involvement. Following the procedures of Koehler *et al.* (8) we conducted lotteries that were otherwise identical except that in one numbers and outcomes were determined by the roll of a single die, and in another they were determined by rolling 30 dice and using the die that landed closest to a predetermined spot.¶ In the 30-dice lottery, perceived involvement in the role of the die that determines the outcome may be reduced by the involvement with the other dice. In our study, the 30-dice lottery is a within subject manipulation with every subject participating in both a single-die lottery and a 30-dice lottery.

Dominant Strategy Elicitation Techniques. We then elicited lottery ticket valuations by using institutions in which truthfully revealing private values is a dominant strategy: a sealed-bid seller's auction and a descending English clock seller's auction. According to the revenue equivalence theorem, prices in the sealed-bid auction and the English clock auction should be the same. However, there is experimental evidence of violations of the revenue equivalence theorem (10). McCabe, Rassenti, and

¶The 30-dice representation of the lottery did not change the payoff distribution in this study. Their paper also includes a second study with a 30-dice representation in which the probability distribution of payoffs is different from the single-die representation.

Table 1. Sample means of recoded data on elicited sealed-bid prices and exit prices in the English auction

	Sealed bid 1	English auc. 1	Sealed bid 2	English auc. 2
Subject rolls, $n = 24$	623.79 (197.78)	501.46 (148.56)	520.83 (143.94)	407.29 (118.07)
Experimenter rolls, $n = 26$	671.64 (185.77)	552.88 (176.07)	559.38 (181.72)	439.04 (166.61)
Pooled, $n = 50$	648.78 (191.20)	528.20 (163.86)	540.88 (164.17)	423.80 (144.79)

SD in parentheses.

Smith (11, 12) found that the English clock auction performed better at dominant strategy elicitation than the sealed-bid auction for nonrisky private value units.

The players have private values for their tickets that depend on their preferences regarding risk. In our design, each player knows his own value, unless he fails to understand the expected value of the lotteries or does not have well-defined preferences. Players are uncertain about other players' values. We assume that the set of possible lottery ticket values is a finite set V. We assume further that each player i believes that for each $j \neq i$, v_j is drawn from the same distribution over V. Under these assumptions, according to traditional game theory, in both elicitation methods bidding the lottery ticket for its true private value is a dominant strategy.

Our experimental task is complex because subjects must understand the auction and lottery procedures as well as the expected value of the lottery ticket they are selling to play optimally. Consequently, we wrote very explicit instructions and included training time in the experiment. The experiments took roughly 2 hr to complete, with most of the time consumed by practice rounds, questionnaires on the instructions, and answering questions and misunderstandings that were exposed in the questionnaires.

A second aspect of our design is that we did not actually implement sales in the sealed-bid auction. Actual sales took place in the English auction. The purpose of this method is to test whether or not involvement effects are robust to the joint changes in real versus hypothetical incentives and the auction mechanism, given that in both cases subjects have no incentive to misrepresent the values they place on their lottery tickets.[‖]

Finally, note that we first endowed subjects with the lottery tickets then allowed them to sell their tickets. Previous findings on endowment effects predict that this design choice will generate higher values than if we had chosen to frame the experiment in terms of willingness to pay for the lottery tickets (14, 15). We use this frame because it is more natural and also more closely replicates the design of Langer's (1) lottery.

Experimental Procedures

We conducted the single-die lottery first and the 30-dice lottery second in five sessions and reversed the order in three sessions, for a total of eight sessions with 10 subjects in each. For each lottery, subjects received two lottery numbers either by rolling or having the experimenter roll the dice then participated in the sealed-bid mechanism and the English seller's auction. The subjects or the experimenter rolled the outcome for each lottery at the end of the experiment, at which point the subject collected his or her earnings and left the experiment.

We paid subjects $5 for showing up. In a single random drawing we bumped extra subjects from the experiment and assigned half of the remaining 10 subjects to the involved condition and half to the uninvolved condition. We did not tell subjects about the two conditions, and the room was designed and used in such a way that subjects could not see the screens or actions of other subjects. We used experiment identification numbers rather than names for record keeping to protect subject anonymity.

Subjects read online instructions about how to sell an item in an English auction. They participated in six paid practice rounds of English seller's auctions with assigned values. In each round, each subject was assigned a new value from a set of values that ranged from zero to 300 cents in 30-cent intervals. The average of the values for each subject was 150 cents. The point of this exercise was to make sure the subjects understood that truthfully revealing their values was the dominant strategy. We were explicit about this in our instructions and had subjects answer a questionnaire to test their understanding. The reason for the large variance in the assigned private values was to help subjects understand the costs of failing to bid their private values. We answered all questions as thoroughly as possible and took the time to explain the auction to everyone who missed a question on the questionnaire.

Subjects then read online instructions describing the first lottery. The two lotteries were identical except that a single die was rolled in the single-die lottery, whereas 30 dice would be rolled in the 30-dice lottery with the outcome being determined by the die that landed closest to a premarked spot. Subjects in the involved treatment were told that they would roll either a single die or 30 fair six-sided dice to determine two different lottery numbers. At the very end of the experiment, they then would roll the die (30 dice) again. If they rolled one of their lottery numbers they would be paid $10. If they did not, then they would earn zero. Subjects in the involved treatment also were given the opportunity to choose before rolling whether or not the upcoming roll would count as a lottery number or be excluded from the set of possible lottery numbers. They were told that numbers rolled more than once would not count. The instructions and procedures for the uninvolved treatment were identical in every respect except that an experimenter rolled the die (dice), and subjects in this condition were not asked to choose whether the upcoming roll should count as a lottery number or be excluded from the set of possible lottery numbers.

Subjects then were called one at a time to a separate room to roll or have the experimenter roll their two lottery numbers. We recorded all roll sequences. When all subjects had lottery numbers, they were sent a new set of online instructions in which they were told that we would buy back lottery tickets from two of the 10 people in the room at the third-lowest asking price. We then elicited their lottery ticket valuations with a second price sealed-bid mechanism by asking them to enter the price at which they would be willing to sell their ticket. We then described how we would buy back two tickets at the third-highest price by using the English clock seller's auction. We were concerned that if we bought back only one ticket, subjects might too readily assume that they would not be able to sell their tickets and drop out before the price clock reached their true private value.

At this point, we handed out a second questionnaire to check that subjects understood the lottery auction. Again, we took the time to encourage questions and answer them as clearly as possible without providing information outside of what was written in the instructions. Instructions were self-paced, and when all subjects were finished with the instructions, we started the English lottery ticket auction.

Both the single-die and 30-dice lotteries were conducted in this manner, one after the other. When the two sets of lottery procedures were finished, we handed out an exit questionnaire with more questions to test their comprehension of the experiment. We called subjects one at a time to play any lotteries that they had not sold, receive their earnings, and leave the experiment. The instructions and questionnaires used in this experiment are provided in the supplemental data on the PNAS web site, www.pnas.org.

This was a fairly high-stakes experiment, relative to those in psychology, with a reasonable degree of risk. In addition to the $5 show-up fee, subjects earned between $9 and $10.50 in the six practice rounds and had expected earnings of $6.66 for the two lotteries combined if they kept their lottery tickets. Subjects could earn up to $10 in each lottery or as little as zero in each. Note that there were no negative earnings. Any perceived losses would be relative to the frame of having been

[‖]Knez, Smith, and Williams (13) report that actual selling prices were lower than hypothetical selling prices in markets for risky assets.

endowed with a lottery ticket that has an expected value of $3.33.

Results

Table 1 presents summary statistics of the pooled sealed-bid elicited values and the English auction prices at which sellers chose to exit. (In the sixth practice round of the English auction, only seven of 80 subjects bid more than 5 cents away from their induced private value.) These data are recoded to create a data set with a common set of individuals for whom we have an exit price in both rounds of the English auction and values that do not exceed the maximum possible earnings of $10 in each lottery. When not specified otherwise, the results presented below use this data set, the recoding of which is described presently.

In the sealed-bid mechanism, six subjects stated selling prices over $10 in the first round and one subject in the second round. Subjects who do this either fail to understand the truth-telling incentives of the auction or feel that participation in the lottery is more valuable than the potential earnings. In the former case we would not be collecting the individual's true valuation of the lottery ticket. The latter case signals preferences that are beyond the scope of this study. Therefore, we deleted these subjects from both rounds of both the sealed-bid and English auctions.

For similar reasons, we deleted one individual who exited the second round of the English auction immediately. The English auction price started at $10 and fell by $.05 every second. Thus, the highest price a subject could bid would be $10. (We were careful to give subjects plenty of warning that the auction was starting. We made a verbal announcement that the "auction will begin in a few seconds" and then when the auction actually began, a beep sounded on each computer.) Because we do not know if this subject would have bid higher if it were possible, we delete all observations on this individual from the data set.

The data observations are the prices at which individuals exited the auction and the ending auction price for the two subjects that remained in the auction until the end and sold their tickets. Because the selling price was equal to the third-highest bid, it is probably higher than the points at which the sellers would have exited had the auction not ended. Thus, the selling prices are upwardly biased measures of the value these subjects place on their tickets. To create a data set of observations from a common set of individuals, we delete all individuals who sold a ticket in either round of the English auction. Of the 32 tickets that sold in the two rounds of the English auction, 20 of the individuals also were the sealed-bid winners.

Note that if subjects in the subject-rolls treatment are more or less likely to sell their tickets than those in the experimenter-rolls treatment that could bias our results. The fraction of subjects in the involved condition who sold their tickets was not significantly different from 0.5, according to the binomial test.

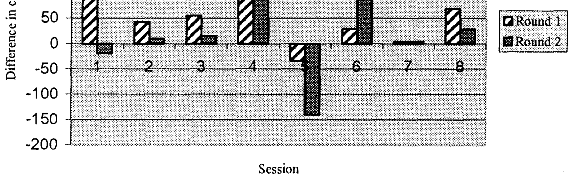

FIG. 1. Differences between mean prices in uninvolved and involved treatments in each session of English auctions 1 and 2.

Table 2. Z-statistics (*P* values) for Wilcoxon signed rank test for pairs of session averages of involved and uninvolved treatments in the English auction

	Round 1	Round 2	Rounds 1 and 2 pooled
Single and 30 pooled	2.10	1.12	2.17
	(.036)	(.263)	(.030)
	8 pairs	8 pairs	16 pairs
Single only	1.75	1.60	2.10
	(.080)	(.109)	(.036)
	5 pairs	3 pairs	8 pairs
30 only	1.60	−.135	.98
	(.109)	(.893)	(.327)
	3 pairs	5 pairs	8 pairs

Table 1 suggests two basic findings. First, the average exit price is lower for those who rolled their own dice in all cases. Second, the average values start high and fall over time within the sealed-bid mechanism and within the English auction, toward the expected value of the lottery, which was $3.33. We attribute this affect to learning.

Session Effects. We tested the null hypothesis that the particular session a subject was in had no significant effect on elicited values and prices. We used the Kruskal-Wallis test, which rejected this hypothesis for the English auction, but not for the sealed-bid elicitation mechanism. In the English auction, subjects may base their behavior on the exit point of other players, so individual observations within a session are not independent. Therefore we use session averages as independent observations rather than individual level data, and we do not pool the sessions. For the sealed-bid elicitation mechanism we used both pooled and unpooled data.

Involvement. Fig. 1 illustrates the involvement effect in the English auction. It displays the average exit price in the uninvolved treatment minus that of the involved treatment for each round in each session. Note that in only three of 16 cases were these negative. In the first round, average prices in the uninvolved condition were higher than the average prices of the involved condition in all but session 5. For round 2, this was the case in six of eight sessions. Recall that in sessions 1–5, the single die lottery was in the first round and the 30-dice lottery in the second round, while the order was reversed in sessions 6–8.

Tables 2 and 3 summarize results of tests of the effects of the subject-rolls treatment on prices in the English auction and sealed-bid elicitation mechanism, respectively. Each table reports a z-statistic and *P* value of a Wilcoxon signed rank test on pairs of session averages. Each pair consists of the means of the subject-rolls treatment and the experimenter-rolls treatment from one session. The null hypothesis is that the distribution of session averages of the involved treatment is not significantly different from that of the session averages of the uninvolved treatment.

Table 3. Z-statistics (*P* values) for Wilcoxon signed rank test for pairs of session averages of involved and uninvolved treatments in elicited (hypothetical) sealed bids

	Round 1	Round 2	Rounds 1 and 2 pooled
Single and 30 pooled	1.54	.84	1.91
	(.123)	(.400)	(.056)
	8 pairs	8 pairs	16 pairs
Single only	1.48	.535	1.68
	(.138)	(.593)	(.093)
	5 pairs	3 pairs	8 pairs
30 only	.535	.677	.981
	(.593)	(.498)	(.326)
	3 pairs	5 pairs	8 pairs

Table 4. P values for Wilcoxon signed rank test for pairs of session averages

	Involved treatment	Uninvolved treatment
Sealed bid 1 = sealed bid 2	1.75 (.080)	2.63 (.009)
English auction 1 = English auction 2	2.10 (.036)	1.84 (.066)

The main result is that when both rounds and the single- and 30-dice lotteries are pooled, the effect of the subject-rolls treatment is significant in both the English auction and the sealed-bid mechanism ($P = 0.030$ and $P = 0.056$ respectively).

In subsamples of these data, the test is significant in the single-die lottery of both institutions when the first and second rounds are pooled together ($P = 0.036$ in the English auction and $P = 0.093$ in the sealed-bid auction). Furthermore, in the English auction the effect of the subject-rolls treatment was significant (at $P \leq 0.109$) in all subsamples of the first round of the auction and all subsamples of the single-die auction. In all cases, those who rolled their own dice had lower mean prices by session than the others.

Learning and Order Effects. Despite two questionnaires and six practice rounds of the English auction, experience had a significant effect on bids in both the sealed-bid and English auctions. Mean bid prices were above the expected value of the lottery in both lotteries but fell significantly with experience. Session averages in the first round of the English auction were higher than session averages in the second round. Similarly, the first-round elicited values were higher and drawn from a significantly different distribution than second-round elicited values. Table 4 shows the P-values of the Wilcoxon signed rank test on the session averages of first- and second-round prices. Mean prices by session in the first round were significantly higher than in the second round.

We also tested whether differences between the first- and second-round bids were significantly affected by the single-die and 30-dice lottery order. The null hypotheses were

$$H_0{:}S1^1 - S2^{30} = S1^{30} - S2^1$$
$$H_0{:}A1^1 - A2^{30} = A1^{30} - A1^1, \quad [1]$$

where $S1$ is the first-round elicited sealed bid, $S2$ is the second-round elicited sealed bid, $A1$ is the first-round English clock auction price, and $S2$ is the second-round English clock auction price. The superscripts 1 and 30 refer to the single-die and 30-dice lotteries, respectively. We used the Mann–Whitney test on individual level data both pooled and unpooled by session. In the individual level data we dropped values of $10 or higher in both auctions and in the English auction only we dropped prices of tickets that sold. We found no significant effects of lottery order on the differences in Eq. **1**.

Discussion

We found a robust effect of involvement in the English auction in the opposite direction of that predicted by the illusion of control hypothesis. A number of uncontrolled factors may have explained previous findings in the illusion of control literature. First, most of the widely cited studies did not control for preferences for the numbers or cards being gambled. Second, many of these studies use either hypothetical or small stakes.

Our results are consistent with studies cited above that provide evidence that (*i*) control over procedures in experiments may improve the moods of subjects, and (*ii*) with minor manipulations intended to improve subjects' moods, they become significantly more risk averse when stakes are large or the probability of losing high. One possible explanation that has been offered by Isen and Geva (6) is that manipulations that improve the moods of subjects increase their aversion to loss.

A second possibility is that involvement accelerates learning, perhaps by making subjects more alert or motivated. In our design, most subjects were demanding prices that were higher than the payoff maximizing strategy. To the extent that this was the result of confusion and subjects are learning with experience, any factor that accelerates learning would exert a downward pressure on prices. It is also possible that the effects of involvement may diminish with experience. We found that in the English auction, the main effect of the subject-rolls treatment was significant in the first round but not the second round.

We found that the effect of the subject-rolls condition did not have a significant effect in the 30-dice lottery when both rounds were pooled or in round 2. These results are consistent with the findings of Koehler *et al.* (8) who found no effect of involvement in the 30-dice representation. Their study was motivated by Gigerenzer's argument (9) that humans may assess risk more easily when lotteries are framed in frequency terms, making their decisions less susceptible to other factors. However, another explanation is that rolling a single die instead of 30 gives a subject more direct involvement in the roll of the relevant die, whereas rolling 30 dice dilutes the involvement with 29 other dice. Thus, the involvement effect of the subject-rolls treatment may be undermined by reduced involvement when subjects have to roll 30 dice.

The risk-seeking behavior we observed decreased significantly with experience. We cannot estimate the point at which exit points and selling prices would converge in repeated auctions. Thus, we cannot make claims about risk preferences in general. Rather, we conclude that for a given level of experience, those who rolled their own dice accepted less risk in the first round of the English auction.

Conclusion

We have shown that involvement, even when isolated from the effects of preferences over the objects of the gamble, influences decision making under risk. Utility theory is silent on this issue. Our findings suggest the importance of researching how the process of choice affects behavior.

One application of the findings in this paper is to the equity premium puzzle. Rajnish Mehra and Edward Prescott (16) showed that the historical average return on equity has so greatly exceeded the average return on nearly default free short-term debt that the difference cannot be explained by theoretically and empirically plausible levels of risk aversion. Shlomo Benartzi and Richard Thaler (17) developed a prospect theoretic explanation, based on Tversky and Kahneman's cumulative prospect theory (18), which they refer to as myopic loss aversion. If people are loss averse, they dislike losses more than they like gains. When loss-averse people review their assets frequently, they experience disutility from losses that they would be less likely to observe if their portfolio evaluation period were longer. The shorter the evaluation period for asset portfolios, the more likely investors are to witness fluctuations in equities that, based on historical trends, would average out to high returns in longer evaluation periods. Thus, with shorter evaluation periods, loss-averse people will increase their investment in safe assets like bonds to avoid the myopic discomfort associated with having to review the losses in risky assets.

Thaler, Tversky, Kahneman, and Schwartz (19) and Gneezy and Potters (20) conducted independent experimental tests of the myopic loss aversion explanation of the equity premium. Both studies found that indeed, in treatments with shorter, more frequent review periods, subjects invested more in safer, lower-paying assets. That is, in both of those experiments, the main manipulation was the frequency with which portfolio evaluations were made.

But with more frequent portfolio review comes additional involvement. Will this have an effect independent of the effects of myopic loss aversion? If so, what will be the effect of potential increases in involvement in the investment process that have been afforded by transaction cost-reducing changes in the institutions through which investments are made?

The effect of involvement on decisions under risk that we report is independent of the effect of viewing losses. Viewing losses and choosing portfolios each may make their own contribution to the equity premium puzzle.

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