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Pay Every Subject or Pay Only Some?

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Abstract

One technique employed by budget-conscious researchers is to pay only some of the subjects for their choices in an experiment. We test the effect of paying some subjects versus paying all subjects in the context of risk preferences, controlling for the difference in stakes induced by paying only some subjects. Over two experiments, we demonstrate that paying only some subjects yields lower levels of risk aversion than does paying all subjects, though it yields more risk aversion than paying all subjects lower stakes with expected values equivalent to the "pay some" condition. We also demonstrate that paying only some subjects not only changes the level of risk aversion but also impacts the ordering of subjects by elicited risk aversion. Neither probability weighting nor standard experimental demographics were correlated with subjects' differences between these conditions. We exploit our multiple measurements of risk aversion to estimate a simple structural model of latent risk aversion, and use these results to derive a correction factor in order to approximate the results as if all subjects were paid high stakes. Our findings imply that probabilistically paying some subjects high stakes meaningfully impacts the elicited level of risk aversion, although it better approximates the experimental ideal of paying all subjects high stakes compared to paying all subjects lower stakes.

JEL Classification: C90, D81

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1 Introduction

Though the gold standard in experimental economics is to pay all subjects for decisions that reflect economically meaningful stakes, in some experiments or surveys a researcher cannot feasibly afford to do so. One technique employed by budget-conscious researchers is to pay only some of the subjects for their choices in an experiment, rather than pay every subject. For example, a researcher measuring time preferences may have all subjects choose between $40 today versus $50 in one year, and then pay one out of ten subjects chosen at random for their choice, rather than pay all subjects for their choice between $4 today versus $5 in one year. One rationale for paying only some subjects is mental accounting. Stakes that are sufficiently low, such as $4 today versus $5 in one year, may fall below an attention or perception threshold, and subjects' choices over such low stakes may not accurately reflect their true preferences. Paying one out of ten subjects $50, rather than all subjects $5, retains the expected budget of $5 per subject but involves stakes that subjects might "take seriously." Another justification for paying only some subjects is to economize on transaction costs associated with payments.

A number of papers (Starmer and Sugden, 1991; Cubitt et al., 1998; Laury, 2006; see Charness et al. (2016) for a review) have tested the validity of choosing at random a subset of to count for payment in an experiment with multiple decisions, rather than paying for every decision in an experiment. The majority of these papers have found no difference in responses from paying for only a subset of questions versus paying for every question. However, less attention has been devoted to the effects, if any, of paying only some for their choices, which is surprising given the relative frequency of this practice in economic experiments.

We test the effect of paying some subjects versus paying every subject in the context of paying [illegible] treatment, but now only one out of eight subjects chosen at random is paid for their choice. In the third treatment, all subjects are paid for their choices, but subjects choose between lotteries with lower stakes. Specifically, the lotteries have expected values that are one-eighth that of the first treatment, which equalizes the expected values between the second and third treatment.

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Our contribution is fourfold. We present the first study with the main focus of testing the effect of paying some versus paying every subject. Though some previous studies (bT01Tf3.Tf()Tj22(

ordering is preserved. For example, a researcher may measure risk preferences to include as a control during analysis of another main parameter of interest, but be unconcerned with the actual levels of risk aversion. Our second contribution is to examine whether the ordering of subjects by risk aversion di¤ers between our treatment conditions. We …nd (Spearman) rank correlations of between 0.54 and 0.76, suggesting that paying only some subjects does a¤ect the level as well as the ordering of subjects' risk aversion. However, we …nd that the condition in which only some subjects are paid high stakes has a greater rank order correlation with the condition in which all subjects are paid high stakes than does paying all subjects lower stakes. That is, probabilistically paying some subjects high stakes elicits risk aversion levels that are more similar (in terms of both levels and rank ordering) to the experimental ideal of paying all subjects high stakes than does paying all subjects lower stakes.

Third, we explore potential mechanisms for di¤erences in subjects' evaluations between treatments. In our second experiment, we run an additional treatment which enables us to …t a probability weighting parameter for each subject. We examine if subjects who exhibit a larger degree of probability weighting also display particularly large di¤erences between our payment treatments. That is, we examine if the subjects who most over-weight small probabilities also over-weight the "one out of eight subjects will be paid" probability in our second treatment, relative to the third treatment in which every subject is paid with smaller stakes. We …nd that probability weighting is not signi[illegible] pc5370k1 (e between the pay every subject versus the pay some subject conditions. We also test whether demographics and alternative hypothetical measures of risk-taking are related to subjects' responses in the various payment conditions. All subjects completed an exit survey, providing demographic characteristics such as gender, g1Tf1.an6. v3.10Td[(i)6()Tjp12160Td[(p)12(r)11(o)38(v)1TJ

taking were consistently predictive of the difference in subjects' responses between the different payment conditions.

Our experiment contains multiple measurements of an individual's risk aversion. We assume that the most costly treatment, where all individuals are paid high stakes, serves as a better measure of an individual's underlying true risk aversion than the lower cost methods in which either only some subjects are paid high stakes or all subjects are paid low stakes. We exploit our multiple measurements to estimate a simple structural model in which an individual's response to each treatment is a product of latent risk aversion plus measurement error. For researchers who cannot afford to pay

subjects made all ten decisions in all three treatments, one of the decisions was randomly selected by a 10-sided die throw in each treatment. A second 10-sided die throw determined the payout for the selected decision
0Tf()Tj/TT01Tf1.2120Td[(tm)Tj/TC29(o)10(n)11(d)]TJ/T101Tf()Tj/T

chance of ending the game with nothing, and a 25% chance of proceeding to the second stage. In the second stage, subjects chose between a 80% chance of $4,000 versus a certain $3,000.[3] In the second scenario, subjects chose between a simple lottery of a 20% chance of $4,000 versus a 25% chance of $3,000. Although these outcomes have the same final probabilities in both scenarios, 78% of respondents preferred the $3,000 in the first scenario which was framed as a compound lottery, whereas only 35% of respondents preferred the $3,000 option in the second scenario. The authors suggest that individuals do not fully account for the 75% chance of ending the game in the first scenario as it is common to both options, and therefore isolated out during the utility evaluations, leading to the preference reversal between two otherwise equivalent outcomes.

A number of papers have tested whether individuals evaluate compound lotteries in accordance with the Expected Utility axiom (Bar-Hillel, 1973; Bernasconi and Loomes, 1992; Miao and Zhong, 2012; Abdellaoui et al., 2015; Harrison et al., 2015; Hajimoladarvish, 2018), with the majority finding that individuals do not reduce compound lotteries in adherence to Expected Utility. If individuals do not treat compound lotteries equivalently to their corresponding simple lotteries, then it seems natural for individuals to evaluate an experiment in which all subjects are paid higher stakes probabilistically as different than one in which all subjects are paid lower stakes.

Several theories have been proposed to account for individuals' failures to reduce compound lotteries (Kreps and Porteus, 1978; Kahneman and Tversky, 1979; Segal, 1990). We consider one avenue for failure to evaluate compound lotteries in accordance with Expected Utility: improper weighting of the first stage of the gamble due to probability weighting. We test whether an individual's degree of probability weighting (whereby individuals overweight small probabilities and under-weight large probabilities) is associated with differences in elicited risk aversion between our treatments. To test this hypothesis, we ran a second experiment which includes the same three treatments above, as well as a fourth treatment,

[3] In actuality, the outcomes were denominated in Israeli currency, not dollars.

" ." Whereas the treatment was constructed by multiplying the monetary prizes of the condition by 1/8th, the condition is constructed by multiplying all probabilities in the condition by 1/8th. The payo¤ options for this treatment, in which all subjects are paid but the options within each MPL have lower probabilities, are shown in Appendix Table 4.

Note that the non-zero payo¤ amounts for the treatmentfourmentsinarethe condi(e)8 (n)3 individuals.[6] For this reason, we employed a within-subject identification rather than the between-subject approach used in some other studies. One concern that arises when using a within-subject approach is that questions or experiences in earlier treatments (e.g., die rolls) might have an effect on a subject's decisions in later treatments. To minimize order effects, the instructions that were read aloud contained payoffs that were different from the actual treatments used to determine earnings. To further account for the possibility that early treatments may influence responses in subsequent treatments, we divided the twelve sessions in Experiment 1 into six "order groups" representing every possible order in which the three treatments could be presented. For example, Order 1 presented the treatment first, followed by the treatment, and finally the treatment. Order 2 presented the treatment first, the individuals with multiple switching rows within an MPL, which violates preference monotonicity. For robustness, we calculate three different measures of individuals' risk tolerance; each measure utilizes different assumptions and yields different estimates for non-monotonic lottery

lottery, EU_A and EU_B, conditional on their risk aversion r. For example, the expected utility of lottery A is given by EU_A p_1 u x_1 $:::p_n$ u x_n, where p_n denotes the probability of x_n: Individuals then choose lottery A or B based upon the di¤erence between the two expected utilities, EU_A EU_B. We assume that the probability that an individual chooses lottery A is A EU_A EU_B, where represents the aversion, and for each treatment, to test if the di¤erent subjects responded similarly between Experiment 1 and Experiment 2. Table 2 presents the p-values from each of these tests. None of the risk aversion measures are signi…cantly di¤erent at even the 10% level between Experiment 1 and Experiment 2, for any treatment, for either the full or the analysis sample.[8]

As a further test of data integrity and poolability, and an interesting question in its own right, we test if the number of switches in each lottery task varies between the experiments and between treatment conditions. Experiment 2 contains an additional treatment, , so subjects in Experiment 2 faced a longer experimental task and higher cognitive load, which may have led to more response errors and a greater number of monotonicity violations. Table 3 presents the number of switches in each lottery task, as well as the p-values from a Wilcoxon rank-sum test for di¤erences between the two experiments. For each of the treatment conditions, there is no signi…cant di¤erence in di¤erC-39.42t1(e)9(0.7T1_0f0.920Td[(o)10(f)]TJ8T1_01Tf()Tj/TT0
i)6 (¤)104 (e)3.3Td [(e)9 (Td [(o)10 (f)]TJ /T1_0 1 Tf ( )Tj /TT0 1 Tf 175)11 (e)9 a1.852d(t)]TJ /20 (f)]TJ 8T1 0 Td (-)Tj EMC
J(-)TjEMC/P850Td[(d)11(i)6(¤)104(e)9(r)123io7T(c)9(e)]TJ/T1_01Tf()Tj/TT012[(e)9(Td[(o)10(f)]TJ/T1_01Tf()Tj/TT01

against . An identical pattern holds: subjects made more risk-averse choices in than in , and in compared to .

Table 4 presents the pooled means of Experiments 1 and 2 together for , , and . For example, subjects chose an average of 6.31 safe choices in , 6.01 safe choices in , and 5.75 safe choices in . Next, we present the main results of our paper, a test of whether subjects' estimated risk aversion di¤ers by payo¤ treatment condition. For each of the three risk tolerance measures, we conduct a Wilcoxon signed-rank test (the non-parametric analog of a paired t-test) for each of the pairwise combinations of our three treatments (vs. , vs. , and vs.). For each of the three risk tolerance measures, we …nd that subjects are signi…cantly more risk-averse in than in . That is, subjects are more risk-averse over otherwise identical lottery questions when all subjects are guaranteed to receive a payment, compared to when only one in eight subjects will receive a payment. Similarly, subjects are signi…cantly more risk-averse, on all three risk measures, in treatment than in . Subjects are more risk-averse in the "high stakes" treatment in which all subjects are paid than in the "low stakes" treatment in which everyone is paid. These two results are not surprising, as the expected value of the lotteries are EV( ) > Thera.872075k5w736(d)11(.)TJ/T101Tf()

signi…cantly more risk-averse in the condition. Our …nding of more risk aversion in the condition compared to the condition is contrary to the isolation e¤ect example of Prospect Theory above, which found less risk aversion when the compound probabilities were multiplied through into equivalent simple lotteries.

3.3 Impact of Payo¤ Treatments on the Rank the

3.4 Correction Factor Using Multiple Measurements and Latent Risk Aversion

Our experiment contains multiple measures of an individual's risk aversion, with the AllHigh condition likely serving as the "best" measurement of true risk aversion. We estimate a simple structural model which depicts an individual's observed risk aversion measure in each treatment ($AllHigh_i$, $SomeHigh_i$, $AllLow_i$) as a function of an individual's unobserved latent risk aversion ($LatentRisk_i$) plus measurement error ϵ. Specifically, design in which all subjects are paid higher stakes. We ﬁrst benchmark the prediction error from simply assuming that the results from paying only some subjects high stakes are equivalent to paying all subjects. For the MLE CRRA measure, treating an individual's response to the condition as the correct estimate for the condition generates a root mean square error (a measure of model ﬁt) of 0.278, relative to the sample mean of 0.532 for the treatment. We next consider the model ﬁt from observing an individual's choices in the condition, and using these results to predict the individual's choices in the condition via the simple regression of $AllHigh_i$ □ □ $SomeHigh_i$ $□_i$. For MLE CRRA, this regression generates the prediction of $\widehat{AllHigh}_i$: : $SomeHigh_i$, and a root mean square error of 0.247, a slight improvement over the previous model. We now consider the prediction of $AllHigh$ using the results from the above structural model. Solving the second equation for the latent risk yields $LatentRisk_i$ $SomeHigh_i$ $□_2$ $□_{2i}$ $= □_2$. Assuming that $□_{2i}$ has a mean of zero, and substituting into the ﬁrst equation yields $\widehat{AllHigh}_i$ $SomeHigh_i$ $□_2$ $= □_2$. Using the values from Table 7 of $□_2$ and $□_2$ for MLE CRRA yields $\widehat{AllHigh}_i$ $SomeHigh_i$: = : , which leads to a root mean square error of 0.147.[10] Thus, our correction generates almost a 50% reduction in the root mean square error relative to simply assuming that paying some subjects yields identical results to paying all subjects. Our correction also improves upon the gn totll up.lt(49p8.3(s)]T

and compared to when these two treatments were separated temporally. We repeat this methodology for the other treatment conditions and their corresponding temporal placement relative to the alternative treatments. Pooling these instances yields comparisons with more than 30 subjects in each group, increasing our power to detect potential order di¤erences. For the MLE CRRA, 0 of 18 of these pooled order comparisons were signi…cantly di¤erent between orders.

In summary, we tested for

4 Comparison to Previous Work

We now compare our results to previous

in the second experiment.

Unlike our findings, Beaud and Willinger (2015) found

who most over-weight small probabilities also display the greatest difference in risk aversion between the conditions with equivalent expected values, we add the risk aversion measure from , to control for the interaction of probability weighting and risk aversion. In our fullest speci…cation, we add the demographic and risk controls as well as the risk aversion measure. In no speci…cation was the coe¢ cient on the probability weighting parameter ever statistically signi…cant.[16] Our …ndings are thus similar to Barseghyan et al. (2011), who examine the concordance of risk preferences between individuals' choices of home and automobile deductibles, and …nd that probability weighting cannot explain individuals' di¤erent risk tolerance between the two.

6 Conclusion

We examined the impact on elicited risk preferences of the relatively common technique of paying only some subjects for their choices, as compared to paying all subjects for their choices. We elicited subjects' risk preferences in three conditions: a high-stakes condition in which all subjects were paid; a high-stakes condition in which only one out of eight subjects were paid; and a low-stakes condition in which all subjects were paid. This lower stakes condition had an expected value equal to one-eighth of the high condition, enabling us to examine if any change in risk that did pay all subjects.

We elicited subjects' probability weighting parameters to test if subjects who most overweight small probabilities displayed the largest differences between our "pay all" versus "pay some subjects" conditions. Probability weighting was not significantly related to the differences in risk preferences between these conditions. Standard experimental demographics, as well as alternative measures of risk preferences, were also not reliably predictive of differences between conditions.

Our experiments were a mixed payment design; in every treatment subjects were paid for a randomly selected question, and then in some treatments only a randomly selected subject was paid. Future work could examine if our [illegible]

References

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Wu.

Table 1 – Percent of Subjects Choosing Safe Option at each Decision Row

		Percent of Subjects Choosing the Safe Option							
		All Subjects				Valid MLE			
Decision	CRRA if Indifferent								
1	-1.71	99.5	95.8	99.0	98.0	100	100	100	98.8
2	-0.95	97.9	95.3	98.4	95.8	100	100	100	96.4
3	-0.49	97.9	93.2	95.8	93.8	100	100	97.7	94.0
4	-0.14	93.8	90.1	91.1	88.5	96.5	96.0	93.1	91.7
5	0.15	79.2	73.4	75.5	78.1	83.2	78.0	78.6	79.8
6	0.41	67.7	59.9	56.8	63.5	68.8	62.4	59.5	66.7
7	0.68	46.4	38.5	28.6	56.3	48.6	39.9	28.9	58.3
8	0.97	23.4	17.7	11.5	33.3	23.1	18.5	11.0	36.9
9	1.37	10.4	7.3	6.8	22.9	10.4	5.8	6.3	23.8
10	N/A	1.6	1.0	0	2.1	0	0	0	0
Observations		192	192	192	96	173	[illegible]		

Table 2 – Summary Statistics for Risk Measures By Experiment 1 and 2

	All Subjects				Valid MLE			
Num Safe Choices								
Experiment 1	: (1:55)	: (1:54)	: (1:54)		: (1:43)	: (1:44)	: (1:3)	
Experiment 2	: (1:73)	: (2:14)	: (1:65)	: (2:04)	: (1:73)	: (1:64)	: (1:65)	: (2:04)
Rank-Sum p-value	:	:	:		:	:	:	
Observations								
Switch CRRA								
Experiment 1	: (:44)	: (:443)	: (:442)		: (:406)	: (:412)	: (:40)	
Experiment 2	: (:4 3)	: (:5 3)	: (:474)	: (:57)	: (:4 1)	: (:461)	: (:47)	: (:576)
Rank-Sum p-value	:	:	:		:	:	:	
Observations								
MLE CRRA								
Experiment 1	: (:31)	: (:366)	: (:367)		: (:2 4)	: (:326)	: (:31)	
Experiment 2	: (:401)	: (:400)	: (:40)	: (:426)	: (:373)	: (:35)	: (:3 2)	: (:3 0)
Rank-Sum p-value	:	:	:		:	:	:	
Observations								

Standard deviations in parentheses.

Table 3 – Summary Statistics for Number of Switches By Experiment 1 and 2

	All Subjects				Valid MLE			
Number of Switches								
Experiment 1	: (:5)	: (:725)	: (:573)		: (:417)	: (:514)	: (:2)	
Experiment 2	: (:744)	: (: 6)	: (:665)	: (:7)	: (:243)	: (:4 6)	: (:42)	: (:460)
Rank-Sum p-value	:	:	:		:	:	:	
Experiment 1 and 2 (Pooled)	: (:673)	: (: 0)	: (:620)	: (:7)	: (:344)	: (:4)	: (:367)	: (:460)
	vs	vs	vs		vs	vs	vs	
Rank-Sum p-value	:	:	:		:	:	:	
Observations	192	192	192	96	173	173	173	84

Table 5 – Di¤ering Risk Preferences to *AllLowProb* in Experiment 2

Num Safe Choices	: (1:73)	: (1:64)	: (1:65)	: (2:04)
	vs	vs	vs	
Signed-Rank p-value	:	:	:	
Switch CRRA	: (:4 1)	: (:461)	: (:47)	: (:576)
	vs	vs	vs	
Signed-Rank p-value	:	:	:	
MLE CRRA	: (:373)	: (:35)	: (:3 2)	: (:3 0)
	vs	vs	vs	
Signed-Rank p-value	:	:	:	
Observations				

denotes ; denotes ; denotes ;and denotes Standard deviations in parentheses.

Table 6 – Rank Correlations of Risk Measures Across Treatments

Num Safe Choices			
	:		
	:	:	
	:	:	:

Switch CRRA			
	:		
	:	:	
	:	:	:

MLE CRRA			
	:		
	:	:	
	:	:	:

Table 7 – Structural Equation Model of Latent Risk Aversion

	Num Safe Choices	Switch CRRA
	–	
	–	

CRRA6(c)9(e)9(s)]TJ/T101Tf()TjEMCA6((c)9h)]T
CRRA6 (354 (e)9 (s)]TJ /TT_0 1 Tf ( )Tj EMCA6 ((c)9

Figure 1: Scatterplots of MLE CRRA by Treatment

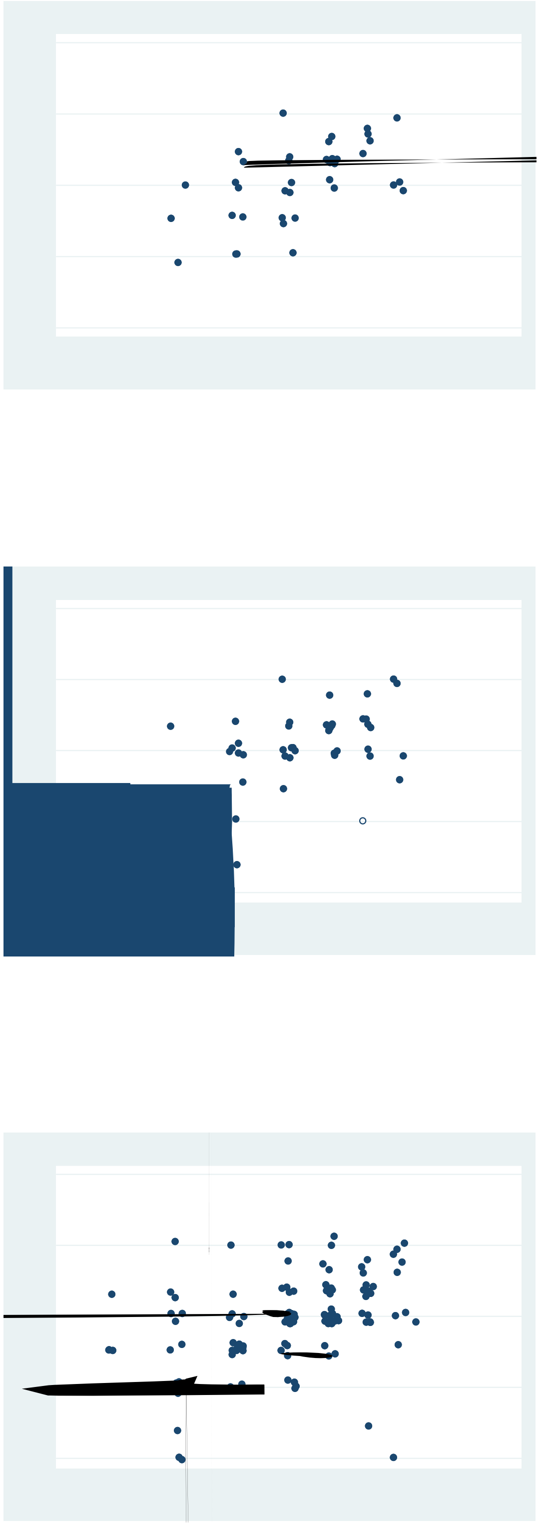

Figure 2: Distribution of MLE CRRA by Treatment

APPENDIX A: EXPERIMENTAL INSTRUMENT

INSTRUCTIONS (for Experiment 1)

You will be making choices between two lotteries, such as those represented as "Option A" and "Option B" below. Note that the actual payoffs amounts for your decisions will differ from those listed in these instructions. The money prizes are determined by throwing a ten-sided die. Each outcome, 1, 2, 3, 4, 5, 6, 7, 8, 9, 10, is equally likely. Thus if you choose Option A, you will have a 1 in 10 chance of earning $2.00 and a 9 in 10 chance of earning $1.60. Similarly, Option B offers a 1 in 10 chance of earning $3.85 and a 9 in 10 chance of earning $0.10.

Decision	**Option A**	**Option B**	**Your Choice** **Circle One**
1st	$2.00 if the die is 1 $1.60 if the die is 2 - 10	$3.85 if the die is 1 $0.10 if the die is 2 - 10	**A** or **B**

Each row of the decision table contains a pair of choices between **Option A** and **Option B**.

You make your choice by circling either "A" or "B" in the far right hand column of the table. Only one option in each row (i.e. for

Decision	Option A	Option B	Your Choice Circle One
. . 9th	$2.00 if the die is 1 - 9 $1.60 if the die is 10	$3.85 if the die is 1 - 9 $0.10 if the die is 10	**A** or **B**

After the random die throw fixes the Decision row that will be used, we need to make a second die throw to determine the earnings for the Option you chose for that row. In Decision 9 below, for example, a throw of 1, 2, 3, 4, 5, 6, 7, 8, or 9 will result in the higher payoff for the option you chose, and a throw of 10 will result in the lower payoff.

Decision	Option A	Option B	Your Choice
9th	$2.00 if the die is 1 - 9 $1.60 if the die is 10	$3.85 if the die is 1 - 9 $0.10 if the die is 10	**A** or **B**
10th	$2.00 if the die is 1 - 10	$3.85 if the die is 1 - 10	**A** or **B**

For decision 10, the random die throw will not be needed, since the choice is between amounts of money that are fixed: $2.00 for Option A and $3.85 for Option B.

Making Ten Decisions: At the end of these instructions you will see tables with 10 decisions in 10 separate rows, and you choose by circling one choice (A or B) in the far right hand column for each of the 10 rows. You may make these choices in any order.

The Relevant Decision: One of the 10 rows (i.e. Decisions) is then selected at random, and the Option (A or B) that you chose in that row will be used to determine your earnings. Note: Please think about each decision carefully, since each row is equa(o)80.95 Td[At the ens i5sens gtereastween u)Tjne

INSTRUCTIONS (for Experiment 2)

ID Number:

You will be making choices between two lotteries, such as those represented as "Option A" and "Option B" below. Note that the actual payoffs amounts for your decisions will differ from those listed in these instructions. The money prizes are determined by the computer equivalent of throwing a ten-sided die. Each outcome, 1, 2, 3, 4, 5, 6, 7, 8, 9, 10, is equally likely. If you choose Option A in the row shown below, you will have a 1 in 10 chance of earning \$2.00 and a 9 in 10 chance of earning \$1.60. Similarly, Option B offers a 1 in 10 chance of earning \$3.85 and a 9 in 10 chance of earning \$0.10.

Decision	**Option A**	**Option B**	**Your Choice** **Circle One**
1st	\$2.00 if the die is 1 \$1.60 if the die is 2 - 10	\$3.85 if the die is 1 \$0.10 if the die is 2 - 10	**A** or **B**

Each row of the decision table contains a pair of choices between **Option A** and **Option B**.

You make your choice by circling either "A" or "B" in the far right hand column of the table. Only one option in each row (i.e. for each Decision) can be circled.

Decision	**Option A**	**Option B**	**Your Choice** **Circle One**
1st	\$2.00 if the die is 1 \$1.60 if the die is 2 - 10	\$3.85 if the die is 1 \$0.10 if the die is 2 - 10	**A** or **B**
2nd . .	\$2.00 if the die is 1 - 2 \$1.60 if the die is 3 - 10	\$3.85 if the die is 1 - 2 \$0.10 if the die is 3 - 10	**A** or **B**

Even though you will make ten decisions, **only one** of these will end up being used. The selection of the one to be used depends on the "throw of the die" that is the determined by a random number generator. No decision is any more likely to be used than any other, and you will not know in advance which one will be selected, so please think about each one carefully. This random selection of a decision fixes the row (i.e. the Decision) that will be used.

For example, suppose that you make all ten decisions and the random number is 9, then your choice, A or B, for decision 9 below would be used and the other decisions would not be used.

To summarize, you will indicate an option, A or B, for each of the rows by circling one choice in the far right hand column.

Then a random number fixes which row of the table (i.e. which decision) is relevant for your earnings.

In that row, your decision fixed the choice for that row, Option A or Option B, and a final random number will determine the money payoff for the decision you made.

In addition, in some cases, there will be a die throw to determine which person in the room will be paid for the set of decisions on a particular sheet. The top of each decision sheet explains who will be paid for that particular decision sheet.

This whole process will be repeated, but the prize amounts may change from one sheet to the next, so look at the prize amounts carefully before you start making decisions.

APPENDIX Table 1: *AllHigh* Condition

EVERYONE IN THE ROOM WILL BE PAID FOR 1 OF THE 10 DECISIONS ON THIS SHEET

Your Dec6sion

Your Dec5sion

Decision	Option A	Option B	Your Decision

APPENDIX Table 3: *AllLow* Condition

<u>EVERYONE IN THE ROOM</u> WILL BE PAID FOR 1 OF THE 10 DECISIONS ON THIS SHEET.

Decision	Option A	Option B	Your Decision Circle One
1	\$4.00 if the die is 1 \$3.20 if the die is 2-10	\$7.70 if the die is 1 \$0.20 if the die is 2-10	**A** or **B**
2	\$4.00 if the die is 1 -2 \$3.20 if the die is 3-10	\$7.70 if the die is 1-2 \$0.20 if the die is 3-10	**A** or **B**
3	\$4.00 if the die is 1-3 \$3.20 if the die is 4-10	\$7.70 if the die is 1-3 \$0.20 if the die is 4-10	**A** or **B**
4	\$4.00 if the die is 1-4 \$3.20 if the die is 5-10	\$7.70 if the die is 1-4 \$0.20 if the die is 5-10	**A** or **B**
5	\$4.00 if the die is 1-5 \$3.20 if the die is 6-10	\$7.70 if the die is 1-5 \$0.20 if the die is 6-10	**A** or **B**
6	\$4.00 if the die is 1-6 \$3.20 if the die is 7-10	\$7.70 if the die is 1-6 \$0.20 if the die is 7-10	**A** or **B**
7	\$4.00 if the die is 1-7 \$3.20 if the die is 8-10	\$7.70 if the die is 1-7 \$0.20 if the die is 8-10	**A** or **B**
8	\$4.00 if the die is 1-8 \$3.20 if the die is 9-10	\$7.70 if the die is 1-8 \$0.20 if the die is 9-10	**A** or **B**
9	\$4.00 if the die is 1-9 \$3.20 if the die is 10	\$7.70 if the die is 1-9 \$0.20 if the die is 10	**A** or **B**
10	\$4.00 if the die is 1-10	\$7.70 if the die is 1-10	**A** or **B**

Result of first random number generated (to determine Decision): ___________

Result of second random number generated (to determine Payoff): ___________

Payoff: ___________

Full-time student

11. How many people participating in this experiment today do you consider to be your friend?
How often do you recycle?
Nearly all the time (every day)
Frequently (a few times a week)
Occasionally (a few times a month)
Never

12. Are you a U.S. citizen?
Yes
No

13. How often do you buy environmentally of socially labeled products (for example, fair trade products, low energy light bulbs, or recycled products)?
Nearly all the time when I shop
Occasionally when I shop
Never

14. During the past two years have you been a member, contributed time, or contributed money to a social organization (for example, soup kitchens or Big Brother-Big Sister).
Yes
No

15. If you are a member of a political party, to which party do you belong?
Democratic
Republican
Libertarian
Green
Other
I am not a member of a political party

16. Which political party best represents your interests?
Democratic
Republican
Libertarian
Green
Other

17. How often do you wear a seatbelt when driving or riding in a car?
Always, or almost always
Most of the time
Some of the time
Never, or almost never

18. If you drive a car, how often do you drive over the speed limit?
Always, or almost always
Most of the time
Some of the time
Never, or almost never
Not applicable; I don't drive a car

19. How often have you gambled or purchased lottery tickets in the last year?
Never
Once or twice
Between three and twelve times

41. What is your primary academic interest area/major area?
    Sciences
    Social Sciences
    Arts and Humanities
    Business

Closing Statement: Thank you for completing the survey. Please remain seated momentarily and someone will come to your desk to pay you for your participation in the experiment.