An experimental test of criminal behavior among juveniles and young adults

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Introduction

Since the seminal paper of Becker (1968), which created the foundation for the economic analysis of criminal behavior, economists have extended the basic theoretical framework.[1] The original framework, as well as its more recent variants, postulate that participation in crime is the result of an optimizing individual's response to incentives such as the expected payoffs from criminal activity, and costs such as the probability of apprehension and the severity of punishment.

Although early empirical research reported evidence suggesting that enhanced deterrence reduces crime (Ehrlich 1975, Witte 1980, Layson 1985), other papers found no significant evidence of deterrence (Myers 1983, Cornwell and Trumbull 1994). The main challenge in empirical analysis has been to tackle the simultaneity between criminal activity and deterrence. Specifically, an increase in criminal activity is expected to prompt an increase in the certainty and severity of punishment (e.g. an increase in the arrest rate and/or the size of the police force), which makes it difficult to identify the causal impact of deterrence on crime. Three types of strategies have been used to overcome the simultaneity problem. The first solution is to find a good instrument which is correlated with deterrence measures but uncorrelated with crime. Examples are Levitt (1997) which uses electoral cycles as an instrument for police hiring and Levitt (2002) which uses the number of per capita municipal firefighters as an instrument for police effort. The second strategy is to use high-frequency time-series data. For example, in

[1] See, for example, Ehrlich 1973, Block and Heineke 1975, Schmidt and Witte 1984, Flinn 1986, Lochner 2004, Mocan et al., 2005.

monthly data, an increase in the police force in a given month will affect criminal activity in the same month, but an increase in crime cannot alter the size of the police force in that same month because of the much longer lag between a policy decision to increase the working police force and the actual deployment of police officers on the street. This identification strategy has been employed by Corman and Mocan (2005) and Corman and Mocan (2000). The third strategy is to find a natural experiment which generates a truly exogenous variation in deterrence, as in Di Tella and Schargrodsky (2004) who use the increase in police protection around Jewish institutions in Buenos Aires after a terrorist attack to identify the impact of police presence on car thefts.

Although these empirical strategies have permitted researchers to refine and improve upon earlier estimates, a convincing natural experiment is very difficult to find. The validity of any instrumental variable can always be questioned, and one can argue that if policy makers have perfect foresight about future crime, monthly data could also suffer from simultaneity.

In this paper, we use a laboratory experiment to investigate individuals' responses to unambiguously exogenous changes in the rewards and penalties pertaining to criminal behavior. The experiments involve decisions about actions that can best be described as petty larceny, and are conducted using high school and college students and real money. The experiment is designed to test the responsiveness of changes in the fundamental tradeoffs that potential criminals face.

We use two similar but different protocols for collecting choice data that can be used to test Becker's model directly. The protocols involve collecting information on (nearly) simultaneous choices made by individuals under a variety of different budget constraints. These choices are used to check whether people's decisions about their criminal behavior change rationally in response to variations in the probability of detection and changes in the size of the fine.

The data are first used to check for transitivity violations. Transitivity is a necessary and sufficient condition for modeling choices as the result of constrained utility maximization. This provides a direct test of Becker's model, which assumes rational choice by criminals. We then estimate demand functions for stolen loot as a function of the loot price, fine, and detection probability. Becker (1962) points out that rational choice is not necessary for choices to satisfy the laws of demand. More fundamentally, aggregate choices may obey the laws of demand even if some individual choices are inconsistent with utility maximization. Therefore, we expect to be able to provide results about the tradeoffs between prices even if choices occasionally, or even frequently, violate the Generalized Axiom of Revealed Preference (GARP).

Section II provides a brief description of revealed preference theory, and the experimental methods for testing it. Section III describes the revealed preference results. Section IV presents the results pertaining to the estimated demand for stolen good. Section V is the conclusion.

II. Revealed Preference

In this section we provide a simplified discussion of revealed preference theory, one which relies on the assumption of continuous budget sets. Harbaugh et al. (2001) give the argument for the case of discrete choice sets, which were used in the experiment of this paper.[2] The basic principle of the experiment can be seen in figure 1. Choices *a* and *b* are irrational by the following argument. When *a* was picked from the budget set *A*, the alternative *b* was within that set. If the person was choosing rationally then $u(a) \geq u(b)$. Because of monotonicity and continuity, we can strengthen this statement to indicate $u(a) > u(b)$, because an alternative bundle with more of at least one good (actually both) than *b* is available within choice set *B*. By the same argument, we also know $u(b) > u(a)$. This is a contradiction. Thus, a person who made these choices could not have been choosing rationally. This is shown formally in Samuelson (1938), who shows that choices which are consistent with the Weak Axiom of Revealed Preference (WARP) are a necessary requirement for data that comes from maximizing a utility function.

This example only uses 2 choice sets, allowing direct comparisons. As will be explained below in detail, in the experiment conducted in this paper we collect choices from 10 choice sets. Therefore, in addition to the above direct tests of Strong Axiom of Revealed Preference (SARP), we can also test for chains of irrational choices that involve indirect comparisons, such as when choices reveal $u(a) > u(b) > u(c) > u(a)$. Houthakker (1950) showed that choice data which do not reveal these sorts of intransitivities, that is which satisfy SARP, are a necessary condition for rational choice; and Afriat (1967) showed that satisfying SARP was sufficient as well. Varian (1982) further generalized revealed preference, to allow indifference curves to have flat spots. His result on the equivalency of utility maximization and satisfying revealed preference is known as the Generalized Axiom of Revealed Preference (GARP).

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[2] In particular, Harbaugh et al. (2001) explain why strong monotonicity of preferences needs to be assumed with discrete choice, rather than the weaker assumption of local non-satiation.

Sippel (1997) is the first example of a properly controlled laboratory experiment applying revealed preference axioms to consumption data. Subjects were asked to choose among several snacks and time-passing goods, and to pay for them using an endowment of an artificial currency. Choices were made almost simultaneously, and the "strategy method" was employed, so that one choice was chosen randomly and then implemented.[3] In each of two treatments, nearly half or more subjects had at least one WARP, SARP, and/or GARP violation. However, the demand analysis supported the view that subjects do not choose randomly, and typically behave according to neoclassical predictions.

Harbaugh, Krause, and Berry (2001) applied the revealed preference test in a series of consumption games on children ages 7 to 11, and on undergraduates. Children and undergraduates were offered choices among eleven different choice sets, each bundle consisting of a number of small bags of chips and boxes of juice. Choice sets were constructed using implicit price and income vectors which form overlapping budget sets.

Harbaugh Krause and Berry (2001) found that nearly half to three quarters of subjects exhibited at least one violation, though the average number of violations tended to be relatively small. On average, even for the youngest children, there were significantly fewer transitivity violations than would be expected under random choice. While the authors discouraged generalization from their experiment., these results

[3] The strategy method involves multiple rounds of the same game, where the experimenter will randomly choose only one (or perhaps a strict subset) of the rounds to calculate the subjects' earnings. In this case, the strategy method provides an incentive for subjects to treat each round as a single play of the game, and maximize expected utility over the entire random lottery. This may actually mitigate against the problem of changing or evolving preferences during the experiment. The strategy method is a common procedure in many experimental settings.

suggested that even children may be rational utility maximizers, at least with respect to relatively familiar consumption goods.

Andreoni and Miller (2002) offered their subjects opportunities to share an endowment with another anonymous partner. Implicit prices and incomes were varied across decisions to create intersecting budget sets, allowing researchers to check for GARP consistency. The majority of subjects showed altruistic behavior, in varying degrees, and almost half of the subjects exhibited behavior that was exactly consistent with at least one of the utility functions estimated, or, equivalently had no GARP violations.

III: Experimental Design

The experiments were conducted in high school math classes in Eugene, Oregon, with the permission of the school district , principals, and teachers. The students were read an assent agreement and were able to opt out of the experiment, but none did. Following standard experimental protocols, we provided complete information to the participants, paid them privately in cash, and there was no deception. Because school attendance rates are high, this procedure provides a fairly representative sample of the area high school age population. However, the sample is not nationally representative – Eugene is a medium sized college town with a population that is richer and whiter than the US as a whole. We also recruited subjects from two upper-division undergraduate courses at the University of Oregon. These students differ from the high school students in many ways. In particular, they have higher GPAs, more money, and are older.

The basic decision in the experiment involved deciding how much to take from your partner, given specified probabilities of getting caught and specified fines that must be paid if you are caught. Everyone made a decision as if they were the criminal, and we then randomly determined the roles for the payoffs, within each pair. Since subjects were randomly and anonymously matched with a partner from their class, they knew the other participants well, but they didn't know exactly who they were stealing from, or who was stealing from them.

People made a series of these decisions, from different choice sets. The sets were constructed from budget sets defined over three goods: stolen loot, the probability of not getting caught (getting away with stealing), and the amount kept, after repayment of the stolen loot and the fine, if caught. Each choice set consisted of a list of bundles of these three goods. The bundles can be thought of as different crimes; that is, some will involve taking a little money, facing a good chance of getting away with it, and a modest fine if caught, while others will involve a higher amount of loot, but a lesser chance of getting away with it, and so on. Taking nothing from your partner is always an option.

The list of bundles for each choice set are constructed with different implicit incomes and prices. These prices can be viewed as the rates of tradeoff between loot, the probability of getting away with the theft, and the smallness of the fine if caught. That is, a high implicit price for loot relative to the price of getting away with it means that, in this choice set, choosing a crime with lots of loot will cost dearly in terms of the chances of getting caught. Incomes can be thought of as the overall extent of criminal opportunities available. A higher income means that, relative to a low income choice set, there are crimes available that involve not only a lot of loot, but also a very good chance of getting away with the crime, and small fines if caught.

The implicit incomes and prices are varied in such that the choice sets overlap, with the intersections of the constraints designed in such a way as to ensure many possibilities for making intransitive choices. The variations in incomes and prices also provide data about decisions under a variety of conditions, which we use to estimate demand functions.

We used 2 protocols, one with neutral language, such as person A, person B, and "take from person B", and uniform starting endowments of $5. Protocol 2 used more loaded language, such as criminal, victim and "steal from the victim" and endowments of $8, $12, or $16. Protocol 1 included duplicate choice sets, to check for consistency. To check for the monotonicity of preferences, protocol 1 included choice bundles that were on the interior of the budget set, while protocol 2 included choices sets that were rays from the origin. The complete protocols are in the Appendix.

Table 1A presents summary information on the choice sets for both Protocol 1 and Protocol 2, and Table 1B displays the menu of bundles for a representative choice set from protocol 1. As can be seen, the stakes are higher for Protocol 2.[4]

[4] The null hypothesis for our revealed preference test is that people are rational. Bronars (1987) finds that power (the probability of not committing a Type II error) is maximized when two-good budgets bisect each other at arbitrarily small angles (that is, they nearly coincide). The three-good analog is that budget planes intersect each other such that the area on either side of the intersection is equal in both budgets, and that they intersect at arbitrarily small angles. In terms of our experimental design, there is a clear tradeoff between Bronars' power to detect revealed preference violations and the ability to detect large violations, in the sense of the Afriat Index. If the choice sets are designed to minimize the chance of a Type II error, we do not present any opportunities for costly mistakes, so we have little idea about subjects' likely responses to costlier mistakes. On the other hand, if we design the choice sets to minimize the chance of a Type I error, we may not be able to detect any revealed preference violations. While we take maximization of Bronars power as the starting point for parameter choice, we are forced to relax it to some degree in order to accommodate the other desirable characteristics, including a balance with the Afriat Index.

Table 1a
Choice Set Characteristics

Protocol 1 budget parameters

Budget	loot_p	prob_p	nfine_p	income
1	.25	1	1	1.2
2	1	2	1	2.9
3	.5	4	1	3.25
4	.5	2	1	2.25
5	.25	1	1	1.2
6	1	2	1	2.75
7	.5	4	1	3.25
8	.5	2	1	2.25
9	1	4	1	3.75
10	.25	2	1	1.75

Protocol 2 budget parameters

Budget	loot_p	prob_p	nfine_p	income
1	2	20	2.5	18.25
2	2	20	5	18.25
3	2	24	2.5	18.25
4	2	24	5	18.25
5	2.25	20	2.5	18.25
6	2.25	20	5	18.25
7	2.25	24	2.5	18.25
8	2.25	24	5	18.25
9	2.5	20	2.5	18.25
10	2.5	20	5	18.25

Table 1b

Bundles from Choice Set 5, Protocol 1

You each start with $5

Mark one choice below	Dollars to take from Person B	Your payment including your starting $5 if you are not discovered	Chance that you are discovered	Dollars paid to experimenter if discovered	Your payment including your starting $5 if you are discovered
	$0	$5	---	---	---
	$1.00	$6.00	25%	$1.55	$3.45
	$1.00	$6.00	50%	$1.30	$3.70
	$1.00	$6.00	75%	$1.05	$3.95
	$1.00	$6.00	75%	$1.25	$3.75
	$2.00	$7.00	50%	$1.55	$3.45
	$2.00	$7.00	75%	$1.30	$3.70
	$3.00	$8.00	75%	$1.55	$3.45

Sample Bundles, from Choice Set 5, Protocol 2

You have ($8, $12, or $16) to start with

Mark one choice below	Amount to steal from the victim	Chance of being caught	Fine if you are caught
	$0.00	0%	$0.00
	$2.00	75%	$0.10
	$2.00	50%	$2.10
	$2.00	25%	$4.10
	$4.00	75%	$1.90
	$4.00	50%	$3.90
	$6.00	75%	$3.70
	$8.00	75%	$5.50

The subjects are told that they must choose one bundle from each choice set. After they and their matched-partner have made their choices in all 10 rounds (10 different choice sets), we randomly determined whose choice is implemented– that is, whether they were the criminal or the victim. Then, one of the criminal's choice sets was randomly chosen, and whatever choice they made from that particular set was implemented. We used a randomized procedure to determine if they got away with any “crime” they might have chosen, or if they were instead caught and must return any loot to the victim and also pay the fine. The criminal and the victim were then paid the resulting amounts in cash.

The protocol is designed to make sure that choices are made carefully. Specifically, we gave the subjects a chance to change their minds after thinking things over, and as explained below, we perform a simple test of whether or not independence holds in this context.

First, we gave the subjects 30 seconds to choose a bundle from each of the 10 choice sets. We told them not to go on to the next choice set until the 30 seconds were up. We call these 10 choices their “first” choices. For the second choices, we had them go through the list again, spending a further 15 seconds on each choice set, and marking any changes they would like to make by crossing out the old and circling the new.

In Protocol 1, we also included duplicate budget sets to investigate whether people were making internally consistent choices. As can be seen in Table 1A, budgets 1 and 5, 3 and 7, and 4 and 8 are the same. In Protocol 2, we conduct no test of independence and therefore we do not duplicate any budget sets. We test for independence in Protocol 1 by giving the subjects another chance to change their minds, after the uncertainty about their role and the actual choice set is resolved. Specifically, after we told them whether they are the criminal or the victim (after they made their second choices), we had them go through the choice sheets yet again. Finally, we told them which specific choice sheet had been chosen for implementation, and they could then change their choice on that sheet, if they wished.

Information about choices at each stage is retained for analysis. Choice sets were ordered starting with the low income version, and going to high. The order of choices within the choice sets was blocked, with half the participants receiving forms where the amount of loot that can be taken is ranked from low to high as one goes down the page, and half vice versa.

Participants were recruited from high school math classes in Eugene, OR. After obtaining permission from the school district and the Principal, we contacted teachers who were willing to allow the experiment in their classes. Experiments with Protocol 1 are performed in a total of six classes, and the Protocol 2 experiments are conducted in a total of 3 classes. Students were matched with other students in their class, so they know the other participants well. However, all interactions and payoffs are anonymous and secret. One might expect that total strangers would have a greater propensity to steal compared to anonymous classmates. Because school attendance rates are high, this procedure provides a fairly representative sample of the area high school age population. However, the sample is not nationally representative – Eugene is a medium sized college town with a population that is richer and whiter than the US as a whole. We also recruited subjects from two upper-division undergraduate courses at the University of Oregon, one for each protocol. These students clearly differ from the high school students in many ways. In particular, they have higher GPAs, more money, and are older, as will be shown below. Protocol 1 experiments were done on 78 high school and 28 college students. The Protocol 2 experiments had 76 high school students and 34 college students.

IV. Results

Consistency and Independence

As described above, in Protocol 1 the budget parameter on choice sets 1 and 5 are identical, as are those for choice sets 3 and 7, and 4 and 8. We checked whether subjects made the same choices when presented with duplicate choice sets. Because there are 114 individuals in Protocol 1, we have 342 pairs of duplicate choice sets (3 pairs for each subject). For the first decision they made on each choice set, 167 of the 342 pairs (49%) are the same. For the final decision, 196 of 342 (57%) are the same. This is reassuring as it suggests that re-thinking about the decision increases consistency. When we regress the number of matched choices on demographics nothing is significant, including whether the subject is a high school student, and the GPA measures. About 75 percent of participants change at least one choice in round 2 (21% in Protocol 2), about 20 percent change in round 3, and no one changes his or her decision in round 4. We take this as evidence that choice behavior under the uncertainty, which is resolved in rounds 3 and 4, generally obeys independence. This is important, because independence is a necessary requirement for our protocol to generate data that can be used to test rationality.

Rationality and Revealed Preference

We check for revealed preference violations using an algorithm from Varian (1995) which has been modified to handle three goods and discrete bundles. Tests of SARP and WARP yield comparative results that are very similar to those from GARP, so only results for GARP are reported here. The revealed preference test has no power to detect irrational behavior if choices are at corners, so we remove those subjects who steal everything and those who steal nothing for this analysis. Table 2 displays the average number of GARP violations for high school subjects, and college subjects, and provides a comparison to bootstrap random choice for both protocols. In the bootstrap random choice, each bundle is weighted by its frequency in the overall choice distribution.

Table 2
Average GARP Violations

	Protocol 1				Protocol 2			
	HS	UO	All	Bootstrap	HS	UO	All	Bootstrap
GARP (no corners)	4.28 (0.43)	4.43 (0.79)	4.32 (0.38)	6.45 (0.02)	1.71 (0.24)	1.56 (0.33)	1.67 (0.20)	2.56 (0.02)
N	78	28	106	10,000	76	32	108	10,000
GARP (no corners, monotonic)	2.00 (0.45)	1.83 (0.97)	1.96 (0.41)	6.45 (0.02)	1.81 (0.25)	1.55 (0.34)	1.73 (0.20)	2.56 (0.02)
N	36	12	48	10,000	67	31	98	10,000

Each subject group exhibits significantly fewer violations than bootstrap choice. The average number of GARP violations across all subjects not choosing at the corners is 4.32 in Protocol 1 and 1.67 in Protocol 2. In Protocol 1 the average number of violations in round 1 is 4.8 and in Protocol 2 it is 1.75. This suggests that, on average, the choice modifications that people make are moving them towards greater rationality. Protocol 2 has a significantly lower number of bootstrap violations than does Protocol 1. This means that, in Protocol 2, the ability to detect violations (in the Bronars power sense) is less, but the ability to detect costly violations is greater. Because the budget sets intersect at greater angles in Protocol 2, any violations are more costly in terms of wasted income than they are in Protocol 1. This makes Protocol 2 more informative about individual rationality than Protocol 1, because making a greater number of less costly violations is not necessarily less rational than making a smaller number of more costly violations.

The lower panel of Table 2 reports average GARP violations for those subjects not did not choose corner bundles and who have at least 8 (out of a possible 10) monotonic choices in Protocol 1, or 2 monotonic choices (out of a possible 3) in Protocol 2. Table 2 demonstrates that, In Protocol 1 average GARP violations drops significantly when individuals with non-monotonic choices. In Protocol 2 average GARP violations for college students is about the same, but it increases slightly for high school students. In each case the average number of violations is still significantly less than bootstrap random choice.

Table 3 presents frequencies for the number of GARP violations per subject. (All of the counts in Table 3 are for the final choices.) Note that, since a minimum of two choices is required to check for a transitivity violation, it is impossible to have just one violation. About 40 percent of the subjects have no GARP violations in Protocol 1 and 54 percent have no GARP violations in Protocol 2.

Table 3
Frequency of GARP Violations

	Protocol 1				Protocol 2			
GARP violations	HS	UO	All	Bootstrap	HS	UO	All	Bootstrap
0	34%	43%	35%	3%	51%	50%	51%	33%
1*	0%	0%	0%	0%	0%	0%	0%	0%
2	6%	0%	5%	3%	14%	19%	16%	19%
3	6%	0%	5%	5%	17%	19%	17%	16%
4	8%	0%	6%	6%	8%	6%	7%	11%
5	9%	11%	9%	10%	3%	3%	3%	8%
6	5%	7%	6%	16%	3%	0%	2%	6%
7	4%	7%	5%	20%	1%	3%	2%	4%
8	6%	7%	7%	21%	3%	0%	2%	2%
9	6%	11%	8%	14%	0%	0%	0%	1%
10	16%	14%	14%	4%	0%	0%	0%	0%
N	78	28	106	10,000	76	32	108	10,000

*Note that it is impossible to have exactly one violation.

There is no obvious standard against which to compare the number of GARP violations. The revealed preference theorems described above require that choices obey the axioms without exception. In practice, this standard is not met. Sippel (1997) used a similar protocol for eight different consumption goods, using 10 different budget sets. He found that 24 of 42 participants violated GARP at least twice. Andreoni and Miller (2002) examined 142 college students' decisions about how much money to keep for themselves and how much to share with another, under eight different budget constraints. They found that nine percent of the participants committed at least some violations of the revealed preference axioms. Harbaugh et al. (2001) looked at decisions over two consumption goods and 11 choice sets. Eleven-year-olds and college students had similar patterns, with about 35 percent displaying GARP violations. The average number of violations was about two.

The task in our experiment is more difficult, in terms of the number of goods, than that in the Andreoni and Miller 92002) or Harbaugh et al. (2001) experiments, but simpler than that of Sippel (1997). We can't make a direct comparison, but our results seem generally consistent with those from by other experiments.

Table 4
Monotonic bundle choices

Monotone Choices	Protocol 1 Sample Frequency	Protocol 1 Sample Running total	Bootstrap Frequency	Bootstrap Running Total
0	0%	0%	0%	0%
1	7%	7%	0%	0%
2	4%	11%	1%	1%
3	5%	16%	3%	4%
4	10%	26%	10%	14%
5	5%	32%	20%	34%
6	12%	44%	27%	61%
7	10%	54%	22%	83%
8	6%	60%	12%	95%
9	16%	76%	4%	99%
10	25%	100%	1%	100%
N	114		10,000	

As in Harbaugh, Krause, and Berry (2001), and Andreoni and Miller (2002), our revealed preference test requires that preferences are strongly monotonic. Rather than take this on faith, our experiment is designed to test this assumption. In Protocol 1 we do this by including dominated bundles in the choice set – that is, bundles with simultaneously lower loot and/or higher probabilities of detection and fine than other options in the choice set. Table 4 gives the frequencies for the number of monotone choices for our sample from Protocol 1, and compares this to what would occur with random selection using a Monte Carlo simulation with 10,000 sets of uniform draws from our choice sets. The average number of monotone choices is 7.0, and 46 percent of our 114 participants made eight or more monotone choices. In comparison, 17 percent have 8 or more monotone choices in the Monte Carlo simulation.

To test for monotonicity in Protocol 2, we include three upward sloping budget sets, similar to Andreoni and Miller (2002). A subject with monotonic preferences would choose the bundle at the very top of the budget line. In this protocol only 20 percent of our subjects made any non-monotonic choices and only 3percent made the maximum of 3 non-monotonic choices. It is important to note that non-monotonic preferences are not necessarily irrational, and can be explained by fairness models.[5] For example, other-regarding preferences may lead a subject to steal less from an anonymous classmate than pure self-interest would dictate.

Sippel (1997) reported that most of his participants spent their entire budget, and those who did not came very close to spending it all. The Harbaugh et al. (2001) paper assumed monotonicity, and did not include any procedure for testing it. Andreoni and Miller (2002) had a larger percentage of monotonic choices (88%) than we find. Our experiment involved a significantly greater number of alternatives, and our monotonicity test was integrated into each budget set for Protocol 1, while the Andreoni paper constructed separate budget sets specifically for the purpose of testing for monotonicity. It seems likely that Andreoni's procedure makes the non-monotonic choices more obvious and less likely to be chosen. Our results from Protocol 2 appear to be consistent with those of the Andreoni paper.

We attempted to explain the rationality of participants' choices with some socio-demographic characteristics collected in a post-experiment survey. (This survey was not conducted on college students in Protocol 2.) Definitions and descriptive statistics of the variables are provided in Table 5. Oldest Child is a dichotomous variable to indicate if the subject is the oldest (or only) child in his or her family. Tenure is the number of years the subject has lived in Oregon. A larger value may be considered as a proxy for enhanced ties to friends and community, and might be expected to have a negative correlation with stealing.

[5] See for example Rabin (1993), Fehr and Schmidt (1999), Bolton and Ockenfels (2000).

Table 5 Descriptive Statistics of Personal Characteristics

		Protocol 1		Protocol 2	
Variable	Definition	High School	College	High School	College
Loot*	The money stolen in each round	1.23 (0.08)	1.77 (0.13)	2.91 (0.18)	4.04 (0.22)
GARP	Number of GARP violations	4.02 (0.42)	4.00 (0.75)	1.59 (0.23)	1.47 (0.31)
Age	Age of the individual	15.98 (0.11)	22.01 (0.17)	16.86 (0.14)	NA
Height	Height of the individual in feet	5.59 (0.03)	5.88 (0.06)	5.60 (0.04)	NA
GPA	High school GPA if the individual is in high school; the average of high school and college GPAs if the in college	3.12 (0.06)	3.34 (0.08)	3.46 (0.06)	NA
Money	How much money the individual spends on his/her own per week	18.34 (1.98)	72.71 (31.34)	20.72 (3.13)	NA
Male	Dichotomous variable (=1) if the person is male	0.51	0.71	0.41	NA
Oldest Child	Dichotomous variable (=1) if the person is oldest child	0.34	0.52	0.48	NA
Tenure	The number of years the person lived in Eugene, Oregon	8.39 (0.39)	5.53 (0.83)	9.75 (0.40)	NA
N		83	31	82	34

* Loot is the average of all 10 rounds
Socio-demographic data were not collected from the college sample in Protocol 2

Because the data are discrete and cardinal, we estimate count data models in an attempt to explain the number of GARP violations using the socio-demographic variables. Table 6 presents the results negative binomial regressions. Poisson regressions yield similar results. The first two columns are estimated from Protocol 1 data, while the third is from Protocol 2 data. The model reported in the first column includes age, gender, GPA, the height of the individual, whether he or she is a high school student, oldest child, and the amount of money spent per week. The second column displays a more flexible specification where the explanatory variables are interacted with the High School dummy, allowing the hypothesis of differential impact of the variables by High School vs. college status.

Among college students, being one year older is associated with about 0.6 fewer GARP violations. While the interaction of Age and High School is positive and significant, the sum of this coefficient and the coefficient of age is not statistically different from zero, indicating that among high school students age does not contribute to the explanation of the number of GARP violations. Gender, height and being the oldest child have no impact on the number of GARP violations. The third specification is similar to the first, but it employs data from protocol 2. Because socio-demographic data were not collected on the college students for Protocol 2, the flexible specification cannot be estimated for this sample. A general conclusion from Table 6 is that the degree of rationality of behavior is not well explained by the available socio-demographic variables.

Crime and deterrence

In this section of the paper we estimate demand functions for stolen money. Specifically, we investigate the determinants of the amount of loot stolen as a function of personal characteristics of the person who steals, the price of the stolen loot, the probability of being caught, and the amount of fine. Table 7 presents the distribution of the number of thefts. During the 10 rounds of the experiment, each individual had the opportunity to steal 10 times. Thus, in Table 7, zero thefts means that the individual never stole during the experiment, and a 10 indicates that he or she stole money in every round. There is substantial variation in the number of thefts, with 49 percent of the subjects stealing money in each round in Protocol 1, and 52 percent in Protocol 2.

Table 7 Number of Thefts

	Protocol 1		Protocol 2	
Number of Thefts*	Number of Individuals	Percentage of Total	Number of Individuals	Percentage of Total
0	5	4%	7	6%
1	2	2%	2	2%
2	2	2%	1	1%
3	4	4%	3	3%
4	6	5%	1	1%
5	2	2%	5	4%
6	6	5%	7	6%
7	6	5%	9	8%
8	13	11%	7	6%
9	12	11%	14	12%
10	56	49%	60	52%

*The number of thefts is the number of rounds where the individual stole money. Thus, 0 indicates that the individual did not steal money during the entire experiment, and 10 indicates that he/she stole in every round.

If people are choosing rationally, then we would expect them to respond to the changes in implicit prices in ways that are consistent with the laws of demand. For example, we would expect that participants will respond to an increase in the cost of choosing a crime with high loot by tending to move toward crimes with less loot but also lower probabilities of detection and/or lower fines. An increase in available criminal opportunities should also be expected to increase loot, assuming it is a normal good. In these regressions, we normalize the implicit prices by dividing through by income. We expect, therefore, to find negative own price effects and positive cross price effects.

Table 8 presents the results of the estimated demand functions. The dependent variable is the amount of loot taken in each round. Each individual contributes 10 observations for the 10 choices that were made. Models are estimated with OLS, and standard errors are adjusted for clustering at the individual level. The control variables included in the regressions may not adequately capture unobserved individual heterogeneity that may be correlated with the propensity to steal. Thus, in some specifications we add individual fixed-effects to control for such unobservables.

The estimated directions of the effects (i.e. the signs of the coefficients) are critically important for an investigation of whether individuals respond to incentives to modify their decisions regarding stealing. The interpretation of the magnitudes is less clear. If agents make the choices that maximize expected value, they would have an elasticity of -0.057 in Protocol 1 and -1.55 in Protocol 2. Using average prices and quantities of stolen loot, the actual choices in Protocol 1 result in an estimated demand elasticity of -1.62, and the Protocol 2 choices result in an estimated demand elasticity of -27.86. Both suggest that stolen loot is an elastic good, but the protocol 2 choices are much more responsive to changes in the price of stolen loot. This indicates that actual behavior is more responsive to price changes than it would be if behavior were consistent with ordinary expected value maximization.

This is a potentially important result, because it indicates that the behavioral restrictions that expected value maximization imposes may be too restrictive. In fact, Becker proposed that criminals are not only rational, but risk-averse expected utility maximizers. The estimates presented in Table 8 suggest that those assumptions are too restrictive. This is an addition to the mounting evidence against the neoclassical expected utility framework. People do make informed and calculated decisions over financial rewards. However, it is too restrictive to simply assume that (expected) money directly enters the utility function. The individual components of the expectation are important.

Table 8
Demand for Loot

Variable	Protocol 1					Protocol 2	
	I	II	III	IV	V	VI	VII
Loot Price	-3.88*** (0.87)	-3.88*** (0.87)	-3.88*** (0.87)	-6.52*** (1.39)	-6.52*** (1.40)	- 41.41*** (5.09)	- 63.98*** (10.84)
Loot Price*High School	- -	- -	- -	3.63** (1.75)	3.63** (1.75)	- -	31.93*** (12.07)
Detection Price	0.99*** (0.35)	0.99*** (0.35)	0.99*** (0.35)	0.75 (0.56)	0.75 (0.56)	-0.20 (0.39)	-0.81 (0.65)
Detection Price*High School	- -	- -	- -	0.33 (0.71)	0.33 (0.71)	- -	0.87 (0.81)
Fine Price	0.06 (0.19)	0.06 (0.19)	0.06 (0.19)	0.04 (0.27)	0.04 (0.27)	9.94*** (0.96)	12.37*** (1.83)
Fine Price*High School	- -	- -	- -	0.02 (0.36)	0.02 (0.36)	- -	-3.43 (2.14)
Age	- -	0.14** (0.06)	0.12* (0.07)	0.13** (0.06)	0.14** (0.07)	- -	- -
Male	- -	-0.07 (0.17)	-0.10 (0.19)	-0.06 (0.17)	-0.09 (0.191)	- -	- -
Height	- -	0.43* (0.24)	0.41 (0.25)	0.42* (0.24)	0.40 (0.25)	- -	- -
Money	- -	- -	0.002*** (0.0003)	- -	0.002*** (0.0003)	- -	- -
High School	- -	0.46 (0.42)	0.32 (0.43)	-0.83 (1.29)	-0.67 (1.30)	- -	-5.30** (2.16)
GPA	- -	- -	-0.06 (0.14)	- -	-0.03 (0.14)	- -	- -
Ascending	- -	- -	-0.07 (0.12)	- -	0.07 (0.12)	- -	- -
Oldest Child	- -	-0.15 (0.13)	-0.17 (0.14)	-0.15 (0.14)	-0.19 (0.13)	- -	- -
Tenure	- -	-0.02 (0.02)	- -	- -	-0.02 (0.02)	- -	- -
Constant	1.30** (0.61)	-3.76*** (1.71)	-2.98 (1.84)	-2.69 (1.85)	-2.52 (1.97)	6.42*** (0.96)	6.42*** (0.96)
n	1140	1140	1140	1140	1140	1160	1160
Adjusted-R^2	0.14	0.21	0.23	0.22	0.24	0.12	0.17

Robust standard errors, adjusted for clustering at the individual level, in parentheses.
*, ** and *** indicate 10, 5 and 1% significance levels respectively.

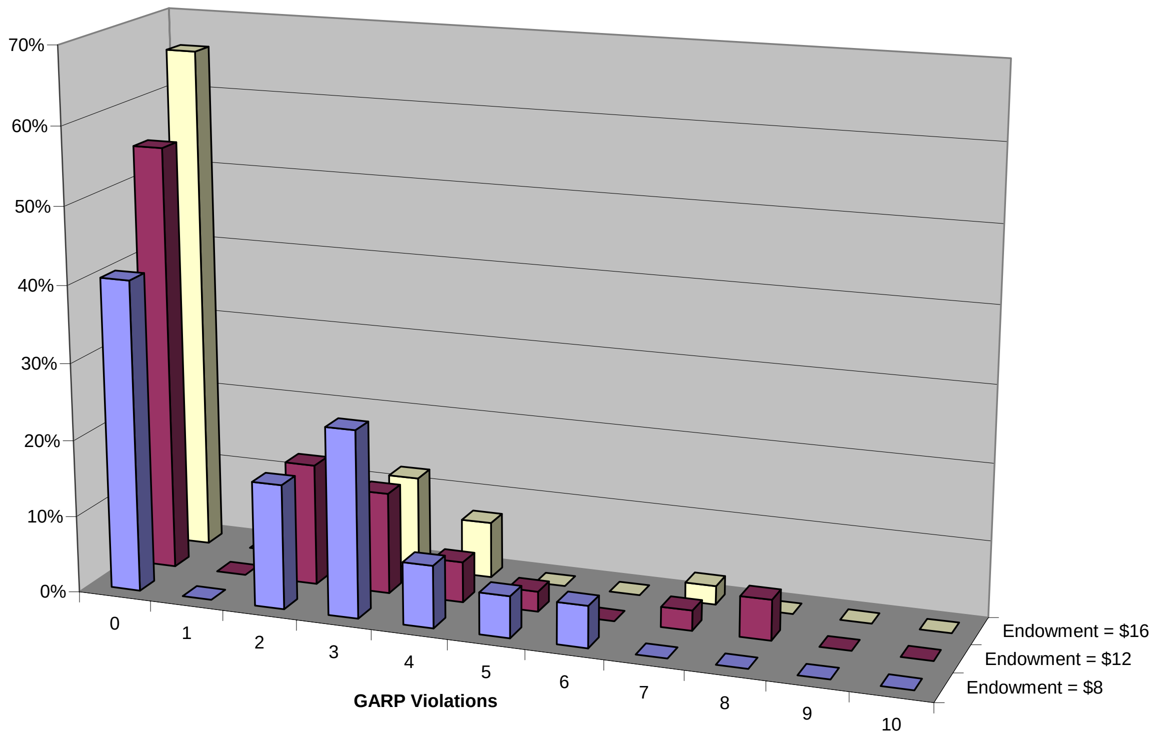
Table 8 demonstrates that coefficient estimates always have the expected sign, though the fine price is not statistically significant in Protocol 1. Columns II and III include coefficients for additional demographic variables. These specifications both suggest that older students tend to steal more. Column II suggests that taller people steal more. Money is included in Column III, and is significant. Columns IV and V include price interactions with the High School dummy variable. As before, the price coefficients all have the expected sign, though only the price of loot is significant. Columns IV and V also indicate that the coefficients of detection price and fine price are the same for college and high school students, but suggests that these effect of the price of loot may be smaller for high school students. Columns VI and VII report estimates from the Protocol 2 data. The only qualitative differences here are that the coefficient on the detection price is the wrong sign, though it is insignificant, and also the price on the fine is the correct sign and significant. Here also, college students appear to be more price sensitive than high school students. We also looked to see whether gender, age, spending money, or tenure had an interactive effect with any of the slope coefficients, but none were significant.

Protocol differences

As previously noted, there are some differences between Protocol 1 and Protocol 2. Protocol 1 was designed to encourage theft using neutral language; and Protocol 2 uses the loaded language in an attempt to provide some context. It is also the case that the stakes in Protocol 2 are higher as the endowments provided in Protocol 2 are bigger. Also, Protocol 2 may be less demanding cognitively as it includes fewer bundles per choice set. Along the same lines, Protocol 2 consists of higher and randomly-varied endowments ($*, $12, or $16).

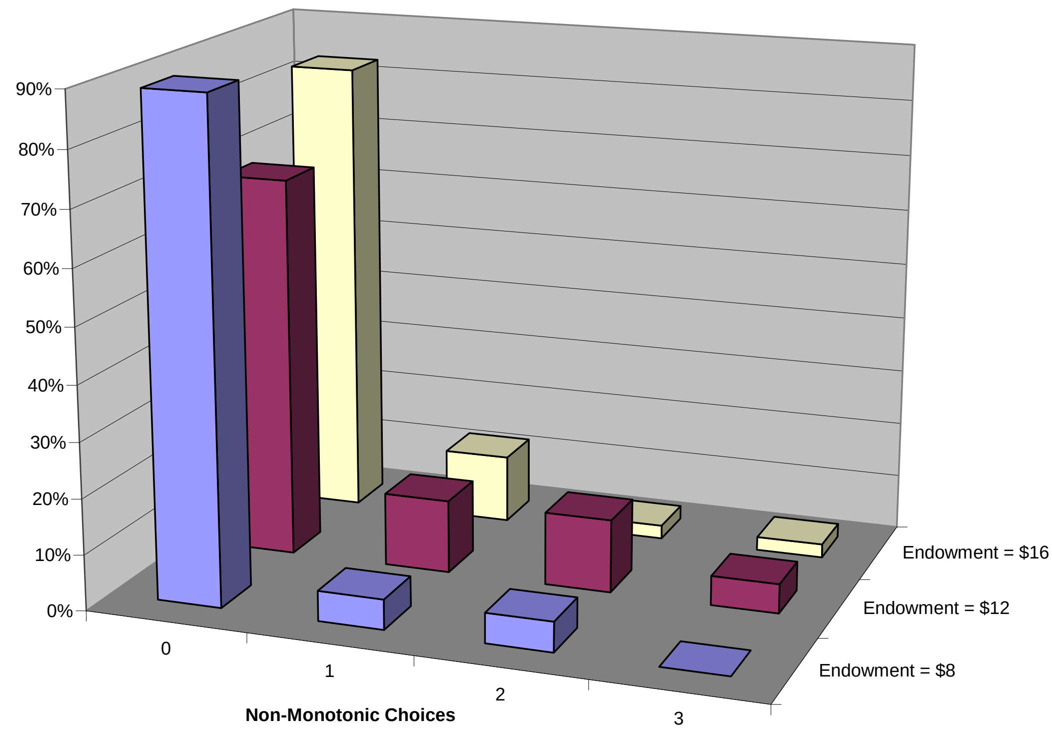
Figure 1 displays the distribution of the number of GARP violations by endowment in Protocol 2. It appears that there is a slight proclivity for those with a greater endowment to have fewer GARP violations. The Mann-Whitney test confirms that the average number of GARP violations between those with an $8 endowment (1.97) and a $16 endowment (1.07) is different at the 5% level. However, the number of GARP violations by those with a $12 endowment (1.66) is not statistically different from the other two groups.

Figure 1 - GARP Violations by Endowment



The distribution of the number of non-monotonic choices by endowment is displayed in Figure 2. Mann-Whitney tests find that there are no statistical differences in monotonic choice by endowment. The same is true for the amount of stolen loot.

Figure 2 - Non-monotonic Choice by Endowment



V. Discussion and Conclusion:

The extent to which criminals and potential criminals respond to variations in deterrence is an important issue, both theoretically and from a public policy perspective. Despite significant progress in recent empirical analyses in identifying the causal effect of deterrence on crime, objections are still raised on the validity of methods proposed to eliminate the simultaneity between crime and deterrence. In this paper we design an experiment where subjects are exposed to exogenous variations in the relative tradeoffs between three important aspects of criminal opportunities – lot, the probability of detection, and the fine. We conduct the experiment with juveniles and young adults (high school students and college students who are younger than 26 years of age), age groups that are frequently labeled as "irrational" and "unresponsive to deterrence".

We find that behavior among this group with respect to petty criminal decisions is not entirely rational. However, it is approximately as consistent with the theoretical requirements of rational choice behavior as choice behavior over consumption goods is. Furthermore, we find that, in aggregate, responses to changes in criminal opportunities are consistent with the laws of demand. Caveats are that the participants in these experiments are not necessarily criminals outside the laboratory, and that the crimes we experiment on involve small financial gains and losses. Given these qualifications, we believe these results strengthen the argument that criminal behavior and the response of criminals to changes in enforcement and penalties can be accounted for by economic models.

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Appendix A – Protocol 1

Welcome:

Today we are conducting an experiment about decision-making. Your decisions are for real money, so pay careful attention to these instructions. This money comes from a research foundation. How much you earn will depend on the decisions that you make, and on chance.

Secrecy:

All your decisions will be secret and <u>we will never reveal them to anyone</u>. We will ask you to mark your decisions on paper forms using a black pen or pencil. If you are discovered looking at another person's forms, or showing your form to another person, we cannot use your decisions in our study and so you will not get paid. Please do not talk during the experiment.

Payment:

Stapled to this page is a card with a number on it. This is your claim check number. Each participant has a different number. Please tear off your card now and write your claim check number on the line on the first page. You are also given a packet. Write your claim check number on top of the first page of that packet, but do not turn the page until instructed to do so. Be sure to keep your claim check number. You will present this number to an assistant at the end of the experiment and you will be given your payment envelope.

The Experiment:

You are going to play a game today. In this game there will be two roles – A and B. Everyone will be randomly assigned one role, and will be matched with another person with the other role. You will not be told who you are paired with, and they will not be told who they are paired with, <u>even after the experiment is over</u>.

Person A will start with \$5, and person B will also start with \$5. Person A will have a chance to take some of person B's money. Taking is not without a risk. After person A makes the decision to take or not to take, there will be a "discovery" phase. During this phase there is a chance that person A will have to return the money taken from person B, and also pay some money to the experimenter. The chance that this happens, and the amount paid to the experimenter if it does, depend on the choice made by person A.

Everyone received one packet, and each packet contains 10 different sheets stapled together. We will show you an example. We call these Choice Sheets.

On each Choice Sheet you will make a choice <u>as if you are person A</u>. You will declare your choice of how much money to take from person B by putting a check mark next to one of the choices with a black pen or pencil.

When we play the game the amount of money you will end up with will really be determined by the choices you make, so you want to consider your choice very carefully. We will give you 60 seconds on the first page and 30 seconds on each subsequent page. Please leave your pencil or black pen on the desk and do not mark your choice until I ask you to do so. When the time is up I will ask you to place a check mark using a pencil next to the choice you want. It is important that you wait until the time is up to mark your choice.

After we go over all 10 Choice Sheets and everyone has made all 10 choices, I will give you a chance to change your mind. This time you will have 15 seconds on each page. Please leave your pencil or black pen on the desk and do not mark your choice until I ask you to do so. To change your choice, clearly cross out (do not erase) your previous choice, and place a check mark next to the choice you want.

Next, we randomly assign roles of A or B by flipping a coin. If it comes up heads, then those whose claim check number is even will be assigned the role of person A, and those whose claim check number is odd will be assigned the role of person B. Should the coin come up tails, then those whose claim check number is odd will be assigned the role of person A, and those whose claim check number is even will be assigned the role of person B.

Now we have to pick which one of the 10 Choice Sheets will count. We will pick a random number from 1 to 10, by having your teacher draw a card from a deck of 10 cards. The Ace will stand for 1. The number of the card will determine which Choice Sheet counts. We will have you turn your packet to that choice sheet.

Then we must complete the discovery phase to see if person A will have to return the money to person B and pay some money to the experimenter. Here is how this will work: We have 5 index cards. On each index card there is a percentage written. They are: 0%, 25%, 50%, 75% and 100%. We will randomly choose one of these index cards.   Everybody looks at their choice on the Choice Sheet that has just been selected. If you are person A, and if the percentage written on the selected index card is less than the chance of being discovered for the choice you made on the Choice Sheet, then you are discovered. You will have to return the money you took from person B and pay to the experimenter the number of money indicated in the choice. Otherwise you keep the money.

We will collect odd numbered and even numbered packets in separate stacks. Then we will mix up each stack , and take one from each stack to match people together. The choice made on person A’s Choice Sheet will be used to determine their payments.

We will proceed through the stacks until we are done. If there is an odd number of people in the room, then at the end there will be one packet left over. This packet will be assigned the role of person A, and will be paid according to the decision on his or her choice sheet.

Note that you don’t know which of your 10 decisions will count, if any. This will be determined purely by chance. So the best thing for you do to is to treat every choice sheet as if it will count, and make the choice on that sheet that you most prefer.

Appendix A – Protocol 2

Welcome:

Today we are conducting an experiment about decision-making. Your decisions are for real money, so pay careful attention to these instructions. This money comes from a research foundation. How much you earn will depend on the decisions that you make, the decisions of others, and on chance.

Secrecy:

All your decisions will be secret and <u>we will never reveal them to anyone</u>. We will ask you to mark your decisions on paper forms using a pen or pencil. If you are discovered looking at another person's forms, or showing your form to another person, we cannot use your decisions in our study and so you will not get paid. Please do not talk during the experiment.

Payment:

You have been given a packet. Stapled to this packet is a card with a number on it. This is your claim check number. Each participant has a different number. Please tear off your card now. Be sure that your claim check number is written on top of the first page of your packet, but do not turn the page until instructed to do so. Be sure to keep your claim check number. You will present this number to an assistant in exchange for your payment envelope.

The Experiment:

You are going to play a game today. In this game you will be randomly and anonymously paired with another person in the room. One of you will be the criminal, and the other will be the victim. You will not know who you are paired with, even after the game is over.

Each person will start with some money, but the amount of money each person gets may be different. You will start with either $16, $12, or $8. Your starting endowment has been determined randomly. The amount you start with is recorded on your packet.

The criminal will have a chance to steal some of the victim's money. However, stealing is not without a risk. If the criminal decides to steal some of the victim's money, there is a chance that the criminal is caught. If the criminal is caught, then he or she will have to return the money taken from the victim, and also pay a fine to the experimenter. The chance that the criminal is caught, and the amount of the fine, depend on the choice made by the criminal.

Everyone received one packet, and each packet contains 13 different sheets stapled together. We will show you an example. We call these Choice Sheets.

On each Choice Sheet everyone will make a choice <u>as if you are the criminal</u>. You will declare your choice of how much money to steal from the victim by putting a check mark next to one of the choices.

When we play the game the amount of money you will end up with will really be determined by the choices you make, so you want to consider your choice very carefully. We will give you 60 seconds on the first page and 30 seconds on each subsequent page. Please leave your pen on the desk and do not mark your choice until I ask you to do so. When the time is up I will ask you to place a check mark next to the choice you want. It is important that you wait until the time is up to mark your choice.

After everyone has made a choice on each of the 13 Choice Sheets, you will have a chance to reconsider each of your decisions just to make sure you have considered each choice carefully. If you wish to change your decision, please cross out your old decision and mark your new decision with a red pen. We will give you 30 seconds on the first page and 15 seconds on each subsequent page. Please leave your pen on the desk and do not mark your choice until I ask you to do so. When the time is up I will ask you to place a check mark next to the choice you want. It is important that you wait until the time is up to mark your choice.

Next we have to determine who will be the criminal and who will be the victim. We randomly assign roles by flipping a coin. If it comes up heads, then those whose claim check number is even will be assigned the role of the criminal, and those whose claim check number is odd will be assigned the role of the victim. Should the coin come up tails, then those whose claim check number is odd will be assigned the role of the criminal, and those whose claim check number is even will be assigned the role of the victim.

Now we have to pick which one of the 13 Choice Sheets will count. We will pick a random number from 1 to 13, by having your teacher draw a card from a deck of 13 cards. The Ace will stand for 1, the Jack, Queen, and King will stand for 11, 12, and 13 respectively. The number of the card will determine which Choice Sheet counts. We will have you turn your packet to that choice sheet.

Note that you don't know which of your 13 decisions will count before you make all of your decisions, if any. This will be determined purely by chance. So, the best thing for you to do is to treat every choice sheet as if it will count, and make the choice on that sheet that you most prefer.

In the final step of the game we have to determine whether or not the criminal is caught. Here is how this will work: We have 4 index cards. On each index card there is a percentage written. They are: 25%, 50%, 75% and 100%. We will randomly choose one of these index cards. Everybody looks at their choice on the Choice Sheet that has just been selected. If you are the criminal, and if the percentage written on the selected index card is less than or equal to the chance of being caught for the choice you made on the Choice Sheet, then you are caught. You will have to return the money you stole from the victim and pay the corresponding fine to the experimenter. Otherwise you keep the money.

Now we will collect your decision packets and calculate your payments. To calculate your payments, we will randomly match one criminal with one victim. To get your envelope, we will ask you to fill out a receipt to be returned to us.