An Experimental Test of Criminal Behavior Among Juveniles and Young Adults

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ABSTRACT

We report results from economic experiments that provide a direct test of the hypothesis that criminal behavior responds rationally to changes in the possible rewards and in the probability and severity of punishment. The experiments involve decisions that are best described as petty larceny, and are done using high school and college students who can anonymously take real money from each other. We find that decisions about whether and how much to steal are, in general, rational and responsive to the variations in tradeoffs, and sometimes, though not always, to the overall availability of criminal opportunities.

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I. Introduction

Becker (1968) created the foundation for the economic analysis of criminal behavior. Since then, economists have extended his basic theoretical framework in several directions, but the basic argument remains – 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.[1] Although some early empirical research reported evidence suggesting that enhanced deterrence reduces crime (Ehrlich 1973, 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.[2]

Recent research has employed three types of strategies to overcome the simultaneity problem. The first solution is to find an instrument which is correlated with deterrence measures but uncorrelated with crime. Examples are Levitt (1997) who uses electoral cycles as an instrument for police hiring, and Levitt (2002) who uses the number of per capita municipal firefighters as an instrument for police effort. The second strategy is to use high-

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

[2] See Ehrlich (1996) for a discussion of related theoretical and empirical issues.

frequency time-series data. For example, in 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 (2000, 2005). 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 experiments 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 – taking money from another individual, with a chance of getting caught and having to repay everything taken, plus a fine. The experiments are done with high school and college students, and are for real money. We collect information on (nearly) simultaneous choices made by individuals under a variety of different criminal opportunities. These choices are used to analyze whether the decisions to steal change rationally in response to variations in the probability of detection and size of the fine.

The data are first used to check for transitivity violations. Since 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 framework, which assumes rational choice by criminals. We then estimate demand functions for stolen loot. In doing so we take advantage of the fact that in an experimental setting we can induce exogenous variations in the extent of criminal opportunities, such as the probability that a theft will be detected, or the fine that ensues if the person is caught. These demand functions allow us to investigate the responsiveness of the decision to steal and the amount of stolen money to variations in the extent of attractiveness of criminal opportunities. Specifically, we are able to estimate elasticities for criminal participation and the amount of money stolen with respect to the terms of trade (or relative prices) between stolen loot and the probability of detection, and between stolen loot and the fine.[3]

Section II provides a brief description of revealed preference theory, and the experimental methods for testing it. Section III describes the experimental design. Section IV presents the results on revealed preference and estimated demand for stolen loot, and section V concludes.

II. Revealed Preference

The basic principle of a revealed preference test can be seen in Figure 1, for the case of two goods and two constraints. Choosing a from A and b from B is irrational by the following argument. When a was picked from the budget set A, the alternative b was within that set, so if the person was choosing rationally then $u(a) \geq u(b)$, where u stands for utility.

[3] Becker (1962) points out that rational choice is not necessary for choices to satisfy the laws of demand. 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 in the case that choices occasionally, or even frequently, violate revealed preference tests of rationality.

We can strengthen this statement to $u(a) > u(b)$ by using continuity and assuming local-nonsatiation, meaning that there is some alternative within any given distance of any bundle that provides strictly higher utility. Applying the same argument starting with the choice of *b* from *B* implies $u(b) > u(a)$, which is a contradiction. Thus, a person who made these choices could not have been choosing rationally.

Samuelson (1938a,b), proves that choices which are not irrational by the above test, i.e. which are consistent with the Weak Axiom of Revealed Preference (WARP), are a necessary requirement for data that come from maximizing a utility function. The example above uses only 2 choice sets, allowing direct comparisons. In the experiment conducted in this paper we collect choices from 10 sets. Therefore we can also test for chains of irrational choices that involve indirect comparisons, such as when choices reveal that $u(a) > u(b) > u(c) > u(a)$. Houthakker (1950) showed that choice data which do not reveal these sorts of intransitivities, that is to say which satisfy the Strong Axiom of Revealed Preference (SARP), are a necessary condition for rational choice. Varian (1982) further generalized revealed preference theory to allow indifference curves to have flat spots. His result on the equivalency of utility maximization and the satisfaction of revealed preference axioms is known as the Generalized Axiom of Revealed Preference (GARP).

These proofs all rely on continuous choice sets. In practice, and in our experiment, choice sets are discrete. Harbaugh, Krause, and Berry (2001) show that with discrete choice sets one needs to strengthen the local nonsatiation requirement to strong monotonicity. This ensures that the discrete alternatives *c* and *d* in Figure 1 have higher utility than the choices *a* and *b* respectively. This means that we don't need continuity to know that choosing *a* means $u(a) > u(b)$, because the available alternative *d* has more of good *y* and the same amount of *x* than does b, and so is better by strong monotonicity. Again, we reverse the argument to show the contradiction, that $u(b) > u(a)$.

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 these goods 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.[4] 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 did not choose randomly, and typically behaved according to neoclassical predictions.

Harbaugh, Krause, and Berry (2001) applied the revealed preference test in consumption experiments with children ages seven to 11, as well as with college undergraduates. Participants were offered choices among 11 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 formed overlapping budget sets. They found that about half-to-three-quarters of subjects exhibited at least one violation, although 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

[4] 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. The strategy method, which is a common procedure in many experimental settings and also used in this paper, provides an incentive for subjects to treat each round as a single play of the game, and to maximize expected utility over the entire random lottery. This may actually mitigate against the problem of changing or evolving preferences during the experiment.

expected under random choice. These results suggested that even children may be rational utility maximizers, at least with respect to familiar consumption goods.

Andreoni and Miller (2002) offered individuals 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. Following standard experimental protocols, we provided complete information to the participants, paid them privately in cash, and there was no deception. Before the experiment, the students read an assent agreement and allowed to opt out of the experiment, but none did. 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 United States as a whole. Experiments are also conducted with economics and business majors from two upper-division undergraduate courses at the University of Oregon. These students differ from the high school students in several dimensions, as will be explained below in detail.[5]

[5] As Levitt and List (2006) underscore, self-selection of subjects into an experiment may diminish the generalizability of the results. In our high school experiments there was no self-selection effect, since all the

Everyone was given an endowment of money to use in the experiment. The basic decision is whether or not, and how much of this money to steal from the randomly-assigned partner (who is anonymous), given specified probabilities of getting caught and specified fines that must be paid if caught. Everyone made a decision as if they were the criminal, and we then randomly determined the participants' actual roles (criminal or victim) within each pair, for the purpose of determining the payoffs. The subjects knew the other participants. However, because they were randomly and anonymously matched with a partner, they did not know who they would steal from, or who stole from them, even after the end of the experiment.

Subjects made a series of these decisions, each one from different choice set. The sets were constructed from budgets defined over three goods: stolen loot, the probability of getting away with the theft, and the amount kept if caught (after paying back the stolen loot, plus the fine). 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 bundles involved taking a little money, facing a good chance of getting away with it and a modest fine if caught; while others involved a larger amount of loot, but a lesser chance of getting away with it; and so on. Taking nothing from the partner is always an option.

The list of bundles for each choice set is 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 particular choice set, choosing a crime with lots of loot will cost dearly in terms of the chances of getting

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students agreed to participate. Selection in terms of being in the classroom was small, because high school attendance is high. Selection on both counts is more significant with our college sample.

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 low probability of getting away with the crime, and small fines if caught. In the remainder of the paper we will call this income variable "opportunity."

We used two different protocols. Protocol 1 used language such as "person A," "person B," and "take from person B" and provided uniform starting endowments of $5. Protocol 2 used different budget sets and also used loaded language, such as "criminal," "victim" and "steal from the victim." Additionally, instead of a uniform endowment of $5, participants were randomly given either $8, $12, or $16. Put differently, in Protocol 2 there was an explicit emphasis on the dishonest or illegal nature of the act. Also the financial stakes could be higher in protocol 2. For example, an endowment of $16 is nearly equivalent to the average weekly spending of a high school student, and it is 1/5th of the weekly spending for a college student (see Table 2).

Protocol 1 included duplicate choice sets to check for the consistency of choices. To check for monotonicity of preferences, Protocol 1 included choice bundles that were on the interior of the budget set, while Protocol 2 included some choice sets constructed using budget lines that were rays from the origin. The logic behind both tests is to investigate the extent to which individuals select bundles that are strictly dominated, in the sense of having less of at least one "good."

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 each protocol. As can be seen, the stakes are higher for Protocol 2.[6] The complete protocols are presented in the Appendix.

The opportunities and prices are varied so 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. These variations in opportunities and prices also allow us to estimate demand functions.

The subjects are told that they must choose one bundle from each choice set (like the ones displayed in Table 1B). After they and their matched partner have made their choices in all 10 rounds (10 different choice sets),[7] we randomly determined who the criminal was, and who the victim was. Then, one of each criminal's choice sets (one of the 10 choice sets) was randomly chosen, and the choice they made from that particular set was implemented. We used a randomized procedure to determine if they got away with the theft they might have chosen to commit, or if they were instead caught and had to 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 protocols were designed to make sure that choices are made carefully. First, we gave the subjects 30 seconds to make a decision on each choice set (to choose a bundle from

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[6] 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. Andreoni and Harbaugh (2006) discuss these issues in more detail.

[7] Protocol 2 included three additional choice sets constructed from budget lines that were rays from the origin, so participants made a total of 13 choices in Protocol 2. These additional choice sets were used to examine monotonicity only, and like the other 10 choice sets, could also have been selected as the choice to be implemented.

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 decisions.

In Protocol 1 we gave individuals two additional chances to change their minds: First after the uncertainty was resolved about whether they were the criminal or the victim, and then again after they were told which choice sheet (from 1 to 10) would count. This information is used to check whether choices obey the independence axiom, as is explained in the results section. Protocol 1 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 do not duplicate any budget sets, nor do we implement independence checks - the second choice is the final choice in Protocol 2.

Choice sets were always ordered starting with the low income version. In Protocol 1 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 (block 1), and the other half of the subjects faced loot amounts that are ranked from high to low (block 2).

Experiments with Protocol 1 are performed in a total of five high school classes and one undergraduate class, for a total of 83 high school and 31 college students. The Protocol 2 experiments were conducted in three high school classes and one undergraduate class, for 82 high school students and 34 college students.

We collected data on socio-demographic characteristics of the subjects in a post-experiment survey in order to investigate their relationship with the rationality of participants' choices.[8] Definitions and descriptive statistics of these variables are provided in Table 2. *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. As shown in Table 2, the college undergraduates have higher GPAs, more money, and are older.

IV. Results

Consistency and Independence

To investigate the consistency of the choices, we analyzed whether subjects made the same choices when presented with the duplicate choice sets in Protocol 1. There are 114 individuals using Protocol 1, and each person faced 3 pairs of duplicate budget sets (Choice sets 1 and 5, 3 and 7, and 4 and 8; see table 1A). Thus, we have 342 pairs of duplicate choice sets. For the first decisions that were 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. The rates of inconsistent choices are much lower than would be true under the alternative of random decisions. In addition, the increase in the proportion of consistent choices from round 1 to round 2 is reassuring as it suggests that thinking about the decision increases consistency. When we regress the number of matched choices on demographics, nothing is statistically significant, including whether the subject is a high school student, and the GPA.

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[8] This survey was not administered to the college students in Protocol 2.

To test GARP and to estimate participation and demand equations, we need to argue that these are the same decisions that would have been made if faced with only one decision and no uncertainty. This amounts to indicating that choices must obey the independence axiom.[9] In our experiment, independence implies that the chosen bundle should not change when a person learns his/her role, or when it is revealed which budget set will be implemented. We find that about 77 percent of participants change at least one choice in round 2 (21% in Protocol 2), about 17 percent change in round 3, and nobody changes his or her decision in round 4. We take this as evidence that choice behavior under uncertainty, which is not resolved until rounds 3 and 4, generally obeys independence.

Protocol differences

As previously noted, there are some differences between Protocol 1 and Protocol 2. Protocol 1 uses neutral language, while Protocol 2 uses loaded language about crime and generally larger stakes. Protocol 2 may be less demanding cognitively as it includes fewer bundles per choice set. The endowments in Protocol 2 vary across individuals. Figure 2 displays the distribution of the number of GARP violations by endowment in Protocol 2. It appears that there is a slight tendency for those with a larger 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. The distribution of the number of non-monotonic choices by endowment is displayed in Figure 3. Mann-

[9] Machina (1987) provides intuition for this axiom, arguing that if people prefer A to B, they should also prefer a coin-flip between A and C to a coin-flip between B and C. The logic is that if the coin comes up for C, their choice is irrelevant, but so long as there is some chance it will come up the other way, one gets what one prefers.

Whitney tests demonstrate that there are no statistical differences in monotonic choice by endowment. The same is true for the amount of stolen loot.

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, therefore 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 eliminate those subjects who always steal everything and those who never steal anything from this part of the analysis. Table 3 displays the average number of GARP violations for both high school 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 3 demonstrates that each subject group exhibits significantly fewer violations than would be expected from bootstrap choice. The average number of GARP violations across all subjects who did not choose 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 it is 1.75 in Protocol 2. This suggests that, on average, the choice modifications made by individuals move them towards greater rationality. Protocol 2 has a significantly lower number of bootstrap violations than does Protocol 1, which suggests that in Protocol 2, the ability to detect violations (in the Bronars' power sense) is less. However, because the budget sets intersect at greater angles in Protocol 2, any violations are more serious than they are in Protocol 1.

The fourth column of Table 3 reports average GARP violations for those subjects who did not choose corner bundles in each and every choice set, and who have at least 8 (out of a possible 10) monotonic choices in Protocol 1, or at least 2 monotonic choices (out of a possible 3) in Protocol 2. There are 36 such high school participants and 12 college student participants in Protocol 1, and 67 high school participants and 31 college student participants in Protocol 2. Table 3 demonstrates that in Protocol 1 average GARP violations drop significantly when individuals with non-monotonic choices are excluded. 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 with bootstrap choices.

Table 3 also presents the calculated Afriat Index (Afriat, 1972) for each sub-sample. The Afriat Index provides a measure of efficiency relative to perfectly rational choice. One minus the Afriat Index can be interpreted as the proportion of income wasted through irrational choice.[10] The indices for each protocol and subgroup are quite close to one, indicating that most violations are trivial in magnitude.

Figure 4 presents frequencies for the number of GARP violations per subject. Note that, since a minimum of two choices are required to check for a rationality 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.

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 seldom met. Sippel (1997) used a

[10] However, with our design this interpretation is not strictly true, since we use discrete rather than continuous choices.

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, Krause, and Berry (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 (2002) or Harbaugh, Krause, and Berry (2001) experiments, but simpler than that of Sippel (1997). Although a direct comparison cannot be made, our results seem generally consistent with those from other experiments.

As in Harbaugh, Krause, and Berry (2001) and Andreoni and Miller (2002), our revealed preference test requires that preferences be strongly monotonic. Rather than take this on faith, our experiment is designed to test this assumption. This is accomplished by including to the choice sets of Protocol 1 dominated bundles – 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 included three upward sloping budget sets, as in 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 3 percent 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.[11] 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. Harbaugh, Krause, and Berry (2001) 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.

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 5 displays the results obtained from negative binomial regressions. Poisson

[11] See for example Rabin (1993) and Fehr and Schmidt (1999).

regressions and ordered-probit models provided similar results. Columns 1-4 display the results based on Protocol 1 data, and column 5 pertains to Protocol 2 data. The model reported in the first column includes all subjects and distinguishes between high school and college students by the dichotomous variable “High School”. The second column presents the results estimated using high school students only, and the third column pertains to college students in Protocol 1. The fourth column displays a specification where the explanatory variables are interacted with the High School dummy. Because socio-demographic data were not collected on the college students for Protocol 2, one cannot identify the differential impact of personal characteristics on GARP violations between college and high school students using Protocol 2.

Column 3 of Table 5 indicates that, among college students, being one year older is associated with about a half fewer GARP violations. Having lived longer in Eugene has a smaller effect in magnate on GARP violations. It appears that being the oldest child in the family is associated with fewer GARP violations. A general conclusion from Table 5, however, is that the degree of rationality of behavior is not well explained by the available personal background characteristics of the subjects.

Crime and deterrence

In this section we investigate the determinants of individuals’ specific decisions regarding whether or not to steal, and the amount of money stolen. As Levitt and List (2006) describe, individuals’ utility maximization may also involve concerns about “doing the right thing” or “moral choices.” In our setting, this aspect of the behavior may be exacerbated even though there is complete anonymity between the partners, because the subjects are classmates. Thus, not surprisingly, there is evidence that individuals do not simply treat the experiment as a chance to maximize expected value or expected utility. In Protocol 1, the average expected value (EV) for the sample is 91% of the maximum possible EV, while for Protocol 2 it is 74%. For expected utility (EU), a simple CARA utility function, $u = x^{\alpha}$, where x is the endowment adjusted by loot or the fine, gives very similar results even when α is as low as 0.6. These averages mask a large degree of heterogeneity – some people take less than the amount that maximizes EV or EU, while others take more.

The hypothesis that people will not steal from others has no support. Table 6 presents the distribution of the number of thefts. During the experiment each individual had the opportunity to steal 10 times (ignoring the monotonicity checks in Protocol 2). Thus, in Table 6, zero thefts means that the individual never stole during the experiment, and a 10 indicates that he or she stole money in every round. As Table 6 demonstrates, there is substantial variation in the number of thefts. Forty-nine percent of the subjects stole some amount in every round of Protocol 1, and 52 percent did the same in Protocol 2.

Given these results, demand functions for stolen money can be estimated to investigate the determinants of the amount of loot stolen as a function of the personal characteristics of the person who steals, and the characteristics of the choice set. 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 (in the sense of a change in constraints, which makes such crimes involve a higher probability of detection, or a higher fine) 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 – that is, a change in the constraint which allows people to take more loot, at less risk of detection, and a lower fine, would also be expected to increase loot, assuming loot is a normal good.

We normalize the implicit prices by dividing through by the price of detection and eliminating that price from the regressions. We expect to find negative own-price effects (an increase in the cost of taking loot --in the sense of having to accept higher probabilities of detection and higher fines-- should reduce the amount taken), and a positive cross-price effect (an increase in the "fine-discount" should cause people to take more loot).

The decision of how much to steal can be thought of as consisting of two steps. The first part is the decision whether or not to steal (participation), and the second part is the decision of how much to steal, conditional on the decision to steal. This process resembles the one typically used to model decisions about cigarette smoking or alcohol consumption (e.g. Gruber and Zinman 2001; Farrell, Manning and Finch 2003). The motivation for this approach is that it allows for variables to have different effects on the decision as to steal, and the decision about how much to steal. Alternatively, one can estimate an unconditional demand function for loot using all observations (those who steal and those who do not).

Tables 7A-7D present the results of the estimated unconditional demand, participation, and conditional demand equations separately for the two protocols, and for high school students and college students. Unconditional demand is estimated as a Tobit (individuals can steal, but not give). In the participation equations, the dependent variable is dichotomous to indicate if the subject stole money in that particular round. Participation equations are estimated as logits and also as linear probability models with personal attributes. The demand for loot is estimated with OLS, including personal background variables. The control variables included in the regressions may not adequately capture unobserved individual heterogeneity that might be correlated with the propensity to steal. Thus, both participation and loot demand equations are also estimated with individual fixed-effects to control for such unobservables. The key variables of interest are the loot price, fine price, and opportunity. The manner is which individual heterogeneity is controlled for should have no impact on the estimated coefficients of these variables because they are exogenous by design. Standard errors are adjusted for clustering at the individual level. In Protocol 2 no personal information was collected from college students. Therefore the regressions that employ data on college students from Protocol 2 do not include personal characteristics.

The estimated participation equations in Table 7A show that, for high school students and for the unloaded language (Protocol 1), the propensity to steal increases with age, but it is otherwise unrelated to other explanatory variables. Unconditional demand estimates reveal that an increase in the loot price generates a decrease in the amount of loot taken. The loot price is estimated with less precision in the unconditional demand equation estimated by tobit, but in this equation in addition to the age effect, height has a positive impact on the demand for stolen loot. Being the oldest child in the family, on the other hand, is negatively related to unconditional demand for stolen money.

These results are generally consistent with those obtained from college students in Protocol 1, reported in Table 7B. Own-price effect (the price of loot) is negative and statistically significant in both unconditional demand and conditional demand equations. Among college students, the number of years lived in Eugene, Oregon (Tenure), and the GPA are negatively related to both the propensity to steal and amount of the stolen money.

On the other hand, an increase in the amount of money the individual spends per week (Money) is positively related to the demand for stolen loot.

For Protocol 2, which uses the loaded language and different choice sets, we find much stronger evidence of responsiveness to prices across all decisions, for both the high school and college samples. For example, loot price is negative and significant and fine price is positive and significant in all models in Table 7C (high school sample). The same is true for college students, reported in Table 7D. In addition, the coefficients of opportunity are also positive and significant in all equations, indicating that larger opportunity sets for crime (i.e. when crime is associated with a lower probability of detection, and lower fines) generate higher propensity to steal and larger amounts of stolen money. We do not have data on personal characteristics of college students in Protocol 2, but male high school students have a higher propensity to steal.

These results underline two points. First, as predicted by economic theory, the subjects' decisions regarding whether to steal and how much to steal are, in general, responsive to the variations in tradeoffs, and sometimes, though not always, to the availability of criminal opportunities. Second, these responses are larger in Protocol 2, which uses the more direct language. Inclusion or exclusion of personal background characteristics does little to influence the estimated parameters, and neither does the inclusion of individual fixed-effects. This is not surprising as the prices and criminal opportunity are exogenous by construction.

Table 8 reports the impact of the variation in prices and criminal opportunity on the decision to steal and the demand for loot from a different perspective. Using Tables7A-7D, the elasticities for prices and for criminal opportunity are calculated for both participation equation and demand for loot. Elasticities are calculated using the OLS estimates for participation and conditional demand equations in all sub samples except for Protocol 2 college students, where the Fixed Effects estimates are used (OLS estimates are virtually identical).

Table 8 shows that an increase in the price of loot always results in a decrease in the amount of stealing, for both protocols and for both high school and college students. In Protocol 1 this response seems to be driven by the reaction of the conditional demand equation, whereas in Protocol 2 both the participation and unconditional demand functions are responsive to the variation in the loot price.[12]

The response to a change in loot price among those who steal (the conditional demand) is about twice as large in Protocol 2 than in Protocol 1. This difference in the responsiveness can be seen in the "fine price" as well. The subjects in Protocol 1 do not change their behavior as fine price changes, while the participants in Protocol 2 consistently increase the amount that they steal from others as the fines for stealing decrease.

The higher degree of responsiveness in Protocol 2 also shows up in the opportunity elasticity. This should be thought of as the degree to which general increases in the attractiveness of crime prompt increases in participation and the amounts of money stolen. The elasticity of criminal opportunity is statistical significance only in the college participants in Protocol 2. The finding that responsiveness in Protocol 2 is greater is consistent with the fact that revealed preference violations in Protocol 2 are more costly than in Protocol 1.

[12] The unconditional demand function elasticities can alternatively be calculated as the sum of elasticities obtained from the participation and conditional demand equations. As can be seen in Table 8, elasticities that can be calculated this way would be very similar in magnitude to those obtained from estimating the unconditional demand equations.

The elasticities reported in Table 8 do not have real-world counterparts. This is not critical because the aim of this study is not to provide parameters that could be used for policy. Rather, the goal is to investigate the extent to which individuals respond to variations in the costs of criminal activity, and whether or not these responses are consistent with the economic theory of criminal behavior.

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 empirical analyses. The issue is important because it involves fundamental arguments about the rationality of individuals in their decisions to engage in illegal acts and whether individuals respond to changes in the costs of crime, such as the probability of punishment and the penalty they face, if caught.

This is the first paper where individuals' responses to potential criminal opportunities and the associated costs are analyzed in an experimental setting. We design an experiment where subjects are exposed to exogenous variations in the relative tradeoffs between three important aspects of criminal opportunities – the amount of loot they can take from another person, the probability of detection, and the fine. We conducted the experiment with juveniles and young adults, age groups that are near the peak ages for participation in petty crime and who are frequently labeled as "irrational" and "unresponsive to deterrence."

We find that behavior among this group with respect to petty criminal decisions is not completely rational. However, it is approximately as consistent with the theoretical requirements of rational choice behavior as are the choice behaviors for consumption goods. Furthermore, we find that, in aggregate, responses to changes in criminal opportunities are consistent with the laws of demand. Our estimates indicate that both participation in crime and the amount of loot stolen respond to exogenous variations in relative prices, with the response of the demand for loot being stronger.

Caveats are that the participants in these experiments are not necessarily criminals outside the laboratory, and that the crimes do not involve very large financial gains and losses. It should be noted, however, that recent evidence indicates that criminals respond to variations in penalties in the same manner as non-criminals. For example, Bar-Ilan and Sacerdote (2004) show that drivers who were previously indicted for property or violent crimes have the same elasticity of traffic violations (red-light running) with respect to the fine, as those who do not have a criminal record. Given these qualifications, these results demonstrate that individuals' decisions to commit crime are consistent with the predictions obtained from economic models, in that exogenous changes in enforcement and penalties do alter criminal behavior.

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Figure 1
Revealed Preference

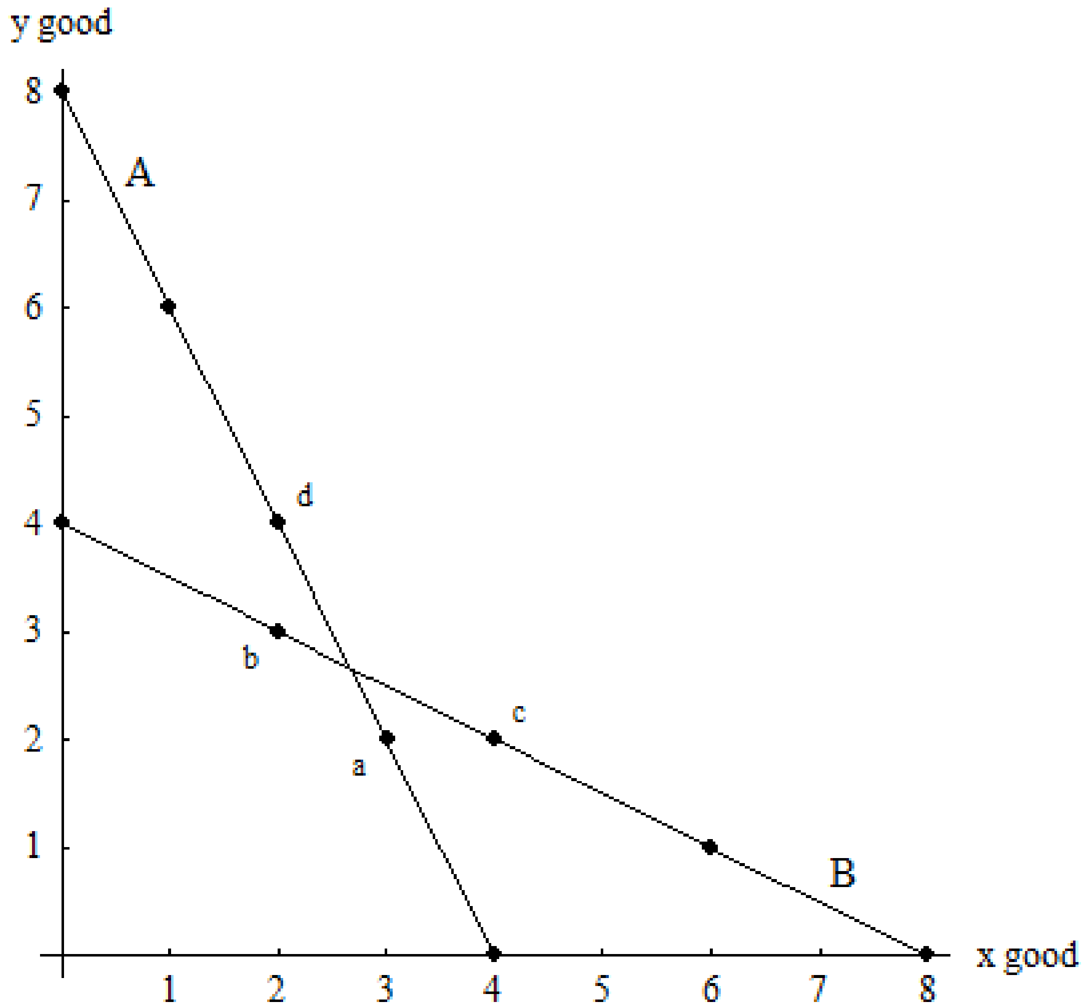

Table 1A

Choice Set Characteristics

Protocol 1 budget parameters

Budget	Loot Price	Probability Price	Fine Price	Opportunity
1	0.21	0.83	0.83	1
2	0.34	0.69	0.34	1
3	0.15	1.23	0.31	1
4	0.22	0.89	0.44	1
5	0.21	0.83	0.83	1
6	0.36	0.73	0.36	1
7	0.15	1.23	0.31	1
8	0.22	0.89	0.44	1
9	0.27	1.07	0.27	1
10	0.14	1.14	0.57	1

Protocol 2 budget parameters

Budget	Loot Price	Probability Price	Fine Price	Opportunity
1	0.11	1.10	0.14	1
2	0.11	1.10	0.27	1
3	0.11	1.32	0.14	1
4	0.11	1.32	0.27	1
5	0.12	1.10	0.14	1
6	0.12	1.10	0.27	1
7	0.12	1.32	0.14	1
8	0.12	1.32	0.27	1
9	0.14	1.10	0.14	1
10	0.14	1.10	0.27	1

Table 1B

Examples of Choice Sets

Sample Bundles from Choice Set #5, in 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, in 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

Table 2

Descriptive Statistics of Personal Characteristics

		Protocol 1		Protocol 2	
Variable	Definition	High School	College	High School	College
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
Classes	Number of classes in the study	5	1	3	1
n		83	31	82	34

Socio-demographic data were not collected from the college sample in Protocol 2

Figure 2
GARP Violations by Endowment

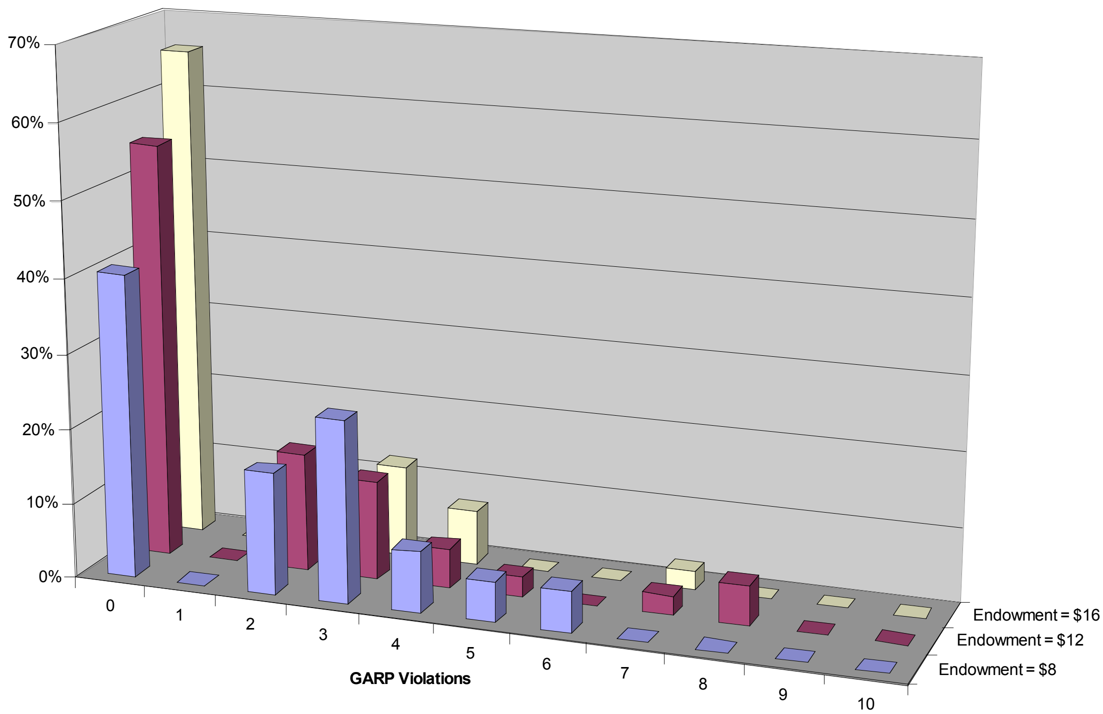

Figure 3
Non-monotonic Choice by Endowment

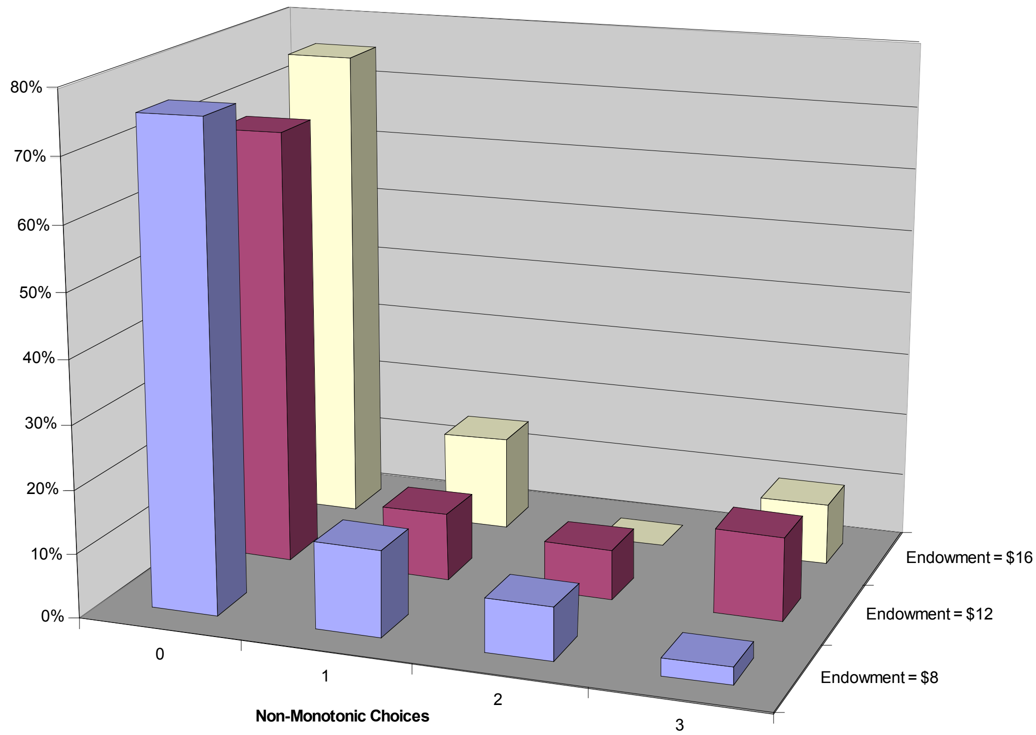

Table 3

Average GARP Violations

		(1) GARP (no corners)	(2) Afriat Index	(3) n	(4) GARP (no corners, monotonic)	(5) Afriat Index	(6) n
Protocol 1	All	4.32 (0.38)	0.991 (0.003)	106	1.96 (0.41)	0.998 (0.002)	48
	HS	4.28 (0.43)	0.989 (0.004)	78	2.00 (0.45)	0.997 (0.002)	36
	UO	4.43 (0.79)	1.00 (0)	28	1.83 (0.97)	1.00 (0.00)	12
	Bootstrap	5.52 (0.03)	0.977 (0.0005)	10,000	3.59 (0.04)	0.979 (0.0006)	4,829
Protocol 2	All	1.67 (0.20)	0.994 (0.001)	108	1.73 (0.20)	0.994 (0.002)	98
	HS	1.71 (0.24)	0.993 (0.002)	76	1.81 (0.25)	0.993 (0.002)	67
	UO	1.56 (0.33)	0.996 (0.002)	32	1.55 (0.34)	0.996 (0.002)	31
	Bootstrap	2.59 (0.02)	0.986 (0.0002)	10,000	2.58 (0.02)	0.986 (0.0002)	9,315

Standard errors in parentheses.

HS: High School sample, UO: University of Oregon undergraduates sample.

Figure 4
Frequency of GARP Violations

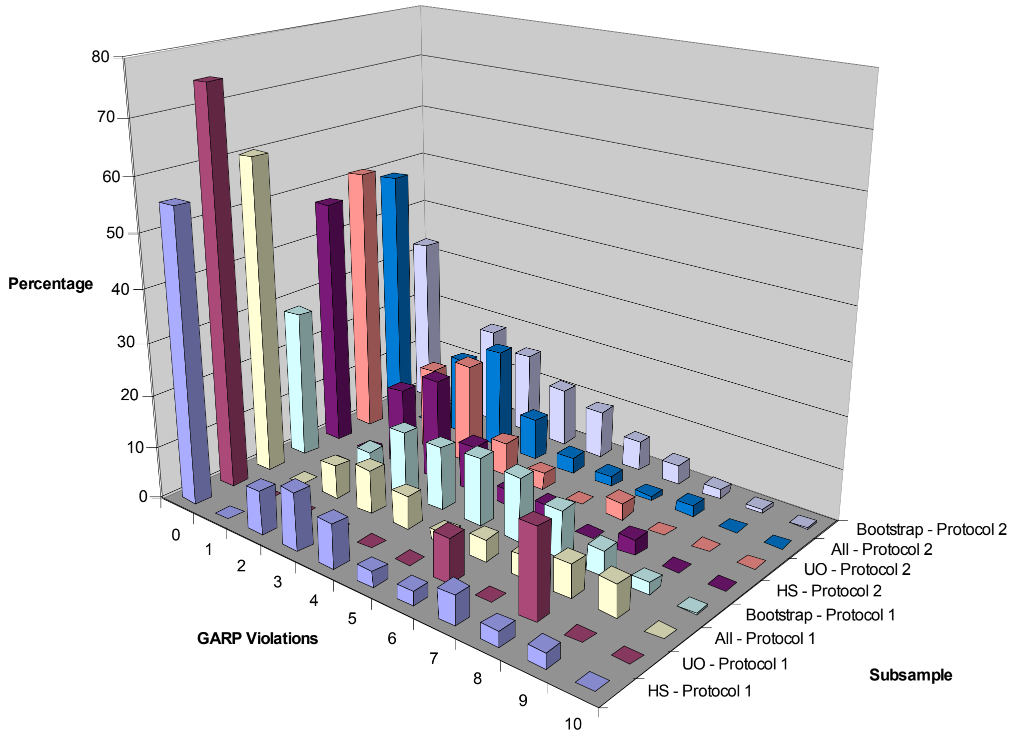

Table 4

Monotonic bundle choices

	Protocol 1 Sample		Monte Carlo	
Monotone Choices	Frequency	Running total	Frequency	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	

Table 5
Negative Binomial Estimates of the Number of GARP Violations

	Protocol 1				Protocol 2
	(1)	(2)	(3)	(4)	(5)
Explanatory Variable	All	HS	COLL	All	HS
Age	0.054	0.159	-0.625**	-0.623**	0.088
	(0.105)	(0.108)	(0.291)	(0.274)	(0.119)
Age*High School	-	-	-	0.782***	-
	-	-	-	(0.295)	-
Male	0.254	0.360	0.837	0.835	-0.369
	(0.276)	(0.311)	(0.591)	(0.577)	(0.362)
Male*High School	-	-	-	-0.475	-
	-	-	-	(0.654)	-
Height	-0.217	-0.789*	0.457	0.456	-0.099
	(0.360)	(0.408)	(0.889)	(0.872)	(0.445)
Height*High School	-	-	-	-1.245	-
	-	-	-	(0.964)	-
GPA	0.080	0.163	-0.638	-0.635	-0.245
	(0.248)	(0.259)	(0.772)	(0.747)	(0.357)
GPA*High School	-	-	-	0.799	-
	-	-	-	(0.794)	-
Money	-0.005	-0.002	-0.012	-0.012	0.003
	(0.005)	(0.007)	(0.007)	(0.007)	(0.005)
Money*High School	-	-	-	0.001	-
	-	-	-	(0.010)	-
Oldest Child	-0.603**	-0.767***	-0.397	-0.397	-0.249
	(0.238)	(0.284)	(0.434)	(0.426)	(0.313)
Oldest Child*High School	-	-	-	-0.371	-
	-	-	-	(0.511)	-
High School	0.232	-	-	-12.177*	-
	(0.612)	-	-	(6.594)	-
Tenure	-0.039	-0.007	-0.052**	-0.111**	0.011
	(0.031)	(0.034)	(0.052)	(0.050)	(0.049)
Tenure*High School	-	-	-	0.104*	-
	-	-	-	(0.061)	-
n	106	78	28	106	74
Log Pseudo-Likelihood	-267.36	-195.91	-65.37	-262.07	-128.79

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

All: All participants not choosing corners.
HS: High school participants not choosing corners.
COLL: College participants not choosing corners.
The number of HS subjects not choosing corners is 76, but two of them did not provide survey responses.

Table 6

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.

Table 7A

Demand for Stolen Loot

Protocol 1, High School Students

	UNCONDITIONAL DEMAND	PARTICIPATION EQUATION			CONDITIONAL DEMAND	
Variable	Tobit	Logit	OLS Fixed Effects	OLS	OLS Fixed Effects	OLS
Loot Price	-1.930	1.147	0.145	0.145	-2.506*	-3.042***
	(1.887)	(2.871)	(0.427)	(0.508)	(1.365)	(0.898)
Opportunity	-0.710	-1.459	-0.208	-0.208	-0.486	-0.087
	(1.312)	(2.023)	(0.297)	(0.357)	(0.948)	(0.659)
Fine Price	-0.253	-0.718	-0.151	-0.151	0.142	0.174
	(0.435)	(0.536)	(0.098)	(0.098)	(0.316)	(0.212)
Age	0.235***	0.377**		0.064**		0.096
	(0.058)	(0.181)		(0.030)		(0.076)
Male	-0.054	-0.119		-0.023		0.013
	(0.150)	(0.573)		(0.108)		(0.221)
Height	0.540***	0.438		0.077		0.421
	(0.201)	(0.762)		(0.135)		(0.262)
Money	0.000	0.004		0.001		-0.001
	(0.003)	(0.009)		(0.001)		(0.003)
GPA	0.081	0.122		0.018		0.047
	(0.123)	(0.453)		(0.079)		(0.150)
Oldest Child	-0.252**	-0.438		-0.080		-0.053
	(0.106)	(0.387)		(0.073)		(0.137)
Tenure	-0.010	-0.004		-0.0002		-0.011
	(0.014)	(0.063)		(0.011)		(0.016)
Constant	-4.403***	-5.856	1.016***	-0.456	2.747***	-1.484
	(1.594)	(5.266)	(0.171)	(0.914)	(0.542)	(1.243)
n	830	830	830	830	621	621
R^2 ("Overall R-sq" in f.e. models)			0.02	0.05	0.16	0.18
Log Likelihood	-1259.4	-445.8				

Robust standard errors, adjusted for clustering at the individual level, in parentheses.

*, ** and *** indicate 10, 5 and 1% significance levels respectively.

Table 7B

Demand for Stolen Loot

Protocol 1, College Students

Variable	UNCONDITIONAL DEMAND	PARTICIPATION EQUATION			CONDITIONAL DEMAND	
	Tobit	Logit	OLS Fixed Effects	OLS	OLS Fixed Effects	OLS
Loot Price	-4.678*	0.406	-0.109	-0.109	-4.344**	-4.738***
	(2.463)	(7.071)	(0.498)	(0.417)	(1.870)	(1.269)
Opportunity	0.180	-3.168	-0.090	-0.090	0.157	0.375
	(1.715)	(5.764)	(0.347)	(0.357)	(1.305)	(0.860)
Fine Price	-0.169	-1.449	-0.120	-0.120	0.165	0.198
	(0.567)	(1.523)	(0.115)	(0.116)	(0.433)	(0.251)
Age	0.137*	0.136		0.020		0.106
	(0.071)	(0.490)		(0.027)		(0.119)
Male	0.141	-1.676		-0.044		0.241
	(0.225)	(1.067)		(0.081)		(0.292)
Height	-0.024	4.028**		0.138		-0.377
	(0.293)	(1.972)		(0.105)		(0.330)
Money	0.002***	0.018		0.00002		0.002***
	(0.000)	(0.017)		(0.00007)		(0)
GPA	-0.365**	-3.767**		-0.113*		-0.112
	(0.165)	(1.509)		(0.059)		(0.178)
Oldest Child	-0.246*	1.067		-0.041		-0.180
	(0.143)	(1.524)		(0.061)		(0.254)
Tenure	-0.042***	-0.146**		-0.013		-0.017
	(0.016)	(0.059)		(0.008)		(0.019)
Constant	1.338	-5.494	1.096***	0.335	2.821***	2.820
	(2.481)	(12.810)	(0.199)	(0.715)	(0.750)	(3.414)
n	310	310	310	310	281	281
R^2 ("Overall R-sq" in f.e. models)			0.03	0.16	0.22	0.30
Log Likelihood	-455.0	-65.7				

Robust standard errors, adjusted for clustering at the individual level, in parentheses.

*, ** and *** indicate 10, 5 and 1% significance levels respectively.

Table 7C

Demand for Stolen Loot

Protocol 2, High School Students

	UNCONDITIONAL DEMAND	PARTICIPATION EQUATION			CONDITIONAL DEMAND	
Variable	Tobit	Logit	OLS Fixed Effects	OLS	OLS Fixed Effects	OLS
Loot Price	-42.659***	-17.15**	-2.719**	-2.822**	-35.683***	-33.864***
	(11.580)	(7.715)	(1.208)	(1.268)	(6.755)	(6.527)
Opportunity	2.083	1.638	0.253	0.237	1.417	0.910
	(2.278)	(1.337)	(0.238)	(0.216)	(1.321)	(1.391)
Fine Price	13.739***	9.041***	1.471***	1.519***	7.349***	6.701***
	(1.701)	(1.547)	(0.177)	(0.244)	(1.017)	(1.266)
Age	-0.60	-0.029		-0.004		-0.044
	(0.087)	(0.153)		(0.026)		(0.131)
Male	1.516***	1.487***		0.223***		0.363
	(0.268)	(0.513)		(0.075)		(0.373)
Height	-0.354	-0.596		-0.076		0.080
	(0.372)	(0.760)		(0.115)		(0.566)
Money	0.006	0.008		0.001		0.001
	(0.004)	(0.013)		(0.001)		(0.004)
GPA	0.562**	0.443		0.066		0.270
	(0.237)	(0.487)		(0.074)		(0.306)
Oldest Child	-0.202	-0.093		-0.011		-0.147
	(0.210)	(0.434)		(0.072)		(0.316)
Tenure	-0.021	-0.043		-0.006		0.017
	(0.030)	(0.056)		(0.009)		(0.042)
Constant	3.327	2.066	0.559***	0.805	4.990***	4.411
	(2.707)	(4.544)	(0.125)	(0.748)	(0.701)	(3.777)
n	790	790	820	790	617	595
R^2 ("Overall R-sq" in f.e. models)			0.04	0.10	0.08	0.09
Log Likelihood	-1625.4	-399.9				

Robust standard errors, adjusted for clustering at the individual level, in parentheses.

*, ** and *** indicate 10, 5 and 1% significance levels respectively.

Table 7D

Demand for Stolen Loot

Protocol 2, College Students

	UNCONDITIONAL DEMAND	PARTICIPATION EQUATION		CONDITIONAL DEMAND
Variable	Tobit	Logit	OLS Fixed Effects	OLS Fixed Effects
Loot Price	-75.798***	-61.621*	-3.843***	-60.396***
	(13.069)	(34.734)	(1.260)	(8.709)
Opportunity	7.316***	11.678*	0.691***	3.953**
	(2.573)	(6.621)	(0.249)	(1.700)
Fine Price	14.933***	10.320*	0.679***	12.164***
	(1.911)	(5.494)	(0.184)	(1.274)
Age				
Male				
Height				
Money				
GPA				
Oldest Child				
Tenure				
Constant	2.920**	-2.720	0.605***	5.073***
	(1.347)	(2.446)	(0.130)	(0.897)
n	340	340	340	312
R^2 ("Overall R-sq" in f.e. models)			0.04	0.24
Log Likelihood	-702.6	-89.4		

Robust standard errors, adjusted for clustering at the individual level, in parentheses.

*, ** and *** indicate 10, 5 and 1% significance levels respectively.

Table 8

Elasticities

Sample		Model (OLS/FE)	Loot Price	Opportunity	Fine Price
Protocol 1	HS	Participation (OLS)	0.051	-0.303	-0.106
			(0.203)	(0.587)	(0.093)
		Conditional Demand (OLS)	-0.475***	-0.057	0.053
			(0.138)	(0.431)	(0.065)
		Unconditional Demand (Tobit)	-0.493	-0.753	-0.129
			(0.482)	(1.394)	(0.222)
	College	Participation (OLS)	-0.032	-0.108	-0.693
			(0.175)	(0.508)	(0.081)
		Conditional Demand (OLS)	-0.622***	0.207	0.052
			(0.156)	(0.473)	(0.067)
		Unconditional Demand (Tobit)	-0.715*	0.114	-0.052
			(0.377)	(1.089)	(0.173)
Protocol 2	HS	Participation (OLS)	-0.386*	0.268	0.353***
			(0.234)	(0.383)	(0.059)
		Conditional Demand OLS)	-0.903***	0.201	0.317***
			(0.164)	(0.307)	(0.059)
		Unconditional Demand (Tobit)	-1.752***	0.708	0.960***
			(0.482)	(0.775)	(0.127)
	College	Participation (FE)	-0.431***	0.642***	0.130***
			(0.141)	(0.231)	(0.035)
		Conditional Demand (FE)	-1.412***	0.767**	0.492***
			(0.205)	(0.330)	(0.052)
		Unconditional Demand (Tobit)	-1.969***	1.572***	0.659***
			(0.344)	(0.555)	(0.087)

Robust standard errors, adjusted for clustering at the individual level, in parentheses.

*, ** and *** indicate 10, 5 and 1% significance levels respectively.

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 we will never reveal them to anyone. 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 as if you are the criminal. 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.