Deterrence, Expected Cost, Uncertainty and Voting: Experimental Evidence

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Abstract: We conduct laboratory experiments to investigate the effects of deterrence mechanisms under controlled conditions. The effect of the expected cost of punishment of an individual's decision to engage in a proscribed activity and the effect of uncertainty on an individual's decision to commit a violation are very difficult to isolate in field data. We use a roadway speeding framing and find that (a) individuals respond considerably to increases in the expected cost of speeding, (b) uncertainty about the enforcement regime yields a significant reduction in violations committed, and (c) people are much more likely to speed when the punishment regime for which they voted is implemented. Our results have important implications for a behavioral theory of deterrence under uncertainty.

JEL Classifications: C91, D03, D81, K42

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The use of deterrence mechanisms – such as the probability of apprehension, fines, jail sentence length, and legal liability – in preventing proscribed activities is theoretically well understood: increases in the expected cost of committing illegal activities will reduce the amount of crimes committed.[1] The concept of deterrence is not new; written accounts date back as far as 18th century, when Jeremy Bentham argued that crime was the product of the exercise of free will and could be deterred by punishment such that the expected discomfort experienced would outweigh the pleasure of engaging in the criminal activity. In the realm of economics, Becker (1968), Stigler (1970), Polinsky and Shavell (1979), and Ehrlich (1996) laid the foundation upon which the theory of deterrence rests. More contemporary extensions of the deterrence literature include Viscusi (1986), Posner (1985), Mookherjee (1997), and Mookherjee and Png (1994).

From a behavioral perspective, scholars in the field of law and economics have begun asking fundamental questions about the individual's interaction with legal institutions. Sunstein (1997) and Jolls, Sunstein, and Thaler (1998) provide some of the earliest analyses on the value of combining behavioral economics and jurisprudence. This research provided a gateway for research in the area of behavioral economics and law. Garoupa (2003) provides an essay on the usefulness of behavioral economics in determining the optimal level of law enforcement. This concept applies directly to the behavioral issues that surround the theory of deterrence.[2] One core issue is the extent to which people understand the concept of deterrence. In other words, is the decision of whether or not to engage in a proscribed activity sensitive to the expected cost? When deterrence mechanisms are not understood, the effectiveness of enforcement tools can be weakened or even nullified.

[1] For simplicity, we shall use the term "deterrence mechanism" as a catch-all phrase for all enforcement tools.

[2] For example, Viscusi and Evans (2006) examine the differences between behavioral (anterior) probabilities and posterior assessments of the risk associated with an event after an individual receives additional information about the likelihood of an event, finding that anterior and posterior probabilities differ considerably. In another analysis, Viscusi and Hakes (2003) examine the use of risk ratings in determining the self-assessed versus actual survival probabilities at different ages. Again, these probabilities differ from one another quite a bit.

A second important issue is the effect of uncertainty on an individual's decision to engage in a proscribed activity.[3] To wit, when an individual is uncertain about features of the deterrence mechanism, he or she might opt to participate in an illegal activity when it is economically sensible to refrain from participating, or *vice versa*. In fact, the uncertainty that surrounds the deterrence mechanism could prove useful for deterrence purposes. Harel and Segal (1999) discuss this issue by asking whether increasing certainty in sentencing and uncertainty with respect to the probability of detection can be justified because this approach would be more unattractive to would-be criminals.[4]

Empirically, examining the degree of effectiveness of enforcement mechanisms can be problematic for a variety of reasons. First, isolating the degree of change of a single enforcement mechanism is problematic with field data. Simultaneous changes in multiple enforcement tools make it quite challenging to ferret out the impact of a particular mechanism on undesirable actions.[5] Next, an individual's knowledge of the change in enforcement mechanisms is uncertain. For example, even if the probability of being apprehended (hereafter probability) for a particular crime were to increase while no other enforcement mechanism changed, should we expect an individual to accurately perceive the new chance of being caught? Additionally,

[3] The concept of uncertainty can take several forms in this analysis. For example, individuals could be unaware of the probability and fine, just the fine, or just the probability of apprehension. Savage (1954) claims that uncertainty may be treated similarly to risk, however Ellsberg (1961) conducts a series of experiments to show that risk and uncertainty are, in fact, different notions. In follow up work by Halevy (2007), a different form of uncertainty (compound lotteries) is determined. In the context of our experiment, implementing the uncertainty from Halevy (2007) allows the probability and fines for several enforcement regimes to be known, but the enforcement regime that is implemented is only probabilistically known. Thus, as seen in Halevy (2007), individuals are uncertain about the expected cost of committing a proscribed activity because they must reduce compound lotteries into simple probabilities. This form of uncertainty provides an environment that permits participants in the experiment to determine their preferred enforcement regime. See the discussion in section 2 for more detail.

[4] Lazear (2004) also discusses the effect of police enforcement on speeding by noting that when few police are available to enforce speeding, it is best to announce their locations *ex ante*. Alternatively, when many police are present, it is best to not announce their locations. This detail does not play a significant role in our analysis, as police budgets or availability are not a direct concern in this analysis.

[5] Becker (1968) noted that enforcement efforts and sanctions are substitutes. For example, it is often the case that decreases in the number of police officers on highways and increases in speeding fines occur simultaneously.

empirical data suffers from measurement error, simultaneity issues with regards to the location of police and the degree of recidivism in a particular region, as well as a selection of data that can only examine those who have not been deterred (offenders) instead of examining both those who have and have not been deterred.

Despite several attempts (discussed in the next section) to empirically isolate the effect of enforcement mechanisms on the number of proscribed activities committed, there still exists a relative void in the law and economics literature regarding the impact of enforcement mechanisms when there is uncertainty on the part of the potential malfeasant. Bebchuk and Kaplow (1992) state: "To guide enforcement policy, empirical research on this point [effect of uncertainty on the decision to commit a crime] would be useful. For example, one might attempt to infer probability perceptions from behavior, which could be accomplished in an experimental setting." Indeed, laboratory methods offer the possibility of testing for effects in a controlled environment.

In this light, we conduct laboratory experiments in which we hold the expected cost of a violation constant while varying the probability of detection, and also examine the effect of uncertainty over this probability. We also test how the expected cost of a violation affects the likelihood of a violation. In our design, each individual decides whether to 'speed', with financial benefits for speeding, but financial costs if one is caught. We vary the probability of being caught and the resulting cost if detected. In addition, in some periods we permit people to vote for one of two probability/cost regimes, implementing the one receiving the majority of votes.

In short, our main results are that increased uncertainty over the deterrence regime leads to a significantly lower violation rate, as the expected psychological cost of a violation appears to be higher in regimes with larger probabilities (and smaller fines). We also find that the violation rate is sharply reduced when the expect cost is higher. Finally, our experiments shed light on whether people are more likely to comply with a regulation for which they have voted. Harel and Segal (1999, p. 280) point out: "The scheme that should be preferred by the policy maker is precisely the scheme that is disfavored by the potential criminal." We provide some experimental support for this view, as violations are much less likely for an individual when he or she has voted against the enforcement regime that has been chosen.

The remainder of this paper is organized as follows. Section 2 offers a literature review, while section 3 presents the details of our experimental design and discusses how this design can be seen as reflecting uncertainty. The experimental results are presented in section 4, and we discuss our results and conclude in section 5.

2. Literature review and background

The topic of enforcement costs and benefits has been studied extensively in the literature on law and economics. However, there are only a handful of controlled experimental studies. Baker et al. (2003) use an experimental setting to test for the effect of uncertainty in an environment where the choice involves an act that could yield financial losses or gains. They find that uncertainty over a deterrence mechanism increases the level of deterrence. However, the experimental framework examines the effect of uncertainty in a risky environment, which is not framed as an environment with an illegal option.[6]

Using auctions and the possibility of collusion among participants, Block and Gerety (1995) test whether attitudes towards certainty and severity of punishment differ between

[6] In addition, the authors use probabilities that are not on the linear portion (probabilities ranging between 0.2 and 0.8) of the S-curve in Wu and Gonzalez (1996). Wu and Gonzalez (1996) noted that individuals did not accurately perceive the probability of an event when it is either very unlikely (close to zero percent) or very like (close to 100 percent).

students and prisoners. They find that the prisoners are more concerned with the likelihood of punishment, while this is reversed for the students. In a related paper, Anderson and Stafford (2003) examine the effect of punishment on free-riding in regulatory compliance and find (a) that compliance is increasing in expected cost of punishment and (b) that punishment severity has a larger effect on compliance than punishment probability. Note that our results differ in that we find that larger probability enforcement regimes (with equivalent expected costs) increase compliance.

There are also studies using data from natural experiments. Bar-Ilan and Sacerdote (2001) measure the change in red lights run when the fine for running the red light changes, Ihlanfeldt (2003) measures the increase in crime due to the construction of commuter rails in Atlanta, and McCormick and Tollison (1984) measure the reduction in the number of fouls committed by NCAA division 1 men's basketball players when the number of referees on the court increases from two to three. Also, Di Tella and Schargrodsky (2004) examine the effect of targeted increases in police enforcement (in the aftermath of terrorist attacks) on the level of crime (notably car thefts) and find significant reductions in crime due to the exogenous increase in law enforcement presence.

However, none of these studies is ideally-suited for testing deterrence theory. The studies by Bar-Ilan and Sacerdote (2001) and Ihlanfeldt (2003) suffer from the fact that the individual committing the crime is uncertain about their probability and amount of the fine that will be charged.[7] Although the basketball players in McCormick and Tollison (1984) are certainly aware of the increased probability of misbehavior being detected, the estimation suffers from

[7] Note that in Bar-Ilan and Sacerdote (2001) the identification comes from the fact that fines are changing. In Ihlanfeldt (2003), it is argued that lower transportation costs reduce the transportation costs of committing a crime and, thus, it assumed that committing a crime is now effectively cheaper. That said, it is not clear that the individual committing the crime will be certain of the probability of being apprehended or fined when they travel to a different city to commit a crime.

unintentional misclassification, as the main dependent variable reported in the box scores – fouls – sometimes included both the number of fouls and the number of rebounds. When corrected, the estimation was only significant at the 10 percent level.[8] Finally, Di Tella and Schargrodsky (2004) provide a somewhat limited analysis in terms of the crimes committed, focusing only on car thefts over a short time period.

Since one of the foci of this article is the effect of uncertainty on deterrence, we wish to address how uncertainty is characterized in our environment. The economics literature distinguishes between risk and uncertainty. The first work to make this distinction was Knight (1921), stating: "Uncertainty must be taken in a sense radically distinct from the familiar notion of Risk, from which it has never been properly separated.... It will appear that a measurable uncertainty, or 'risk' proper, as we shall use the term, is so far different from an unmeasurable one that it is not in effect an uncertainty at all."[9] Ellsberg (1961) provides persuasive examples that people having preferences distinguish between risk (known probabilities) and uncertainty (unknown probabilities). However, Heath and Tversky (1991) point out that ambiguity also makes people shy away from taking either side of a bet, as not knowing important information about the environment is psychologically uncomfortable and effectively reduces confidence.

Different researchers appear to have very strong convictions with respect to the issue of what constitutes uncertainty. One approach is normative and views ambiguity as a situation in which the decision-maker cannot assign probabilities to events. According to this approach, being ambiguity averse is perfectly rational and is a natural response to lack of information.[10] In much of the research designed to examine the Ellsberg paradox, it is assumed that individuals

[8] See Hutchinson and Yates (2007) for further details.
[9] Other very notable early work in this area also includes von Neumann and Morgenstern (1947) and Savage (1954).
[10] But see Fox and Tversky (1995), who find that ambiguity aversion is reduced or eliminated in a between-subjects design.

can reduce compound objective lotteries. An alternative approach is more descriptive and is based on the observation that there exists a very strong empirical association between ambiguity aversion and violation of reduction of compound lotteries. For example, Halevy (2007) discusses uncertainty – from the perspective of the decision maker – that arises from an individual's inability to comprehend compound lotteries or to calculate probabilities of final outcomes in compound lotteries according to the laws of probability. Since this is difficult or impossible to justify normatively, and can be safely viewed as a mistake or bias, the holders of this view tend to agree that ambiguity aversion is a form of bounded rationality.[11] From this perspective, the use of compound lotteries in decision-making can in effect induce uncertainty. This is the perspective that we take in this paper, and we do in fact find consistent evidence that speeding rates are lower when a compound lottery is present.[12]

In our experiment, we have environments with and without voting. In the case without voting, there is a compound lottery involving the probability that a particular deterrence regime will be chosen and the probability and consequences of detection. Since in principle all of the probabilities are known, some people would view this as a form of risk, rather than uncertainty; alternatively, if people are unable to reduce compound lotteries, then our experimental results in these environments can be informative as to how subjects respond to ambiguity or uncertainty. In our environment with voting for a preferred regime (with equivalent expected costs of committing a crime), the voters are informed of the regime that is implemented so that there is no compound lottery and ambiguity is averted. This environment provides a situation with risk, whereby individuals are aware of the regime that is implemented (and corresponding probability of being caught) before deciding whether or not to speed. This allows us to test environments

[11] See Halevy (2007) for more detail. We thank Yoram Halevy for valuable discussions on this topic.

[12] In a certain sense, if one considers the inability to reduce compound lotteries as reflecting a weaker form of uncertainty, our experimental results might be considered to be a lower bound on the effects of uncertainty.

containing risk versus uncertainty.

Thus, one can view our environments as either comparing behavior under risk with behavior under uncertainty or comparing behavior under different degrees of uncertainty. In any case, it seems fair to say that our paper examines the effect of uncertainty on an individual's willingness to commit proscribed activities.

3. Experimental Design

We conducted nine experimental sessions at the University of California at Santa Barbara. Participants were recruited using ORSEE (Greiner 2004) from a campus-wide database of students who had registered for participation in paid experiments and our experiment was programmed using z-tree (Fishbacher 2007). A total of 125 students participated in the experiment, with no person permitted to participate in more than one session. The number of people in each session ranged from seven to 19, but was always an odd number. Average earnings for an experiment lasting less than one hour were about $15, including a $5 payment for showing up on time. Each session had an odd number of people present (to avoid ties in the voting stage described below).

One consideration was how to choose a proscribed activity. While we could ask the participants about serious crimes such as murder, extortion, etc., we thought it would be unlikely that they would indicate they would choose such activities, even in the lab. Thus, we wished to find a proscribed activity in which people frequently engage, as this seemed more likely to avoid strong emotional connotations. Speeding seems a natural choice that has also been discussed empirically discussed (see Ashenfelter and Greenstone (2004) and DeAngelo and Hansen (2009)).

We had two experimental settings. In both, we had 30 periods, which consisted of three blocks of 10 periods. In each period, a participant faced a choice of whether or not to speed. If the participant chose not to speed, the payoff for that period was $0.60. If the participant instead chose to speed, the payoff for the period was $1.00 less a possible fine if caught.[13] After each period, participants received feedback concerning the outcome. In each period of both settings, each participant faced the same regime. Sample experimental instructions are given in Appendix A.

We start by describing the first experimental setting, in which 87 people participated. In periods 1-10, people were first told that there was a 50% chance of being in each of two regimes, with the outcome randomly determined. In regime 1, the probability of being caught was 1/3 and the fine if caught was $0.90; in regime 2, the probability of being caught was 2/3 and the fine if caught was $0.45. After being so informed, participants then decided whether or not to speed.[14] The expected fine from speeding was $0.30 in each case; since the net expected earnings from speeding ($0.70) is greater than the earnings from not speeding ($0.60), a risk-neutral person should prefer to speed in either regime. In periods 11-20, people first voted in each period for either regime 1 or regime 2. While voting is admittedly not totally realistic, it nevertheless establishes a link between enforcement preference and compliance. The regime receiving the most votes (note that the odd number of participants in a session ensured that there were no ties) was implemented and the participants were informed of the applicable regime; the decision of whether or not to speed then followed.

[13] Our design does not explicitly take into account the guilt, shame or stigma that might result from being caught committing an infraction. In a sense, this could be considered to be a part of the fine, but one that is costless to administer. Although we see no way to test for this in the data, it seems reasonable that people may have an aversion to simply being caught, which would lead to less speeding than otherwise.

[14] An implicit assumption of this experimental design is that speeding beyond the posted speed limit has a negative externality, as discussed in Ashenfelter and Greenstone (2004) and DeAngelo and Hansen (2009), in that it can increase the number of fatalities on the roadway. There are, however, opponents to the belief that increases in speed augment fatalities (see Lave (1985)).

In periods 21-30, people voted on two new regimes to test whether a higher expected cost of speeding would lead to lower speeding rates. In the first of these, the probability of being caught speeding was 3/5 and the fine if caught was $0.833; in the second, the probability of being caught speeding was 4/5 and the fine if caught was $0.625. Thus, the expected fine if one chose to speed was $0.50 in each of these regimes; since the net expected earnings from speeding ($0.50) is less than the earnings from not speeding, a risk-neutral person should prefer to not speed in either regime.[15] Each participant faced the same regime in each period.

Our second experimental setting, in which 38 people participated, was intended to control for the possibility that the act of voting changed one's preferences. As before, in periods 1-10, people were first told that there was a 50% chance of being in each of two regimes. In regime 1, the probability of being caught was 1/3 and the fine if caught was $0.90; in regime 2, the probability of being caught was 2/3 and the fine if caught was $0.45. In periods 11-20, instead of having voting, we simply imposed one of the two regimes, and the regime was varied from one period to the next. People were told with certainty which regime would apply in the coming period. Finally, periods 21-30 were the same environment as periods 11-20 in our first setting: people first voted in each period for either regime 1 or regime 2. The regime receiving the most votes was implemented and the participants were informed of the applicable regime.[16]

We chose to pay for only three periods, with one drawn randomly from periods 1-10, another from 11-20, and the third from 21-30. We then multiplied the payoffs from these three

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[15] Of course, people may very well not be risk neutral, so that some risk-averse people might choose not to speed in periods 1-20, while some risk-seeking people might choose to speed in periods 21-30. Nevertheless, if an individual's *per se* attitude towards risk does not change over the course of an experimental session, risk preferences may not affect our within-subject analysis.

[16] We chose to expose people to regimes before initiating voting, as it seemed that one should have some experience with some actual regimes before beginning to vote; this sets up the possibility of order effects. Nevertheless, we can test for trends over time in the decision to speed over each 10-period block, by regime and treatment. We find a significant effect in only one of the 12 tests (periods 21-30 in the first treatment with the regime with the smaller fine), as might be expected for 12 comparisons and a 5% significance level.

periods by five, and added the $5 show-up fee. By not paying for each period (a speeding participant could otherwise know that he or she had lost his or her endowment early in the session and subsequently take chances, knowing that losses could not be enforced), we avoided the possibility that bankruptcy issues could interfering with decisions.[17]

4. Experimental Results

We find strong evidence that people are less likely to speed when the expected cost of speeding is higher, when the regime that an individual has voted against is actually implemented, and when there is uncertainty over which regime will apply. In this section, we first discuss each setting in turn, providing summary statistics and nonparametric statistical tests based on each individual's tendencies. We then present comprehensive regression analysis for both settings.

4.1 Results for Setting 1

Figure 1 provides a visual illustration of the overall speeding rates in setting 1, according to each 10-period block ("Don't know regime" represents periods 1-10, "Voted and know regime" represents periods 11-20, and "Voted and know regime, high cost" represents periods 21-30). Table 1 provides more detailed information. Figure 1 shows a modest difference between the speeding rates in the leftmost columns, with a large difference in speeding rates between the leftmost columns. The difference in the first case reflects the effect of uncertainty regarding the regime in place, although this could also be affected by the act of voting for a regime. This difference in speeding rates is statistically significant according to the nonparametric binomial test (see Siegel and Castellan 1988), using each individual's overall

[17] It is true that this creates another layer of compounding, so that we do not in fact have a pure distinction between risk and uncertainty. Nevertheless, this aspect of compounding seems considerably easier, and in any case, we have a distinction induced by the greater complexity of the lotteries involving voting.

speeding rates in the two classes.[18] The speeding rate was higher for 40 people when people voted and then learned the regime than when they did not know the regime, was the same for 33 people, and was lower for 14 people, yielding $Z = 3.54$ and $p = 0.000$.[19] The larger difference in speeding rates according to whether the expected cost is higher or lower is also statistically significant according to the nonparametric binomial test; the speeding rate was higher with the lower expected cost than with the higher expected cost for 61 people, the same for 23 people, and reversed for only three people, yielding $Z = 7.25$ and $p = 0.000$ (recall that the difference between these cases is that the expected cost of speeding is 0.3 units in the first case and 0.5 units in the second case). Thus, behavior is quite sensitive to the expected cost.

Figure 1 - Speeding rates by category, Setting 1

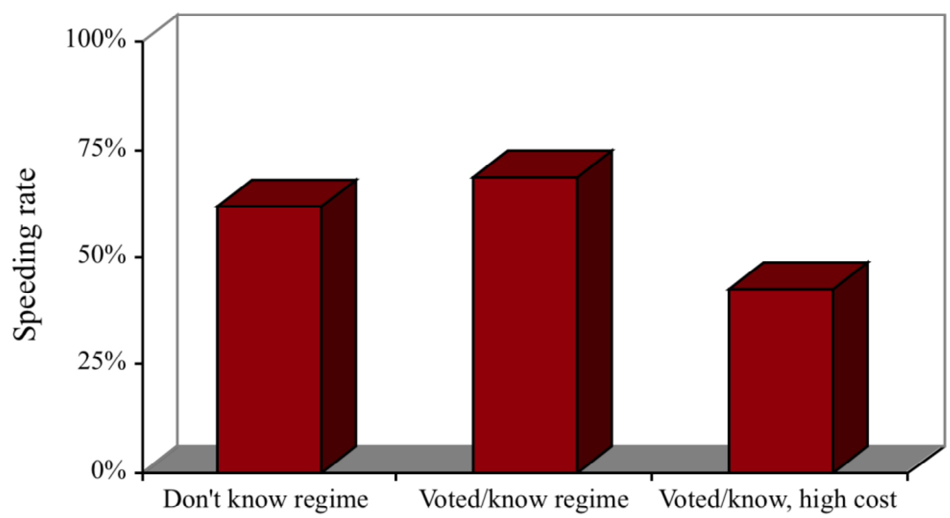


Table 1 – Speeding rates by block of periods, Setting 1

Category	Overall rate	Observations, regime 1	Rate in regime 1	Observations, regime 2	Rate in regime 2

[18] We can use this test because we have within-subject data on the various blocks and regimes. The logic of this test is that if people are behaving randomly, we should expect as many people to speed more frequently in regime 1 as there are people who speed more frequently in regime 2. If these numbers differ a great deal, this indicates that behavior is not random.

[19] In this paper, we round off each *p*-value to the third decimal place. All tests are two-tailed, unless otherwise indicated.

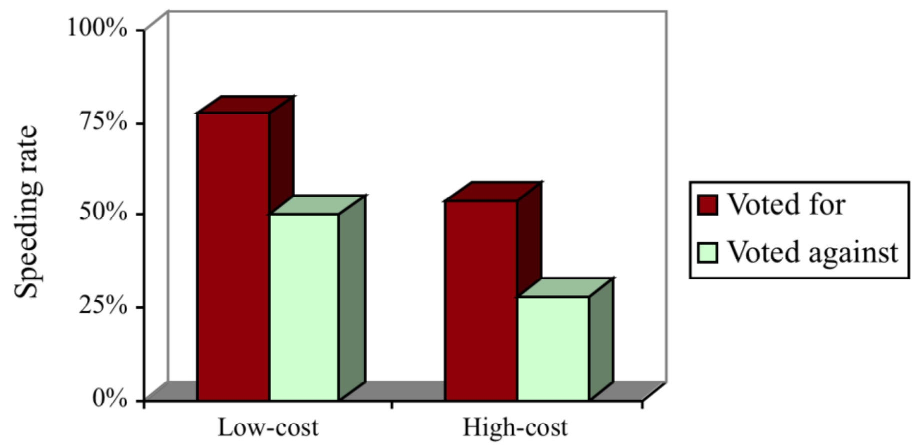
Don’t know regime	.616 (.016)	461	.616 (.023)	409	.617 (.024)
Voted/know regime	.685 (.016)	513	.671 (.021)	357	.706 (.024)
Voted/know, high cost	.424 (.016)	686	.461 (.019)	184	.288 (.033)

Standard errors are in parentheses.

Table 1 also breaks down speeding behavior according to the regime in place. Since people did not know *ex ante* which regime was in place in the first set of periods, it is reassuring that the speeding rates were nearly identical for each regime. When people in the low-cost case are able to vote for regime 1 (with a 1/3 chance of detection and a 0.90 fine if detected speeding) or regime 2 (with a 2/3 chance of detection and a 0.45 fine if detected speeding), we see that 53.9% of the votes were for the regime with a higher fine and a lower probability of detection. However, there is no significant difference in the speeding rate depending on the regime in place when people voted in the low-cost case; the binomial test gives $Z = 1.04$ and $p = 0.298$. The preference for regime 1 over regime 2 (respectively, a fine of 0.625 units with a probability of detection of 4/5 versus a fine of 0.833 with a detection probability of 3/5) is stronger in the high-cost case, with 78.9% of the votes for regime 1. In this case, we do have a substantial and significant difference in speeding rates depending on the regime; the binomial test gives $Z = 3.88$ and $p = 0.000$.

Why do people both prefer regime 1 and speed more frequently under regime 1 in the voting, high-cost case? It turns out that there is a striking relationship between the choice of whether to speed in a regime and whether or not the person had voted in favor of this regime. This is illustrated in Figure 2, which shows speeding rates in the low- and high-cost cases depending on whether one voted for the regime actually implemented.

Figure 2 - Speeding rates by vote-match, Setting 1



The differences are large (77.5% versus 50.4% with low cost, and 53.9% versus 28.0% (with high cost) and highly significant; the binomial test on individual rates across voting outcomes gives $Z = 3.75$ and $p = 0.000$ for the low-cost case and $Z = 4.20$ and $p = 0.000$ for the high-cost case.[20]

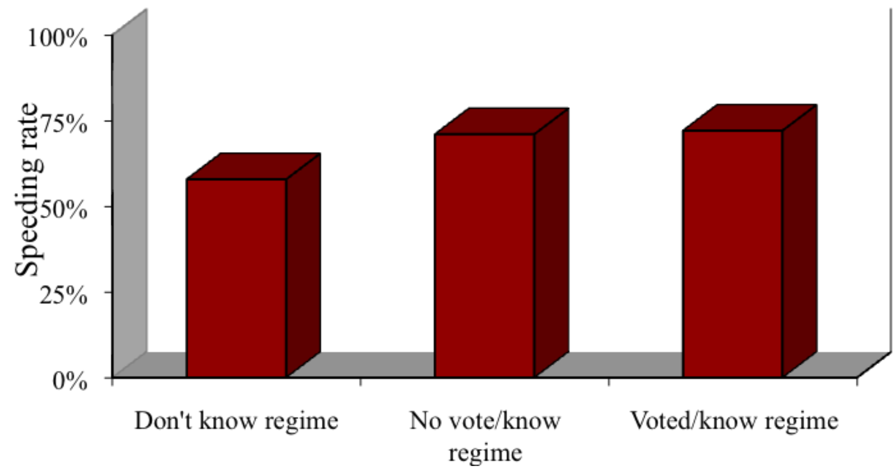
4.2 Results for Setting 2

Figure 3 shows a difference between the speeding rates according to whether people knew the regime in place (without voting in either case), with no difference in overall speeding rates according to whether people voted (knowing the regime in either case).[21]

[20] Note that these results extend what is observed in the tax compliance literature. That is to say, we do not observe increased compliance with the law when the preferred regime is instituted (as seen in Alm et al. (1993), Alm et al. (1999), and Feld and Tyran (2002)).

[21] We also note that there is no significant difference between either the speeding rates across the first 10 periods of settings 1 and 2 (61.6% versus 57.9%), or across periods 11-20 of setting 1 and periods 21-30 of setting 2; this is reassuring since they are identical decisions for each comparison. The Wilcoxon rank sum test (See Siegel and Castellan 1988) gives $Z = 0.08$ for the first comparison and $Z = -1.05$, neither close to statistical significance.

Figure 3 - Speeding rates by category, Setting 2



The comparison across the leftmost columns provides a particularly clean test of the effect of uncertainty on the decision to speed, as in setting 2 there is no voting in periods 11-20. This difference in speeding rates is statistically significant according to the binomial test, using each individual's overall speeding rates in each category. The speeding rate was higher with a known regime for 17 people, the same for 15 people, and lower with a known regime for six people, yielding $Z = 2.29$ and $p = 0.022$. The very small difference in speeding rates according to whether one voted is not statistically significant, as the speeding rate was higher without voting for 17 people, the same for 10 people, and lower without voting for 11 people, yielding $Z = 1.13$ and $p = 0.258$.

Table 2 also breaks down speeding behavior according to the regime in place. Once again, people did not know *ex ante* the regime that would be in place in the first 10 periods, and we see that the speeding rates are similar for each regime (the binomial test for differences gives Z = 0.83 and $p = 0.406$). When there is no voting but there is awareness of the regime in place, people speed more frequently in regime 2 (with the higher detection rate); however, while the difference amounts to 14.4 percentage points, it is not statistically significant; the binomial test gives $Z = 1.51$ and $p = 0.131$.

Table 2 – Speeding rates by block of periods, Setting 2

Category	Overall rate	Observations, regime 1	Rate in regime 1	Observations, regime 2	Rate in regime 2
Don't know regime	.579 (.025)	217	.567 (.034)	163	.595 (.039)
No vote/know regime	.711 (.023)	191	.639 (.035)	189	.783 (.030)
Voted/know regime	.721 (.023)	342	.719 (.024)	38	.737 (.073)

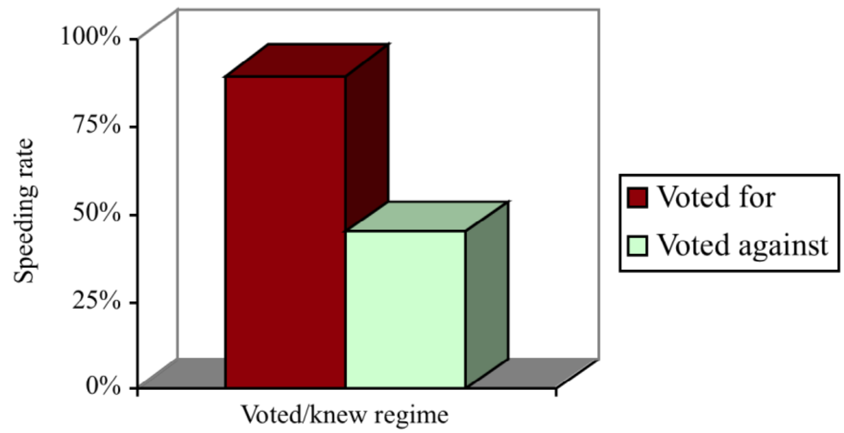
Standard errors are in parentheses

The preference for regime 1 over regime 2 (respectively, a 1/3 chance of detection and a 0.90 fine if detected speeding versus a 2/3 cost of detection and a 0.45 fine if detected speeding) in the last category is slightly (but not significantly) higher than that in periods 11-20 of setting 1 (these categories feature identical choices), with 59.7% of the votes for regime 1. Here we have a very small and insignificant difference in speeding rates depending on the regime. However, once again there is a striking relationship between the choice of whether to speed in a regime and whether or not the person had voted in favor of this regime. This is illustrated in Figure 4, which shows speeding rates in the voted/know category depending on whether one voted for the regime actually implemented.

The speeding rate is double (90.0% versus 45.0%) when the participant had voted for the regime that was implemented; this difference in speeding rates is significant; the binomial test on individual rates gives $Z = 2.65$ and $p = 0.008$.[22]

[22] Note that the speeding rates in block 3 in setting 2 are not significantly different than those in block 2 of setting 1.

Figure 4 - Speeding rates by vote-match, Setting 2



Before turning to our regression analysis, we would like to comment on one aspect of our results in both settings, which reflects on our theoretical result that people will perceive a larger expected cost of speeding in the regime with the higher probability. In our experiments, participants should vote for the regime that has the smaller perceived cost of speeding. We see that people vote more often for the regime with the higher fine and the smaller probability of detection.[23] Thus, since in all blocks involving voting, people prefer a higher fine and a smaller chance of detection, the regime with a **higher** probability and a **lower** potential fine is less attractive overall to would-be speeders. If the goal of a policy-maker is to make speeding unattractive to drivers, choosing this latter regime should be more effective, particularly since people are much more likely to speed when the regime for which they voted is in place.

4.3 Regression analysis

We present a separate series of regressions for each setting. Table 3 shows probit

[23] In addition to the results already presented, in block 3 of setting 1, 61.0% of the votes are in favor of regime 1 (with a detection probability of 3/5 and a potential fine of 0.833 units as opposed to a detection probability of 4/5 and a potential fine of 0.625 units with regime 2).

regressions for setting 1.[24]

Table 3: Random-effects Probit Regressions for Determinants of Speeding in Setting 1

	Dependent variable			
Independent variables	(1) Speeding	(2) Speeding (Periods 11-30)	(3) Speeding (Periods 11-30)	(4) Speeding (Periods 11-30)
Didn't know regime	-0.282*** (0.074)	-	-	-
Voted/know, high cost	-0.972*** (0.074)	-1.129*** (0.083)	-0.105 (0.244)	-0.103 (0.245)
Regime 2	-	-0.280*** (0.089)	0.090 (0.124)	0.072 (0.168)
Vote match	-	0.804*** (0.083)	0.796*** (0.084)	0.755*** (0.282)
Voted/know, high cost*Regime 2	-	-	-0.777*** (0.177)	-0.779*** (0.177)
Vote match *Regime	-	-	-	0.031 (0.201)
Constant	0.571*** (0.077)	0.694*** (0.158)	0.197 (0.194)	0.221 (0.249)
Rho	0.534*** (0.035)	0.534*** (0.035)	0.536*** (0.038)	0.587*** (0.039)
N	2610	1740	1740	1740
Log likelihood	-1312.7	-827.6	-817.8	-817.8

Standard errors are in parentheses; ***, **, and * indicate significance at $p = 0.01$, $p = 0.05$, and $p = 0.10$. Specifications (2)-(4) include data only periods 11-30. Vote match =1 if the individual voted for the regime that was implemented. We use clustered standard errors at the level of the individual subject.

Specification (1) tests for unconditional differences in speeding rates across blocks. As we found in our nonparametric tests, the speeding rate when the regime in place was unknown is significantly lower than when it is known and the expected cost is low (the omitted category), and the speeding rate with a high expected cost is significantly lower than with a low expected

[24] In both Table 3 and Table 4, the random-effects assumption that the unobserved individual effect is not correlated with any of the explanatory variables is violated when voting behavior is included in the regressions. To check for the robustness of the regression results, we have therefore performed both logit regressions and OLS regressions with fixed effects. The results are qualitatively nearly identical, with no serious changes in the significance levels of any of the explanatory variables. Since the random-effects regressions are appropriate for some of the columns, we choose to report these results in all cases.

cost. Specifications (2)-(4) adds the regime and the factor of whether the individual voted for the implemented regime; since voting is only permitted in periods 11-20 and 21-30, we only include the data from these categories in the regressions. We see that, as seen in Figures 2 and 4, people are much more likely to speed when their preferred regime is in force. While specification (3) indicates that people are significantly less likely to speed under regime 2, the interaction term in specification (4) shows that this effect is entirely driven by behavior in block 3. Finally, there is no difference across regimes in the effect of having voted for the implemented regime.

Table 4 shows similar regressions for setting 2. Specification (1) tests for unconditional differences in speeding rates across 10-period blocks. The speeding rate when the regime is unknown is significantly lower than when it is known without voting, showing the effect of uncertainty on speeding rates, while there is no difference between speeding rates according to whether one has voted (and learned the regime). Specifications (2) - (4) indicate that this is entirely driven by the informed, no-voting case, where there is a somewhat higher speeding rate when the likelihood of being caught is higher but the cost is lower (Regime 2). Once again, whether one voted for the implemented regime is a significant factor in specifications (4) and (5). No other coefficients are significant in specifications (4) and (5), with the direction of the coefficient for regime reversing when the interaction term is included. Thus, we see that, once again, there is less speeding with uncertainty (comparing the informed, no-voting case – the omitted variable – to the uninformed, no-voting case) and that the speeding rate is quite sensitive to whether or not one has voted for the regime that has been implemented.[25]

[25] Having been caught (or not having been caught) speeding in the last period has no significant effect in our regressions. In principal, there is no reason to expect such an effect, as the outcome from speeding in the past period should have no effect on the actual expected cost of speeding. In fact, while there is no effect in the aggregate (people sped 80.2% of the time after having been caught speeding in the last period, compared to 76.6% of the time when they sped in the last period and were not caught), there is a considerable effect on individuals. In fact, the difference in these two rates exceeds 25 percentage points for 39% of the population. However, these differences go in opposite directions, effectively eliminating any aggregate effect. While it might seem natural that people are less

Table 4: Random-effects Probit Regressions for Determinants of Speeding in Setting 2

	Dependent variable				
Independent variables	(1) Speeding	(2) Speeding (Periods 1-10)	(3) Speeding (Periods 11-20)	(4) Speeding (Periods 21-30)	(5) Speeding (Periods 21-30)
Didn't know regime	-0.544*** (0.116)	-	-	-	-
Voted/know regime	0.074 (0.119)	-	-	-	-
Regime 2	-	0.092 (0.224)	0.571*** (0.169)	-0.382 (0.336)	0.429 (0.640)
Vote match	-	-	-	1.299*** (0.263)	2.917*** (1.151)
Vote match *Regime 2	-	-	-	-	-1.225 (0.887)
Constant	0.744*** (0.096)	-0.368 (0.344)	-0.098 (0.307)	1.027** (0.476)	-0.263 (0.827)
Rho	0.575*** (0.035)	0.869*** (0.030)	0.487*** (0.087)	0.766*** (0.075)	0.666*** (0.075)
N	1140	380	380	380	380
Log likelihood	-496.0	-127.1	-190.6	-128.2	-127.7

Standard errors are in parentheses; ***, **, and * indicate significance at $p = 0.01$, $p = 0.05$, and $p = 0.10$. Specifications (2) includes data only from periods 1-10, specification (3) only includes data from periods 11-20, and specifications (3) and (4) only include data from periods 21-30. Vote match =1 if the individual voted for the regime that was implemented. We use clustered standard errors at the level of the individual subject.

We can also make some comparisons across settings. For example, comparing the no vote/know regime in setting 2 to the uncertainty treatment in setting 1, we see that the rate of speeding is higher in the no vote/don't know environment. A Wilcoxon-Mann-Whitney ranksum test on individual speeding rates (this is across different individuals in the two different settings) finds the difference to be marginally significant ($Z = 1.29$, $p = 0.099$, one-tailed test justified by *ex-ante* hypothesis). This is slight further evidence that uncertainty leads to lower violation rates.

likely to engage in a certain behavior after having a bad outcome (even though there is nothing to actually be learned), many people also seem to believe in the "law of averages".

A second comparison is between the two voting environments with low cost, periods 11-20 in setting 1 and periods 21-30 in setting 2, where there should be no difference across the identical decision environments. The violation rates are .685 and .721, respectively; the ranksum test on individual rates gives $Z = 1.046$, $p = 0.296$ (two-tailed test of the null hypothesis that the violation rates are not significantly different from each other).

In closing Section 3, we would like to point out that if the expected cost of speeding is the same for each period within a category, people should either always speed or never speed. However, there is substantial within-subject variance within a category for many participants. If we define consistency within a category as choosing to speed either 0, 1, 9, or 10 times (allowing up to one deviation from absolute consistency), we observed consistency within a category for about half of the participants overall.[26] While a frequent lack of consistency in the speeding decision might seem a bit disconcerting, we don't feel that people change their decisions to reflect a change in risk attitudes *per se*. Instead, we propose two potential explanations for the inconsistent behavior of half of the population: aversion to the stigma of getting caught, as well as a willingness to experiment. In order to explore whether individuals have an adverse reaction to being caught, we explored the factors that appear to affect the willingness to speed or not, such as being caught in previous periods, not speeding in previous periods, speeding and not getting caught in previous periods, and cumulative profits.[27] When we examine build up effects (e.g. being apprehended two periods in a row), we find little evidence to explain the choice of whether to speed or not. In fact, overall, the only statistically significant findings that we observe is an increase in the willingness to speed when an individual has been caught in the two previous

[26] The precise figures are 48.9%, 58.6%, and 37.9% for the 87 participants in blocks 1, 2, and 3, respectively, in setting 1, as well as 68.4, 44.7%, and 68.4% for the 38 participants in blocks 1, 2, and 3, respectively, in setting 2.

[27] We report the results of probit regression models in Appendix B.

periods and a decrease in the willingness to speed when an individual did not speed in the two previous periods. However, most of the explanatory variables that we included in this analysis do not explain individual choices to speed or not.

Alternatively, individuals may have been investigating their options within the experiment. This is not unusual in experimental work. For example, people frequently switch their choices in Charness and Levin (2005), even with identical parameters across periods, and also change their bids from period to period in Charness and Levin (2009), in an environment in which probabilities are completely transparent. Eliaz and Fréchette (2008) find that people like to diversify even when it is costly to do so and provides no benefit: they are willing to pay to switch from a lottery that pays a prize in only one particular state of nature to a lottery that pays a prize in more than one state of nature, even though the overall distribution over prizes remains the same. Rubinstein (2002) finds that if individuals must make a decision that has many options (for example, guessing the color of a card that is pulled out of a deck containing 5 different colored cards), the individuals will tend to use a "probability matching" strategy. That is to say, they will attempt to mimic the percentage of black, blue, green, etc. colored cards in the deck.[28]

5. Discussion

The theory of deterrence has implications for the prevention of proscribed activities, the level of punishment that individuals receive when committing a violation, the awards of a judge or jury (e.g. compensatory, punitive, and nominal damages), and the policies that should be instituted to prevent violations. Understanding the deterrence mechanisms that can be implemented and their expected effect on prevention of violations has very serious implications

[28] From a theoretical standpoint, Halevy and Feltkamp (2005) discuss hedging in an environment with uncertainty.

for society. Three key features of our analysis include (a) whether people understand the concept of deterrence (i.e., is the frequency of proscribed behavior sensitive to the expected cost), (b) whether it is worth implementing a higher probability regime that is costlier than a lower probability regime with the same expected cost, and c) how uncertainty affects the violation rate. The answers to these questions are important not only for the prevention of speeding on roadways, but are also relevant for much larger issues such as the prevention of tax evasion, burglary, homicide, and medical malpractice.

Empirical data that permits a careful examination of the impact of enforcement regimes on proscribed activities is very difficult to obtain. An even more difficult data-acquisition task arises when one attempts to test for the effect of uncertainty pertaining to the enforcement tool on an individual's incentive to commit a violation. Although there are many reasons why this task is so daunting, one such reason is that field data pertaining to an individual's decision to commit a violation is difficult to track and, even if collected, would be incomplete because of the overwhelming number of violations committed but not detected. Given the difficulty in obtaining accurate and complete field data, we performed laboratory tests of whether or not people understand deterrence and of the effect of uncertainty on deterrence tools. The experimental setting provides the ability to inject and extract uncertainty from the deterrence mechanisms that the participants in the experiments face, allowing us to discern the impact of uncertainty on the decisions of subjects.

When using scenarios to understand the impact of uncertainty on the effectiveness of deterrence mechanisms, Sunstein et al. (2000) draws our attention to the fact that subjects might not promote optimal deterrence. We find that individuals who are directly involved with the legal process understand and respond to deterrence mechanisms.   As noted in setting 1, the speeding rates decreased significantly when the expected cost of speeding increased.

One potential aspect of the theory of deterrence that has been theoretically discussed but has received little empirical attention is the effect of uncertainty about the magnitude of a deterrence tool, from the perspective of the potential violator. For example, people who are considering committing a crime most likely do not accurately perceive the probability of being apprehended; instead, they perceive their probability with some level of error. A similar argument can be made about the punishment that an individual will receive for committing a crime, as they might not know how a judge/jury would rule.[29] Although Bebchuk and Kaplow (1992) and Sah (1991) have discussed the effect of uncertainty on the perception of the probability in theoretical settings, the effect has not been examined empirically. Moreover, the comparison of alternative enforcement regimes – both theoretically and empirically – does not appear to be discussed in the law and economics literature.

In order to examine the effect of uncertainty on the individual's decision of whether or not to violate a rule, the first 10 periods in both settings offered two equally likely enforcement regimes with identical expected costs. The uncertainty about which regime would actually be in place led to a compound lottery. The participants in the experiment understood that each regime was equally likely and, *ex ante*, had to decide whether or not to speed. However, in the last 20 periods of setting 1 (and the last 10 periods of setting 2), the individuals first voted on a regime and were then told which regime was in place. The removal of uncertainty allows us to both understand the effect of uncertainty as well as the perception of the relative expected costs. Individuals are more likely to speed in both settings 1 and 2 when this uncertainty is removed. In addition, since individuals typically (in five cases of six) speed more frequently when the

[29] In the roadway speeding environment it is true that knowledge of the fines is available to all potential violators. However, most individuals are not accurately informed about these fines. Moreover, judges often allow violators to plead a lesser charge.

probability of detection is lower (see Tables 1 and 2), this seems consistent with the theory that expected costs are perceived to be lower in a regime with a lower probability and a higher fine, even though the mathematical expected costs are the same.

To the extent that our laboratory environment reflects the field, these findings provide interesting potential policy implications. First, it appears that more uncertainty leads individuals to violate less than they would otherwise, so that an enforcement regime is likely to be more effective when uncertainty is present.[30] Second, we find even stronger evidence, which is consistent with the law-and-economics literature that states that a policy-maker should prefer the low probability/low fine. That is, given our finding that individuals speed approximately the same amount under either regime and since increases in the probability can only be accomplished by increases in enforcement efforts, a low probability/high fine should yield approximately the same level of deterrence at a lower cost.

If we can extend alternative regimes in this paper to situations when damages can be assessed, we can say more about the amount of punitive damages. In particular, we can comment on the levels of punitive damages that should be assessed when an individual violates. To start, we find (limited) evidence that the expected cost is perceived to be larger when there are lower fines and higher probabilities relative to lower probabilities and higher fines, despite the fact that the expected costs are identical. Therefore, if an individual commits a violation in the high probability/low fine environment, he or she values the violation at a level that is greater than the perceived expected cost. If the purpose of punitive damages is to deter the individual from committing the crime in the future, then imposing a positive level of damages is justified in this environment.

[30] For example, law enforcement agencies can implement uncertainty over the probability of being caught by setting up enforcement "blitzes" that randomly change. Eeckhout et al. (2010) discuss random crackdowns by law enforcement and determine that this could be part of an optimal policing strategy, specifically for speeding.

Similarly, in a low probability/high fine environment, an individual that commits a violation might perceive the expected cost as being smaller than the actual expected cost.[31] In this instance, it would seem less reasonable to assess lower punitive damages, if any at all. This line of reasoning does have a particular intuitive appeal. Individuals that are aware that they will be inspected frequently but still commit a violation are more negligent than an individual that is inspected infrequently. Thus, we should assess higher punitive damages to those individuals that are more negligent.

On a final policy note, the voting on regimes in this experiment provides evidence about the behavior of drivers when they vote in favor of/against a regime. That is to say, when individuals vote for a regime, they tend to speed considerably more frequently in that regime than when they vote against the regime that is instituted. Therefore, it would seem that implementing the regime that is less desired by the potential violators will lead to a reduction in the number of violators.

Our study is subject to a number of limitations; we mention three of these. First, there is no actual crime being committed in the laboratory. Second, our simple environment does not take into account the externalities present when another person decides to speed. Third, we do not permit heterogeneity with respect to the costs and benefits of speeding. While it does not seem possible to observe criminal behavior in the laboratory, the second and third points are suitable topics for future investigation. It also seems valuable to extend this research to examine different types of enforcement concerns, such as profiling in law enforcement. In any case, we feel that our study represents an important step in the area of deterrence and that future research

[31] As noted in Sunstein et al (2000), a low probability of apprehension could suggest stealthiness on the part of the perpetrator. This claim is not refuted in our analysis; however, it should be noted that a lower probability of apprehension could appear to encourage stealthiness because the expected cost is perceived to be lower (relative to a higher probability/lower fine combination that has equivalent expected costs).

can build upon our results.

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Appendix A – Sample Experimental Instructions

Experiment Instructions – Setting 1

General Rules: No talking. No use of cell phones. No looking at neighbor's screens.

You are about to participate in an experiment on decision-making carried out by a researcher from UCSB. During this session, you can earn money. The amount of your earnings depends on your decisions and on the decisions of the other participants in this session. The session consists of 30 rounds. Your earnings will be determined by randomly choosing one payout from rounds 1-10, 11-20, and 21-30. The payout mechanism is explained in detail below. Your earnings will be paid to you in cash in private.

Your decisions are anonymous and confidential.

The experiment is split into 2 separate sections. The first section consists of 10 rounds and the second consists of 20 rounds. Instructions for the first 10 rounds are provided below. Instructions for the last 20 rounds will be given upon completion of the first 10 rounds.

Rounds 1-10: You are attempting to travel to the same destination 10 separate times - much like a commuter travels to work every day - and will receive a payout for each separate trip. You will have the option to speed or not when traveling. There will be two potential enforcement regimes - which you will be told before deciding whether to speed or not - and the probability that either regime will be instituted is equally likely. An enforcement regime is both a probability of getting caught when speeding and a corresponding fine, denoted f, which will be imposed if you are caught speeding. An individual that speeds and arrives at the destination without being apprehended will receive a payout of $1.00 per round. An individual that speeds and is apprehended for speeding receives a payout of $1.00-$f$ per round, where f is the fine that is imposed by the enforcement regime that is instituted. Note that $0<f<1$ always. Lastly, an individual that obeys the law and does not speed receives a payout of $0.60 per round.

There are two potential enforcement regimes:

Regime A: Chance of getting caught = 1/3, Fine = $0.90

Regime B: Chance of getting caught = 2/3. Fine = $0.45

Regime A and Regime B are equally likely - i.e. there is a 50% chance that either regime is randomly chosen.

Examples

Situation 1: Regime A is randomly selected. There are three possible payouts:
Payout 1: Participant does not speed and receives a payout of $0.60.
Payout 2: Participant speeds, is not caught and receives a payout of $1.00.
Payout 3: Participant speeds, is caught and receives a payout of $0.10 (= $1.00-$0.90).

Situation 2: Regime B is randomly selected. There are three possible payouts:
Payout 1: Participant does not speed and receives a payout of $0.60.
Payout 2: Participant speeds, is not caught and receives a payout of $1.00.
Payout 3: Participant speeds, is caught and receives a payout of $0.55 (= $1.00-$0.45).

Payouts: The payout that you will receive for rounds 1-10 will be determined by randomly choosing one of the 10 rounds and then multiplying the payout of that round by 5. Consider the following possible payouts:

Example Payout 1: Suppose that round 5 is randomly chosen as the payout round and that you decide to not speed in round 5 and so your payout for round 5 is $0.60. Therefore, your payout for rounds 1-10 is $3.00 (=5*$0.60).

Example Payout 2: Suppose that round 3 is randomly chosen as the payout round and that you decide to speed in round 3 and were not caught and so your payout for round 3 is $1.00. Therefore, your payout for rounds 1-10 is $5.00 (=5*$1.00).

Example Payout 3: Suppose that round 7 is randomly chosen as the payout round. In round 7 you decided to speed, Regime A is instituted and you are caught speeding. Your payout for round 7 is $0.10. Therefore, your payout for rounds 1-10 is $0.50 (=5*$0.10).

Example Payout 4: Suppose that round 9 is randomly chosen as the payout round. In round 9 you decided to speed, Regime B is instituted and you are caught speeding. Your payout for round 9 is $0.55. Therefore, your payout for rounds 1-10 is $2.25 (=5*$0.55).

Total Payouts: Your total payout is determined by multiplying a randomly chosen round in each block (1-10, 11-20, 21-30) by 5 and then adding a $5.00 show up payment. The payout is explained mathematically below:

$$\textit{Total Payout} = (\textit{Payout}_{1\text{-}10} + \textit{Payout}_{11\text{-}20} + \textit{Payout}_{21\text{-}30})*5 + \$5$$

Rounds 11-20: As in the first ten rounds, you are attempting to travel to the same destination 10 separate times - much like a commuter travels to work every day - and will receive a payout for each separate trip. You will have the option to speed or not when traveling. In each of these rounds you and the other members of the experiment will now have two decisions to make. First, you will vote on an enforcement regime and the regime receiving the majority vote will be posted so that everyone is aware of the enforcement regime. After observing the winning regime, you will then be asked to decide whether you will speed or not. As in the first 10 rounds, an individual that speeds and arrives at the destination without being apprehended will receive a payout of $1.00 per round. An individual that speeds and is apprehended for speeding receives a payout of $1.00-$f$ per round, where f is the fine that is imposed by the enforcement regime that is instituted. Note that $0<f<1$ always. Lastly, an individual that obeys the law and does not speed receives a payout of $0.60 per round.

Rounds 21-30: As in rounds 11-20, in each of these rounds you and the other members of the experiment will have two decisions to make. First, you will vote on an enforcement regime and the regime receiving the majority vote will be posted so that everyone is aware of the enforcement regime. However, the regimes are different.

Regime A: Chance of getting caught = 3/5, Fine = $0.833

Regime B: Chance of getting caught = 4/5. Fine = $0.625

Whichever regime (Regime A or Regime B) that receives the most votes will be implemented and posted. After observing the winning regime, you will then be asked to decide whether you will speed or not. As in the first 10 rounds, an individual that speeds and arrives at the destination without being apprehended will receive a payout of $1.00 per round. An individual that speeds and is apprehended for speeding receives a payout of $1.00-*f* per round, where *f* is the fine that is imposed by the enforcement regime that is instituted. Note that $0<f<1$ always. Lastly, an individual that obeys the law and does not speed receives a payout of $0.60 per round.

Examples

Situation 1: Regime A is implemented because it receives the most votes. There are three possible payouts:
Payout 1: Participant does not speed and receives a payout of $0.60.
Payout 2: Participant speeds, is not caught and receives a payout of $1.00.
Payout 3: Participant speeds, is caught and receives a payout of $0.167 (= $1.00-$0.833).

Situation 2: Regime B is implemented because it receives the most votes. There are three possible payouts:
Payout 1: Participant does not speed and receives a payout of $0.60.
Payout 2: Participant speeds, is not caught and receives a payout of $1.00.
Payout 3: Participant speeds, is caught and receives a payout of $0.375 (= $1.00-$0.625).

Payouts: The payout that you will receive for rounds 21-30 will be determined by randomly choosing one of the 10 rounds and then multiplying the payout of that round by 5. Consider the following possible payouts:

Example Payout 1: Suppose that round 25 is randomly chosen as the payout round and that you decide to not speed in round 25 and so your payout for round 5 is $0.60. Therefore, your payout for rounds 21-30 is $3.00 (=5*$0.60).

Example Payout 2: Suppose that round 23 is randomly chosen as the payout round and that you decide to speed in round 3 and were not caught and so your payout for round 23 is $1.00. Therefore, your payout for rounds 21-30 is $5.00 (=5*$1.00).

Example Payout 3: Suppose that round 27 is randomly chosen as the payout round. In round 27 you decided to speed, Regime A is instituted and you are caught speeding. Your payout for round 7 is $0.167. Therefore, your payout for rounds 21-30 is $0.835 (=5*$0.167).

Example Payout 4: Suppose that round 29 is randomly chosen as the payout round. In round 29 you decided to speed, Regime B is instituted and you are caught speeding. Your payout for round 9 is $0.375. Therefore, your payout for rounds 21-30 is $1.875 (=5*$0.375).

Total Payouts: Your total payout is determined by summing your payouts for all thirty rounds, dividing this total in half, and then adding a $5.00 show up payment. The payout is mathematically explained below:

$$\textit{Total Payout} = (\textit{Payout}_{1\text{-}10} + \textit{Payout}_{11\text{-}20} + \textit{Payout}_{21\text{-}30})*5 + \$5$$

Note that the minimum total payout that you could receive is $6.84, which could be obtained by getting caught speeding in Regime A in all rounds. The maximum payout is $20.00 = ($5.00+$15.00), which could be obtained by speeding and not getting caught in all rounds.

Experiment Instructions – Setting 2

General Rules: No talking. No use of cell phones. No looking at neighbor's screens.

You are about to participate in an experiment on decision-making carried out by a researcher from the University of California at Santa Barbara.. During this session, you can earn money. The amount of your earnings depends on your decisions and on the decisions of the other participants in this session. The session consists of 30 rounds. Your earnings will be determined by randomly choosing one payout from rounds 1-10, one payout from rounds 11-20, and one payout from rounds 21-30. The payout mechanism is explained in detail below. Your earnings will be paid to you in cash in private.

Your decisions are anonymous and confidential.

The experiment is split into 3 separate sections. Each section consists of 10 rounds. Instructions for each set of 10 rounds are provided below.

Rounds 1-10: You are attempting to travel to the same destination 10 separate times - much like a commuter travels to work every day - and will receive a payout for each separate trip. You will have the option to speed or not when traveling. There will be two potential enforcement regimes - which you will be **told** before deciding whether to speed or not - and the probability that either regime will be instituted is equally likely. An enforcement regime is both a probability of getting caught when speeding and a corresponding fine, denoted *f*, which will be imposed if you are caught speeding. An individual that speeds and arrives at the destination without being apprehended will receive a payout of $1.00 per round. An individual that speeds and is apprehended for speeding receives a payout of $1.00- *f* per round, where *f* is the fine that is imposed by the enforcement regime that is instituted. Note that the fine is always positive and less than $1.00. Lastly, an individual that obeys the law and does not speed receives a payout of $0.60 per round.

Examples

There are two potential enforcement regimes:

Regime A: Chance of getting caught if speeding = 1/3, Fine = $0.90

Regime B: Chance of getting caught if speeding = 2/3, Fine = $0.45

Regime A and Regime B are equally likely - i.e. there is a 50% chance that either regime is randomly chosen.

Situation 1: Regime A is randomly selected. There are three possible payouts:
Payout 1: Participant does not speed and receives a payout of $0.60.
Payout 2: Participant speeds, is not caught and receives a payout of $1.00.
Payout 3: Participant speeds, is caught and receives a payout of $0.10 (= $1.00-$0.90).

Situation 2: Regime B is randomly selected. There are three possible payouts:
Payout 1: Participant does not speed and receives a payout of $0.60.
Payout 2: Participant speeds, is not caught and receives a payout of $1.00.
Payout 3: Participant speeds, is caught and receives a payout of $0.55 (= $1.00-$0.45).

Payouts: The payout that you will receive for rounds 1-10 will be determined by randomly choosing one of the 10 rounds and then multiplying the payout of that round by 10. Consider the following possible payouts:

Example Payout 1: Suppose that round 5 is randomly chosen as the payout round and that you had decided to not speed in round 5 and so your payout for round 5 is $0.60. Therefore, your payout for rounds 1-10 is $6.00 (=10×$0.60).

Example Payout 2: Suppose that round 3 is randomly chosen as the payout round and that you had decided to speed in round 3 and were not caught and so your payout for round 3 is $1.00. Therefore, your payout for rounds 1-10 is $10.00 (=10×$1.00).

Example Payout 3: Suppose that round 7 is randomly chosen as the payout round. In round 7 you had decided to speed, Regime A is instituted and you are caught speeding. Your payout for round 7 is $0.10. Therefore, your payout for rounds 1-10 is $1.00 (=10×$0.10).

Example Payout 4: Suppose that round 9 is randomly chosen as the payout round. In round 9 you had decided to speed, Regime B is instituted and you are caught speeding. Your payout for round 9 is $0.55. Therefore, your payout for rounds 1-10 is $5.50 (=10×$0.55).

Total Payouts: Your total payout is determined by summing your payouts for all thirty rounds, dividing this total in half, and then adding a $5.00 show up payment. The payout is mathematically explained below:

$$Total\ Payout = (Payout_{1\text{-}10} + Payout_{11\text{-}20} + Payout_{21\text{-}30})*5 + \$5$$

Note that the minimum total payout that you could receive is $6.50 =($5.00+$1.50), which could be obtained by getting caught speeding in Regime A in all rounds. The maximum payout is $20.00 = ($5.00+$15.00), which could be obtained by speeding and not getting caught in all rounds.

Rounds 11-20: As in the first ten rounds, you are attempting to travel to the same destination 10 separate times - much like a commuter travels to work every day - and will receive a payout for each separate trip. You will have the option to speed or not when traveling. The main difference between the rounds 1-10 and rounds 11-20 is that you will be told which regime is in place before deciding whether to speed or not. In other words, you will know with certainty the enforcement regime that is in place. Recall that an enforcement regime is both a probability of getting caught when speeding and a corresponding fine, denoted f, which will be imposed if you are caught speeding. An individual that speeds and arrives at the destination without being apprehended will receive a payout of $1.00 per round. An individual that speeds and is apprehended for speeding receives a payout of $1.00- f per round, where f is the fine that is imposed by the enforcement regime that is instituted. Note that $0<f<1$ always. Lastly, an individual that obeys the law and does not speed receives a payout of $0.60 per round.

Rounds 21-30: In each of the last 20 rounds you and the other members of the experiment will now have two decisions to make. First, you will vote on an enforcement regime and the regime receiving the majority vote will be posted so that everyone is aware of the enforcement regime. After observing the winning regime you will then be asked to decide whether you will speed or not. As in the first 10 rounds, an individual that speeds and arrives at the destination without being apprehended will receive a payout of $1.00 per round. An individual that speeds and is apprehended for speeding receives a payout of $1.00-$f$ per round, where f is the fine that is imposed by the enforcement regime that is instituted. Note that $0<f<1$ always. Lastly, an individual that obeys the law and does not speed receives a payout of $0.60 per round.

Situation 1: Suppose 7 out of 11 people vote in favor of Regime A and so Regime A is instituted. An individual who chooses to speed has a one in three chance of being caught speeding and paying a fine of $0.90. There are three possible payouts that a participant could receive:

Payout 1: Participant does not speed and receives a payout of $0.60.
Payout 2: Participant speeds, is not caught and receives a payout of $1.00.
Payout 3: Participant speeds, is caught and receives a payout of $0.10 (= $1.00-$0.90).

Situation 2: Suppose 6 out of 11 people vote in favor of Regime B and so Regime B is instituted. An individual who chooses to speed has a two in three chance of being caught speeding and paying a fine of $0.45. There are three possible payouts that a participant could receive:

Payout 1: Participant does not speed and receives a payout of $0.60.
Payout 2: Participant speeds, is not caught and receives a payout of $1.00.

Payout 3: Participant speeds, is caught and receives a payout of \$0.55 (= \$1.00-\$0.45).

Payouts: The payout that you will receive for rounds 21-30 will be determined by randomly choosing one of the 10 rounds and then multiplying the payout of that round by 5. Consider the following possible payouts:

Example Payout 1: Suppose that round 25 is randomly chosen as the payout round and that you had decided to not speed in round 25 and so your payout for round 5 is \$0.60. Therefore, your payout for rounds 1-10 is \$6.00 (=10×\$0.60).

Example Payout 2: Suppose that round 23 is randomly chosen as the payout round and that you had decided to speed in round 23 and were not caught and so your payout for round 3 is \$1.00. Therefore, your payout for rounds 1-10 is \$10.00 (=10×\$1.00).

Example Payout 3: Suppose that round 27 is randomly chosen as the payout round. In round 27 you had decided to speed, Regime A received the majority vote and you are caught speeding. Your payout for round 7 is \$0.10. Therefore, your payout for rounds 1-10 is \$1.00 (=10×\$0.10).

Example Payout 4: Suppose that round 29 is randomly chosen as the payout round. In round 29 you decided to speed, Regime B received the majority vote and you are caught speeding. Your payout for round 9 is \$0.55. Therefore, your payout for rounds 1-10 is \$5.50 (=10×\$0.55).

Total Payouts: Your total payout is determined by summing your payouts for all thirty rounds, dividing this total in half, and then adding a \$5.00 show up payment. The payout is mathematically explained below:

$$\textit{Total Payout} = (\textit{Payout}_{1\text{-}10} + \textit{Payout}_{11\text{-}20} + \textit{Payout}_{21\text{-}30})*5 + \$5$$

Note that the minimum total payout that you could receive is \$6.50 =(\$5.00+\$1.50), which could be obtained by getting caught speeding in Regime A in all rounds. The maximum payout is \$20.00 = (\$5.00+\$15.00), which could be obtained by speeding and not getting caught in all rounds.

Appendix B

To examine potential determinants of consistency in the choices that individuals make when determining whether or not to speed, we run probit regressions for the decision to speed on whether or not the individual was caught in previous periods, was not caught in previous periods, did not speed in previous periods, and which regime that the individual faced. We report elasticities in the table below.

Probit Regressions for Determinants of Speeding

Independent variables	Speed Current period (Block 1)	Speed Current period (Block 2)	Speed Current period (Block 3)
Caught previous period	-0.039 (0.040)	0.027 (0.039)	-0.121 (0.162)
Caught previous 2 periods	0.002 (0.009)	0.018*** (0.005)	0.024*** (0.011)
Not caught previous 2 periods	0.049 (0.186)	0.012 (0.007)	0.003 (0.005)
Did not speed previous period	-0.163*** (0.075)	0.039 (0.052)	-0.366 (0.292)
Did not speed previous 2 periods	-0.110*** (0.034)	-0.187*** (0.038)	-0.160*** (0.061)
Regime	-	0.037 (0.069)	
N	868	868	868

Standard errors are in parentheses; ***, **, and * indicate significance at $p = 0.01$, $p = 0.05$, and $p = 0.10$. All specifications report elasticities between the dependent and independent variable. *Regime* is an indicator variable that takes on a value of zero if regime 1 is in place and one if regime 2 is in place.