The Price of Expungements

Romain Espinosa, Gregory Deangelo, Bruno Deffains, Murat Mungan, Rustam Romaniuc

The Price of Expungements*

Romain Espinosa[1,2], Gregory DeAngelo[3], Bruno Deffains[4], Murat Mungan[5], and Rustam Romaniuc[6]

[1]CNRS, CREM - Université Rennes 1†
[2]Rennes School of Business, France
[3]Claremont Graduate University
[4]Université Paris 2, CRED, Institut Universitaire de France
[5]George Mason University
[6]Burgundy School of Business, Université Bourgogne Franche-Comté, CEREN EA 7477

October 2020

Abstract

Expungement mechanisms allow first-time offenders to seal their criminal record. Theory predicts that the stigma of a criminal record can hinder the reintegration of criminals for whom legal activities are less lucrative. In theory, expungements priced at the reservation level can facilitate the reintegration of criminals without making first-time crime more attractive. This paper considers a behavioral perspective and offers experimental evidence about the impact of expungements priced at different levels. To do this, we set up a laboratory experiment where subjects repeatedly face opportunities to commit crime (take money from another subject). In addition to stochastic formal sanctions – imposed by the experimenter – we introduce endogenously determined social sanctions. In our main treatments of interest, subjects who choose the wrongful action have the opportunity to expunge their record prior to the second stage, thus avoiding social sanctions as long as they do not recidivate. Overall, our experiment shows that, from a general deterrence perspective, it is better to implement expungements at very high prices. We offer an explanation for this result based on the idea that the price of expungements may signal the moral reprehensibility of the offense.

Keywords: Expungement, specific deterrence, general deterrence, recidivism, social sanctions, legal norms.

JEL codes: K14, C91, K42.

*We thank the participants in Florence (IMEBESS - 2018), Lille (Behavioral and Experimental Public Choice Workshop - 2018), Charleston (Public Choice Society - 2018), Nancy (AFED - 2018), Nice (ASFEE - 2018) for their fruitful comments on previous versions of this paper. Financial support from the *Institut Universitaire de France*, George Mason University's School of Law, and West Virginia University is gratefully acknowledged.

†Corresponding author. Email: romain.espinosa@univ-rennes1.fr

1 Introduction

The United States houses the largest prison population rate, with 655 prisoners per 100,000 citizens in 2018 (Walmley [2018]). With more than 2 million prisoners and nearly 70 million American adults with a criminal record, there has been a call across the political spectrum for reversing mass criminalization and incarceration and, most importantly, for addressing the collateral consequences of convictions and arrests.[1] From a theoretical standpoint, Becker [1968]'s seminal model suggests that lower employment options increase the amount of criminal behavior. Recent empirical research has shown considerable support for this claim. For example, recidivism is affected by the availability of jobs for re-entering offenders (Galbiati et al. [2017]; Yang [2017]; Schnepel [2018]), the length of time that a former felon spends searching for a job (Engelhardt [2010]), the experience of incarceration in the life cycle (Apel and Sweeten [2010]; Western [2002]) and the amount of education that an individual has received (Lochner and Moretti [2004]).

While the collateral consequences of a criminal record are observable in terms of labor market and recidivism outcomes, the social implications of a criminal record could be just as severe, but less observable. To start, many housing applications are denied due to criminal records, effectively resulting in geographic sorting of individuals with criminal records.[2] Such close proximity to other individuals with prior criminal tendencies could increase the likelihood that an individual engages in future criminal acts (Bayer et al. [2009]). Additionally, a prior criminal record can impact a person's ability to advance beyond a criminogenic phase, as criminal backgrounds are a determining factor in college admissions. Finally, having a known criminal conviction can lead to shunning or isolation from the community, which has been associated with addiction, depression and criminal behavior (Raphael [2010]; Blossom and Apsche [2013]). In sum, while criminal records can offer some information about an individual's tendencies, they also put significant boundaries in front of an individual who is attempting to pursue an alternative, less criminal path.

To counteract some of the negative ramifications associated with the stigma of a criminal record, policies have attempted to suppress this information. For example, in an effort to reduce barriers to employment for people with criminal records, the recent practice of "banning the box" requires employers to delay asking about an applicant's criminal record until late in the hiring process. In fact, 34 states, the District of Columbia and the federal government have all enacted similar policies. The allure of such a program is considerable, as it has the potential to eliminate the negative ramifications associated with criminal records. However, the "ban the box" policy is essentially costless for formerly incarcerated individuals.[3] As noted in Agan and Starr [2018] and Doleac and Hansen [2018], the "ban the box" policy obfuscated the signal that could have been obtained from knowing a potential job candidate's

[1] The American Bar Association's Database lists more than 45,000 potential collateral consequences associated with having a conviction (National Inventory of Collateral Consequences of Conviction, http://www.abacollateralconsequences.org/search/). See also Demleitner [1999] where a variety of social negative consequences are discussed.

[2] In fact, some forms of post-prison release (e.g. sex offenders) place strict requirements on where an individual is permitted to live, largely ensuring that they live in a population of former offenders.

[3] The criminal record is often divulged in the very last stage of the job application process, but it is believed that the resources devoted to finding a specific candidate will lead the employer to continue considering the potential employee, even if they have a criminal record. See Doleac and Hansen [2018] footnote 6 for a detailed discussion.

prior criminal record, which is what is predicted by the theory proposed in Mungan [2018]. In its place, employers appear to depend on statistical discrimination in determining the likelihood that an individual has a prior criminal record. In sum, it appears that policies such as "ban the box" do not achieve their desired purpose, as low-skilled minorities face reduced likelihood of employment in locations adopting these policies.

Another, costly, mechanism for removing one's criminal record from the public record exists through the use of expungements. Expungement refers to the legal practice of having one's criminal record sealed such that it is inaccessible to the public through a formal request to the court. Although there are many variations of this practice[4], the commonality among these instruments is that they make a person's criminal record less visible. There are at least two important benefits to expunging one's record. First, their criminal record is not visible to a potential employer. Second, the individual's criminal file is not accessible by the general public, thereby enabling a person to mitigate both the pecuniary and social costs associated with having a criminal record. Thus, expungements may increase specific deterrence (i.e., reduce incentives to recidivate) because a person who has an expunged record has more to lose by committing crime (in the form of social sanctions) than a person who has a visible criminal record.

However, expungements raise a number of concerns. First, if expungements were free for first time offenders, one would expect them to reduce general deterrence, since they reduce the expected costs associated with committing a first offense. Thus, allowing expungements is likely to generate a trade-off between specific and general deterrence. From a theoretical perspective, this trade-off vanishes if one can charge a person a price for expungements that equals his reservation price (see Mungan [2017]). There is no general deterrence effect, because the person is indifferent between not expunging his record and suffering social sanctions and expunging at a cost that equals the expected social sanction associated with having a criminal record. But, the specific deterrence effect is still present, because a person with an expunged record still has more to lose than a person with a visible record, and thus is less likely to commit crime again. However, research in behavioral economics has shown that the mere fact of introducing a price incentive for morally reprehensible acts may decrease the frequency of desired behaviors instead of increasing it (see Gneezy and Rustichini [2000a] for an experimental analysis of the signal that a price may convey about the social appropriateness of a behavior). Mungan [2017] provides theoretical support for the proposition that expungements can decrease specific deterrence without increasing general deterrence if priced appropriately. In this paper, we empirically identify the effect of expungement on criminal behavior and show how this effect varies with expungement cost.

Empirical investigations of the impact of expungement on criminal recidivism are complicated by numerous factors, which is likely the reason that empirical investigations using observational data do not exist (see Prescott and Starr [2019]). To start, wealthier people are often more likely to utilize expungements, because in many cases there are significant costs and barriers to obtaining them. Since criminal conduct and income are likely related through observable and unobservable channels, it would be difficult if not impossible to disentangle the impact of income versus expungements on the likelihood

[4] Examples include expungements for judicial diversion, dismissal or not guilty verdict, and expungements of non-violent offenses.

an individual recidivates. Also, the discretion of the court might permit expungements for certain crimes, but not other crimes. For example, drunk driving might be an expungeable offense, whereas distribution of drugs might not be. Once again, this could result in the discretion of the court not permitting expungement for crimes that have a higher likelihood of being associated with minorities.[5]

In scenarios where we cannot identify the causal effect of a particular policy on criminal outcomes using observational data, it is natural to turn to laboratory experiments. Indeed there is precedent for examining criminal behavior and legal instruments using lab experiments. For example, Anderson et al. [2017] note that a decreasing penalty structure could optimally deter criminal activity, but that increasing penalty structures are ubiquitous in the criminal code. As such, they explore declining versus escalating penalty structures in the lab. Although the examination of law enforcement on criminal activities has been studied using observational data (e.g. Levitt 1999), identifying causal estimates are difficult (see DeAngelo and Hansen [2014] for example). So, to examine the impact of law enforcement strategies on deterrence, numerous experimental studies have been utilized (Schildberg-Hoerisch and Strassmair [2012]; DeAngelo and Charness [2012]; Friesen [2012]). Lastly, and most closely related to the current work, Pager [2007] experimentally demonstrates that criminal records "mark" their owners with a negative job credential, which renders illegal behaviors more appealing for individuals with a criminal record compared to those with no criminal record.[6]

In this article we use a laboratory experiment to study the effects of expungements on general and specific deterrence. We compare the criminal behavior in a treatment without expungement with treatments with expungment options priced at different levels (low, medium, high). Our analysis generates multiple conclusions. First, offenders are less likely to expunge when expungement price is high. Second, the proportion of crimes in the first stage is lowest in the treatment where expungement costs are highest. Third, the overall proportion of offenses in the second stage is not statistically different between our treatments, although the few that commit crime in the first stage in the overpriced treatment have a higher probability of recidivating in the second round.

Overall, our experiment shows that, from a general deterrence perspective, it is better to implement expungements at high prices. We present a potential explanation for this result. We argue that the price to expunge one's criminal record may be interpreted by some subjects as a proxy of the moral reprehensibility of the offense. In this case, subjects who have strong social preferences may find the idea of committing an offense less attractive as the price of the expungement increases. However, our results suggest that this relationship is non monotonic since we find no difference in crime rates between the treatment with a moderate price and the treatment with a low price for expungement, which is in line with Gneezy and Rustichini [2000b].

[5] Similar selection effects may play out at the aggregate level. Indeed, states that allow expungements may be different from states where expungements are not allowed (e.g. the social sanctions may be higher in the latter states than in the former).

[6] Earlier experimental studies include Schwartz and Skolnick [1962] in which the researchers prepared four sets of resumes to be sent to prospective employers, varying the criminal record of applicants. In each condition, employers were less likely to consider applicants who had any prior contact with the criminal justice system. Several later studies have verified these findings, varying the types of crimes committed by the hypothetical applicant (Finn and Fontaine [1985]; Cohen and Nisbett [1997]) or the national context (Boshier and Johnson [1997]; Boshier and Johnson [1971]). Each of these studies find that contact with the criminal justice system leads to worse employment opportunities.

2 Experimental Design

We propose an experiment that allows us to capture the impact of expungement on criminality. We introduce three treatment variations with respect to the expungement price (underpriced, moderately priced, and overpriced) and a control condition without an expungement option. The experiment is summarized in Figure 1.

Expungement Game: The *Expungement Game* is composed of two main stages. In the two stages, each subject is randomly matched with another participant[7] from whom he can take a fraction of that subject's initial endowment.[8] If a participant decides to do so, he obtains a proportion X of the other participant's endowment M.

Each stage is preceded by a real effort task where subjects need to put in effort to obtain a given amount of money.[9] The real-effort task is meant to generate an endowment effect among participants and designed such that all participants could finish it before the end of the timer. In our experiment, all participants completed the task and received a fixed amount of $M = 1250$ prior to playing each stage. After each stage, participants received an amount of $R = 450$ that went directly to their private account and could not be stolen.

After all participants completed the first real-effort task – that is, prior to the first stage – three participants in the room were randomly selected to play as *social controllers*.[10] These participants were excluded from the rest of the game, and only played the real-effort tasks in order to earn as much as the other participants.[11] Social controllers were asked to decide on the level of social sanctions that will be imposed on individuals who will have a criminal record during the game (the record could be the result of stealing in the first or in the second stages of the game). They could decide either not to sanction thefts, or to impose a sanction $c = 200$. If at least one out of the three social controllers decided to impose a social sanction, then the other participants incurred a sanction $c = 200$ at the end of the first and second second stages if they had a criminal record.

In the two main stages of the game, all participants had to declare whether they would steal for two kinds of crime opportunities $X \in \{X_L = 10\%, X_H = 36\%\}$.[12] Participants were told that only one of the two options would be selected by the computer at the end of the experiment to be implemented for each stage.[13]

Participants who decided to steal, for at least one of the two crime opportunities, had a probability

[7]For the second stage, each subject is matched with someone different from the first stage.

[8]Note that the matching procedure is akin to a circle, meaning that one subject can take a fraction of another's endowment, who in turn can take from someone else. All choices are made simultaneously. This way, we avoid reciprocity concerns.

[9]The task was first developed and used by Levitt [2012] and consists of a single screen displaying a number of sliders. This screen does not vary across experimental participants or across repetitions of the task. When the screen containing the effort task is first displayed to the participant all of the sliders are positioned in a random manner at a number different than 50. By using the mouse, the participant can position each slider at any integer location between 0 and 100 inclusive. The participant's points score in the task, interpreted as effort exerted, is the number of sliders positioned at 50 at the end of the allotted time. Participants had to position 30 sliders at 50 in order to obtain 1250 ECUs. They had five minutes to complete the tasks. Every subject, in our experiment, completed the tasks within five minutes.

[10]In the experiment, we used neutral language. For example, the social controllers were called *role C players*.

[11]We did this in order to ensure that negative emotions, such as envy, will not affect their decisions.

[12]We used a neutral wording in the instructions of the game. Participants had the possibility to "take" from the associated player. See instructions in the appendix.

[13]The probability of drawing a high or low crime opportunity depends on the subject's role, as we explain below.

$p = 0.5$ of being detected. If detected, the computer imposed a formal sanction $s = 200$. In addition, at the end of the stage, participants with a criminal record received the social sanction decided by the social controllers. The only difference between the first decision to steal and the second (corresponding to the first and the second stages described above) is that at the second decision, participants were asked to choose whether they want to steal conditional on having been detected after the first decision to steal. Because we used the strategy method (Selten [1967]), participants who decided to steal at least once at the first decision (for one of the crime opportunities) had to make four choices at the second decision, i.e., for high vs. low crime opportunity, and having previously been detected or not.

Our treatment manipulation consists in offering the possibility to expunge one's criminal record after the first stage. We vary the price of the expungement option in a between-subjects design. To expunge one's criminal record after the first stage, participants were proposed to pay an amount K. If they agreed to do so, their record was sealed, and although they could still incur the formal sanction in case of detection, the social sanction did not apply to them.

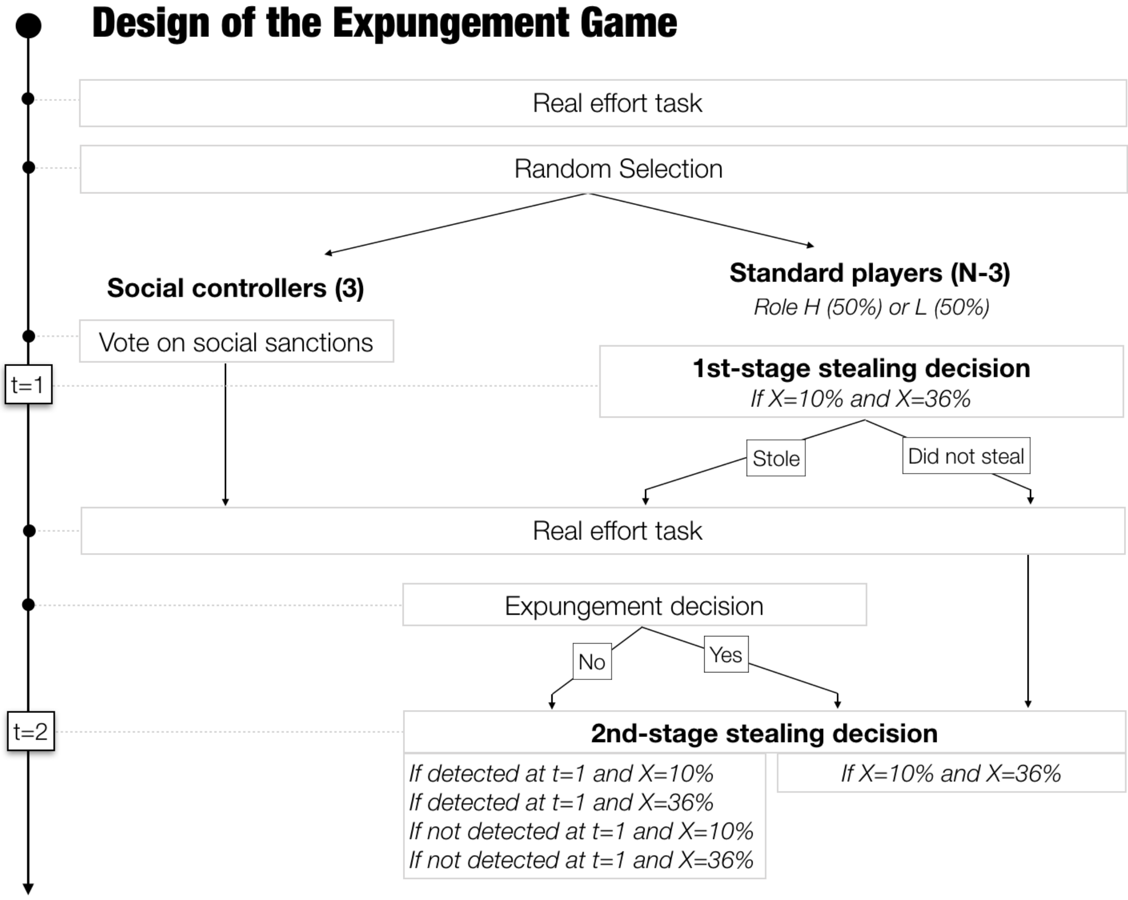
At the beginning of the experiment, participants were randomly sorted into two types. Players of type L are characterized by low propensities of crime, with a probability of $q_L = 0.5$ of drawing $X = 10\%$, while players of type H are players with high criminal opportunities. Their probability of drawing $X = 10\%$ at the first and second stages is $q_H = 0.2$. Under this specification, our model yields clear theoretical predictions that we discuss in the next section.

The participants' revenue was determined as follows. The social controllers received a fixed amount of money equal to the sum of revenues of the two real effort tasks (i.e. 2.500 ECU in total). The standard players also received this amount (2.500 ECU) but were affected by the stealing decisions. Whenever a participant decided to steal and this decision was selected by the computer to be implemented (strategy method), his revenue increased by $X \times 1250$ (where X was stochastically determined ex-post based on the player's role L or H). The other player lost this amount of money. If he was not detected, his revenue remained unchanged. In case of detection, he lost 200 ECU from the centralized sanction system (s=200) each time he was detected taking money from another participant. Participants also incurred a 200 ECU loss per stage if their record was not-empty at the end of each stage.

Treatment variations: Our protocol includes four treatment variations with three different price levels for expungement and a control condition without the possibility to expunge one's record. Our expungement prices are set as follows: one in which only type L individuals are incentivized to expunge their record (i.e., $K = 135$, Moderately priced condition), one in which none are incentivized to expunge their record (i.e., $K = 235$, Overpriced condition), and one in which both types of players are incentivized to expunge their record (i.e., $K = 35$, Underpriced). The treatment without the possibility to expunge one's record is called No Expungement.

Experimental procedures: The experiment consists of 12 sessions conducted by the same experimenter between January 2018 and January 2019 at the Laboratory for Experimental Anthropology,

Figure 1: Summary of the experiment



Catholic University of Lille, France.[14] A total of 246 subjects participated in our experiment. Subjects interacted through individual computer terminals using the oTree software (Chen et al. [2016]). The terminals were separated by lateral partitions to ensure complete anonymity. The exchange rate was 150 ECUs = 1 €. Subjects earned an average of 27 €, and payments were made privately at the end of the session. Sessions lasted for two hours, including the reading of the instructions and distribution of payments.

We devoted three sessions to each of the three treatments presented above and three other sessions to the control condition without the possibility to expunge one's record.

Control Games: Before playing the Expungement Game, participants were invited to play three games. First, we elicited risk aversion using a Holt and Laury [2002]'s lottery game. Second, we elicited guilt and envy parameters using a modified ultimatum game and a dictator game as proposed by Blanco et al. [2011].

3 Predictions

We now derive the game theoretical predictions of the above design. We consider the optimal strategies for the two types of players, assuming that the rationality, risk-neutrality and selfishness of all players is common knowledge. We proceed by backward induction to determine Nash equilibria. Note that the predictions below are robust to non-extreme risk-aversion levels.

The experimental design is made of two main stages (the first and second stealing decisions) and the expungement decision that occurs between stage one and two.

3.1 Decision to steal at the second stage

In the above experimental design, individuals who have a clean criminal record (because they did not steal at the first stage, stole but were not caught, or because they expunged their record), are incentivized to steal at the last stage if the benefits of stealing outweigh the benefits of not stealing. Let X_2 denote the opportunity of crime for an individual at the second stage. The condition for stealing is:

$$\begin{aligned} MX_2 + p(R-c-s) + (1-p)R &> R \\ X_2 &> p\frac{c+s}{M} \end{aligned} \tag{1}$$

Similarly, individuals who have a criminal record will steal if and only if:

$$\begin{aligned} MX_2 + p(R-c-s) + (1-p)(R-c) &> R-c \\ X_2 &> p\frac{s}{M} \end{aligned} \tag{2}$$

[14] Subjects were invited via the ORSEE software (Greiner [2015]), from a pool of more than 3,000 volunteers.

Given the above calibration, individuals who have a high opportunity of crime at the second stage (i.e., $X_2 = X_H$) will steal ($X_2 > p\frac{c+s}{M}$), while those who have a low opportunity of crime (i.e., $X_2 = X_L$) will steal if they have a criminal record ($X_2 > p\frac{s}{M}$) but will not steal if they have a clean record ($X_2 < p\frac{c+s}{M}$).

3.2 Decision to expunge

Proceeding backward, we now analyze the decision to expunge for those who have a criminal record. Individuals are incentivized to expunge their record if the benefits of expunging outweigh the benefits of not expunging. With probability q_i, a type i individual will obtain $X_2 = X_L$ at the second stage. In this case, s/he will steal only if s/he does not expunge. With probability $1 - q_i$, s/he will draw $X_2 = X_H$, and will therefore be better off by stealing at the second stage, regardless of the expungement decision. The condition for expungement can be written as:

$$\begin{aligned} q_i R + &(1-q_i)[MX_H + p(R-c-s) + (1-p)R] - K \geq \\ &q_i[MX_L + p(R-c-s) + (1-p)(R-c)] + (1-q_i)[MX_H + p(R-c-s) + (1-p)(R-c)], \end{aligned} \tag{3}$$

which simplifies to:

$$q_i(c + ps - MX_L) + (1-q_i)(1-p)c \geq K. \tag{4}$$

Given the above calibration, in the Moderately priced condition ($K = 135$), individuals of type L who have a criminal record after the first stage are incentivized to expunge their criminal record, while individuals of type H are not. In the Underpriced condition ($K = 35$), all individuals are expected to expunge their record. On the contrary, in the Overpriced condition ($K = 235$), no individual will expunge their criminal record.

3.3 Decision to steal at the first stage

We now turn to the decision to steal at the first stage. We analyze, for each type of player, whether participants maximize their monetary earnings by stealing or not. We consider two types of cases: when expungement is possible (Underpriced, Moderately priced and Overpriced) and when it is not (No Expungement).

When expungement is possible: Let us consider a type i individual. His immediate benefits of stealing equal $MX_1 + p(R - c - s) + (1 - p)R$, while his immediate benefits of not stealing are R. The future benefits depend on four factors: (i) the decision to steal at this stage, (ii) the realization of detection, (iii) the expungement decision, and (iv) the random draw of X_2. If one decides not to steal at the first stage, s/he will not need to expunge, and will steal at the second stage only if s/he gets $X_2 = X_H$.

In this case, the future benefits for that individual are $q_iR + (1 - q_i)[MX_H + p(R - c - s) + (1 - p)R]$. In case an individual i decides to steal at the first stage, his future payoff depends on the probability of detection and, if applicable, the expungement decision. If s/he refuses to expunge his record, his expected future benefits equal to $p[M\mathbb{E}(X|i) + p(R - c - s) + (1 - p)(R - c)] + (1 - p)[q_iR + (1 - q_i)(MX_H + p(R - c - s) + (1 - p)R)]$. On the other hand, if s/he decides to expunge his record, his future benefits will be $p[q_iR + (1 - q_i)(MX_2 + p(R - c - s) + (1 - p)R - K] + (1 - p)[q_iR + (1 - q_i)(MX_2 + p(R - c - s) + (1 - p)R)]$. Thus, the conditions to steal at the first stage can be written as:

$$\text{If does not expunge:} \qquad M_1X + p[Mq_iX_L - c(2 - p(1 - q_i)) - s(1 + pq_i)] \geq 0 \tag{5}$$

$$\text{If expunges:} \qquad MM_1X - p(c + s + K) \geq 0 \tag{6}$$

Given the above calibration, individuals are incentivized to steal at the first stage whenever they draw $X_1 = X_H$ in all treatment conditions. Moreover, none of them are expected to steal if they draw $X_1 = X_L$. These conditions hold irrespective of the treatment conditions (i.e., for $K = \{35, 135, 235\}$).

When expungement is not possible: Under this condition, the predictions are the same as in Overpriced, since participants are predicted not to expunge their record after the first stage. So, in No Expungement, participants are also incentivized to steal at the first stage whenever they draw $X_1 = X_H$ but not if they draw $X_1 = X_L$.

3.4 Treatment effect

Considering the optimal strategies of rational and selfish agents, we can form the following game-theoretical predictions. First, we show above that first decisions to steal are not supposed to be impacted by the treatment variations. When $X_1 = X_H$, all participants are expected to steal, while they are all expected not to steal when they draw $X_1 = X_L$.

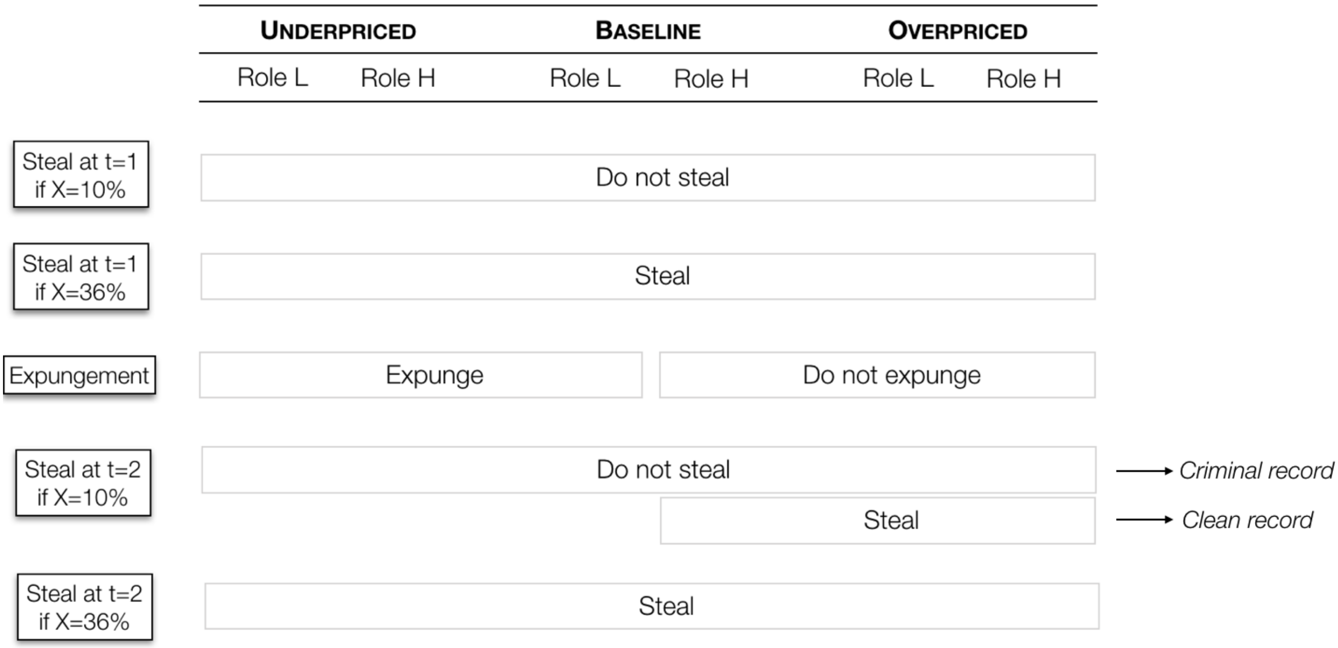
Prediction 1: Decisions to steal at the first stage are similar across treatment variations.

Second, our treatment variations mainly target the expungement decision, which is hypothesized to be facilitated with lower prices. It follows:

Prediction 2: We expect the highest expungement proportion when expungement price is low (Underpriced), and the lowest level of expungement when the price is high (Overpriced). We expect an intermediate level of expungement decisions in the Moderately priced treatment.

Regarding the second stage, the impact of treatment variations depends on the random draw X_2. When individuals face high crime opportunities, we predict that all participants are incentivized to steal. When they face low crime opportunities, decisions to steal are determined by the expungement decision. In the Underpriced condition, all participants are expected to expunge, which implies that

Figure 2: Summary of the game theoretical predictions



none should steal at the last period. In the Overpriced condition, no player is assumed to expunge. Finally, participants in the Moderately priced condition are expected to expunge if they have high probabilities of high crime opportunity only (i.e., type H individuals).

Prediction 3.1 : We expect similar levels of crime across treatments when participants face high crime opportunities at the second stage of the experiment.

Prediction 3.2 : In case of low opportunities of crime, we expect the highest crime rates for the second stage when individuals cannot expunge (No Expungement) and when they can expunge at a high price (Overpriced), and the lowest when they can expunge at a low price (Underpriced). We hypothesize an intermediate level of crime when individuals face a medium price (Moderately priced).

These predictions reflect the benefits of expungement when appropriately priced: More affordable expungement can cause a reduction in recidivism by helping criminals with small crime gains not to recommit crime, while they would do so if they had to keep incurring social sanctions (Prediction 3-2). Appropriate pricing of expungement should not affect the decision to steal at the first crime opportunity to avoid more criminals to engage in crime in the first place (Prediction 1).

4 Results

We present our results in the order in which they were elicited, starting with the decision to steal at the first stage, followed by the expungement decision and finally the decision to steal at the second stage.

4.1 Decision to Steal at the First Stage

We first start by looking at the decision to steal at the first stage. Results are graphically displayed in figure 3 with whiskers depicting one standard error, and summary statistics and univariate tests are presented in tables 1 and 2, respectively. First, we observe that about 1 out of 4 participants decide to steal when $X = 10\%$. This decision does not significantly vary across treatment variations. Univariate tests fail at rejecting the equality hypothesis for all pairwise comparisons.[15] These results also hold when controlling for guilt, envy, and risk aversion using probit estimations (see table 3).

Second, we observe significantly different decisions to steal across treatment variations when the value of crime is high. Indeed, about 2 out of 3 participants decide to steal when they can take 36% of the other participant's endowment in Underpriced and Moderately priced, and about 1 out of 2 participants decide to do so in No Expungement, while only about 1 out of 3 participants take money from another in Overpriced. The differences between the first three treatments (Underpriced, Moderately priced, and No Expungement) and Overpriced are statistically significant in univariate tests[16] and also hold when controlling for confounding factors, except for the difference between No Expungement and Overpriced which becomes insignificant (see table 3).

Exploring the determinants of the stealing decision, we observe in table 3 that the level of guilt negatively correlates with the decision to steal for both X_L and X_H. Given that participants receive the same endowment at the beginning of each phase, the decision to steal will increase the risk of generating unequal payoffs at the end of the experiment. Because participants are not paired (i.e., my potential thief is someone different from my potential victim), no reciprocity is at stake here. Participants who dislike payoff inequality in their favor (guilt) are thus less likely to steal as it generates this type of inequalities.

Result 1: (a) Expungement does not significantly affect first crime rates if priced at low and moderate levels, (b) and significantly reduces crime rates if priced at a high level, when the stakes of criminal behavior are high.

4.2 Expungement decision

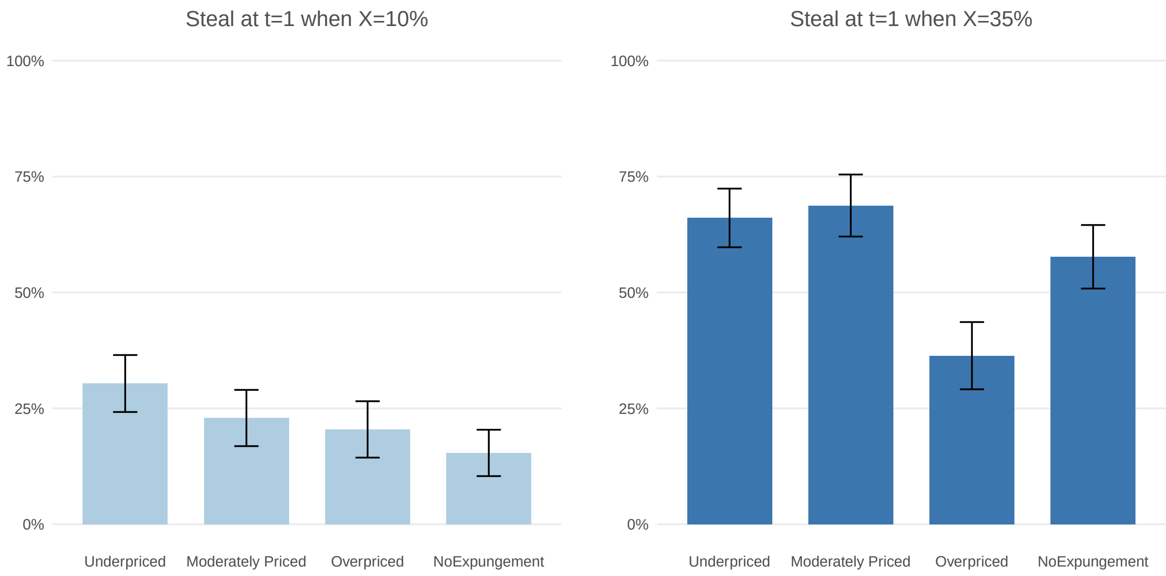
We now turn to the study of decisions to expunge one's criminal record. We compute the proportion of individuals who are willing to expunge their record, conditional on having been detected at the previous stage. Proportions across treatments are displayed in figure 4, and summary statistics are presented in table 1.

At first sight, we observe that the treatment variations greatly affect decisions to expunge. Treatments with more costly expungement are associated with lower expungement rates. In the Underpriced treatment, all individuals who decided to steal at least once at the first stage decided to expunge their

[15] Underpriced vs. Moderately priced: p=0.394; Overpriced vs. Moderately priced: p=0.775; Underpriced vs. Overpriced: p=0.262; Overpriced vs. No Expungement: p=0.517; Underpriced vs. No Expungement: p=0.065; Moderately priced vs. No Expungement: p=0.338

[16] Underpriced vs. Overpriced: p=0.002; Moderately priced vs. Overpriced: p=0.003; No Expungement vs. Overpriced: p=0.037

Figure 3: Decision to steal at the first stage (by crime opportunity and by treatment).



record in the situation where they were detected. This proportion reduces to 73% for the Moderately priced treatment, and to 11% for the Overpriced treatment. These differences are both significant in univariate[17] and multivariate[18] analyses.

In addition, we can see from table 3 that the decision to expunge also significantly correlates with the level of risk aversion. Individuals who took safer decisions in the lottery choice game are more likely to expunge their record. This result mainly comes from the strategy method used in our experiment. As participants do not know whether they were detected when they decided to steal at the first stage, risk-averse participants may have sought to use the expungement mechanism as an insurance for their future income. Expunging one's record will generate an immediate cost that will have a zero benefit if the individual had not been detected but that will prevent the decrease in income associated with the social sanction at the last period if the individual was effectively detected. This result is consistent with a rational behavior for risk averse participants.

Result 2: Lower expungement prices are associated with higher expungement rates.

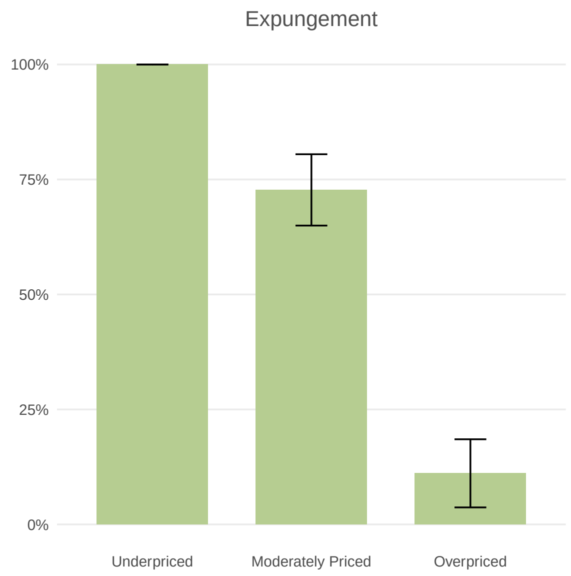
4.3 Decision to Steal at the Second Stage

We now explore the decision to steal at the second stage. We distinguish here between participants who stole at least once at the first stage (and who had to choose whether to expunge their record), and those who did not. The first type of participants had to make four decisions: whether or not to steal in case they were detected, interacted with whether they had low or high criminal opportunities ($X = 10\%$ or $X = 36\%$). On the other hand, participants who did not steal at the first stage were only asked whether

[17] Underpriced vs. Moderately priced: p<0.001; Overpriced vs. Moderately priced: p<0.001; Underpriced vs. Overpriced: p<0.001.

[18] Overpriced vs. Moderately priced: p<0.001. The dummy variable associated with Underpriced is dropped in the probit regressions because it predicts success perfectly.

Figure 4: Decision to expunge one's record (by treatment).



they wanted to steal in case they faced low or high opportunities of crime ($X = 10\%$ or $X = 36\%$).

Figure 5 displays the proportion of participants, among those who committed a crime in the previous stage, that recidivate when they have been detected. The associated statistics and univariate tests are presented in table 1, while table 3 shows the results of probit regressions. We observe clear differences across treatments. When participants have low opportunities of crime (i.e., $X = 10\%$), approximately 25% of participants decide to recidivate when the expungement price is low, moderate or when there is no expungement.[19] This proportion jumps to 67% of the participants when the expungement price is high. The difference between the low or moderate expungement price and no expungement on the one side and the high expungement price on the other side is highly significant.[20]

Furthermore, we observe similar patterns when there is high crime opportunity (i.e., $X = 36\%$). In the low and moderate expungement price treatments, about 6 out of 10 criminals decide to steal when they have been detected and they face high opportunities of crime (7 out of 10 in the 'no expungement' treatment). When the expungement price is highest, about 9 out of 10 criminals decide to steal. The 30 percentage-point difference between the treatments with low and moderate prices for expungement and the Overpriced treatment is statistically significant.[21]

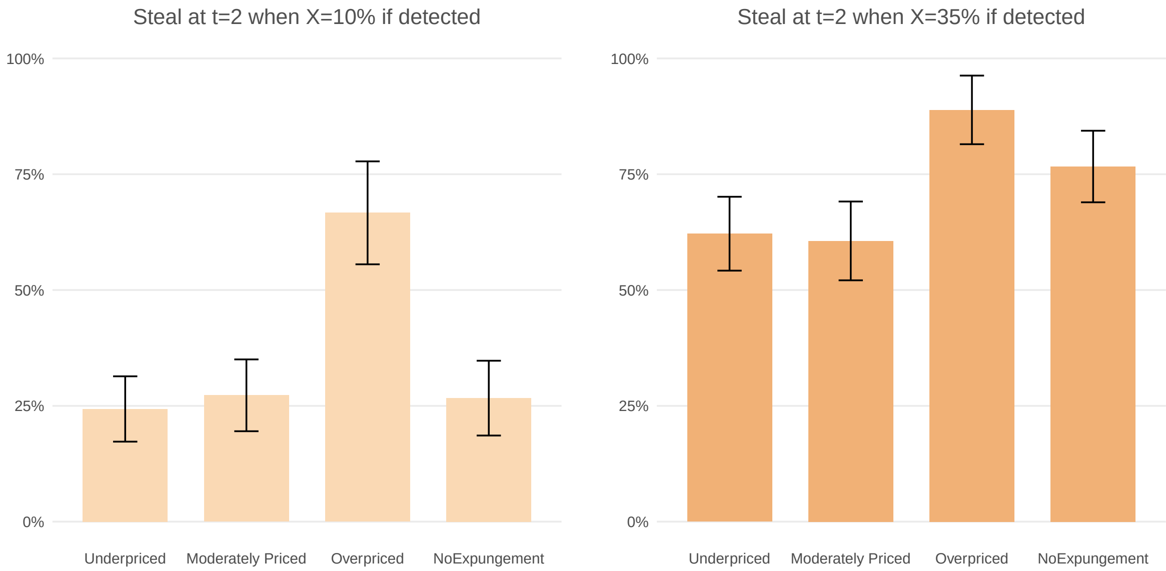
Result 3: (a) Allowing expungement is associated with similar levels of recidivism rates for detected criminals when priced at low or moderate levels, (b) but is associated with higher recidivism rates when priced at a high level.

[19] Univariate tests: $p = 0.778$ for Moderately priced vs. Underpriced; $p = 0.957$ for Moderately priced vs. No Expungement, and $p = 0.827$ for Underpriced vs. No Expungement.

[20] For univariate analysis: $p = 0.006$ for Overpriced vs. Moderately priced, $p = 0.002$ for Overpriced vs. Underpriced, $p = 0.007$ for Overpriced vs. No Expungement. For equality of marginal effects in the probit regression: $p = 0.005$ for Overpriced vs. Moderately priced, $p = 0.007$ for Overpriced vs. Underpriced, and $p = 0.002$ for Overpriced vs. No Expungement

[21] For univariate analysis: $p = 0.034$ for Overpriced vs. Moderately priced, $p = 0.041$ for Overpriced vs. Underpriced. For equality of marginal effects in the probit regression: $p = 0.001$ for Overpriced vs. Moderately priced, $p = 0.005$ for Overpriced vs. Underpriced.

Figure 5: Decision to recidivate conditional on having been detected (by crime opportunity and by treatment).



Given that participants were asked to make decisions both in the case that they were detected and in case that they were not (strategy method), we now investigate decisions to recidivate when they evaded public detection. Figure 6 shows the proportion of criminals who decided to recidivate when they were not previously detected. We observe similar patterns as before. When subjects face low opportunities of crime, a high price for expungements is associated with a higher probability to steal a second time compared to treatments with lower prices for expungement.[22] Similarly, we observe the same differences when criminal opportunities are larger (i.e., $X = 36\%$), with the highest level of recidivism in the Overpriced treatment.[23] We can also see a small increase in the Moderately priced treatment relative to the Underpriced treatment, but the difference is far from statistically significant.[24]

Result 4: (a) Allowing expungement is associated with similar levels of recidivism rates for non-detected criminals when priced at low or moderate levels, (b) but is associated with higher recidivism rates when priced at a high level (mostly for low values of crime).

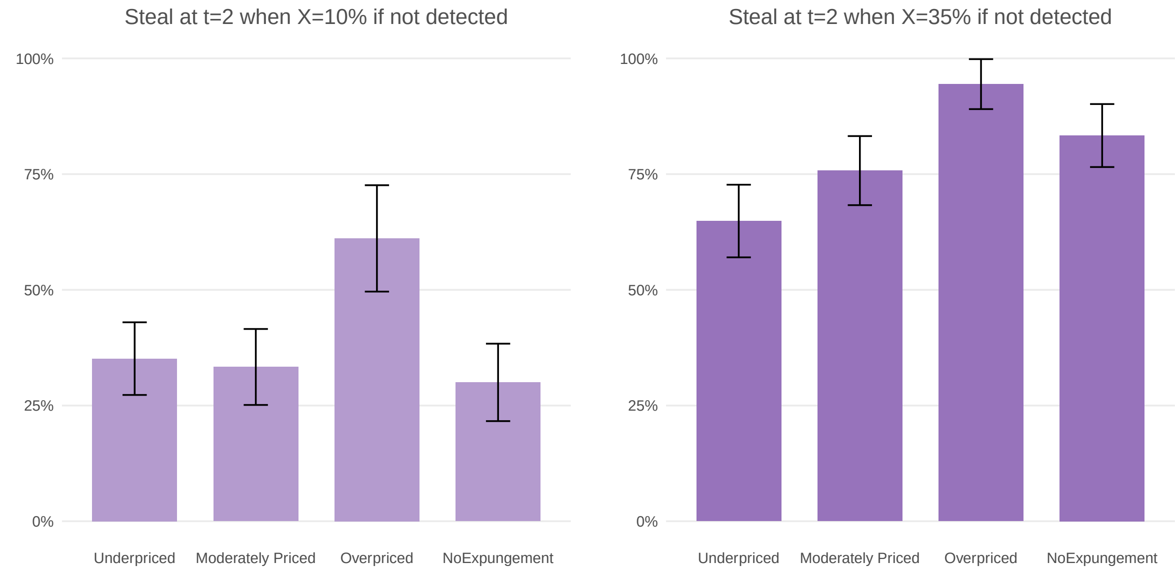
Finally, we consider the decisions of individuals who did not steal at the first stage. Their decision to steal at the second stage is relatively high. Table 1 shows that from 0% to 13% of these participants decide to steal when they have low opportunities of crime in the second stage. Differences across treatments

[22]For univariate analysis: $p = 0.056$ for Overpriced vs. Moderately priced, $p = 0.068$ for Overpriced vs. Underpriced. For equality of marginal effects in the probit regression: $p = 0.052$ for Overpriced vs. Moderately priced, $p = 0.064$ for Overpriced vs. Underpriced.

[23]For univariate analysis: $p = 0.094$ for Overpriced vs. Moderately priced, $p = 0.018$ for Overpriced vs. Underpriced. For equality of marginal effects in the probit regression: $p = 0.162$ for Overpriced vs. Moderately priced, $p = 0.068$ for Overpriced vs. Underpriced.

[24]For univariate analysis: $p = 0.3208$ for Underpriced vs. Moderately priced. For equality of marginal effects in the probit regression: $p = 0.4661$ for Underpriced vs. Moderately priced.

Figure 6: Decision to recidivate conditional on not having been detected (by crime opportunity and by treatment).



are not statistically significant.[25] Moreover, we further observe relatively few thefts when there is a high crime opportunity. The proportion of participants who did not steal at the first stage but who decided to steal at the second when $X = 36\%$ ranges from 6.7% to 15.8%. Here again, differences across treatments are not statistically significant.[26]

Result 5 (a) Allowing expungement is associated with similar levels of second crime rates for non-criminals when priced at low or moderate levels, (b) but is associated with lower crime rates when priced at a high level (mostly for high values of crime).

4.4 Selection Effect vs. Causal Effect

So far the results show that expungement prices significantly affect first offense rates and recidivism rates. The fact that treatment variations affect the proportion of offenders at the first stage implies that the population at the subsequent stages is not directly comparable across treatments. The observed differences in the second stage could indeed result from a selection effect, i.e., the composition of the sample of individuals who can reoffend, and not only from a causal effect of expungement on the thieves.

To illustrate this, consider the decision to commit a crime at the first stage. A participant will steal

[25] For univariate tests: p=0.101 for Underpriced vs. Moderately priced; $p = 0.558$ for Overpriced vs. Moderately priced, $p = 0.216$ for Underpriced vs. Overpriced, $p = 0.654$ for Overpriced vs. No Expungement, $p = 0.347$ for Underpriced vs. No Expungement, and $p = 0.336$ for Moderately priced vs. No Expungement. For equality of marginal effects in the probit regression: $p = 0.372$ for Overpriced vs. Moderately priced, $p = 0.695$ for Overpriced vs No Expungement and $p = 0.696$ for Moderately priced vs. No Expungement. The dummy variable associated with Underpriced is dropped in the probit regression because it predicts failure perfectly.

[26] For univariate tests: p=0.412 for Underpriced vs. Moderately priced; p=0.613 for Overpriced vs. Moderately priced; and p=0.679 for Underpriced, p=0.085 for Overpriced vs. No Expungement, p=0.233 for Underpriced vs. No Expungement, and p=0.068 for Moderately priced vs. No Expungement. For equality of marginal effects in the probit regression: p=0.655 for Underpriced vs. Moderately priced; p=0.408 for Overpriced vs. Moderately priced; and p=0.150 for Underpriced; p=0.010 for Overpriced vs. No Expungement and p= 0.200 for Moderately priced vs. No Expungement

if the associated benefits (B) outweigh the associated costs (C). The costs are expected to increase with the price of expungement (K). Assume now that participants are heterogenous regarding observable and unobservable parameters that make them more or less likely to commit crime. Let us note δ_i the individual propensity to steal. It is straightforward to see that a participant will steal at least once at the first stage if and only if : $B + \delta_i > C(K)$. We can define the values δ_U, δ_M and δ_O as the individuals who are indifferent between stealing and not stealing in each treatment. Given that the costs of stealing are increasing with the expungement price, it follows that $\delta_U < \delta_M < \delta_O$. In other words, only the most crime prone participants will steal at the first stage in the Overpriced treatment, while even low crime propensity participants will steal at the first stage in the Underpriced treatment. If we want to assess the impact of expungement *ceteris paribus*, i.e., for a constant composition of criminals on the unobservables, we need to integrate this selection stage in our estimations.

To account for the selection effect induced by the variations in the anticipation of the expungement prices, we propose a Heckman-like two-step estimation. The first step consists in estimating the probability of stealing at the first period (at least once: whether X=10% or X=36%). The second step re-estimates the probability of the subsequent actions, but includes the inverse Mills ratio to capture for the selection effect described above. For simplicity, we consider a linear probability model for the second stage.[27] To ensure the full specification of the model, we use the role assigned to each participant as an exclusion variable. The assigned role (i.e., H or L) affects the probability that a participant effectively faces a high or low opportunity of crime. At the second stage, the role of the participant does not matter, since the strategy method ensures that choices are made in a non-stochastic manner. At the first decision to steal, however, participants anticipate that their future opportunity of crime is stochastic, and their first decision to steal can have an impact on their subsequent payoffs in case they get detected. In other words, the role assigned to a participant affects his/her decision to steal at the first stage, but not at the second. An empirical validation can be found in the previous probit regressions, where this variable was included in all specifications and whose impact was only significant for the decision to steal at the first opportunity of crime (see table 3).

Results of the Heckman specification are presented in table 4. First, results of the first step confirm the above findings: participants are more likely to steal at least once at the first stage when expungement is priced at low and moderate levels.[28] We observe that our exclusion variable has a statistically significant impact on the first decision to steal, which is necessary for the two-step estimation. Second, we find that lower expungement prices have a causal impact on the decision to steal at the first stage beyond the selection effect.[29] Third, and more importantly, we find that result 3 holds, when we control for the selection effect: detected criminals are more likely to reoffend when expungement price is high.[30] This implies that the higher rates of recidivism of detected criminals are not solely driven by a more crime

[27]Using a linear probability model for the second stage allows us to rely on two-step Heckman estimations rather than Maximum Likelihood estimations. The main advantage is that two-step estimation yields the same estimation of the first stage, while the ML estimation yields different estimates of the first stage for each subsequent action considered in the second stage.

[28]Underpriced vs. Overpriced: p=0.007; Moderately priced vs. Overpriced: p=0.007.

[29]Underpriced vs. Moderately priced: p=0.003; Underpriced vs. Overpriced: p<0.001; Moderately priced vs. Overpriced: p<0.001.

[30]For $X = 10\%$: p=0.008 for Underpriced vs. Overpriced; p=0.019 for Moderately priced vs. Overpriced. For $X = 36\%$: p=0.010 for Underpriced vs. Overpriced; p=0.007 for Moderately priced vs. Overpriced.

prone population, but also result from a causal impact of lower expungement possibilities. Fourth, results indicate however that the previous findings showing that criminals are more likely to recidivate when they were not detected (i.e., result 4) were driven by a selection effect. Indeed, we observe no statistical difference, once accounted for the selection stage, across treatments for the decision to recidivate when individuals were not detected for their first offense.[31] Finally, we also investigate how selection of non-criminals at the first stage affects their decision to steal at the second stage. Columns (7) and (8) of table 4 shows the results of a two-step estimation, where the results for the first stage correspond to the inverse of column (1). We observe here no statistical differences across groups (confirming result 5): expungement price does not affect the decision to steal at the second stage.[32]

Result 6: Compared to allowing expungement at a high price, low and moderate expungement prices significantly (a) increase the probability of expungement, (b) increase the probability to commit a first offense, and (c) reduce the probability of recidivism when detected.

4.5 Efficiency of expungement

The above results have showed opposite results regarding the efficiency of expungement. On the one hand, expungement priced at low or moderate levels increases first-offense rates (result 1), i.e., has a welfare-decreasing general deterrence effect. On the other hand, when expungement is priced at a low or a moderate level, it decreases the probability of recidivism for detected criminals (result 3). Cheap or moderately priced expungements therefore have a welfare-increasing specific deterrence effect. Using the probability of occurrence of each event (i.e., realization of X and detection in case of crime), we are able to compute, in our setting, the total crime rate for each of the two stealing stages.

Figure 7 displays the proportion of thefts that would occur in a society calibrated on the experiment's parameters, using answers based on the strategy method. First, as the previous results showed, we observe significantly more crimes when expungement is priced at a low or moderate level.[33] Overall, we observe a 25 percentage-point difference between treatments when expungement is priced at a low or moderate level and when it is priced at a high level. In other words, cheap or moderately priced expungement induces about twice more crime at the first stage than highly priced expungement.

Second, and more interestingly, the figure further shows that there is no statistical difference across treatments for the total crime rate at the second stage. This results from the fact that recividism is higher in Overpriced treatment but the pool of criminals is also smaller in Overpriced treatment. The two effects cancel each other out, resulting in no statistical effect of the price of expungement.[34]

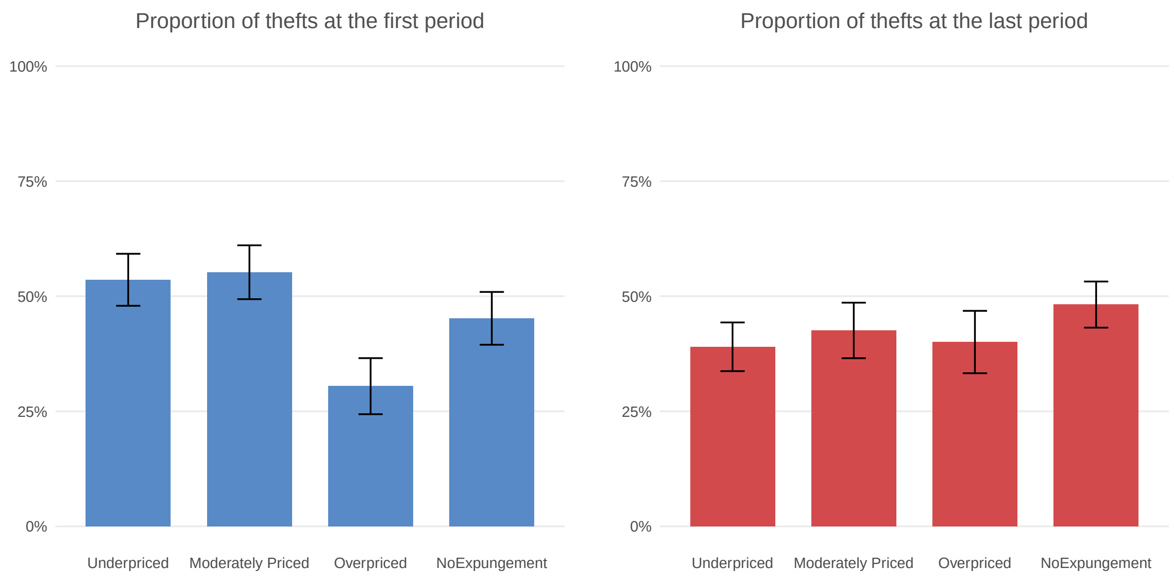
[31] When $X = 10\%$: p=0.912 for Underpriced vs. Moderately priced, p=0.157 for Underpriced vs. Overpriced, p=0.232 for Moderately priced vs. Overpriced. When $X = 36\%$: p=0.403 for Underpriced vs. Moderately priced, p=0.153 for Underpriced vs. Overpriced, p=0.411 for Moderately priced vs. Overpriced.

[32] When $X = 10\%$: p=0.238 for Underpriced vs. Moderately priced, p=0.423 for Underpriced vs. Overpriced, p=0.942 for Moderately priced vs. Overpriced. When $X = 36\%$: p=0.452 for Underpriced vs. Moderately priced, p=0.981 for Underpriced vs. Overpriced, p=0.594 for Moderately priced vs. Overpriced.

[33] Moderately priced vs. Underpriced: p=0.841; Moderately priced vs. Overpriced: p=0.004;Underpriced vs. Overpriced: p=0.007.

[34] Moderately priced vs. Underpriced: p=0.658; Moderately priced vs. Overpriced: p=0.781;Underpriced vs. Overpriced: p=0.903.

Figure 7: Efficiency analysis: overall crime rates for the two stages.



Result 7: At the aggregate level, low or moderately priced expungement increases crime rates at the first stage compared to highly priced expungement, but does not impact criminal rate at the second stage.

5 Conclusion

In this article, we studied experimentally the effects of expungements priced at different levels on overall criminal behavior in a dynamic game. Although there are many variations of this practice, the commonality among them is that they make the person's criminal records less visible, and they thereby mitigate the informal costs associated with being an ex-convict. However, behavioral research has shown that introducing a price for morally reprehensible behaviors may have undesirable consequences (see Gneezy and Rustichini [2000a]). What is more, the relationship between the price and the targeted behavior turned out to be non monotonic – strong price incentives may lead to behavioral effects that go in the opposite direction of the behavioral reactions that one observes from the introduction of weak price incentives targeted at the same behaviors (see Gneezy and Rustichini [2000b]). The question that this paper addressed is therefore important given previous results in behavioral economics.

Our experiment reveals a number of interesting behavioral reactions. Our first set of results show that increasing the price of expungements causes subjects to purchase expungements less frequently, which in turn generates a recidivism effect, and, moreover, higher expungement prices result in a clear reduction in the number of first time offenses. These findings are original in that they support the hypothesis that subjects' willingness to commit offenses are in fact affected by expungement prices, even in cases where focusing on monetary considerations alone would indicate otherwise. This suggests that expungements should be considered as an additional tool in affecting criminal behavior.

In addition to these general results, one may interpret our experiment as suggesting that high expungement prices are crime-minimizing, since the ‘overpriced’ treatment generates the lowest offense rate in the first stage, and there is no significant difference between the offense levels across treatments in the second offense stage. We believe that one must exercise caution in interpreting the results of our experiment to support specific claims of this nature. This is primarily because, out of the universe of all possible expungement prices, we have focused only on three specific prices, which we believed would generate significantly different responses by subjects –based on our computations of the behavior of expected-monetary-reward-maximizers in section 2. Thus, our experiment is naturally silent on the potential impacts of expungements made available at prices different than the three we have selected. It is of course possible for the crime minimizing expungement to be different than the three prices we have considered.

In the process of analyzing subjects’ responses, we have uncovered a relationship which existing standard theories are unable to predict but which can be accounted for by a more nuanced behavioral explanation. In particular, although standard law enforcement theories would predict increases in the expungement price to weakly and monotonically decrease the offense rate in the first stage, we found that switching from the ‘overpriced regime’ to the ‘no expungement’ regime had the opposite effect, and that this effect was statistically significant for individuals who faced high returns from the offense. We elaborate on this point to provide our best interpretation of this result.

As we have noted, in the first stage, about 15-30% of subjects steal despite facing a low return theft opportunity, and differences in these rates across treatments is statistically insignificant. Thus, there appears to be a small percentage of non-marginal offenders who are undeterred regardless of the treatment. However, there is some variation across treatments in the percentage of subjects who steal when they confront a high return opportunity. A theory that only focuses on subjects who make monetary calculations would predict this percentage to decline in the expungement price, and for it to be maximal when expungements are not available. This is because increasing the price of expungments is akin to increasing the price of an option that is exercisable only upon stealing. However, a comparison between the ‘overpriced’ treatment and the ’no-expungement’ treatment reveals that when the expungement option is removed there is a marked increase in the rate at which marginal offenders decide to steal. Before concluding, we present a potential explanation of this counter-intuitive result based on previous research in behavioral law and economics (in particular, see Gneezy and Rustichini [2000a]).

The price of an expungement provides at least two types of information to subjects. First, it provides information on the monetary cost one must incur to remove one’s offense record. Thus, an increase in the price of an expungement (weakly) increases the expected cost to committing an offense, and has the impact of (weakly) enhancing general deterrence. Second, it provides information on how much the experimenter is demanding from the offender to remove his offense record, and this may be interpreted by some subjects as a proxy of the severity or moral reprehensibility of the offense. If this type of information updating is present, subjects who have strong social preferences may find the idea of committing an offense less attractive as the price of the expungement increases.

Quite interestingly, when one moves from the ‘overpriced’ treatment to the ‘no-expungement’ treatment, one removes information regarding the moral gravity of the offense: subjects are left with their prior beliefs about how morally wrong the conduct is. Therefore, if the ‘overpriced’ treatment causes subjects to hold the belief that stealing is even more reprehensible than what they initially believed, one would expect the ’overpriced’ regime to cause a partial general deterrence effect through the second information channel relative to the ‘no expungement’ regime. Moreover, because the expungement price is very high in the ’overpriced’ regime, almost no subject is willing to purchase it, and, thus, there is no marginal deterrence effect due to changes in the information provided through the first channel. Thus, the only consequential effect of moving from the ’overpriced’ expungement regime to a ’no expungement’ regime is the removal of some information that suggests that committing the offense is more reprehensible than one’s prior beliefs. These dynamics are consistent with the behavior of the subjects: in the first stage, only 16 percent of the subjects were marginal offenders under the ‘overpriced’ regime whereas this proportion increased to around 42 percent in the ‘no expungement regime’. This difference is statistically significant, and generates a p-value of 0.013.

The above explanation would suggest an important social information function of expungements which has not yet been analyzed in the literature. If the cost of expungements is interpreted as the cost of correcting one’s mistakes, and, thus a proxy for the moral gravity of an offense, it can influence the behavior of some people who place significant value on conforming to moral norms. Thus, in circumstances where there is an under-appreciation of how wrongful an act is, one relatively harmless way of informing people would be to increase the costs of expunging one’s offense record, rather than increasing the size of the sanction to perform a similar function. However, the increase in the price of expungements needs to be substantial as suggested by the absence of any differences in criminal rates between our treatments with moderate and low prices of expungement. This result is congruent with Gneezy and Rustichini [2000b] who found a non monotonic relationship between the level of monetary compensation and effort. More research focusing on the social information function of expungement may prove to be useful.

Last but not least, we explored here a specific setting regarding the costs of social sanctions. We assumed indeed that the per period cost of having a criminal record is independent of the number of previous criminal actions. Whenever an individual has committed at least one crime for which he was detected, he had to bear a social sanction of c per period. It could be however that social sanctions do not solely depend on the fact of having a criminal record but also on the number of registered criminal acts. Changing the social sanction schemes to include the number of detected offences would alter the decision to steal at the last period, and, thus, the effect of expungement on specific deterrence.

A Tables

Table 1: Summary statistics.

	Underpriced	Mod. priced	Overpriced	No Expungement
1st theft $X = 10\%$	0.304	0.229	0.205	0.154
	(0.061)	(0.061)	(0.061)	(0.050)
1st theft $X = 36\%$	0.661	0.688	0.364	0.577
	(0.063)	(0.067)	(0.073)	(0.069)
(1st theft $X = 36\%$)-(1st theft $X = 10\%$)	0.357	0.458	0.159	0.423
	(0.065)	(0.073)	(0.072)	(0.069)
Expunge	1	0.727	0.111	.
	.	(0.078)	(0.074)	.
2nd theft $X = 10\%$ if detected	0.243	0.273	0.667	0.267
	(0.071)	(0.078)	(0.111)	(0.081)
2nd theft $X = 36\%$ if detected	0.622	0.606	0.889	0.767
	(0.080)	(0.085)	(0.074)	(0.077)
2nd theft $X = 10\%$ if not detected	0.351	0.333	0.611	0.300
	(0.078)	(0.082)	(0.115)	(0.351)
2nd theft $X = 36\%$ if not detected	0.649	0.758	0.944	0.833
	(0.078)	(0.075)	(0.054)	(0.068)
2nd theft opportunity $X = 10\%$ if didn't steal before	0	0.133	0.077	0.045
	.	(0.088)	(0.052)	(0.044)
2nd theft opportunity $X = 36\%$ if didn't steal before	0.158	0.067	0.115	0.318
	(0.084)	(0.064)	(0.063)	(0.099)

Standard errors in parentheses.

Table 2: Univariate equality tests.

	Under=Base	Over=Base	Under=Over	Under=NoExp	Base=NoExp	NoExp=Over
[1] 1st theft $X = 10\%$	p=0.394	p=0.775	p=0.262	p=0.065	p=0.338	p=0.517
[2] 1st theft $X = 36\%$	p=0.772	p=0.002	p=0.003	p=0.370	p=0.253	p=0.037
[3] (1st theft $X = 36\%$)-(1st theft $X = 10\%$)	p=0.297	p=0.006	p=0.053	p=0.485	p=0.724	p=0.013
[4] Expunge	p<0.001	p<0.001	p<0.001			
[5] 2nd theft $X = 10\%$ if detected	p=0.778	p=0.006	p=0.002	p=0.827	p=0.957	p=0.007
[6] 2nd theft $X = 36\%$ if detected	p=0.894	p=0.034	p=0.041	p=0.203	p=0.171	p=0.294
[7] 2nd theft $X = 10\%$ if not detected	p=0.874	p=0.056	p=0.068	p=0.656	p=0.777	p=0.034
[8] 2nd theft $X = 36\%$ if not detected	p=0.321	p=0.094	p=0.018	p=0.090	p=0.458	p=0.260
[9] 2nd theft opportunity $X = 10\%$ if didn't steal before	p=0.101	p=0.558	p=0.216	p=0.347	p=0.336	p=0.654
[10] 2nd theft opportunity $X = 36\%$ if didn't steal before	p=0.412	p=0.613	p=0.679	p=0.233	p=0.068	p=0.085
[11] Aggregate crime at the 1st theft opportunity	p=0.995	p=0.008	p= 0.0092	p=0.225	p=0.217	p=0.111
[12] Aggregate crime at the 2nd theft opportunity	p=0.622	p=0.806	p=0.859	p=0.256	p=0.595	p=0.540

P-values of two-sided proportions tests for [1], [2], [4], [5], [6], [7], [8], [9], and [10]. Ranksum tests for [3], [11], and [12].

Table 3: Probit Estimations: Marginal effects

Variable	1st theft opportunity		Expunge	2nd theft opportunity					
				if detected		if not detected		didn't steal before	
	$X = 10\%$	$X = 36\%$		$X = 10\%$	$X = 36\%$	$X = 10\%$	$X = 36\%$	$X = 10\%$	$X = 36\%$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
No Expungement	Reference	Reference	.	Reference	Reference	Reference	Reference	Reference	Reference
Underpriced	0.180*	0.157	omitted	0.0541	-0.0381	0.0917	-0.103	omitted	-0.0388
	(0.100)	(0.102)		(0.127)	(0.126)	(0.135)	(0.119)		(0.0670)
Mod. priced	0.122	0.205**	0.839***	0.0295	-0.103	0.0795	-0.0331	0.0327	-0.0691
	(0.103)	(0.0951)	(0.139)	(0.128)	(0.125)	(0.136)	(0.113)	(0.0837)	(0.0540)
Overpriced	0.0134	-0.178	0.328	0.481***	0.292***	0.382**	0.168	-0.0168	-0.139**
	(0.0977)	(0.120)	(0.446)	(0.153)	(0.0721)	(0.158)	(0.105)	(0.0430)	(0.0540)
Role L	-0.0877	-0.227***	0.0908	-0.0553	-0.141	-0.0170	-0.0407	-0.0297	-0.126*
	(0.0587)	(0.0795)	(0.191)	(0.0935)	(0.0932)	(0.0967)	(0.0833)	(0.0391)	(0.0668)
Safe Choices	-0.0172	-0.0281	-0.203***	0.0337	-0.0178	-0.0153	-0.00223	-0.0219	-0.0243
	(0.0205)	(0.0267)	(0.0651)	(0.0303)	(0.0281)	(0.0304)	(0.0264)	(0.0136)	(0.0196)
Guilt	-0.326***	-0.653***	-0.328	-0.203	-0.475*	-0.281	-0.264	-0.113	-0.371***
	(0.114)	(0.152)	(0.375)	(0.240)	(0.243)	(0.252)	(0.207)	(0.0709)	(0.131)
Envy	0.0212	-0.0200	0.0566	0.0242	-0.0133	0.00363	-0.0287	0.0149	0.0230
	(0.0209)	(0.0275)	(0.0750)	(0.0360)	(0.0326)	(0.0352)	(0.0297)	(0.0157)	(0.0219)
Observations	182	182	47	108	108	108	108	56	74
Mod. priced = Underpriced	p=0.517	p=0.597	.	p=0.839	p=0.570	p=0.920	p=0.507	.	p=0.655
Mod. priced = Overpriced	p=0.265	p=0.001	p<0.001	p=0.005	p=0.001	p=0.052	p=0.162	p=0.372	p=0.408
Underpriced = Overpriced	p=0.075	p=0.005	.	p=0.007	p=0.005	p=0.064	p=0.068	.	p=0.150

Robust Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Underpriced omitted in column 1 because it perfectly predicts expungement.

Underpriced omitted in column 8 because it perfectly predicts not stealing.

Table 4: Heckman Regressions

Variable	Steal at 1st theft opportunity	Expunge	2nd theft opportunity					
			if detected		if not detected		didn't steal before	
			$X = 10\%$	$X = 36\%$	$X = 10\%$	$X = 36\%$	$X = 10\%$	$X = 36\%$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Underpriced	2.591***	1.328***	0.0988	0.791***	0.457**	0.794***	-0.0807	-0.259
	(0.663)	(0.147)	(0.215)	(0.257)	(0.228)	(0.207)	(0.296)	(0.520)
Mod. priced	2.723***	1.099***	0.0977	0.697**	0.470*	0.885***	-0.00197	-0.347
	(0.698)	(0.160)	(0.234)	(0.278)	(0.249)	(0.227)	(0.291)	(0.516)
Overpriced	1.817***	0.343*	0.596**	1.342***	0.740***	1.054***	-0.00839	-0.263
	(0.662)	(0.182)	(0.267)	(0.305)	(0.285)	(0.259)	(0.233)	(0.410)
Safe Choices	-0.0601	-0.0722***	0.0122	-0.00588	-0.0326	-0.0135	-0.0137	-0.00190
	(0.0804)	(0.0237)	(0.0346)	(0.0408)	(0.0368)	(0.0335)	(0.0181)	(0.0323)
Guilt	-1.723***	-0.241	-0.0525	0.0749	-0.383	-0.347	0.0144	0.0568
	(0.458)	(0.248)	(0.365)	(0.418)	(0.389)	(0.354)	(0.163)	(0.290)
Envy	-0.0408	0.0119	0.0211	-0.00968	0.0141	-0.0166	-0.0110	0.0417
	(0.0902)	(0.0236)	(0.0344)	(0.0414)	(0.0365)	(0.0332)	(0.0183)	(0.0328)
Gender	-0.529**	0.0479	0.220*	0.209	0.167	0.0431	0.0824	0.129
	(0.264)	(0.0834)	(0.122)	(0.145)	(0.129)	(0.118)	(0.0750)	(0.131)
Role L	-0.588**							
	(0.242)							
Lambda		0.165	-0.141	-0.539	0.0321	0.0325	0.110	0.326
		(0.200)	(0.295)	(0.330)	(0.315)	(0.287)	(0.147)	(0.256)
Observations	137	81	81	81	81	81	56	56
Mod. priced = Underpriced	p=0.652	p=0.003	p=0.993	p=0.482	p=0.912	p=0.403	p=0.238	p=0.452
Mod. priced = Overpriced	p=0.003	p<0.001	p=0.019	p=0.007	p=0.232	p=0.411	p=0.942	p=0.594
Underpriced = Overpriced	p=0.007	p<0.001	p=0.008	p=0.010	p=0.157	p=0.153	p=0.423	p=0.981

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

B Instructions

Note: The instructions presented below correspond to the treatment where K = 135. Instructions associated with Part 1 are not displayed and are similar to the original papers (see Holt and Laury [2002] and Blanco, Engelmann, and Normann [2011]).

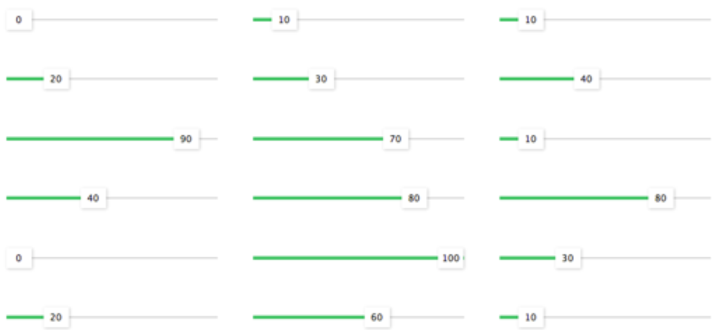
Part 2

This part contains several phases. As in part 1 of the experiment, the final outcome from each phase will be communicated to you at the very end of the experiment. The exchange rate from ECU to euros is 150 ECU = 1 euro.

Phase 1

You have 5 minutes to position 30 sliders at 50. For each correctly positioned slider the number 50 will be displayed on top of the slider. You will earn 1250 ECU by positioning the 30 sliders at 50 within 5 minutes. This will also allow you to continue your participation in this experiment.

The image below illustrates an example of 18 sliders positioned in a random manner.



In the upper-right corner of the computer screen, you will be informed about the total number of sliders that you correctly positioned. .

If you have any questions, please raise your hand, and an experimenter will answer your question privately.

(instructions for Phases 2 and 3 were distributed after Phase 1 was completed)

Phase 2

You will be randomly paired with another participant in the room. You will not be able to know the identify the other participant and s/he will not be able to know your identity. You will need to decide whether you want to take a fraction from the other participant's endowment of 1250 ECU.

You will first be informed about your role. The computer will randomly attribute you one of the following roles: role A, role B, or role C.

- If you have the role A: you will have 80% chance to be able to take 450 ECU from the other participant's endowment, and 20% chance to be able to take 125 ECU;
- If you have the role B: you will have 50% chance to be able to take 450 ECU from the other participant's endowment, and 50% chance to be able to take 125 ECU;
- If you have the role C, you will need to take a different decision, one that will affect the earnings of participants with roles A and B (see below).

For participants with roles A and B, you will need to indicate whether you want to take a fraction of the other participant's endowment:

- In case you can take 450 ECU from the other participant's endowment,
- And in case you can take 125 ECU from the other participant's endowment.

IMPORTANT: In case you decide to take a fraction of the other participant's endowment, there is a 50% chance that you will be detected by the central computer. If you are detected, the computer will automatically deduct 200 ECU from your final earnings. In case you decide to take a fraction of the other participant's endowment and you are detected, this will be added to **your personal record (that is private)**. You will be informed at the very end of the experiment if you were detected.

***

Decision of participants with role C: For this second phase of part 2, participants with roles A and B will receive an additional revenue that will be equal to either 450 ECU or 250 ECU. The decision taken by participants with role C will determine whether participants with roles A and B will receive 450 or 250 ECU. At the beginning of this phase, the computer will randomly select 3 participants who will have the role C. More specifically, participants with the role C will be asked to vote for one of the following options:

1. Send 450 ECU to all participants with roles A and B independently of their decision to take or not a fraction of another participant's endowment,
2. Or, send 450 ECU to participants with roles A and B who have not been detected or have not taken a fraction of the another participant's endowment (i.e., who have a clean personal record) and send 250 ECU to all those who have been detected.

If at least one of the participants with a role C votes for option 2, then this option will be implemented. If no one votes for option 2, then option 1 will be implemented. Thus, one vote for option 2 suffices for this option to be implemented.

For participants with roles A and B: you will be informed about the decision of role C participants before deciding whether you want to take a share of another participant's endowment. The outcome of the vote will be displayed on the computer screen of participants with roles A, B, and C before the start of the second phase – i.e. before participants with roles A and B are asked to choose whether they want to take a fraction from the endowment of another participant.

Participants with role C will be inactive after the voting stage. That is, they will not be paired with another participant and will not have the opportunity to take a fraction of another participant's endowment. Participants with role C will automatically receive an additional revenue of 450 ECU for this phase independently of their vote.

***

For participants with roles A and B who decide to take a fraction from the endowment of another participant: at the end of the second phase, you will be offered the possibility to clear your personal record by paying **135 ECU**. Since you will only be informed whether you were detected at the very end of the experiment, you will be asked to indicate whether you wish to clear your record conditional on having been detected.

Phase 3

As in phase 1, you will have 5 minutes to position 30 sliders at 50. For each correctly positioned slider the number 50 will be displayed on top of the slider. You will earn an *additional amount* of 1250 ECU by positioning the 30 sliders at 50.

***

You will be paired with another participant in the room (a different one from the phase 2 of this part). You will then be asked to indicate whether you want to take a fraction of that participant's endowment. Note that you keep the same role as in the previous phase of the experiment. As in the previous phase:

- If you have the role A: you will have 80% chance to be able to take 450 ECU from the other participant's endowment, and 20% chance to be able to take 125 ECU;
- If you have the role B: you will have 50% chance to be able to take 450 ECU from the other participant's endowment, and 50% chance to be able to take 125 ECU;
- If you have the role C, you will be inactive after the completion of the slider task.

For participants with roles A and B who previously decided not to take a fraction of another participant's endowment (either when they could take 450 ECU or when they could take 125 ECU): you will need to indicate, as before, whether you wish to take a fraction of the other participant's endowment for the following two scenarios:

- In case you can take 450 ECU from the other participant's endowment,
- And in case you can take 125 ECU from the other participant's endowment.

For participants with roles A and B who previously decided to take a fraction of another participant's endowment (either when they could take 450 ECU or 125 ECU, or in both cases): you will need to indicate whether you wish to take a fraction of the other participant's endowment for the following four scenarios:

- In case **you were detected** in the previous phase and that you can now take 450 ECU from the other participant's endowment;
- In case **you were detected** in the previous phase and that you can now take 125 ECU from the other participant's endowment;

- In case **you were not detected** in the previous phase and that you can now take 450 ECU from the other participant's endowment;
- In case **you were not detected** in the previous phase and that you can now take 125 ECU from the other participant's endowment

Note that, as before, you will be informed at the end of the experimentwhether you were able to take 450 ECU or 125 ECU from the other participant's endowment in this phase. For this phase, as for the previous one, there is a 50% chance that you will be detected in case you decide to take a fraction of the other participant's endowment. You will be informed at the end of the experimentwhether you were detected. As in the previous phase, in case you decide to take a fraction of another participant's endowment and you are detected, this will be added to your personal record (that is private).

Finally, in this phase, as in the previous one, participants with roles A and B will receive an additional revenue that will be equal to either 450 ECU or to 250 ECU. The amount that participants with roles A and B will receive depends on the outcome of participants C's vote at the beginning of this part. That is, the outcome of the vote remains the same. If at least one of the participants with role C voted to send only 250 ECU to participants with roles A and B who have been detected, then participants A and B who do not have a clean personal record (either from the previous phase or from the current one) will receive 250 ECU and those who have a clean record will receive 450 ECU.

Participants with role C will automatically receive 450 ECU for this phase independently of their vote at the beginning of this part of the experiment.

In order to verify your comprehension of these instructions, you will be asked to answer a few questions. You will only be able to proceed to the next stage of the experiment if you answer correctly all of the questions.

If you have questions, please raise your hand and an experimenter will privately answer your questions.

C Comprehension questions

The three role C players will have to vote to select one of the two following options:

- Option 1: Send 450 ECU to all players in the room.
- Option 2: Send 450 ECU to all players who decided not to take from another player's revenue (or who have not been detected doing so) and send 250 ECU to those who have been detected doing so.

Q1: If one of the three role C players vote for Option 2 and the others vote for Option 1, what Option will be implemented?

- Option 1
- Option 2

Q2: If Option 2 is implemented and that you are detected for taking from another player's revenue (this information will be revealed at the end of the experiment):

- You receive 250 ECU (instead of 450) as a result of the choice of the role C players and the computer will take another 200 ECU from your final payoff.

- You receive 250 ECU (instead of 450) as a result of the choice of the role C players and the computer will not take any money from your final payoff.

Q3: Let's assume that Option 2 is implemented and that you are detected for taking from another player's revenue (this information will be revealed at the end of the experiment). This information will be added to your record. You have the possibility to clear your record. If you decide to clear your record and that you don't take from the other participant's revenue at the last stage of the game, you will:

- Receive 450 ECU (instead of 200 ECU) at the last stage of the experiment as a result of the choice of the role C players.
- Receive 200 ECU (instead of 450 ECU) at the last stage of the experiment as a result of the choice of the role C players.

References

A. Agan and S. Starr. Ban the box, criminal records, and racial discrimination: A field experiment. *Quarterly Journal of Economics*, 133(1):191—-235, 2018.

L.R. Anderson, G.J. DeAngelo, W. Emons, B. Freeborn, and H. Lang. Penalty structures and deterrence in a two-stage model: Experimental evidence. *Economic Inquiry*, 55(4):1833–1867, 2017.

R. Apel and G. Sweeten. The Impact of Incarceration on Employment during the Transition to Adulthood. *Social Problems*, 57(3):448–479, 2010.

P. Bayer, R. Hjalmarsson, and D. Pozen. Building Criminal Capital behind Bars: Peer Effects in Juvenile Corrections. *Quarterly Journal of Economics*, 124(1):105–147, 2009.

G. Becker. Crime and punishment: An economic approach. *Journal of Political Economy*, 76(2):169–217, 1968.

Mariana Blanco, Dirk Engelmann, and Hans Theo Normann. A within-subject analysis of other-regarding preferences. *Games and Economic Behavior*, 72(2):321–338, 2011.

P. Blossom and J. Apsche. Effects of loneliness on human development. *International Journal of Behavioral Consultation and Therapy*, 7(4):28–29, 2013.

R. Boshier and D. Johnson. Deliquincy and stigmatisation. *British Journal of Criminology*, 11:185–187, 1971.

R. Boshier and D. Johnson. Does conviction affect employment opportunities. *British Journal of Criminology*, 14:264–268, 1997.

D.L. Chen, M. Shonger, and C. Wickens. oTree—An open-source platform for laboratory, online, and field experiments. *Journal of Behavioral and Experimental Finance*, 9:88–97, 2016.

D. Cohen and R.E. Nisbett. Field experiments examining the culture of honor: The role of institutions in perpetuating norms about violence. *Personality and Social Psychology Bulletin*, 23(11):1188–1199, 1997.

G. DeAngelo and G. Charness. Deterrence, expected cost, uncertainty, and voting: Experimental evidence. *Journal of Risk and Uncertainty*, 44(1):73–100, 2012.

G. DeAngelo and B Hansen. Life and death in the fast lane: Police enforcement and traffic fatalities. *American Economic Journal: Economic Policy*, 6(2):231–257, 2014.

N.V. Demleitner. Reforming juvenile sentencing. *Federal Sentencing Reporter*, 1999.

J. Doleac and B. Hansen. Does "ban the box" help or hurt low-skilled workers? Statistical discrimination and employment outcomes when criminal histories are hidden. *NBER Working Paper No. 22469*, 2018.

B. Engelhardt. The effect of employment frictions on crime. *Journal of Labor Economics*, 28(3):677–718, 2010.

R.H. Finn and P.A. Fontaine. The association between selected characteristics and perceived employability of offenders. *Criminal Justice and Behavior*, 12:353–365, 1985.

L. Friesen. Certainty of punishment versus severity of punishment: An experimental investigation . *Southern Economic Journal*, 79(2):399–421, 2012.

R. Galbiati, A. Ouss, and A. Philippe. Jobs, news, and re-offending after incarceration. *TSE working paper*, 17-843, 2017.

U. Gneezy and A. Rustichini. A fine is a price. *Journal of Legal Studies*, 29(1):1–17, 2000a.

U. Gneezy and A. Rustichini. Pay enough or don't pay at all. *Quarterly Journal of Economics*, 115(3): 791–810, 2000b.

B. Greiner. Subject pool recruitment procedures: organizing experiments with ORSEE. *Journal of the Economic Science Association*, 1(1):114–125, 2015.

Charles A Holt and Susan K Laury. Risk aversion and incentive effects. *American economic review*, 92 (5):1644–1655, 2002.

S. Levitt. A structural analysis of disappointment aversion in a real effort competition. *American Economic Review*, 102(1):469–503, 2012.

L. Lochner and E. Moretti. The Effect of Education on Crime: Evidence from Prison Inmates, Arrests, and Self-Reports. *American Economic Review*, 94(1):155–189, 2004.

M. Mungan. Reducing crime through expungements. *Journal of Economic Behavior and Organization*, 137:398–409, 2017.

M. Mungan. Statistical (and Racial) Discrimination, "Ban the Box", and Crime Rates. *American Law and Economics Review*, 20(2):512–535, 2018.

D. Pager. The mark of a criminal record. *American Journal of Sociology*, 108(5):937–975, 2007.

J.J. Prescott and S.B. Starr. Expungement of Criminal Convictions: An Empirical Study. *Harvard Law Review*, forthcoming, 2019.

S. Raphael. The Causes and Labor Market Consequences of the Steep Increase in U.S. Incarceration Rates. In *Labour in the era of globalization*, pages 375–413. 2010.

H. Schildberg-Hoerisch and C. Strassmair. An experimental test of the deterrence hypothesis. *Journal of Law, Economics, and Organization*, 28(3):447—-459, 2012.

K.T. Schnepel. Good jobs and recidivism. *Economic Journal*, 128(608):447–469, 2018.

R. Schwartz and J. Skolnick. Tho studies of legal stigma. *Social Problems*, 10:133–142, 1962.

R. Selten. Die Strategiemethode zur Erforschung des eingeschränkt rationalen Verhaltens im Rahmen eines Oligopolexperiments. In *Beiträge zur experimentellen Wirtschaftsforschung*, pages 136–168. 1967.

R. Walmley. *World Prison Population List, twelfth edition.* Institute for Criminal Policy Research, 2018.

B. Western. The Impact of Incarceration on Wage Mobility and Inequality. *American Sociological Review*, 67(4):526–546, 2002.

C. Yang. Does public assistance reduce recidivism? *American Economic Review*, 107(5):551–555, 2017.