Marginal Deterrence at Work*

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April 28, 2017

Abstract

We test the rational economic model of marginal deterrence of law enforcement — i.e., the need for graduating the penalty to the severity of the crime. We use a unique data set, which combines individual-level data on sentence length for a representative sample of US inmates with proxies for maximum punishment and monitoring costs across US states over 50 years. We show that the penalty is increasing in the level of the offense. Consistent with the marginal deterrence framework, we also document that a decrease in maximum penalty or an increase in monitoring cost are associated with longer sentences and higher monitoring rates. We also provide evidence that the effects of maximum penalty and monitoring cost are stronger in states where income inequality is higher. Finally, we show that steeper sanctions are associated with less harmful crimes. Overall, these findings favor the marginal deterrence framework over the maximal penalty principle and other competing theories of justice.

JEL Classification: K14, K40.

Keywords: Marginal Deterrence, Enforcement Policies, Individual-Level Data, Death Penalty.

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*We thank Giuseppe Dari-Mattiacci for useful comments. Pietro Panizza and Enrico Turco provided excellent research assistance. Financial support from the Einaudi Institute for Economics and Finance is gratefully acknowledged. The usual disclaimer applies.

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1. Introduction

Since the pioneering work by Becker (1968), law enforcement has attracted considerable attention by economists, and for good reasons. The development of efficient enforcement systems is inextricably linked with economic growth, financial development and the rise of modern democracies. Designing 'good' laws is, in fact, not enough to guarantee these objectives without an effective deterrence.

Several prominent scholars in law and economics have, over the past decades, debated intensively on the notion of optimal enforcement of law. In models where agents simply consider whether to commit a single harmful act or not, the threat of severe sanctions is enough to deter people from infringing the law: the *maximal punishment* principle (Becker, 1968). Yet, when agents can also choose the severity of the harm their actions inflict to society, 'hanging people for a sheep' might not be a good idea (Friedmand and Sjostrom, 1993). Notably, undeterred individuals will have a reason to commit less, rather than more harmful acts, if expected sanctions rise with harm. This tendency is sometimes said to reflect *marginal deterrence* — i.e., the need for graduating penalties to the severity of the harm.

Yet, even if the anecdotal evidence seems to corroborate the idea that penalties are sensitive to the severity of harm, it is surprising that the empirical literature on this issue is rather narrow, despite the intense theoretical debate, and that it lacks a systematic analysis of the practical relevance of marginal deterrence as opposed to competing theories. In order to fill this gap, in this paper we investigate empirically if, and to what extent, the rational economic model of marginal deterrence of law enforcement provides a reasonable description of the enforcement policies actually chosen by regulators and law enforcers.

As shortly explained before, older theoretical contributions on these issues have assumed that each individual chooses whether or not to commit one act — i.e., the "single-act" framework developed in Becker (1968), Landes and Posner (1975), Polinsky and Shavell (1984) and Friedman (1981), among others. However, starting from the seminal contribution by Stigler (1970), the literature has recognized the role of marginal deterrence. The logic behind this theory is fairly intuitive: stepping up enforcement against one level of the activity may induce a switch to a more harmful act instead. Friedman and Sjostrom (1993), Mookherjee and Png (1992, 1994), Reinganum and Wilde (1986), Shavell (1991, 1992), and Wilde (1992) study the conditions under which the presence of marginal deterrence induces optimal penalties that are graduated to the severity of the harm produced by different law infringements.

Our objective is three-fold. First, we provide evidence of a systematic correlation between penalties and harm severity, which provides prima facie evidence for the marginal deterrence approach. Second, in order to argue that in real life penalties actually reflect marginal deterrence we test the main implications of the theory underlying the logic of marginal deterrence. Third, we show that steeper sanctions induce less harmful crimes.

Specifically, the analysis endeavours to answer the following questions. Are actual punishments (Reinganum and Wilde, 1986; Shavell, 1992; Wilde, 1992) and their expected values (Friedman and Sjostrom, 1993; Mookherjee and Png, 1994) higher for more severe crimes? While these are general predictions of the marginal deterrence framework, they also apply to other non-economic based theories of justice, according to which penalties should not reflect deterrence. For example, a positive relationship between punishments and harm severity would obtain also when penalties are set according to a retributive principle.[1] Hence, in order to clarify whether a positive correlation between penalties and harm severity can be rationalized with marginal deterrence, we also test some comparative-statics predictions offered by the general environment analyzed by Mookherjee and Png (1994). Specifically, is it true that if the maximum possible punishment drops or if the cost of monitoring increases the regulator should reduce the expected penalties on all illegal acts?

While evidence of a positive relationship between maximum punishment and expected penalties would bring support to marginal deterrence, one could argue that a similar pattern can be generated by a punishment logic based on retribution. Essentially, as long as crimes are punished proportionally to the harm they generate, optimal penalties in states in which the maximum punishment is higher should be (on average) higher than penalties in other states, because courts in the former states should be relatively more likely to apply tougher standards than courts in the latter. Yet, while this effect seems to give rise to a parallel shift of penalties in states with higher maximum punishment, this is not the case with marginal deterrence, as we document below. In addition, and even more convincingly, evidence of a negative correlation between monitoring cost and expected penalties would unambiguously point in the direction of marginal deterrence, since competing theories of justice would posit that penalties should not feature this dependence. In sum, testing these relationships also allows us to provide the first assessment of the empirical validity of the marginal deterrence principle as opposed to the maximal penalty principle and to other competing theories of justice.

To perform the empirical analysis, we build a novel and unique data set, which com-

[1] Retributive justice is a theory of justice postulating that the best response to a crime should only reflect the social harm of the crime rather than extrinsic social purposes such as deterrence, rehabilitation of the offender etc. (see, e.g., Perry, 2006).

bines information from different sources. In particular, we use individual-level data on a representative sample of inmates of US prisons from the “Survey of Inmates in State and Federal Correctional Facilities”. For each individual, the survey contains information on the crime committed, the current sentences, the criminal history and many demographic characteristics. The survey also contains information on the state and year in which the crime was committed. Using this information, we merge the individual-level data with a number of state characteristics that are relevant to test the theory. First, we employ data on the existence and use of death penalty in each state over time (e.g., Donohue and Wolfers, 2005; Katz, Levitt and Shustorivich, 2003) to build proxies for maximum punishment. Second, we use data on police wages and on the cost of gathering weapons and ammunitions across police departments (Bove and Gavrilova, 2017; Masera, 2016) to measure monitoring costs in different states over time.

We start by showing that penalties are indeed strongly increasing in the offense level of crimes. Across US states, the average slope of the punishment-severity schedules equals 0.021, indicating that an additional level of offense is associated with a 2.1% increase in sentence length. The punishment-severity schedules are positively sloped in all but two states, and 75% of the positive slope coefficients are also statistically significant at conventional levels.

Next, we provide robust evidence that sentence length is higher in states with a death penalty law in place, and it is lower in states where monitoring is more costly. According to our baseline estimates, the presence of a death penalty law in a state is associated with a 10-13% longer sentence, while a 1% increase in police wages is associated with a 0.3-0.7% drop in sentence length. Since the marginal deterrence framework of Mookherjee and Png (1994) converges to the single-act model if criminals are all equal, we also conjecture and empirically verify that the effects of maximum punishment and monitoring cost are stronger in states where the private benefits from crime — which we proxy using income inequality data — are more heterogeneous.

Although our individual-level data are fairly good to assess the effect of a change in the maximum possible punishment and in the cost of monitoring on the penalty level, they are less so to test the effect of the same comparative statics on the level of monitoring chosen by the regulator, since for the latter purpose we must aggregate information at the state level. Nevertheless, consistent with Mookherjee and Png (1994), we document that a decrease in the maximum punishment or an increase in the cost of monitoring are associated with a lower monitoring rate, which we proxy at the state level using information on the share of policemen in total employment. Specifically, the difference in police employment shares between states with and without a death penalty law equals

16% of the sample mean of this variable; at the same time, a 1% increase in police wages across states is associated with a reduction in the monitoring rate of approximately 2% of the sample mean.

Finally, we provide evidence on the effectiveness of marginal deterrence. The latter requires penalties to be graduated to harms, so as to avoid criminals switching to more harmful acts. Accordingly, if marginal deterrence works, we should expect steeper sanctions to be associated with less harmful crimes. Indeed, we find that in states where punishment-severity schedules are steeper, an increase in the slope of this schedule is associated with a significant reduction in the average offense level. Moreover, we find that steeper schedules are associated with relatively fewer inmates committing more serious crimes.

Our analysis is obviously related to the literature on the determinants of crime, which has studied the role of direct policies such as the size of the police force,[2] the incarceration rate[3] and capital punishment,[4] as well as the effect of more indirect factors such as abortion[5] or gun laws.[6] Unlike all these papers, we do not investigate the determinants of crime, but we rather focus on the determinants of the enforcement policies meant to fight it. Specifically, we show that the enforcement policies and the toughness of sanctions in different US states are by and large consistent with the rational economic model of marginal deterrence of law enforcement.

The rest of the paper is organized as follows. Section 2 summarizes the empirical predictions of marginal deterrence for optimal punishment. Section 3 describes the data. Section 4 presents the baseline results. Section 5 reports some robustness checks. Section 6 extends the analysis to the monitoring rate and to the role of inequality. Section 7 provides additional evidence on the effectiveness of marginal deterrence. Section 8 concludes.

2. Theoretical Background

To begin with, in order to gain intuition on the empirical strategy that we will develop in the next sections, we shortly summarize the basic logic of the model developed by

[2]See for instance Cameron (1988), Cornwell and Trumbell (1994), Di Tella and Schargrodsky (2004), Moody and Marvell (1996), Levitt (1996) and McCrary (2002).

[3]See for instance Abrams (2006), Chen and Shapiro (2004), Drago et al. (2009), Johnson and Raphael (2006), Kessler and Levitt (1999), Levitt (1996) and Webster, Doob and Zimring (2006).

[4]See among others Cohen-Cole et al. (2009), Donohue and Wolfers (2005), Ehrlich (1975, 1977), Katz, Levitt and Shustorivich (2003) and Passell and Taylor (1977).

[5]See among others Dills and Miron (2006), Donohue and Levitt (2001, 2004, 2008), Foote and Goetz (2008), Joyce (2003, 2009) and Lott and Whitley (2007).

[6]See Ayres and Donohue (2003a, 2003b), Black and Nagin (1998), Helland and Tabarrok (2004), Lott and Mustard (1997), Lott (1998, 2003) and Plassmann and Whitley (2003).

Mookherjee and Png (1994), which provides the most general environment to study marginal deterrence of enforcement of law. Mookherjee and Png (1994) study a model in which the level of the criminal activity is a continuous variable, and individuals derive heterogeneous benefits from infringing the law. In their baseline analysis, they consider an environment in which, although the monitoring system detects all acts at a common rate, regardless of their harmfulness, acts of differing severity may be penalized at different rates. The policy they consider specifies both a monitoring rate and penalties. For given policy, each individual will choose an harmful act to maximize the difference between the benefits from infringing the law — which are heterogeneous — and the expected penalty for the act — which is type independent. A consequence is that higher types — i.e., those who benefit the most from a harmful act — cannot choose less harmful acts. In the limit when all individuals derive the same benefits from infringing the law (no heterogeneity) the model converges to the single-act framework, where only one harmful act is chosen in equilibrium.

In this environment the authors characterize the optimal policy for a regulator that maximizes an utilitarian welfare function, which attributes equal weight to private benefits, external harms and enforcement costs. The model does not allow for infinitely large punishments, otherwise any desired pattern of deterrence could be achieved at minimal cost by combining arbitrarily low monitoring with sufficiently steep penalties. The monitoring rate is assumed to be non-contingent on the severity of the harm produced by criminals, whereas penalties are contingent on it.[7]

If enforcement were costless and the regulator could distinguish individuals' types, each individual would be induced to choose the first-best action, namely, the one that trades off each individual's marginal benefit against the corresponding marginal harm. This decision rule changes when enforcement becomes costly and when the regulator cannot distinguish individuals' types: the second best features a distortion that is standard in adverse selection environments à la Mirlees and reflects how costly monitoring is, the heterogeneity of the profitability of crime across individuals as well as the maximal punishment that the society can inflict to law breakers.

In a nutshell, the analysis offered by Mookherjee and Png (1994) shows that — in a marginal deterrence setting — if society wishes to reduce the harmful act chosen by a given individual, it necessarily must raise expected penalties for all more harmful acts.

[7]This is an important assumption in this and other models of marginal deterrence (see also Reinganum and Wilde, 1986; Shavell, 1992; Wilde, 1992). All these papers assume that the probabilities of apprehension (monitoring rate in Mookherjee and Png, 1994) for two or more alternative harmful acts are determined by the same decision, so the only way of changing the expected penalty for one act without changing the expected penalty for another is by altering the actual penalty.

Penalties, however, cannot be raised beyond the given maximum. Hence, it is not optimal to match the first-best degree of deterrence for any type.

In this setting, an increase in the maximum possible penalty increases the scope for deterrence through the means of higher penalties. The same increase in the scope for deterrence results from a fall in the monitoring cost. Intuitively, when monitoring (hence deterrence) becomes less costly, society should move closer to the first-best pattern of deterrence, stepping up expected penalties for all illegal acts.

Using the data described in the next section, in Section 4 we will test the following predictions:

Prediction 1. The optimal actual punishment for a crime is higher for more severe crimes.

Reinganum and Wilde (1986), Shavell (1992) and Wilde (1992) investigate the question of whether the optimal punishment rises with the seriousness of the offense. Provided that the probability of actual punishment is not decreasing in the seriousness of the crime — as it is typically the case in reality — our results also support the conclusions of Friedman and Sjostrom (1993) and Mookherjee and Png (1994), who show theoretically that the expected punishment (i.e., the combination of probability and actual punishment) is increasing in the level of offense.[8]

Prediction 2. If the maximum possible punishment is higher, the regulator should, other things being equal, increase the expected penalty on all illegal acts.

Prediction 3. If the cost of monitoring is higher, the regulator should, other things being equal, reduce the expected penalty on all illegal acts.

Although Predictions 2 and 3 are stated in terms of expected penalty, we will rather look at actual punishment. The reason is twofold. First, by looking at actual punishment we can fully exploit our individual-level information on a large sample of inmates of US prisons, and control for a large set of individual and state confounders in the empirical analysis. To calculate expected penalty we must instead aggregate information at the state level (an exercise that we do later on as a robustness check) and thus rely on stronger assumptions for identification. Second, in Section 6 we show that if the maximum possible punishment is higher, or if the cost of monitoring is lower, the regulator increases the monitoring

[8] As underlined by Friedman and Sjostrom (1993) if a given crime is punished in expected terms more severely than another crime, this does not necessarily imply that the actual punishment for the first crime is also higher. From a theoretical point of view, some extra assumptions are required.

rate. Therefore, if both the monitoring rate and the actual punishment go in the right direction, the same must be true for the combination of the two — i.e., the expected punishment. Interestingly, our strategy allows us to compare empirically the marginal deterrence model with the single-act one. In the single-act framework of Polinsky and Shavell (1992) an increase in the cost of monitoring should reduce monitoring and raise penalties. We find, instead, that such an increase reduces both monitoring and penalties in accordance with the marginal deterrence framework.

3. Data and Descriptive Evidence

3.1. Data

To test the theoretical predictions laid down in the previous section, we assemble a unique data set by combining information from different sources. We draw individual-level data on a nationally-representative sample of inmates of US prisons from the latest wave of the "Survey of Inmates in State and Federal Correctional Facilities". This survey was run by the Bureau of Justice Statistics in 2004 across inmates of State and Federal prisons. For each individual, the survey reports information on his/her current offense, current sentence, criminal history, demographic characteristics, family background, and weapon, drugs and alcohol use. The total number of interviewed inmates is 18,185. Out of these, we focus on the subsample of 7,963 individuals who (i) are currently sentenced to serve time, (ii) have not received either a life or a death sentence (as in these cases we could not compute sentence length), and (iii) for whom we have complete information on all the variables used in the analysis.

The "Survey of Inmates in State and Federal Correctional Facilities" also contains information on the state in which each criminal committed his/her offense and on the year of arrest. Using this information, we merge the individual-level data with a number of state characteristics that are relevant to test the theoretical predictions of marginal deterrence. First, we use information on death penalty to construct proxies for the maximum penalty applied in each state and year. We obtain information on the existence of a death penalty law from the "Death Penalty Information Center". As shown in Table 1, the existence of a death penalty law varies markedly across states and time. Specifically, four states (Maine, Michigan, Minnesota and Wisconsin) never had a death penalty law in place over our sample period (1953-2003), while in the remaining states, the number of years in which a death penalty law was in place ranges from 4 (Alaska and Hawaii) to 50 (Arkansas, Arizona, Connecticut, Florida, Georgia, Indiana, Louisiana, Nebraska, Nevada, Oklahoma and Utah).[9] We complement the information on the existence of a death penalty law with data on the number of actual capital executions in each state and year, sourced from the "Death Penalty Information Center". As shown in Table 1, there is large variation also in this variable, with the cumulated number of executions ranging from 0 (in 19 states) to 313 (in Texas) over the sample period. Second, to construct our main proxy for monitoring cost, we use the monthly wage of policemen in each state and year. We source this information for the period 1982-2003 from the "Criminal Justice Expenditure and Employment Extracts" of the Census Bureau "Annual Government Finance Survey" and "Annual Survey of Public Employment". Following Bove and Gavrilova (2017) and Masera (2016), we also use alternative proxies based on the physical cost faced by police departments in each state for gathering military equipment (details below).

3.2. Descriptive Statistics

Table 2 reports descriptive statistics on the individual characteristics. The inmates in our sample were arrested over a period of 50 years, from 1953 to 2003. Their average age is 36 and their sentence length equals 4,438 days (roughly 12 years) on average. The vast majority (79%) of inmates are males and US born (88%). The number of white and black inmates is roughly equivalent (49% and 42%, respectively), as is the number of married and divorced individuals (20% and 21%, respectively). The majority of inmates (68%) have a high-school diploma, but we also observe a substantial share of individuals with either a university degree (17%) or just primary education (12%). One-quarter of inmates are held in Federal prisons and the remaining three-quarters in State prisons. One-fifth of inmates have a parent who was sentenced in the past, and roughly 15% of them have spent some time in jail before the current sentence. Almost one-fourth of individuals have used a weapon during the offense.

The last three rows of the table report information on the offense level of crimes committed by inmates. We obtain this information from Chapter 2 of the "US Federal Sentencing Guidelines Manual", which sets rules for a uniform sentencing of individuals who are convicted to felonies and serious misdemeanors (punished with at least one year of prison) in the US Federal court system. The Guidelines assign to each crime one of 43 base offense levels. We manually map the description of the crime committed by each inmate (as provided in the survey) to one of these offense levels. As shown in Table 2,

[9] These 11 states reintroduced the death penalty after suspending it for one year in 1972, following the "Furman" decision by the U.S. Supreme Court, which deemed capital punishment unconstitutional and transformed all pending death sentences to life imprisonment.

more than 50% of crimes have offense levels between 12 and 22 - e.g., marijuana or hashish trafficking (offense level of 13), attempted sexual assault (17) and unarmed robbery (21). The frequency of less and more serious crimes is lower.

3.3. Punishment-Severity Schedules

We now study how sentence length varies across offense levels. According to Prediction 1, more severe crimes should be punished with longer sentences. Thus, in Figure 1a we plot the median sentence length across all inmates on the 43 offense levels. We normalize all sentences by the sentence length of the first offense level. The relation is sharply increasing, with the median sentence of the most severe crimes exceeding that of the least severe offenses by 20 times.

In Figure 1b we repeat the exercise after controlling for observable characteristics of the inmates. To this purpose, we start by regressing the log sentence length (number of days) on demographic, crime and state-level controls, plus a full set of year dummies. The demographic controls are age and its square, gender, race and marital status dummies, indicators for US born inmates and for inmates with sentenced parents, dummies for educational levels and for inmates who ever served in the US Armed Forces, and an indicator for inmates of Federal prisons. The criminal record controls are a dummy for use or possession of weapons during the offense, an indicator for whether the inmate spent any time in other correctional facilities before the arrest, and a dummy for whether the inmate ever used heroin. Finally, the state-level covariates are the shares of Catholics, Protestants and Muslims in the state adult population, the state unemployment rate, the log population of the state, the number of violent crimes, robberies and property crimes per state inhabitant, and the state GDP. Then, we compute the residuals from this regression, exponentiate them, and plot the median of the resulting variable on the 43 offense levels. The main evidence is now even stronger.

Next, we estimate state-specific punishment-severity schedules, in order to study whether the aggregate relationship documented in the previous graphs also holds across individual states. To this purpose we regress, separately for each state, log sentence length on offense level (a variable ranging from 1 to 43 depending on the severity of the crime), plus demographic and crime controls and a time trend. We restrict to states with at least 40 inmates, so as to have enough degrees of freedom. The coefficients on offense level obtained from these regressions give the slopes of the state-specific schedules. These coefficients indicate by how much sentence length changes (in percentage) in a given state for each additional level of offense. The results are displayed in Figure 2. Across all states, the average slope equals 0.021, indicating that sentence length increases by 2.1% on average for each additional level of offense. The slopes range from -0.048 to 0.047. However, they are positive in all but two states, and 28 (75%) of the positive slopes are also statistically significant at conventional levels.[10] Overall, these results indicate that the positive relationship between sentence length and offense level is a robust relationship across US states. In Section 7 we will use the estimated state-specific slopes presented in this section to provide additional evidence on the effectiveness of marginal deterrence.

4. Baseline Results

In this section, we test Predictions 2 and 3. To this purpose, we estimate various specifications in which the log sentence length is regressed on proxies for either the maximum punishment (Prediction 2) or the cost of monitoring (Prediction 3) in the state and year in which a given crime was committed. In our main analysis we focus on inmates whose sentences are longer than 365 days, so as to make sure that our results are not driven by very small sentences, whose duration may be noisy; as shown below, our results are however robust to using alternative samples of different sentence length. Our main proxy for maximum punishment is the existence of a death penalty law in a given state and year. As our main measure of monitoring cost we use instead the monthly wage of policemen in each state and year.

The results are reported in Tables 3 and 4. We start, in column (1), by regressing the log sentence length on each proxy, controlling for full sets of dummies for the year of arrest and for the 43 offense levels. Accordingly, our identification strategy consists of comparing inmates who have committed crimes of similar severity within a given year, across states characterized by different levels of maximum punishment and monitoring cost. Given that the existence of a death penalty law and the police wage vary across states and years, whereas the length of sentence is individual specific, we correct the standard errors for clustering within state-year pairs. Consistent with Predictions 2 and 3, the results show a positive coefficient on the death penalty dummy and a negative coefficient on the police wage, implying that sentences are longer in states where the maximum punishment is higher or the cost of monitoring is lower.

In the remaining columns, we include further controls to account for differences across inmates in observable characteristics, which may affect the length of sentence. Specifically,

[10] The only states for which the slope is negative are Utah and Massachusetts, although the slope is statistically significant only in the former state. These results may partly reflect the limited sample size, which equals 42 inmates for Utah and 56 for Massachusetts.

in column (2) we add controls for demographic characteristics. These are: age and age squared; gender, race and marital status dummies; dummies for US born inmates and for inmates with sentenced parents; dummies for educational levels; and dummies for inmates who ever served in the US Armed Forces or are held in Federal prisons. In column (3) we add controls for the criminal record of inmates, namely, a dummy for use or possession of weapons during the offense, a dummy for whether the inmate spent any time in other correctional facilities before arrest, and a dummy for whether the inmate ever used heroin. Finally, in column (4) we add state-level covariates, namely, the shares of Catholics, Protestants and Muslims in the state adult population, the state unemployment rate, GDP and population, and the number of violent crimes, robberies and property crimes per state inhabitant. The main evidence is preserved across the board. Quantitatively, our estimates for the death penalty coefficient range between 0.10 and 0.13, implying that in states with a death penalty law, sentences are 10-13% longer than in other states. Similarly, the estimated coefficients on the police wage range between 0.3 and 0.7, implying that a 1% increase in police wages is associated with a 0.3-0.7% drop in sentence length.

5. Robustness Checks

We now submit the baseline results to an extensive sensitivity analysis, which aims at showing that our main evidence is preserved when using alternative proxies and estimation samples, when accounting for possible underlying trends, and when exploiting different identification strategies.

5.1. Alternative Proxies

We start by showing that our baseline evidence holds across alternative proxies for the maximum penalty and the cost of monitoring. The results are reported in Tables 5 and 6. As for the maximum penalty, we switch from the simple existence of a death penalty law to a measure of its actual adoption. In particular, following a large empirical literature (e.g., Donohue and Wolfers, 2005; Katz, Levitt and Shustorivich, 2003), we use the number of executions in a given state and year. We include this variable either in levels (column 1), or per 100,000 state inhabitants (column 2), or per 1,000 state prisoners (column 3). In all cases, we find that a more intensive use of the death penalty is associated with longer sentences. Quantitatively, an increase in executions equal to the difference between the 10th and the 90th percentile of the distribution (i.e., 17 executions) implies a 8.5% longer sentence. A commensurate increase in per-capita or per-prisoner executions (0.11 and 0.20, respectively) is associated with a 8-11% rise in sentence length.

Next, we discuss alternative proxies for the cost of monitoring. Our first measure, used in column (1) of Table 6, is the fraction of each state's land devoted to forests. This variable captures the fact that monitoring criminals is more difficult and costly in states where natural conditions are more prohibitive. Accordingly, the coefficient on this variable is negative and highly statistically significant, with a point estimate of 0.273. This implies that a 1 percentage point (p.p.) increase in the share of forest land is associated with a 0.27% drop in sentence length.

As a second proxy, we follow Bove and Gavrilova (2017) and Masera (2016), and exploit a program created by the National Defense Authorization Act — i.e., the 1033 Program — which gave US police departments the possibility of obtaining military equipment and weapons from a number of disposition centers of the U.S. Government Defense Logistics Agency (DLA). The cost of obtaining such material is increasing in the distance between the police department and the disposition center, because the department must cover all transportation costs. Accordingly, monitoring criminals should be ceteris paribus more costly for police departments that are located further away from the disposition centers. We build on this intuition to construct our second proxy for monitoring cost. We start by geocoding all police departments in each US state and all DLA disposition centers (see Figure 3). Then, we compute the distance between each department and its closest disposition center. Finally, we calculate the mean or median distance across all departments in each state. As shown in columns (2) and (4), a 1% increase in distance is associated with a 0.1% decrease in sentence length. In columns (3) and (5), we further take advantage of the fact that the 1033 Program was signed into law in 1996, and thus became effective only afterwards. We therefore interact the distance variables with a dummy equal to 1 in 1996 and later years. As expected, the interaction term is negative and very precisely estimated, whereas the linear coefficient — which captures the effects of distance prior to 1996 — is close to zero.

5.2. Alternative Samples

In this section, we test the robustness of our baseline estimates across alternative samples. The results are reported in Tables 7 and 8. In column (1), we use the whole sample of inmates, including those whose sentences are shorter than one year. The coefficients are very close to our baseline estimates in terms of size, but they are slightly less precise, consistent with these sentences being more noisy. In column (2), we revert to the baseline sample and we further exclude the 1% of inmates with sentences exceeding 75 years. The results are virtually unchanged, suggesting that our evidence is not driven by extremely long sentences. In column (3), we further address the potential issue of outliers in the dependent variable by running a median regression. The coefficients are in the same ballpark as our baseline estimates.

In column (4), we restrict the sample to inmates who committed crimes in their own state of residence (79% of inmates in our sample). The information on the state in which crimes were committed is not always reported for individuals who migrated in a different state to perform illegal activities. Accordingly, this robustness check ensures that our results are not driven by measurement error in the state variable. Reassuringly, our evidence is unaffected. In column (5), we find even stronger results when excluding Alaska and Hawaii, the two states in which the death penalty was in place for the smallest number of years. Similarly, in column (6) we show that our baseline results are preserved when excluding Texas and Virginia, which are the two toughest states against crime, since they have the highest number of cumulated executions over the sample period. Column (7) confirms our main evidence for a subsample without Maine and North Dakota, the two states with the smallest number of inmates; column (8) does the same excluding Texas and Florida, the two states with the largest number of prisoners. Finally, columns (9) and (10) show that our results hold strong in the subsample of inmates of State prisons, but are not present in the subsample of inmates of Federal prisons. Interestingly, this is consistent with the fact that, in case of Federal offenses, sentences are largely uniform across states, since they are set by the US Federal Sentencing Guidelines. Therefore, for Federal offenses, there is little useful variation across states to achieve identification.

5.3. Underlying Trends and Alternative Identification Strategies

In Tables 9 and 10, we show that our results are not spuriously driven by heterogeneous trends in sentence length across states. To this purpose, we augment our specifications with interactions between the year dummies and the initial value of the state characteristic named in each column's heading: unemployment rate, population size, the share of Catholics in the population and the violent crime rate. These interactions absorb cross-state differences in trends based on pre-existing observable attributes of the states. The results are very close to the baseline estimates.

Next, we discuss alternative identification strategies. So far, our approach consisted of comparing sentence length across states, for crimes with similar severity and for inmates with similar observable characteristics. The large set of controls included in our baseline specifications and robustness checks allays concerns with omitted variables potentially correlated with sentence length, maximum punishment and monitoring cost. One may still be concerned, however, that our results are driven by unobserved heterogeneity across states. To address this concern, in panel a) of Tables 11 and 12, we re-estimate our main specifications including state fixed effects, which absorb time-invariant differences across states. We also control for a full set of US Census division-year fixed effects, which absorb time-varying differences across states located in different areas.[11] The coefficients are now identified using time variation in sentences, death penalty and police wage within states, after controlling for common shocks to states belonging to the same region. Our evidence is unchanged and the coefficients are slightly larger than before.

In panel b), we instead run Instrumental Variables regressions, in a further attempt to isolate the exogenous variation of our main regressors. Following a vast empirical literature (surveyed, e.g., in Donohue and Wolfers, 2005), we instrument death penalty using the share of votes cast for the Republican candidate in each state during the most recent Presidential election. We instrument instead the police wage using the average wage paid in the private non-farm sector of the state. As shown in column (12), both instruments have strong predictive power in the first stage, and the second-stage coefficients remain similar to our baseline OLS estimates. In column (13) we show that neither instrument has an independent effect on sentence length after controlling for the endogenous regressors, pointing in favor of the exclusion restriction. Finally, column (14) reports results from a reduced-form specification, in which sentence length is regressed directly on the instruments. Consistent with previous results, we find sentences to be longer in states with a larger Republican share or with lower private wages.

6. Extensions

6.1. Monitoring

As already mentioned, our individual-level data is fairly good for assessing the effect of a change in the maximum punishment and in the cost of monitoring on sentence

[11] Census divisions are nine groups of states defined as follows. Division 1 (New England): Connecticut, Maine, Massachusetts, New Hampshire, Rhode Island and Vermont. Division 2 (Middle Atlantic): New Jersey, New York and Pennsylvania. Division 3 (East North Central): Illinois, Indiana, Michigan, Ohio and Wisconsin. Division 4 (West North Central): Iowa, Kansas, Minnesota, Missouri, Nebraska, North Dakota and South Dakota. Division 5 (South Atlantic): Delaware, District of Columbia, Florida, Georgia, Maryland, North Carolina, South Carolina, Virginia and West Virginia. Division 6 (East South Central): Alabama, Kentucky, Mississippi and Tennessee. Division 7 (West South Central): Arkansas, Louisiana, Oklahoma and Texas. Division 8 (Mountain): Arizona, Colorado, Idaho, Montana, Nevada, New Mexico, Utah and Wyoming. Division 9 (Pacific): Alaska, California, Hawaii, Oregon and Washington.

length. Testing the same comparative statics on the level of monitoring chosen by the regulator requires the use of aggregate data at the state level, which raises more concerns with identification. Nevertheless, we now show some basic evidence on the following predictions, which come from two comparative-statics results in Mookherjee and Png (1994):

Prediction 4. If the maximum possible punishment is lower, the regulator should, other things being equal, reduce the monitoring rate.

Prediction 5. If the cost of monitoring is higher, the regulator should, other things being equal, reduce the monitoring rate.

To bring these predictions to the data, we use the employment share of policemen in each state and year as a proxy for the monitoring rate. We obtain information on full-time police employment over 1982-2003 from the "Criminal Justice Expenditure and Employment Extracts" of the Census Bureau "Annual Government Finance Survey" and "Annual Survey of Public Employment". We then study how the employment share of policemen behaves in states with and without the death penalty, as well as in states with different monitoring costs.

The results for the effects of death penalty are reported in Table 13. In columns (1) and (2), we regress the proxy for monitoring rate on a dummy equal to one for states and years with a death penalty law in place, plus a full set of year dummies; in column (1), we omit the controls for other state characteristics, which are instead added in column (2). In both cases, we find a positive and significant coefficient, implying that states with a death penalty law display a higher monitoring rate compared to other states. According to the point estimates, the former states have a 0.4 p.p. higher employment share than the latter, i.e., a difference equal to 16% of the sample mean (2.5%). In columns (3) and (4) we replace the death penalty dummy with the number of executions, expressed in per-capita or per-prisoner terms, respectively. We continue to find positive and statistically significant coefficients. The point estimates imply that an increase in per-capita or per-prisoner executions equal to the difference between the 10th and the 90th percentile of the distribution is associated with a 0.1 p.p. increase in the employment share of policemen, i.e., roughly 4% of the mean of this variable.

Table 14 reports the results for the effect of monitoring costs. In columns (1) and (2) we regress the employment share of policemen on the log police wage, excluding and including the other state controls. The coefficient is negative and statistically significant in both cases, implying that a 1% increase in the proxy for monitoring costs is associated with a reduction in the monitoring rate approximately equal to 2% of the sample mean. In columns (3)-(8) we use the average or median distance between police departments and DLA disposition centers as alternative proxies for the monitoring cost. The results in columns (3) and (6) imply that a 1% increase in average (median) distance is associated with a reduction in monitoring rate equal to 16% (12%) of the sample mean. Columns (4)-(5) and (7)-(8) show that this effect is concentrated in the years following the approval of the 1033 Program (i.e., since 1996 onwards), irrespective of whether we control or not for state fixed effects to condition on unobserved, time-invariant, state characteristics.

6.2. Inequality

The marginal deterrence framework of Mookherjee and Png (1994) converges to the single-act model if criminals are all equal. Accordingly, we conjecture that the effects of maximum punishment and monitoring cost are stronger in states where the private benefits from crime are more heterogeneous.

To test this conjecture, we use different measures of income inequality as proxies for heterogeneity in the private benefits from crime in different states. The idea is that richer people may have lower benefit from committing offenses. We estimate our baseline specification for the length of sentence including among the regressors various indexes of income inequality in each state and year, plus the interactions of these indexes with death penalty and police wage. We expect these interactions to be positive and negative, respectively, implying that the effects of maximum penalty and monitoring costs on sentence length are relatively stronger in more unequal states. We use four indexes of inequality, namely, the Gini coefficient, the real mean deviation, the Theil index and the top 10% income share, as sourced from Frank (2009) and Frank et al. (2015). The results are reported in Table 15, where panel a) focuses on the effect of death penalty and panel b) on the effect of police wage. In all cases, we find the interaction coefficients to be highly significant and correctly signed, implying that, consistent with our conjecture, within-state inequality is an important mediator of the effects of maximum penalty and monitoring cost on sentence length.

7. Additional Evidence on Marginal Deterrence

The evidence presented in the previous sections provides support for some of the comparative statics of the marginal deterrence framework of Mookherjee and Png (1994). In particular, the results for the cost of enforcement are unlikely to be explained by alternative theories of justice. Instead, one may argue that the effect of the death penalty may not be direct evidence of marginal deterrence, in that this effect may also be consistent with non-economic explanations such as the retributive principle, under the presumption that the existence of a death penalty law in a state testifies to a moral attitude of the states' citizens who believe that the best response to a crime is a punishment inflicted for its own sake rather than to serve an extrinsic social purpose, such as deterrence or rehabilitation of the offender (retributive theory). This moral attitude might be responsible for making judges become tougher on all (including minor) crimes. In this section, we therefore present additional evidence in support of the marginal deterrence framework.

To this purpose, we use the slopes of the state-specific punishment-severity schedules estimated in Section 3.3 to divide US states into two groups, characterized by steep and flat schedules, respectively. The former (latter) states have slope coefficient above (below) the sample median. The idea is that states with steeper schedules are those in which, other things being equal, marginal deterrence is more likely to be an issue since neither the maximal punishment principle nor the retribution theory are fully consistent with schedules being excessively steep — i.e., the former predicts only one level of punishment while the latter seems more coherent with a smooth decision rule linking punishments and harms.[12] Accordingly, it seems reasonable to expect the predictions of the marginal deterrence framework to hold stronger in states featuring relatively steeper punishment-severity schedules.

The result are reported in Table 16. We start by re-estimating our baseline regressions for the maximum punishment and the cost of monitoring separately on the two sub-sample of states. As shown in columns (1) and (2), the death penalty dummy enters with a large and statistically significant coefficient only in the subsample of states with steeper schedules; the coefficient on death penalty is instead very small and imprecisely estimated in the other sub-sample. Similarly, columns (3) and (4) show that the coefficient on the log police wage is negative and highly significant in the sub-sample of states with steeper slopes, whereas it is small and statistically not significant in the remaining states. These results suggests that the comparative statics of Mookherjee and Png (1994) hold stronger in states that behave more in keeping with the marginal deterrence framework.

Next we provide evidence on the effectiveness of marginal deterrence. The reason why

[12] Of course, the hidden assumption we are forced to impose here is that when regulators behave according to marginal deterrence they do so optimally — i.e., according to the rule identified in Mookherjee and Png (1994). Obviously, this assumption may or may not be plausible depending on the regulators' actual degree of sophistication and the accuracy of the information they own about the environment. However, estimating the effects of unintentional mistakes in policy design seems a quite hard task given the information available that we prefer to leave aside.

marginal deterrence requires penalties to be graduated is to avoid criminals switching to more harmful acts, which would follow if the enforcement was leveled upward. Then, if marginal deterrence does work, we would expect that steeper sanctions should be associated with less harmful crimes. We use two complementary approaches. First, in columns (5) and (6), we regress the log mean offense level in a state on the slope coefficient of that state, separately for the two sub-samples defined above. To ease the interpretation of the results, we standardize the slope coefficients to have mean zero and standard deviation one. Note that, in states with steeper schedules, an increase in the slope coefficient is associated with a significant reduction in the average offense level. The estimated coefficient implies that a one standard deviation increase in the slope of the punishment-severity schedule reduces the offense level by 8.6% on average. On the contrary, there is no significant relationship between offense levels and schedule steepness in the remaining states. Second, in columns (7) and (8), we study how the number of inmates who have committed crimes of different severity changes as the punishment-severity schedule becomes steeper. To this purpose, we compute the number of inmates per state and offense level, and regress this variable on state fixed effects, offense level fixed effects, and the interaction between the offense level variable and the slope coefficient. If this interaction term is negative, then steeper schedules are associated with relatively fewer inmates in more serious crimes. The results show that the coefficient on the interaction term is indeed negative and precisely estimated in the sub-sample of states with steeper schedules. No significant relationship instead holds for the remaining states.

8. Concluding Remarks

The simple takeaway of the theoretical debate on marginal deterrence is that penalties should be graduated to the severity of the harm, and that the Beckerian view of the maximal punishment principle holds only in very specific environments where people can commit only one harmful act (crime). Surprisingly, how this insight is applied in real life has not been tested so far. To fill this important gap we have tested the rational economic model of marginal deterrence of enforcement law and its main predictions. By using a unique data set, which combines individual-level data on sentence length for a representative sample of US inmates with proxies for maximum punishment and monitoring costs across US states over 50 years, we have documented that the actual penalties are increasing in the level of the offense.

While this is a general prediction of the marginal deterrence framework, it also applies to other non-economic based theories of justice, according to which penalties should not reflect deterrence. For example, a positive relationship between punishments and harm severity would obtain also when penalties are set according to a retributive principle.

Hence, in order to clarify whether a positive correlation between penalties and harm severity can be rationalized with marginal deterrence, we also tested some comparative-statics predictions offered by Mookherjee and Png (1994). Specifically, we have documented an inverse relationship between sentence length and maximum penalty, while a positive relationship was found between sentence length and monitoring costs. Interestingly, the effects of maximum penalty and monitoring cost are stronger in states where income inequality is higher, suggesting that more inequality exacerbates marginal deterrence and calls for sentences that are more responsive to harms. In sum, testing these relationships also allowed us to provide the first assessment of the empirical validity of the marginal deterrence principle as opposed to the maximal penalty principle and to other competing theories of justice.

To conclude, we want to stress that — although deterrence is based on a rational conception of human behavior in which individuals freely choose between alternative courses of action to maximize pleasure and minimize pain — behavioral aspects might well play a complementary role. For instance Bindler and Hjalmarsson (2016), exploiting the differential timing in the abolition of capital punishment across offenses, are able to retrieve the effect of changes in punishment severity on jury verdicts. This provides empirical evidence that capital punishment may impact the ability of a jury to be impartial.

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Table 1 - Death penalty and executions across US states

State	Years with Death Penalty Law in Place	Cumulated Number of Executions (1953-2003)
Alabama (AL)	1953-1971, 1976-2003	28
Alaska (AK)	1953-1956	0
Arizona (AZ)	1953-1971, 1973-2003	22
Arkansas (AR)	1953-1971, 1973-2003	25
California (CA)	1953-1971, 1974-2003	10
Colorado (CO)	1953-1971, 1975-2003	1
Connecticut (CT)	1953-1971, 1973-2003	0
District of Columbia (DC)	1953-1971	0
Delaware (DE)	1953-1957, 1962-1971, 1975-2003	13
Florida (FL)	1953-1971, 1973-2003	57
Georgia (GA)	1953-1971, 1973-2003	34
Hawaii (HI)	1953-1956	0
Idaho (ID)	1953-1971, 1977-2003	1
Illinois (IL)	1953-1971, 1977-2003	12
Indiana (IN)	1953-1971, 1973-2003	11
Iowa (IA)	1953-1964	0
Kansas (KS)	1953-1971, 1994-2003	0
Kentucky (KY)	1953-1971, 1975-2003	2
Louisiana (LA)	1953-1971, 1973-2003	27
Maine (ME)	-	0
Maryland (MD)	1953-1971, 1978-2003	3
Massachusetts (MA)	1953-1971, 1983-1984	0
Michigan (MI)	-	0
Minnesota (MN)	-	0
Mississippi (MS)	1953-1971, 1974-2003	6
Missouri (MO)	1953-1971, 1976-2003	61
Montana (MT)	1953-1971, 1974-2003	2
Nebraska (NE)	1953-1971, 1973-2003	3
Nevada (NV)	1953-1971, 1973-2003	9
New Hampshire (NH)	1953-1971, 1991-2003	0
New Jersey (NJ)	1953-1971, 1982-2003	0
New Mexico (NM)	1953-1971, 1979-2003	1
New York (NY)	1953-1971, 1995-2003	0
North Carolina (NC)	1953-1971, 1977-2003	30
North Dakota (ND)	1953-1971	0
Ohio (OH)	1953-1971, 1974-2003	8
Oklahoma (OK)	1953-1971, 1973-2003	69
Oregon (OR)	1953-1971, 1979-2003	2
Pennsylvania (PA)	1953-1971, 1974-2003	3
Rhode Island (RI)	1953-1971, 1973-1983	0
South Carolina (SC)	1953-1971, 1974-2003	28
South Dakota (SD)	1979-2003	0
Tennessee (TN)	1953-1971, 1974-2003	1
Texas (TX)	1953-1971, 1974-2003	313
Utah (UT)	1953-1971, 1973-2003	6
Vermont (VT)	1953-1971	0
Virginia (VA)	1953-1971, 1976-2003	89
Washington (WA)	1953-1971, 1976-2003	4
West Virginia (WV)	1953-1964	0
Wisconsin (WI)	-	0
Wyoming (WY)	1953-1971, 1977-2003	1

Source: Death Penalty Information Center.

Table 2 - Descriptive statistics on individual-level variables

	Mean	S.D.	Min.	Max.	Obs.
Length of sentence (days)	4438	7110	15	367704	7963
Year of arrest	1999	4	1953	2003	7963
Age	36	11	17	81	7963
Male	0.79	0.41	0	1	7963
White	0.49	0.50	0	1	7963
Black	0.42	0.49	0	1	7963
Asian	0.01	0.10	0	1	7963
Other race	0.08	0.27	0	1	7963
Married	0.20	0.40	0	1	7963
Widowed	0.02	0.15	0	1	7963
Divorced	0.21	0.41	0	1	7963
Separated	0.05	0.22	0	1	7963
Never married	0.52	0.50	0	1	7963
Elementary school	0.12	0.32	0	1	7963
High school	0.68	0.47	0	1	7963
College	0.17	0.37	0	1	7963
Graduate school	0.03	0.18	0	1	7963
Sentenced parent	0.19	0.39	0	1	7963
US born	0.88	0.33	0	1	7963
Federal prison	0.25	0.43	0	1	7963
Served US Armed Forces	0.10	0.30	0	1	7963
Used weapon	0.23	0.42	0	1	7963
Time in jail before arrest	0.14	0.35	0	1	7963
Use heroin	0.15	0.36	0	1	7963
Offense level 1-11	0.12	0.33	0	1	7963
Offense level 12-22	0.52	0.50	0	1	7963
Offense level 23-43	0.35	0.48	0	1	7963

Source: Survey of Inmates in State and Federal Correctional Facilities, 2004. The sample consists of inmates who are currently sentenced to serve time, have not received either a life or a death sentence, and have no missing information for any of the variables used in the analysis. Crimes' offense levels are obtained by manually matching the description of the crimes reported in the Survey of Inmates in State and Federal Correctional Facilities with the base offense levels reported in Chapter 2 of the US Federal Sentencing Guidelines Manual.

Table 3 - Length of sentence and maximum penalty (baseline estimates)

	(1)	(2)	(3)	(4)
Death penalty	0.131***	0.133***	0.131***	0.101**
	[0.048]	[0.047]	[0.047]	[0.045]
Demographic controls	no	yes	yes	yes
Criminal record controls	no	no	yes	yes
State controls	no	no	no	yes
Offense level dummies	yes	yes	yes	yes
Year dummies	yes	yes	yes	yes
Observations	7427	7427	7427	7035
R2	0.42	0.43	0.43	0.42

The dependent variable is the length of sentence, expressed in number of days and in logs. *Death penalty* is a dummy equal to one if a death penalty law is in place in a given state and year. *Demographic controls* include: age and age squared; a dummy for male inmates; race dummies (black, asian and other races; excluded category: white); marital status dummies (widowed, divorced, separated and never married; excluded category: married); a dummy for US born inmates; a dummy for inmates with sentenced parents; dummies for 20 educational levels (highest grade of school attended); a dummy for inmates of federal prisons; and a dummy for inmates who ever served in the US Armed Forces. *Criminal record controls* include: a dummy for use or possession of weapons during the offense; a dummy for whether the inmate spent any time in other correctional facilities before arrest; and a dummy for whether the inmate ever used heroin. *State controls* include: the shares of catholics, protestants and muslims in the state adult population; the state unemployment rate; the log population of the state; the number of violent crimes, robberies and property crimes per state inhabitant; and the state GDP. *Offense level dummies* are indicator variables for 43 categories of crimes with different levels of severity. *Year dummies* are indicator variables for the year of arrest. The sample includes inmates whose sentence is longer than one year. Standard errors are corrected for clustering by state-year and reported in square brackets. ***, **, *: indicate significance at the 1, 5 and 10% level, respectively. See also notes to previous tables.

Table 4 - Length of sentence and monitoring cost (baseline estimates)

	(1)	(2)	(3)	(4)
Police wage	-0.686***	-0.695***	-0.700***	-0.304**
	[0.075]	[0.073]	[0.073]	[0.124]
Demographic controls	no	yes	yes	yes
Criminal record controls	no	no	yes	yes
State controls	no	no	no	yes
Offense level dummies	yes	yes	yes	yes
Year dummies	yes	yes	yes	yes
Observations	7169	7169	7169	6843
R2	0.42	0.43	0.43	0.42

The dependent variable is the length of sentence, expressed in number of days and in logs. *Police wage* is the average gross monthly police payroll in each state and year (expressed in logs). ***, **, *: indicate significance at the 1, 5 and 10% level, respectively. See also notes to previous tables.

Table 5 - Length of sentence and maximum penalty (alternative proxies)

	(1)	(2)	(3)
Executions	0.005**		
	[0.002]		
Per capita executions		0.734***	
		[0.247]	
Per prisoner executions			0.537***
			[0.150]
Demographic controls	yes	yes	yes
Criminal record controls	yes	yes	yes
State controls	yes	yes	yes
Offense level dummies	yes	yes	yes
Year dummies	yes	yes	yes
Observations	7035	7035	6970
R2	0.43	0.43	0.43

The dependent variable is the length of sentence, expressed in number of days and in logs. *Executions* is the number of capital executions in each state and year. This variable is expressed in levels in column (1), per 100,000 state inhabitants in column (2) and per 1,000 state prisoners in column (3). ***, **, *: indicate significance at the 1, 5 and 10% level, respectively. See also notes to previous tables.

Table 6 - Length of sentence and monitoring cost (alternative proxies)

	(1)	(2)	(3)	(4)	(5)
Share of forests	-0.273***				
	[0.084]				
Population density					
Police dept.-DLA disp. cent. distance (av.)		-0.099**	0.069		
		[0.043]	[0.090]		
Police dept.-DLA disp. cent. distance (av.) * Post 1996			-0.182**		
			[0.092]		
Police dept.-DLA disp. cent. distance (med.)				-0.102***	0.040
				[0.037]	[0.075]
Police dept.-DLA disp. cent. distance (med.) * Post 1996					-0.154**
					[0.078]
Demographic controls	yes	yes	yes	yes	yes
Criminal record controls	yes	yes	yes	yes	yes
State controls	yes	yes	yes	yes	yes
Offense level dummies	yes	yes	yes	yes	yes
Year dummies	yes	yes	yes	yes	yes
Observations	6970	7035	7035	7035	7035
R2	0.43	0.42	0.43	0.43	0.43

The dependent variable is the length of sentence, expressed in number of days and in logs. *Share of forests* is the fraction of each state's land devoted to timberland. *Police dept.-DLA disp. cent. distance* is the distance between each police department and its closest DLA disposition center: columns (2) and (3) use the log average of this measure across all police departments in each state; columns (4) and (5) use the log median distance. *Post 1996* is a dummy equal to one in 1996 and all subsequent years. ***, **, *: indicate significance at the 1, 5 and 10% level, respectively. See also notes to previous tables.

Table 7 - Length of sentence and maximum penalty (alternative samples)

	All Sentences	No Large Sentences	Median Regression	Off. in State of Residence	Excl. Alaska and Hawaii	Excl. Texas and Virginia	Excl. Maine and N. Dakota	Excl. Texas and Florida	Federal Inmates	State Inmates
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Death penalty	0.106*	0.088**	0.095***	0.080*	0.109**	0.092**	0.097**	0.081*	-0.029	0.140**
	[0.058]	[0.044]	[0.035]	[0.048]	[0.047]	[0.044]	[0.045]	[0.042]	[0.064]	[0.055]
Demographic controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Criminal record controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
State controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Offense level dummies	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Year dummies	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Observations	7562	6983	7035	5512	6975	5946	7026	5520	1833	5202
R2	0.40	0.42		0.43	0.42	0.42	0.42	0.41	0.56	0.40

The dependent variable is the length of sentence, expressed in number of days and in logs. Column (1) includes inmates with sentences shorter than one year. Column (2) excludes also inmates with sentences longer than 75 years. Column (3) reports estimates of a median regression. Column (4) restricts to the subsample of inmates who committed the offense in their state of residence. Column (5)-(8) excludes inmates who committed the offense in, respectively, Alaska and Hawaii, Texas and Virginia, Maine and North Dakota, and Texas and Florida. Column (9) restricts to inmates of Federal prisons. Column (10) restricts to inmates of State prisons. Standard errors (reported in square brackets) are corrected for clustering by state-year, except in column (3), where they are corrected for heteroskedasticity. ***, **, *: indicate significance at the 1, 5 and 10% level, respectively. See also notes to previous tables.

Table 8 - Length of sentence and monitoring cost (alternative samples)

	All Sentences	No Large Sentences	Median Regression	Off. in State of Residence	Excl. Alaska and Hawaii	Excl. Texas and Virginia	Excl. Maine and N. Dakota	Excl. Texas and Florida	Federal Inmates	State Inmates
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
					a) Using Police Wage					
Police wage	-0.437**	-0.286**	-0.205***	-0.279**	-0.330**	-0.345***	-0.314**	-0.284**	-0.168	-0.390**
	[0.182]	[0.123]	[0.077]	[0.127]	[0.128]	[0.131]	[0.124]	[0.137]	[0.143]	[0.154]
Observations	7365	6798	6843	5352	6796	5780	6834	5361	1743	5100
R2	0.39	0.41		0.42	0.42	0.41	0.42	0.40	0.55	0.39
					b) Using Average Distance between Police Dpt. and DLA Deposits					
Police dept.-DLA disp. cent. distance (av.)	-0.097*	-0.100**	-0.131***	-0.095**	-0.123***	-0.103**	-0.096**	-0.189***	-0.032	-0.101*
	[0.053]	[0.042]	[0.033]	[0.047]	[0.046]	[0.043]	[0.043]	[0.045]	[0.050]	[0.055]
Observations	7562	6983	7035	5512	6975	5946	7026	5520	1833	5202
R2	0.40	0.42		0.43	0.42	0.42	0.42	0.41	0.56	0.40
					c) Using Median Distance between Police Dpt. and DLA Deposits					
Police dept.-DLA disp. cent. distance (med.)	-0.105**	-0.100***	-0.124***	-0.102**	-0.121***	-0.087**	-0.101***	-0.192***	-0.017	-0.120**
	[0.047]	[0.036]	[0.027]	[0.040]	[0.038]	[0.036]	[0.037]	[0.038]	[0.040]	[0.047]
Observations	7562	6983	7035	5512	6975	5946	7026	5520	1833	5202
R2	0.40	0.42		0.43	0.43	0.42	0.43	0.41	0.56	0.40

The dependent variable is the length of sentence, expressed in number of days and in logs. All regressions include the same controls as in column (4) of Table 4. ***, **, *: indicate significance at the 1, 5 and 10% level, respectively. See also notes to previous tables.

Table 9 - Length of sentence and maximum penalty (underlying trends)

	Trends Based on Initial:			
	Unemployment Rate	Population Size	Share of Catholics	Violent Crime Rate
	(1)	(2)	(3)	(4)
Death penalty	0.129***	0.111**	0.103**	0.099**
	[0.047]	[0.048]	[0.045]	[0.044]
Demographic controls	yes	yes	yes	yes
Criminal record controls	yes	yes	yes	yes
State controls	yes	yes	yes	yes
Offense level dummies	yes	yes	yes	yes
Year dummies	yes	yes	yes	yes
Observations	7035	7035	7035	7035
R2	0.43	0.43	0.43	0.43

The dependent variable is the length of sentence, expressed in number of days and in logs. Each column includes a full set of interactions between the year dummies and the initial (first year) value of the characteristic indicated in the column's heading. ***, **, *: indicate significance at the 1, 5 and 10% level, respectively. See also notes to previous tables.

Table 10 - Length of sentence and monitoring cost (underlying trends)

	Trends Based on Initial:			
	Unemployment Rate	Population Size	Share of Catholics	Violent Crime Rate
	(1)	(2)	(3)	(4)
	a) Using Police Wage			
Police wage	-0.287**	-0.311**	-0.268**	-0.338***
	[0.120]	[0.123]	[0.122]	[0.122]
Observations	6843	6843	6843	6843
R2	0.42	0.42	0.42	0.42
	b) Using Average Distance between Police Dpt. and DLA Deposits			
Police dept.-DLA disp. cent. distance (av.)	-0.113***	-0.107**	-0.099**	-0.083*
	[0.042]	[0.043]	[0.042]	[0.042]
Observations	7035	7035	7035	7035
R2	0.43	0.43	0.43	0.43
	c) Using Median Distance between Police Dpt. and DLA Deposits			
Police dept.-DLA disp. cent. distance (med.)	-0.120***	-0.110***	-0.103***	-0.091**
	[0.036]	[0.037]	[0.036]	[0.036]
Observations	7035	7035	7035	7035
R2	0.43	0.43	0.43	0.43

The dependent variable is the length of sentence, expressed in number of days and in logs. All regressions include the same controls as in column (4) of Table 4. In addition, each column includes a full set of interactions between the year dummies and the initial (first year) value of the characteristic indicated in the column's heading. ***, **, *: indicate significance at the 1, 5 and 10% level, respectively. See also notes to previous tables.

Table 11 - Length of sentence and maximum penalty (alternative identification strategies)

	a) State and Division-Year Fixed Effects											b) IV and Reduced-Form Regressions		
	Whole Sample	All Sentences	No Large Sentences	Median Regression	Off. in State of Residence	Excl. Alaska and Hawaii	Excl. Texas and Virginia	Excl. Maine and N. Dakota	Excl. Texas and Florida	Federal Inmates	State Inmates	Whole Sample	Whole Sample	Whole Sample
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Death penalty	0.491**	0.470**	0.306**	0.403***	0.519**	0.494**	0.500**	0.497**	0.491**	0.006	0.624***	0.253**	0.091***	
	[0.202]	[0.201]	[0.155]	[0.119]	[0.254]	[0.202]	[0.205]	[0.202]	[0.205]	[0.168]	[0.229]	[0.123]	[0.033]	
Republican share													0.241	0.377**
													[0.188]	[0.184]
Demographic controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Criminal record controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
State controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Offense level dummies	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Year dummies	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Observations	7035	7562	6983	7035	5512	6975	5946	7026	5520	1815	5200	7035	7035	7035
R2	0.48	0.47	0.47		0.49	0.48	0.47	0.48	0.47	0.60	0.48	0.42	0.43	0.42
Estimator	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	2SLS	OLS	OLS
First-Stage Results														
Private wage	-	-	-	-	-	-	-	-	-	-	-	1.492***	-	-
	-	-	-	-	-	-	-	-	-	-	-	[0.095]	-	-
Kleibergen-Paap F-statistic	-	-	-	-	-	-	-	-	-	-	-	248.0	-	-

The dependent variable is the length of sentence, expressed in number of days and in logs. *Republican share* is the share of votes cast for the republican candidate in a given state during the last Presidential election. The regressions in panel a) also include state and Census division-year fixed effects. ***, **, *: indicate significance at the 1, 5 and 10% level, respectively. See also notes to previous tables.

Table 12 - Length of sentence and monitoring cost (alternative identification strategies)

	a) State and Division-Year Fixed Effects											b) IV and Reduced-Form Regressions		
	Whole Sample	All Sentences	No Large Sentences	Median Regression	Off. in State of	Excl. Alaska and Hawaii	Excl. Texas and Virginia	Excl. Maine and N. Dakota	Excl. Texas and Florida	Federal Inmates	State Inmates	Whole Sample	Whole Sample	Whole Sample
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Police wage	-0.400**	-0.337*	-0.337*	-0.336	-0.446**	-0.384**	-0.300*	-0.397**	-0.504***	0.026	-0.512**	-0.428*	-0.263*	
	[0.178]	[0.198]	[0.178]	[0.264]	[0.191]	[0.177]	[0.174]	[0.178]	[0.192]	[0.264]	[0.206]	[0.235]	[0.144]	
Private wage													-0.112	-0.289*
													[0.185]	[0.160]
Demographic controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Criminal record controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
State controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Offense level dummies	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Year dummies	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Observations	6843	7365	6798	6843	5352	6796	5780	6834	5361	1727	5099	6843	6843	6843
R2	0.47	0.47	0.46		0.47	0.47	0.46	0.47	0.46	0.59	0.47	0.42	0.42	0.41
Estimator	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	2SLS	OLS	OLS
<u>First-Stage Results</u>														
Private wage	-	-	-	-	-	-	-	-	-	-	-	0.676***	-	-
	-	-	-	-	-	-	-	-	-	-	-	[0.062]	-	-
Kleibergen-Paap F-statistic	-	-	-	-	-	-	-	-	-	-	-	120.2	-	-

The dependent variable is the length of sentence, expressed in number of days and in logs. *Private wage* is the log average annual compensation of private non-farm employees in each state and year. The regressions in panel a) also include state and Census division-year fixed effects. ***, **, *: indicate significance at the 1, 5 and 10% level, respectively. See also notes to previous tables.

Table 13 - Enforcement and maximum penalty

	(1)	(2)	(3)	(4)
Death penalty	0.004***	0.003**		
	[0.001]	[0.001]		
Per capita executions			0.020***	
			[0.005]	
Per prisoner executions				0.008***
				[0.002]
State controls	no	yes	yes	yes
Observations	1046	647	647	646
R2	0.06	0.26	0.26	0.23

The dependent variable is the employment share of policemen in each state and year. The sample consists of a panel of 51 US states between 1982 and 2003. All regressions include year fixed effects. ***, **, *: indicate significance at the 1, 5 and 10% level, respectively. See also notes to previous tables.

Table 14 - Enforcement and monitoring cost

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Police wage	-0.005***	-0.007***						
	[0.002]	[0.002]						
Police dept.-DLA disp. cent. distance (av.)			-0.004***	-0.002				
			[0.001]	[0.001]				
Police dept.-DLA disp. cent. distance (av.) * Post 1996				-0.003**	-0.002**			
				[0.002]	[0.001]			
Police dept.-DLA disp. cent. distance (med.)						-0.003***	-0.001	
						[0.001]	[0.001]	
Police dept.-DLA disp. cent. distance (med.) * Post 1996							-0.002*	-0.001**
							[0.001]	[0.001]
State controls	no	yes	yes	yes	yes	yes	yes	yes
Observations	1026	634	647	647	647	647	647	647
R2	0.81	0.85	0.27	0.27	0.87	0.26	0.26	0.87

The dependent variable is the employment share of policemen in each state and year. The sample consists of a panel of 51 US states between 1982 and 2003. All regressions include year fixed effects; columns (1), (2), (5) and (8) also include state fixed effects. ***, **, *: indicate significance at the 1, 5 and 10% level, respectively. See also notes to previous tables.

Table 15 - Length of sentence, maximum penalty and monitoring cost (heterogeneity)

	a) Maximum Penalty				b) Monitoring Cost			
	Gini Index	Rel. Mean Deviation	Theil Index	Share Top 10%	Gini Index	Rel. Mean Deviation	Theil Index	Share Top 10%
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Death penalty	-2.323***	-1.809**	-0.306*	-0.571*				
	[0.885]	[0.817]	[0.180]	[0.339]				
Death penalty * Inequality	4.270***	2.365**	0.585**	1.655**				
	[1.569]	[1.022]	[0.249]	[0.822]				
Police wage					3.399**	3.382***	0.414	1.519***
					[1.355]	[1.303]	[0.330]	[0.579]
Police wage * Inequality					-6.240***	-4.395***	-0.907**	-4.259***
					[2.288]	[1.554]	[0.380]	[1.298]
Inequality	-2.059	-0.466	-0.533**	-1.578*	52.750***	37.622***	7.536**	35.082***
	[1.492]	[0.970]	[0.263]	[0.813]	[18.742]	[12.784]	[3.171]	[10.767]
Demographic controls	yes	yes	yes	yes	yes	yes	yes	yes
Criminal record controls	yes	yes	yes	yes	yes	yes	yes	yes
State controls	yes	yes	yes	yes	yes	yes	yes	yes
Offense level dummies	yes	yes	yes	yes	yes	yes	yes	yes
Year dummies	yes	yes	yes	yes	yes	yes	yes	yes
Observations	7035	7035	7035	7035	6843	6843	6843	6843
R2	0.43	0.43	0.43	0.43	0.42	0.42	0.42	0.42

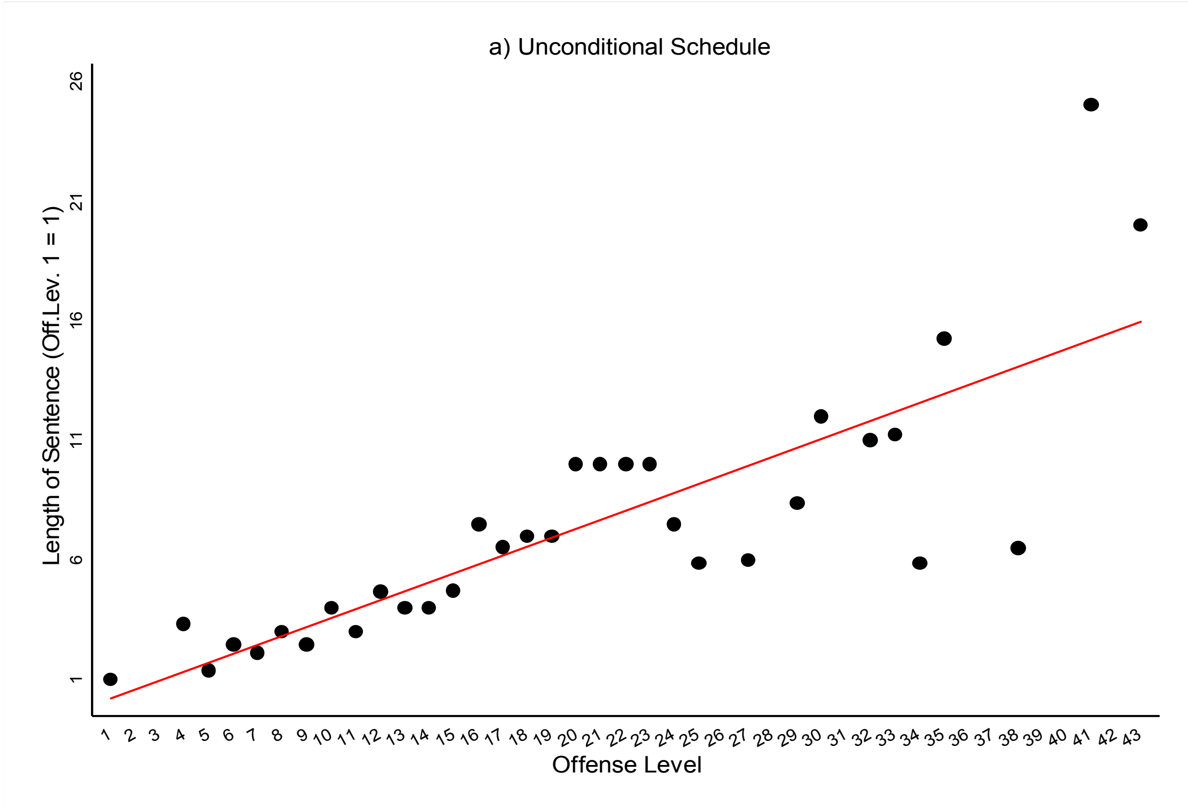
The dependent variable is the length of sentence, expressed in number of days and in logs. In each column, *inequality* is measured using the variable indicated in the column's heading. ***, **, *: indicate significance at the 1, 5 and 10% level, respectively. See also notes to previous tables.

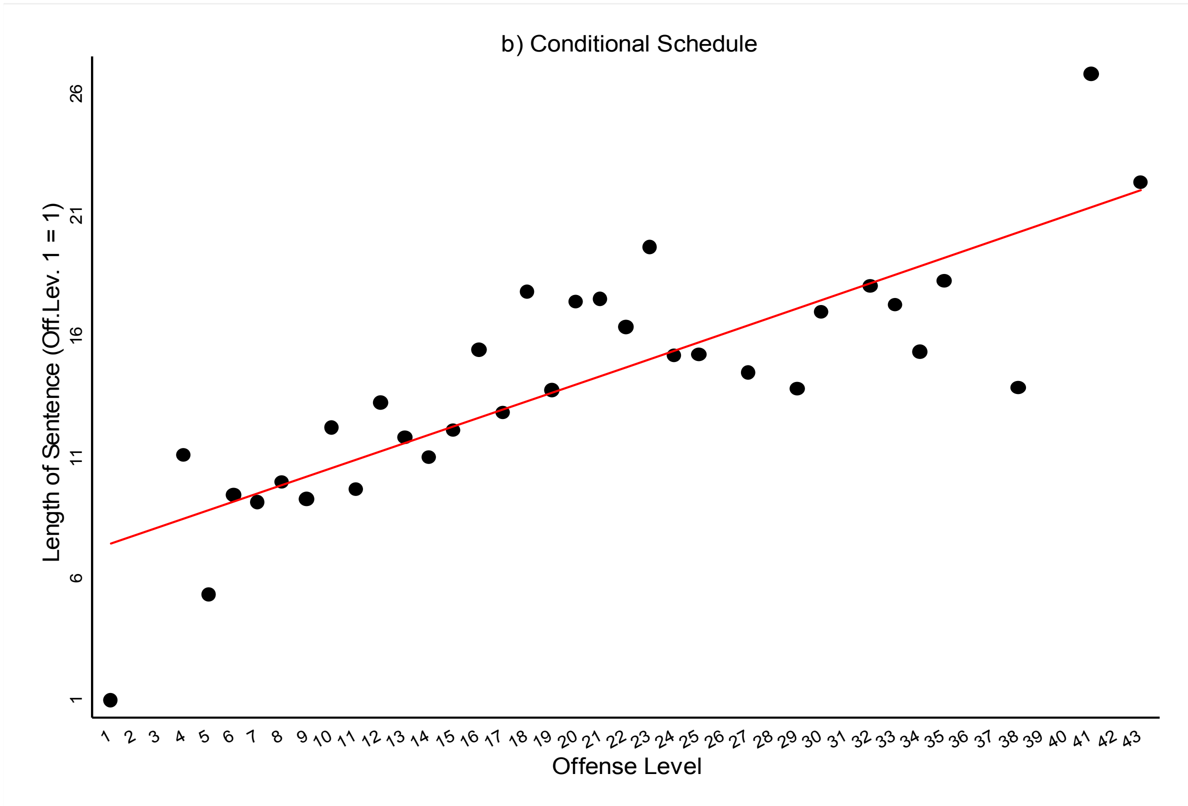
Table 16 - Additional Evidence on Marginal Deterrence

	Dependent Variable: Sentence Length		Dependent Variable: Sentence Length		Dependent Variable: Average Offense Level		Dependent Variable: Number of Inmates	
	Steep Schedule	Flat Schedule	Steep Schedule	Flat Schedule	Steep Schedule	Flat Schedule	Steep Schedule	Flat Schedule
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Death penalty	1.057**	0.046						
	[0.419]	[0.068]						
Police wage			-0.905***	-0.267				
			[0.270]	[0.198]				
Schedule slope					-0.086**	0.022		
					[0.031]	[0.015]		
Schedule slope * Offense level							-0.125*	0.017
							[0.062]	[0.022]
Observations	3,166	3,668	3,105	3,552	20	20	414	413
R2	0.48	0.40	0.47	0.39	0.17	0.06	0.50	0.57

The dependent variables are indicated in the columns' heading, and are: the length of sentence, expressed in number of days and in logs (columns 1-4); the average offense level of inmates in the state where the offense occurred (columns 5 and 6); and the number of inmates by state and offense level (columns 7 and 8). The regressions in columns (1)-(4) include the same controls as in column (4) of Table 4. The regressions in columns (7) and (8) include state and offense level fixed effects. Standard errors reported in square brackets are corrected for clustering by state-year in columns (1)-(4), for heteroskedasticity in columns (5) and (6), and for clustering by state in columns (7) and (8). States with steep (flat) schedules are those for which the estimated slope of the punishment-severity schedule is above (below) the sample median. ***, **, *: indicate significance at the 1, 5 and 10% level, respectively. See also notes to previous tables.

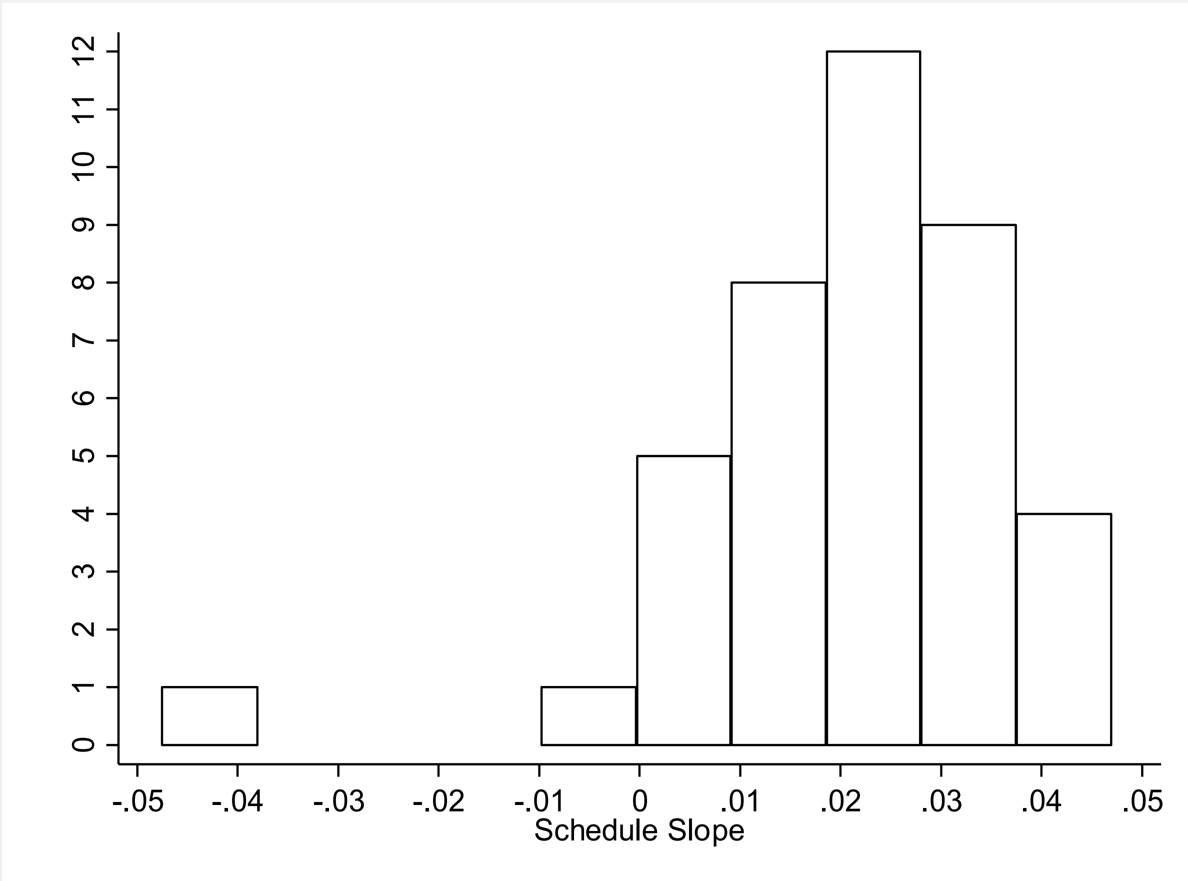
Figure 1 - Punishment-Severity Schedules





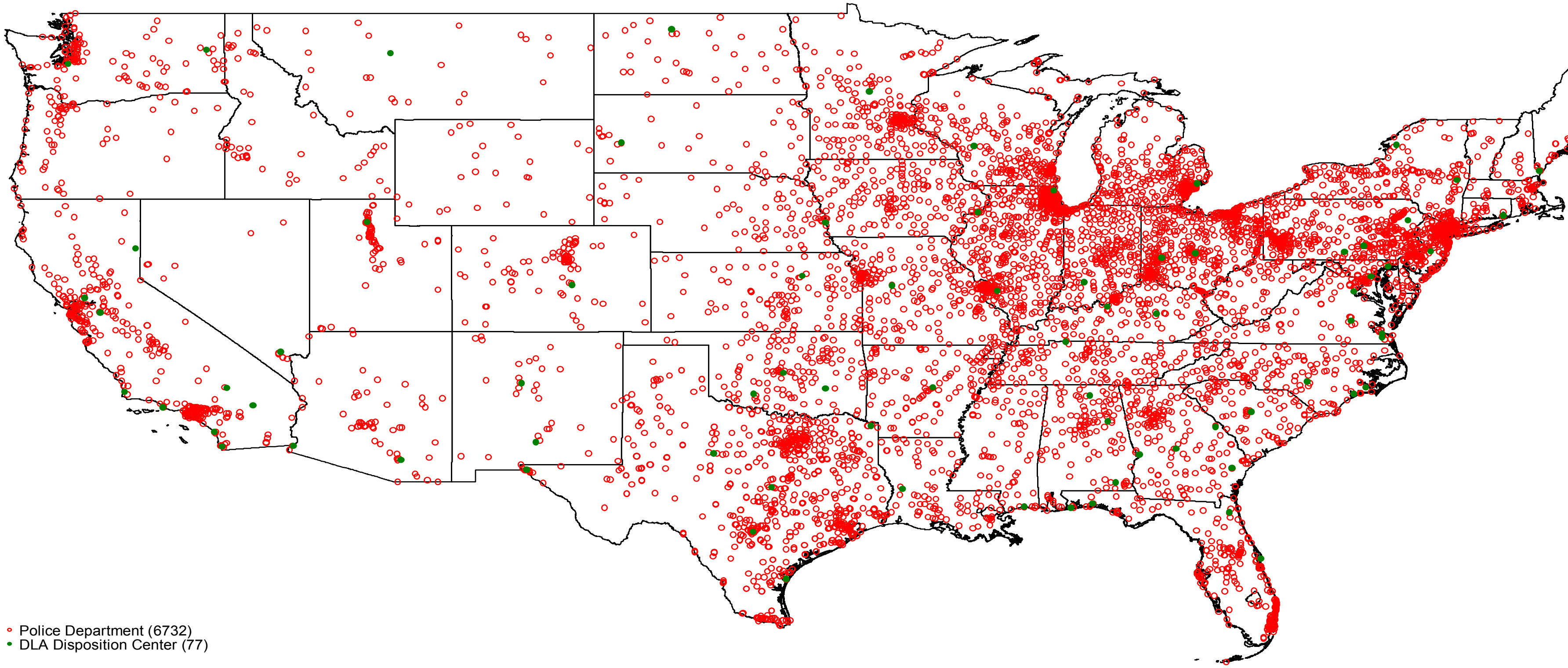
The figure plots the median sentence length across all inmates on the base offense level assigned to their crime. Figure 1a) reports the unconditional relation whereas Figure 1b) plots the schedule conditional on individual and state characteristics.

Figure 2 - Punishment-Severity Schedules: Distribution of Slope Coefficients across States



The figure plots the distribution of the slopes of state-specific punishment-severity schedules. The slopes are estimated by running, separately for each state, a regression of log sentence length on offense level, demographic controls, crime controls, and a time trend. The slope for each state is the coefficient on offense level from the corresponding regression. Only states with at least 40 inmates are considered.

Figure 3 - Police departments and DLA disposition centers



The figure plots the position of each state police department (red hollow dots) and DLA disposition center (green dots). Data for Alaska and Hawaii are available to us and used in the regressions, but are not displayed in the figure to make it more readable.