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Marginal Deterrence at Work

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 combine 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. Consistent with the theory of marginal deterrence, we show that sentence length is increasing in maximum penalty and decreasing in monitoring cost. We also provide evidence that steeper sanctions are associated with less severe crimes, consistent with marginal deterrence being e¤ective. Overall, these findings favor the marginal deterrence framework over competing theories of justice.

JEL-Codes: K140, K400.

Keywords: marginal deterrence, enforcement policies, individual-level data, death penalty.

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1. Introduction

Since the pioneering work by Becker (1968), law enforcement has attracted considerable attention by economists as well as other social scientists, 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, in fact, might not suffice 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 illegal act or not, the threat of severe sanctions is enough to deter people from infringing the law: the maximum punishment principle (Becker, 1968). Yet, when agents can also choose the severity of the offense, 'hanging people for a sheep' might not be a good idea (Friedman and Sjostrom, 1993). Notably, undeterred individuals will have a reason to commit less, rather than more severe acts, if expected sanctions rise with severity. This tendency is sometimes said to reflect *marginal deterrence* — i.e., the need for graduating penalties to the severity of the offense.[1]

Yet, the empirical literature lacks a systematic analysis of the practical relevance of marginal deterrence as opposed to competing theories of justice. In order to fill this gap, in this paper we provide the first empirical assessment of marginal deterrence, by showing that this rational economic model of law enforcement provides a reasonable description of the actual policies chosen by regulators and law enforcers. First, we document a systematic positive correlation between penalties and crime severity. Second, we test the main implications of the theory underlying the logic of marginal deterrence. Third, we provide evidence on the effectiveness of marginal deterrence by showing that steeper sanctions are associated with less severe crimes.

To perform the empirical analysis, we build a novel and unique data set, which combines information from multiple sources. In particular, we use individual-level data on a representative sample of inmates of US prisons from the 2004 Survey of Inmates in State and Federal Correctional Facilities of the US Bureau of Justice Statistics. For each

[1] Older theoretical contributions have assumed that each individual chooses whether or not to commit one (illegal) 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 severe 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.

individual, the survey contains information on the crime committed, the current sentence, the criminal history and many demographic characteristics. We merge the individual-level data with three other pieces of information that are relevant to test the theory. First, we employ data on the existence and use of death penalty in each state over time 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. Finally, we use the US Federal Sentencing Guidelines Manual to construct an objective measure of the severity of each crime.

We start by documenting that penalties are strongly increasing in the severity of crimes. Across US states, the average slope of the punishment-severity schedule 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.

A positive relationship between punishments and crime severity is consistent with marginal deterrence, but it would obtain also when penalties are set according to alternative theories of justice such as the retributive or the incapacitation principle (see, e.g., Perry, 2006). Retributive justice postulates that the best response to a crime should only reflect the severity of the crime rather than extrinsic social purposes, such as deterrence and rehabilitation of the offender (Bhuller et al., 2016). By contrast, proponents of the incapacitation theory of punishment advocate that offenders should be prevented from committing further crimes either by their (temporary or permanent) removal from society or by some other method that restricts their physical ability to re-offend in some other way (see, e.g., Zimring and Hawkins, 1995).

In order to clarify whether a positive correlation between penalties and crime severity can be rationalized with marginal deterrence as opposed to other theories of justice, we then move to test some comparative-statics predictions offered by the general environment of marginal deterrence analyzed by Mookherjee and Png (1994). Specifically, we find that sentences are on average longer in states where maximum punishment is higher, and shorter in states where monitoring is more costly. According to our estimates, the presence of a death penalty law in a state is associated with a 20-30% longer sentence, while a 10% increase in police wages is associated with a 3-6% drop in sentence length.

A causal interpretation of these estimates would require maximum punishment and monitoring cost to be exogenous to sentence length. The theory is largely silent on the determinants of these variables, thereby providing little guidance to search for causality.

Yet, we argue that our correlations, besides being consistent with the theory of marginal deterrence, are also remarkably strong and robust to be purely coincidental. In particular, we show that similar results obtain when controlling for state-level trends that may induce simultaneity bias, when using specifications with state fixed-effects to absorb time-invariant heterogeneity across states, and when employing an IV strategy to get rid of shocks that may affect sentence length, maximum penalty and monitoring cost simultaneously. We also find similar results when using alternative estimation samples and alternative proxies, including the number of capital executions to measure the actual usage of death penalty across states.

We corroborate our baseline estimates with evidence on three additional predictions of the Mookherjee and Png (1994) model. First, since the model converges to the single-act framework (Becker, 1968) if criminals are all equal, we conjecture and empirically verify that the correlations of sentence length with maximum punishment and monitoring cost are stronger in states where the private benefits from crime — as proxied for using income inequality data — are more heterogeneous. Second, we document that a higher maximum punishment or a lower cost of monitoring are associated with a higher monitoring rate, as proxied for at the state level using information on the employment share of policemen. Third, we show that maximum punishment and monitoring cost affect sentence length not only in absolute but also in marginal terms, as the correlation between these variables is stronger for more severe crimes.

Finally, we provide evidence on the effectiveness of marginal deterrence. The latter requires penalties to be graduated to crime severity, so as to avoid criminals switching to more severe acts. Accordingly, if marginal deterrence works, we should expect steeper sanctions to be associated with less severe 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 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

[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, e.g., Abrams (2006), Bhuller et al. (2016), Chen and Shapiro (2004), Drago et al. (2009), Johnson and Raphael (2006), Kessler and Levitt (1999), Levitt (1996) and Webster, Doob and Zimring (2006).

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. Taken together, our novel body of evidence shows 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. Our results also have implications for the debate on the deterrence effect of death penalty. While previous papers mainly focus on the impact that the introduction of death penalty produces on the crimes for which such an extreme form of punishment is designed — e.g., first degree murder — we are the first to document a systematic relationship between capital punishment and punishments for less severe crimes, a prediction that is consistent with marginal deterrence but not necessarily with other theories of justice.

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 studies the relationships between maximum punishment, monitoring cost and sentence length. Section 5 provides additional evidence. Section 6 concludes.

2. Theoretical Background

In order to gain intuition on the empirical strategy that we will use in the next sections, we shortly summarize the basic logic of the model developed by 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 severity, 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 a crime 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

[4] See among others Alesina and La Ferrara (2014), 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).

a crime — cannot choose less severe 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 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 act 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 maximum 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 severity of the act chosen by a given individual, it necessarily must raise expected penalties for all more severe acts.[8] It follows that:

Prediction 1. Higher punishments should be associated with more severe crimes.

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

[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 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.

[8] Reinganum and Wilde (1986), Shavell (1992) and Wilde (1992) investigate the question of whether the optimal punishment rises with the severity of the offense. Provided that the probability of actual punishment is not decreasing in the severity 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. 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.

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 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 that, by looking at actual punishment, we can fully leverage our individual-level information on a large sample of inmates of US prisons, and control for a large set of individual- and state-level characteristics in the empirical analysis. To calculate expected penalty we must instead use more aggregated data at the state level. However, in Section 5 we show that the monitoring rate is increasing in maximum punishment and decreasing in monitoring cost. Therefore, since 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.

While evidence of a positive relationship between maximum punishment and expected penalties (Prediction 2) would bring support to marginal deterrence, one could argue that a similar pattern could also be generated by a punishment logic based on retribution or incapacitation. Under the presumption that a higher maximum punishment 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), this moral attitude might be responsible for making judges become tougher on all (including less severe) crimes. Our results seem to refuse this critique. In particular, this effect would give rise to a parallel shift of penalties in states with a death penalty law in place. However, marginal deterrence implies that a higher maximum punishment is associated with longer sentences not only in absolute but also in marginal terms, as we find in Section 5.[9]

[9] Moreover, evidence of a negative correlation between monitoring cost and penalties (Prediction 3) unambiguously points in the direction of marginal deterrence, since competing theories of justice would posit that penalties should not feature this dependence.

3. Data and Preliminary Evidence

3.1. Data and Descriptive Statistics

We assemble a novel and unique data set by combining information from multiple sources. We draw individual-level data on a nationally-representative sample of 18,185 inmates of US prisons from the latest available 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. We focus on the sub-sample 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 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 two 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 punishment 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).[10] 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, we proxy for monitoring cost using the monthly wage of policemen in each state and year, obtained for the period 1982-2003 from the Criminal Justice Expenditure and Employment Extracts of the Census Bureau Annual Government Finance Survey and Annual Survey

[10] 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.

of Public Employment. Alternatively, we follow Bove and Gavrilova (2017) and Masera (2016) and use the physical cost faced by police departments in each state for gathering military equipment (details below).

To obtain an objective measure of the severity of each crime, we manually map the description of the crime committed by each inmate (as provided in the survey) to one of the offense levels obtained from Chapter 2 of the US Federal Sentencing Guidelines Manual. The latter 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. Each crime is assigned to one of 43 different offense levels, with higher numbers indicating more severe crimes. For instance, marijuana or hashish trafficking has an offense level of 13, attempted sexual assault of 17, unarmed robbery of 21, and first-degree murder of 43.

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 of inmates are males (79%) 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 have a high-school diploma (68%), 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. Finally, more than 50% of crimes have offense levels between 12 and 22; the frequency of less and more serious crimes is lower.

3.2. 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 against 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 and the states. 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.[11] Then, we compute the residuals from this regression, exponentiate them, and plot the median of the resulting variable against 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.[12] Overall, these results indicate that the positive correlation between sentence length and offense level is a robust relationship across US states. In Section 5 we will use the estimated state-specific slopes presented in this section to provide additional evidence on the effectiveness of marginal deterrence.

4. Maximum Penalty and Monitoring Cost

4.1. Baseline Results

We now test Predictions 2 and 3. To this purpose, we estimate specifications of the following form:

$$\ln Days_{icst} = \alpha_c + \alpha_t + \beta \cdot X_{st} + \gamma \cdot Controls_{icst} + \varepsilon_{icst},$$

[11] 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.

[12] 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.

where $Days_{icst}$ is the sentence length (in number of days) of inmate i, who committed crime of type c in state s and was arrested in year t; $Controls_{icst}$ is a vector of demographic, crime- and state-level controls (details below); α_c and α_t are offense level and year fixed effects, respectively; X_{st} is our proxy for either maximum punishment or monitoring cost; and ε_{icst} is an error term. We correct the standard errors for clustering within state-year pairs and weight the regressions by the number of inmates in each state. This specification allows us to compare sentence length across inmates who have committed crimes of similar severity (as captured by the fixed effects α_c) in a given year (as captured by α_t) and who have similar observed characteristics (as captured by $Controls_{icst}$), but have acted in states characterized by different levels of maximum punishment and monitoring cost. We expect $\beta > 0$ for maximum punishment and $\beta < 0$ for monitoring cost, implying that sentences are ceteris paribus longer in states where the maximum punishment is higher (Prediction 2) or the cost of monitoring is lower (Prediction 3).

The baseline results are reported in Table 3, where panel a) refers to maximum penalty and panel b) to monitoring cost. In columns (1) and (6), we only control for year and offense level fixed effects. In keeping with the comparative-statics predictions of Mookherjee and Png (1994), the results show a positive and statistically significant coefficient on the death penalty dummy and a negative and precisely estimated coefficient on the police wage.

In the remaining columns, we include further controls to account for differences in observable characteristics across inmates and states. In columns (2) and (7), we control for demographics. 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 columns (3) and (8), we add controls for the criminal record of inmates: 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. In columns (4) and (9), we add state-level covariates: 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. Finally, in columns (5) and (10), we reach our preferred specification by replacing the offense level and year dummies with offense level $\times$ year fixed effects (α_{ct}), which control for possible heterogeneous trends in sentence length across crime types. The main evidence is preserved across the board. Quantitatively, our estimates for the death penalty coefficient range between 0.2 and 0.3, implying that in states with a death penalty law in place, sentences are 20-30% longer than in other states. Similarly, the estimated coefficients on the police wage range between 0.3 and 0.6, implying that a 10% increase in police wages is associated with a 3-6% drop in sentence length.[13]

4.2. Robustness Checks

We now perform an extensive sensitivity analysis, which aims at showing that the baseline correlations are preserved when using alternative proxies for maximum punishment and monitoring cost, alternative estimation samples, and a battery of alternative strategies for dealing with possible remaining confounders.

4.2.1. Alternative Proxies

In Table 4, we estimate our preferred specifications (columns 5 and 10 of Table 3) using alternative proxies for maximum penalty and monitoring cost. As for maximum punishment, our concern is that the dummy for the existence of a death penalty law may not fully account for the actual differences in maximum punishment across states. The reason is that some of the states where death penalty is in place resort occasionally to the capital sentence. To address this concern, 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 each state and year. We include this variable either per 100,000 state inhabitants (column 1) or per 1,000 state prisoners (column 2). In both cases, we find that a more intensive use of the death penalty is associated with longer sentences. Quantitatively, an increase in per-capita or per-prisoner executions equal to the difference between the 10th and the 90th percentile of the distribution (i.e., 0.11 and 0.2, respectively) implies an increase in sentence length by 6% or 9%, respectively.

Next, we construct an alternative proxy for monitoring cost, to address the concern that the variation in police wage across states may reflect factors that simultaneously influence sentence length. Following Bove and Gavrilova (2017) and Masera (2016), we 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

[13] In unreported specifications, we have re-estimated our most complete models with sentence length in levels rather than in logs and a Poisson estimator, to account for the fact that sentence length is a count variable. The results were qualitatively unchanged, with the coefficient on death penalty being equal to 0.177 (s.e. 0.058) and that on police wage to −0.470 (s.e. 0.129).

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 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. Compared to the police wage proxy, this variable is more likely to vary across states for factors unrelated with sentence length, but it does not exhibit time variation and may suffer from some measurement error, since police departments may choose not to source equipment from the DLA disposition centers.

As shown in columns (3) and (5), the results imply that a 10% increase in distance is associated with a 1.4% decrease in sentence length. In columns (4) and (6), we further take advantage of the fact that the 1033 Program was signed into law in 1996, and thus became effective only afterward. 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.

4.2.2. Alternative Samples

In this section, we test the robustness of our baseline estimates across alternative samples. The results are reported in Table 5. In column (1), we use the whole sample of inmates, including those with sentences shorter than one year, which were excluded from our baseline regressions because the duration of these sentences is typically noisy and the Federal guidelines only apply to felonies and serious misdemeanors. In column (2), we revert to the baseline sample and we further exclude the 1% of inmates with sentences exceeding 75 years. In column (3), we restrict the sample to inmates who committed crimes in their own state of residence (79% of inmates in our sample), since the information on the state may be reported with error for individuals who migrated to perform their illegal activities. The main evidence is largely unaffected, suggesting that our results are not driven by influential observations or measurement issues.

In column (4), we restrict to inmates who are sentenced for a single crime, in order to measure sentence length uniformly across inmates, since in our data we observe sentence and offense level for the most serious crime. In column (5), we account for recidivism by limiting the sample to individuals who have never been in jail before. In column (6), we exclude inmates who have committed crimes of the highest level of offense (43).

This accounts for the possibility that, in states without a death penalty, we may observe inmates who would have been sentenced to death if their crime was committed in a state where the death penalty is instead in place; in this case, these inmates would not appear in our data set. The results are unchanged.[14] Finally, columns (7) and (8) show that our results hold strong in the sub-sample of inmates of State prisons, but are not present in the sub-sample of inmates of Federal prisons. Interestingly, this is consistent with the fact that, in the case of Federal offenses, sentences are largely uniform across states, since they are set by the Federal guidelines. Therefore, for Federal offenses, there is little useful variation across states to achieve identification.

4.2.3. Confounding Factors

So far, our results support the main predictions of the Mookherjee and Png (1994) model. One may be concerned, however, that our correlations are coincidental, resulting from unobserved factors that are simultaneously correlated with sentence length, maximum punishment and monitoring cost. We believe that our empirical approach should largely allay this concern, for a number of reasons. First, our specifications control for a wealth of observable characteristics, both at the state and at the inmate level, as well as for heterogeneous trends across crime types. These controls absorb a large number of factors that may confound the results. Second, we obtained similar results when using alternative proxies, which exploit different sources of variation in maximum punishment and monitoring cost, as well as a number of alternative samples, which give emphasis to different sources of variation in the data. We view as unlikely that a third factor could make both correlations consistent with theory, across all these proxies, samples and specifications. Third, in the next sections we will provide additional results on marginal deterrence and its effectiveness. This large and varied body of evidence always points to marginal deterrence being the underlying principle of US enforcement policies. Nevertheless, we will now discuss a number of possible remaining confounders and show that our correlations are robust to accounting for them.

Underlying trends We start by showing that our estimates are not spuriously driven by heterogeneous trends across states. To this purpose, in Table 6 we augment our specifications with interactions between the year dummies and the initial value of the

[14] In untabulated regressions, we found similar results when excluding: Alaska and Hawaii, the two states in which the death penalty was in place for the smallest number of years; Texas and Virginia, which are the two toughest states against crime, since they have the highest number of cumulated executions over the sample period; Maine and North Dakota, the two states with the smallest number of inmates; and Texas and Florida, the two states with the largest number of prisoners.

state characteristic named in each column's heading: unemployment rate (column 1), population size (column 2), the share of Catholics in the population (column 3) and the violent crime rate (column 4). The basic idea is that states with different initial characteristics may have experienced a different evolution (e.g., in terms of enforcement policies or attitudes towards crime) over time. These interaction terms absorb such cross-state differences in trends. The results are close to our baseline estimates.

Unobserved heterogeneity Until now, our approach consisted of comparing sentence length across states, for crimes of similar severity and for inmates with similar observable characteristics. Despite the large set of controls included in our specifications, one may still worry that our results are driven by unobserved heterogeneity across states. For instance, more conservative states may favor the death penalty and be harsher on crime more in general. To address this concern, in columns (1) and (3) of Table 7 we re-estimate the baseline specification including state fixed effects, which absorb time-invariant differences across states. We also control for a full set of US Census division $\times$ year fixed effects, which absorb time-varying differences across states located in different areas.[15] 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. If anything, the results are now even stronger.[16]

Unobserved shocks Finally, one may worry that our estimates are spuriously driven by unobserved shocks moving maximum punishment and monitoring cost simultaneously with sentence length. In columns (2) and (4) of Table 7, we run Instrumental Variables regressions, in a further attempt to isolate the variation in death penalty and police wage that is exogenous to such shocks. Following a vast empirical literature (surveyed, e.g., in Donohue and Wolfers, 2005), we instrument death penalty using the share of votes cast in each state for the Republican candidate, during the Presidential election closest to each sample period. This instrument captures cross-state variation in death penalty due to

[15] 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.

[16] These specifications cannot be estimated with the distance-based proxy for monitoring cost since this variable is constant over time.

differences in political preferences over the medium run (four years), and thus gets rid of variation due to temporary shocks. We instead instrument police wage using the average wage paid in the private non-farm sector of the state. The private sector wage acts as an alternative wage for prospective policemen. It therefore helps isolating variation in police wage due to structural transformations driving changes in policemen supply. Both instruments have strong predictive power in the first stage, and the second-stage coefficients remain reassuringly similar to our baseline OLS estimates.

5. Additional Evidence

In this section we corroborate our previous results by providing evidence on a number of additional predictions of the marginal deterrence framework.

5.1. Heterogeneity across Offense Levels

Marginal deterrence implies that a higher maximum punishment or a lower monitoring cost are associated with higher penalties not only in absolute but also in marginal terms. To provide evidence on these predictions, we modify our baseline specification as follows:

$$\ln Days_{icst} = \alpha_{ct} + \beta_1 \cdot X_{st} + \beta_2 \cdot X_{st} \cdot OffLev_c + \gamma \cdot Controls_{icst} + \varepsilon_{icst},$$

where $OffLev_c$ is a variable ranging from 1 to 43 across offense levels.[17] If $\beta_2 > 0$ when X_{st} is maximum punishment, then a higher maximal penalty is associated with higher sentences also in marginal terms, as in this case the overall coefficient on X_{st} ($\beta_1 + \beta_2 \cdot OffLev_c$) is larger for more severe crimes. A similar argument holds when X_{st} is monitoring cost if $\beta_2 < 0$.

The results are reported in Table 8. The interaction coefficients are correctly signed and highly statistically significant. Using the point estimates, Figure 4 plots the coefficient on death penalty and monitoring cost against the offense level (solid red line) together with the corresponding 95% confidence intervals (dashed black lines). The coefficient on either variable grows sharply with the level of severity of the crime. For death penalty, the coefficient ranges from virtually zero for offense levels below 17 to 0.525 (s.e. 0.105) for offense level 43. Similarly, in the case of monitoring cost the coefficient is small and statistically not significant for offense levels below 18 and reaches −0.791 (s.e. 0.214) for offense level 43.

[17] The linear term in $OffLev_c$ is subsumed in the offense level × year fixed effects (α_{ct}).

5.2. 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 coefficient on maximum punishment and monitoring cost should be larger for states where the private benefits from crime are more heterogeneous. To test this conjecture, we augment our baseline specification as follows:

$$\ln Days_{icst} = \alpha_{ct} + \beta_1 \cdot X_{st} + \beta_2 \cdot Inequal_{st} + \beta_3 \cdot X_{st} \cdot Inequal_{st} + \gamma \cdot Controls_{icst} + \varepsilon_{icst},$$

where $Inequal_{st}$ is a measure of income inequality, which we use as a proxy for heterogeneity in the private benefits from crime in different states. We expect $\beta_3 > 0$ when X_{st} is maximum punishment and $\beta_3 < 0$ when X_{st} is monitoring cost.

The results are reported in Table 9, where each column refers to a different inequality index: the Gini coefficient (columns 1 and 5), the real mean deviation (columns 2 and 6), the Theil index (columns 3 and 7) and the top 10% income share (columns 4 and 8). We source these indexes from Frank (2009) and Frank et al. (2015). In all cases, we find the interaction coefficients to be statistically significant and correctly signed. Consistent with our conjecture, this suggests that within-state inequality is an important mediator of the effects of maximum penalty and monitoring cost on sentence length.[18]

5.3. Monitoring Rate

Our individual-level data are well suited for studying the relationship between maximum punishment or monitoring cost and sentence 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 provide suggestive evidence on two additional comparative-statics results in Mookherjee and Png (1994): (1) if the maximum possible punishment is higher, the regulator should, other things being equal, increase the monitoring rate; and (2) if the cost of monitoring is lower, the regulator should, other things being equal, increase the monitoring rate.

To proxy for the monitoring rate, we use the employment share of policemen in each state and year. We obtain information on full-time police employment over 1982-2003

[18] We have also computed the overall coefficients on maximum punishment and monitoring cost ($\beta_1 + \beta_3 \cdot Inequal_{st}$) for different levels of the inequality indexes. Taking the Gini index as an example, the coefficient on death penalty ranges from -0.083 (s.e. 0.114) in the state with the lowest inequality to 0.659 (s.e. 0.216) in the state with the highest inequality. The coefficient on monitoring cost varies instead from 0.296 (s.e. 0.235) to -0.794 (s.e. 0.265).

from the Criminal Justice Expenditure and Employment Extracts of the Census Bureau Annual Government Finance Survey and Annual Survey of Public Employment. With this data in hand, we 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 are reported in Table 10. Panel a) refers to maximum punishment. In particular, in column (1) we regress our proxy for monitoring rate on a dummy equal to one for states and years with a death penalty law in place, controlling for state covariates and year dummies. In columns (2) and (3), we instead use the number of executions — expressed in per-capita or per-prisoner terms, respectively — in place of the death penalty dummy. Consistent with the theoretical prediction, we find positive and statistically significant coefficients, suggesting that the monitoring rate is higher in states where maximum punishment is also higher. Panel b) contains the results for monitoring cost. In column (4), we regress our proxy for monitoring rate on police wage. Consistent with the theoretical prediction, we find a negative and statistically significant coefficient, suggesting that the monitoring rate is higher in states where monitoring is less costly. In columns (5)-(10), we instead use the distance-based proxies for monitoring cost. These variables enter with a negative coefficient (columns 5 and 8), and the correlations stem from the years following the approval of the 1033 Program (i.e., since 1996 onwards), irrespective of whether we control (columns 7 and 10) or not (columns 6 and 9) for state fixed effects to condition on time-invariant state characteristics.

5.4. Effectiveness of Marginal Deterrence

The evidence presented above provides support for some of the comparative statics of the marginal deterrence framework of Mookherjee and Png (1994). In this section, we present evidence on the effectiveness of marginal deterrence. To this purpose, we use the slopes of the state-specific punishment-severity schedules estimated in Section 3 to divide US states into groups characterized by schedules of different steepness. States with steeper schedules are those in which, other things being equal, marginal deterrence is more likely to be at work, given that in this framework steep sanctions are needed to obtain deterrence.

Accordingly, we expect the predictions of the marginal deterrence framework to hold stronger in states featuring relatively steeper punishment-severity schedules. In panels a) and b) of Table 11, we therefore re-estimate our baseline specifications separately on different groups states. In the first two columns, we divide states in two groups with schedule slope below and above the sample median, respectively. As expected, both the death penalty dummy and the police wage enter with larger coefficients in the sub-sample of states with steeper schedules. In the remaining three columns, we instead classify states in three groups, with schedule slope in the bottom, top or intermediate quartiles of the distribution. Strikingly, the coefficients on maximum punishment and monitoring cost grow monotonically in size, and become statistically more significant, as we move from states with relatively flatter schedules to states with relatively steeper sanctions. 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.

The reason why marginal deterrence requires penalties to be graduated is to avoid criminals switching to more severe acts, which would follow if the enforcement was leveled upward. Then, if marginal deterrence does work, we would expect steeper sanctions to be associated with less severe crimes. We now provide evidence on this using two complementary approaches. First, in panel c) of Table 11, we regress the log mean offense level in a state on the slope coefficient for that state, separately for states with schedule slope below and above the median. To ease the interpretation of the results, we standardize the slope coefficients to have mean zero and standard deviation one. The results show 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 is associated with a 8.6% decrease in offense level on average. On the contrary, there is no significant relationship between offense levels and schedule steepness in the remaining states.

Second, in panel d) 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 in each state-offense level cell, and regress this variable on state fixed effects, offense level fixed effects, and the interaction between the offense level variable and the slope coefficient. The results show that the coefficient on the interaction term is negative and precisely estimated in the sub-sample of states with steeper schedules. The point estimate implies that a one standard deviation increase in the schedule slope is associated with a 0.13 additional reduction in the number of inmates for each additional level of offense. Hence, steeper schedules are associated with relatively fewer inmates committing more serious crimes. No significant relationship holds instead for the remaining states.

Finally note that, as a corollary, our results also contribute to the important debate on the deterrent effect of capital punishment. Previous studies have focused on the effect of death penalty on first-degree murder (see, e.g., the references in footnote 4). Our combined evidence that death penalty makes the punishment-severity schedule steeper (Figure 4) and that a steeper schedule induces individuals to switch to less severe crimes (panels c) and d) of Table 11) suggests that capital punishment is effective also at preventing crimes which are not directly targeted by the death penalty.

6. Concluding Remarks

The simple takeaway of the theoretical debate on marginal deterrence is that penalties should be graduated to the severity of the crime. Surprisingly, so far there has been no attempt to test this insight — i.e., whether actual penalties reflect or not marginal deterrence. The main contribution of this paper is to test the rational economic model of marginal deterrence of law enforcement and its main predictions.

To this purpose, we have assembled a novel and 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, as well as with an objective measure of crime offense levels. First, we have documented that actual penalties are increasing in the level of the offense. Second, we have shown that penalties are increasing in maximum punishment and decreasing in monitoring cost, consistent with the comparative-statics predictions of Mookherjee and Png (1994). Finally, we have provided evidence that steeper sanctions are associated with less severe crimes, consistent with marginal deterrence being effective. Testing these relationships allowed us to provide the first assessment of the empirical validity of the marginal deterrence principle as opposed to other competing theories of justice.

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 on the ability of a jury to be impartial. However, by showing that marginal deterrence is at work, our evidence suggests that the rational economic model of law enforcement accounts well for the actual enforcement policies chosen by regulators.

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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 - Baseline estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	a) Maximum Penalty					b) Monitoring Cost				
Death penalty	0.283***	0.304***	0.303***	0.211***	0.203***					
	[0.067]	[0.065]	[0.065]	[0.060]	[0.058]					
Police wage						-0.607***	-0.627***	-0.634***	-0.254*	-0.362**
						[0.082]	[0.081]	[0.083]	[0.154]	[0.152]
Demographic controls	no	yes	yes	yes	yes	no	yes	yes	yes	yes
Criminal record controls	no	no	yes	yes	yes	no	no	yes	yes	yes
State controls	no	no	no	yes	yes	no	no	no	yes	yes
Offense level dummies	yes	yes	yes	yes	no	yes	yes	yes	yes	no
Year dummies	yes	yes	yes	yes	no	yes	yes	yes	yes	no
Offense level * Year dummies	no	no	no	no	yes	no	no	no	no	yes
Observations	7427	7427	7427	7035	7035	7169	7169	7169	6843	6843
R2	0.47	0.48	0.48	0.48	0.52	0.46	0.47	0.48	0.46	0.51

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. *Police wage* is the average gross monthly police payroll in each state and year (expressed in logs). *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. Regressions are weighted by the number of inmates in each state. ***, **, *: indicate significance at the 1, 5 and 10% level, respectively. See also notes to previous tables.

Table 4 - Alternative proxies

	(1)	(2)	(3)	(4)	(5)	(6)
	a) Maximum Penalty		b) Monitoring Cost			
Per capita executions	0.500**					
	[0.213]					
Per prisoner executions		0.473***				
		[0.145]				
Police dept. distance (av.)			-0.143**	0.144		
			[0.057]	[0.114]		
Police dept. distance (av.) * Post 1996				-0.306***		
				[0.116]		
Police dept. distance (med.)					-0.143***	0.091
					[0.049]	[0.094]
Police dept. distance (med.) * Post 1996						-0.250***
						[0.096]
Observations	7035	6970	7035	7035	7035	7035
R2	0.52	0.52	0.52	0.52	0.52	0.52

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 per 100,000 state inhabitants in column (1) and per 1,000 state prisoners in column (2). *Police dept. distance* is the distance between each police department and its closest DLA disposition center: columns (3) and (4) use the log average of this measure across all police departments in each state; columns (5) and (6) use the log median distance. *Post 1996* is a dummy equal to one in 1996 and all subsequent years. All regressions include demographic controls, criminal record controls, state controls and offense level x year dummies, as in columns (5) and (10) of Table 3. ***, **, *: indicate significance at the 1, 5 and 10% level, respectively. See also notes to previous tables.

Table 5 - Alternative samples

	All Sentences	No Large Sentences	Off. in State of Residence	Single Count	No Jail Before	No Murder	Federal Inmates	State Inmates
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				a) Maximum Penalty				
Death penalty	0.191***	0.181***	0.189***	0.230***	0.174***	0.209***	0.040	0.223***
	[0.072]	[0.057]	[0.061]	[0.071]	[0.063]	[0.059]	[0.091]	[0.072]
Observations	7562	6983	5512	5194	6075	7004	1833	5202
R2	0.51	0.52	0.53	0.54	0.53	0.52	0.65	0.50
				b) Monitoring Cost				
Police wage	-0.501**	-0.350**	-0.342**	-0.470**	-0.363**	-0.355**	-0.291	-0.397**
	[0.214]	[0.155]	[0.154]	[0.182]	[0.146]	[0.152]	[0.201]	[0.186]
Observations	7365	6798	5352	5066	5907	6813	1743	5100
R2	0.50	0.51	0.52	0.53	0.52	0.51	0.64	0.49
Police dept. distance (av.)	-0.088	-0.129**	-0.139**	-0.150**	-0.154***	-0.146**	-0.034	-0.166**
	[0.074]	[0.054]	[0.058]	[0.065]	[0.056]	[0.057]	[0.062]	[0.070]
Observations	7562	6983	5512	5194	6075	7004	1833	5202
R2	0.51	0.52	0.53	0.54	0.53	0.52	0.65	0.50

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) restricts to inmates who have committed the offense in their state of residence. Column (4) restricts to inmates with a single count. Column (5) excludes inmates who have been in jail before. Column (6) excludes inmates who have committed crimes of offense level 43. Column (7) restricts to inmates of Federal prisons. Column (8) restricts to inmates of State prisons. ***, **, *: indicate significance at the 1, 5 and 10% level, respectively. See also notes to previous tables.

Table 6 - Underlying trends

	Trends Based on Initial:			
	Unemployment Rate	Population Size	Share of Catholics	Violent Crime Rate
	(1)	(2)	(3)	(4)
		a) Maximum Penalty		
Death penalty	0.226***	0.199***	0.207***	0.194***
	[0.058]	[0.058]	[0.058]	[0.056]
Observations	7035	7035	7035	7035
R2	0.52	0.52	0.52	0.53
		b) Monitoring Cost		
Police wage	-0.384***	-0.395***	-0.318**	-0.343**
	[0.148]	[0.148]	[0.149]	[0.154]
Observations	6843	6843	6843	6843
R2	0.51	0.51	0.51	0.51
Police dept. distance (av.)	-0.143***	-0.119**	-0.122**	-0.081
	[0.055]	[0.053]	[0.056]	[0.054]
Observations	7035	7035	7035	7035
R2	0.52	0.52	0.52	0.52

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 7 - Alternative estimation approaches

	Additional FE	Instrumental Variables	Additional FE	Instrumental Variables
	(1)	(2)	(3)	(4)
Death penalty	0.415*	0.253**		
	[0.261]	[0.123]		
Police wage			-0.577***	-0.428*
			[0.185]	[0.235]
Observations	7035	7035	6843	6843
R2	0.55	0.42	0.54	0.42
Estimator	OLS	2SLS	OLS	2SLS
<u>First-Stage Results</u>				
Republican share		1.492***		
		[0.095]		
Private wage				0.676***
				[0.062]
Kleibergen-Paap F-statistic		248.0		120.2

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 most recent Presidential election. *Private wage* is the log average annual compensation of private non-farm employees in each state and year. The regressions in columns (1) and (3) also include state and Census division x year fixed effects. ***, **, *: indicate significance at the 1, 5 and 10% level, respectively. See also notes to previous tables.

Table 8 - Heterogeneity across offense levels

	(1)	(2)
Death penalty	-0.164	
	[0.120]	
Death penalty * Offense lev.	0.016***	
	[0.005]	
Police wage		0.046
		[0.229]
Police wage * Offense lev.		-0.019***
		[0.007]
Observations	7035	6843
R2	0.52	0.51

The dependent variable is the length of sentence, expressed in number of days and in logs. *Offense lev.* is a varable ranging from 1 to 43, with higher numbers indicating more severe crimes according to the US Federal Sentencing Guidelines Manual. ***, **, *: indicate significance at the 1, 5 and 10% level, respectively. See also notes to previous tables.

Table 9 - Inequality

	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)
	a) Maximum Penalty				b) Monitoring Cost			
Death penalty	-2.663**	-1.909*	-0.397	-0.662				
	[1.161]	[1.101]	[0.247]	[0.456]				
Death penalty * Inequality	4.950**	2.556*	0.796**	2.058*				
	[2.037]	[1.364]	[0.316]	[1.086]				
Police wage					4.084**	4.274***	0.379	0.802
					[1.618]	[1.492]	[0.411]	[0.623]
Police wage * Inequality					-7.267***	-5.395***	-0.827*	-2.601*
					[2.733]	[1.765]	[0.453]	[1.344]
Inequality	-1.152	0.352	-0.493	-1.568	63.558***	47.302***	7.216*	22.195**
	[2.029]	[1.331]	[0.338]	[1.140]	[22.526]	[14.589]	[3.801]	[11.174]
Observations	7035	7035	7035	7035	6843	6843	6843	6843
R2	0.52	0.52	0.52	0.52	0.51	0.51	0.51	0.51

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

Table 10 - Monitoring rate

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	a) Maximum Penalty			b) Monitoring Cost						
Death penalty	0.003**									
	[0.001]									
Per capita executions		0.020***								
		[0.005]								
Per prisoner executions			0.008***							
			[0.002]							
Police wage				-0.007***						
				[0.002]						
Police dept. distance (av.)					-0.004***	-0.002				
					[0.001]	[0.001]				
Police dept. distance (av.) * Post 1996						-0.003**	-0.002**			
						[0.002]	[0.001]			
Police dept. distance (med.)								-0.003***	-0.001	
								[0.001]	[0.001]	
Police dept. distance (med.) * Post 1996									-0.002*	-0.001**
									[0.001]	[0.001]
Observations	647	647	646	634	647	647	647	647	647	647
R2	0.26	0.26	0.23	0.85	0.27	0.27	0.87	0.26	0.26	0.87

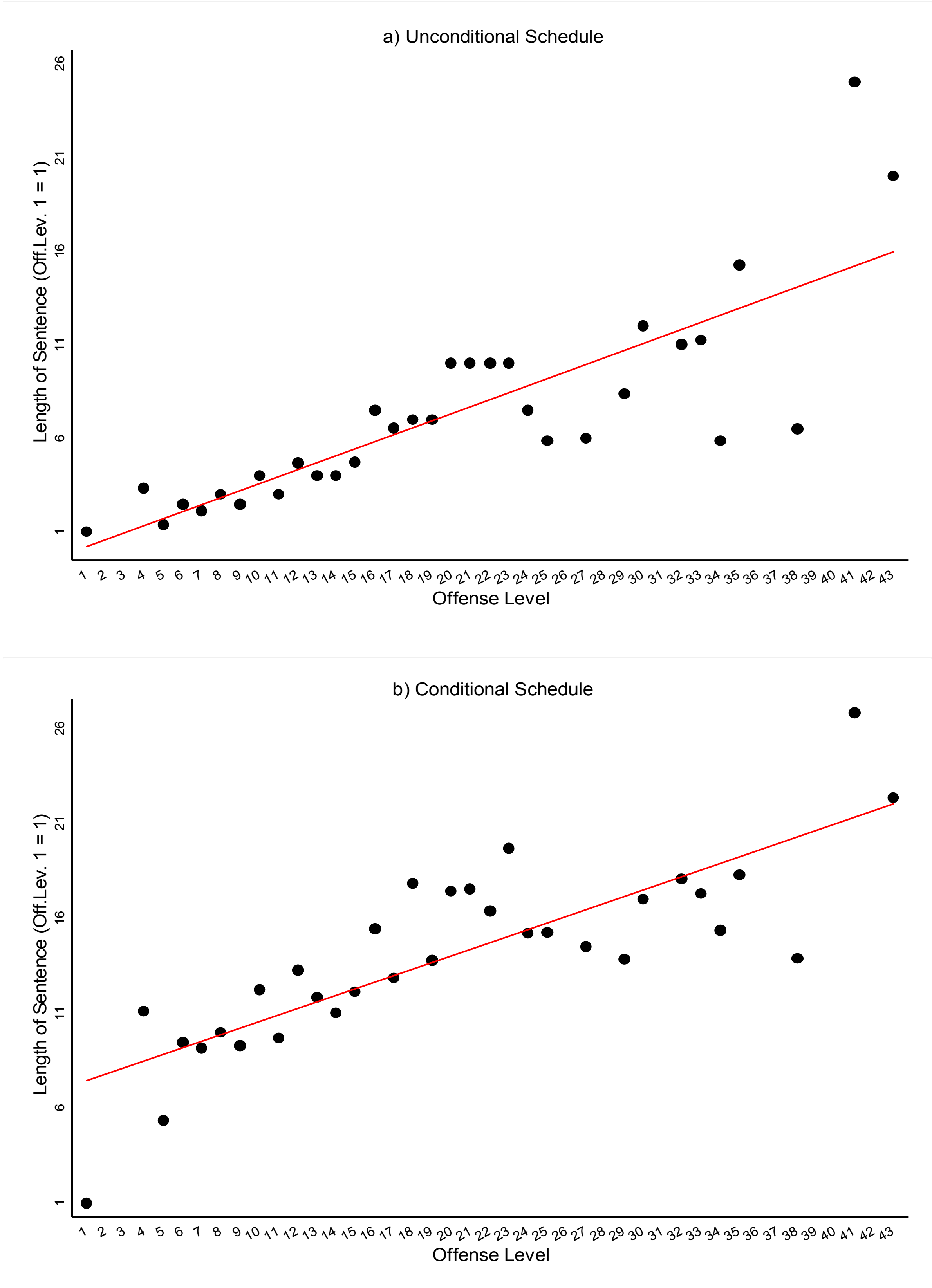
The dependent variable is the employment share of policemen in each state and year. The sample consists of a panel of 51 US states between 1982 and 2003. All regressions include state controls and year fixed effects; columns (4), (7) and (10) also include state fixed effects. ***, **, *: indicate significance at the 1, 5 and 10% level, respectively. See also notes to previous tables.

Table 11 - Effectiveness of Marginal Deterrence

	a) Dependent Variable: Sentence Length				
	Schedule Slope (Below Median)	Schedule Slope (Above Median)	Schedule Slope (Below 25th pct)	Schedule Slope (25th - 75th pct)	Schedule Slope (Above 75th pct)
Death penalty	0.172**	0.926*	-0.050	0.161*	1.044***
	[0.077]	[0.507]	[0.121]	[0.085]	[0.308]
Observations	3668	3166	1038	4844	952
R2	0.51	0.59	0.57	0.53	0.69
	b) Dependent Variable: Sentence Length				
	Schedule Slope (Below Median)	Schedule Slope (Above Median)	Schedule Slope (Below 25th pct)	Schedule Slope (25th - 75th pct)	Schedule Slope (Above 75th pct)
Police wage	-0.326	-0.748**	-0.105	-0.428**	-1.546***
	[0.220]	[0.321]	[0.390]	[0.201]	[0.507]
Observations	3552	3105	961	4755	941
R2	0.50	0.58	0.57	0.52	0.68
	c) Dependent Variable: Average Offense Level			d) Dependent Variable: Number of Inmates	
	Schedule Slope (Below Median)	Schedule Slope (Above Median)		Schedule Slope (Below Median)	Schedule Slope (Above Median)
Schedule slope	0.022	-0.086**			
	[0.015]	[0.031]			
Schedule slope * Offense level				0.017	-0.125*
				[0.022]	[0.062]
Observations	20	20		414	416
R2	0.06	0.17		0.57	0.50

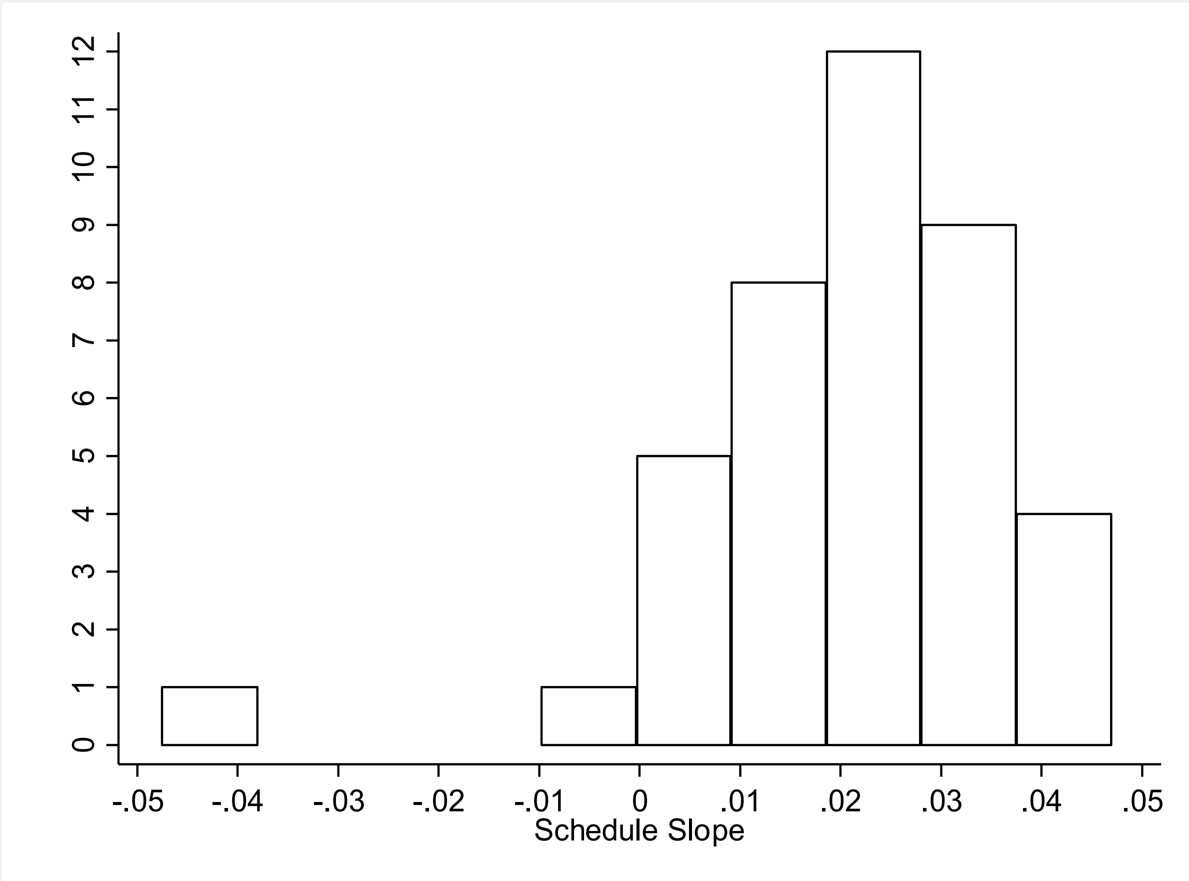
The dependent variables are indicated in the panels' headings, and are: the length of sentence, expressed in number of days and in logs (panels a and b); the average offense level of inmates in the state where the offense occurred (panel c); and the number of inmates by state and offense level (panel d). The regressions in panels a) and b) are weighted by the number of inmates in each state and include demographic controls, criminal record controls, state controls and offense level x year dummies. The regressions in panel d) include state and offense level fixed effects. *Schedule slope* is the slope of the state-specific punishment-severity schedule (see Figure 2), standardized to have mean equal to zero and standard deviation equal to one. Standard errors reported in square brackets are corrected for clustering by state-year in panels a) and b), for heteroskedasticity in panel c), and for clustering by state in panel d). ***, **, *: indicate significance at the 1, 5 and 10% level, respectively. See also notes to previous tables.

Figure 1 - Punishment-severity schedules



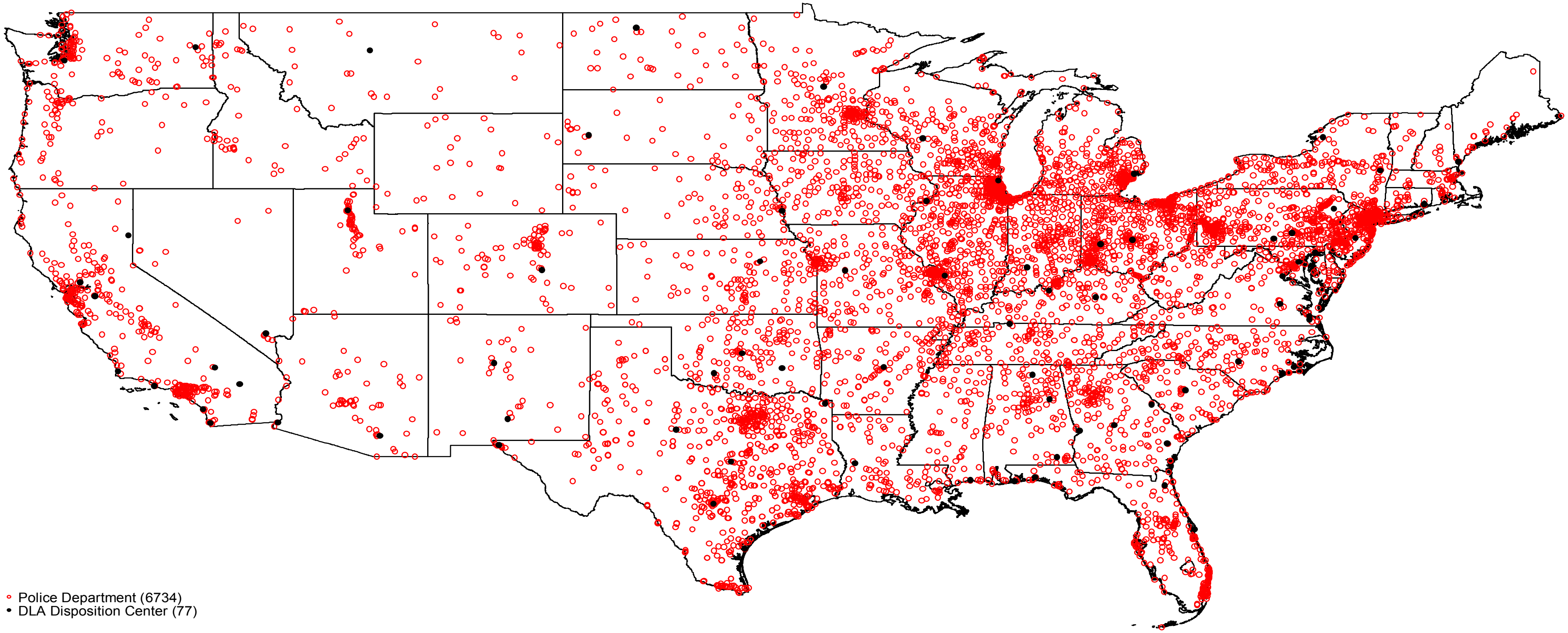
The figure plots the median sentence length across all inmates on the base offense level assigned to their crime. Figure 1a) reports the unconditional relation whereas Figure 1b) plots the schedule conditional on individual and state characteristics.

Figure 2 - Distribution of slope coefficients across states



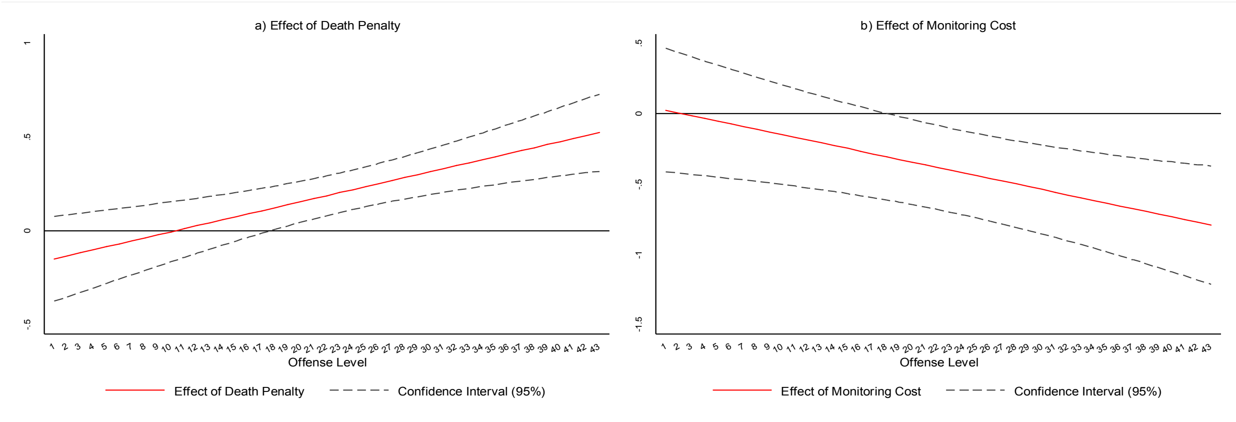
The figure plots the distribution of the slopes of state-specific punishment-severity schedules. The slopes are estimated by running, separately for each state, a regression of log sentence length on offense level, demographic controls, crime controls, and a time trend. The slope for each state is the 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 (black 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.

Figure 4 - Heterogeneity across offense levels



The figure plots the effect of maximum penalty (panel a) and monitoring cost (panel b) on sentence length for different offense levels. Results are based on the estimates reported in columns (1) and (2) of Table 8.