Journal of Economic Behavior and Organization 166 (2019) 586–612

Contents lists available at ScienceDirect

Journal of Economic Behavior and Organization

journal homepage: www.elsevier.com/locate/jebo

Marginal deterrence at work☆

Rosario Crinò[a,b,c], Giovanni Immordino[d], Salvatore Piccolo[e,*]

[a] Department of Economics and Finance, Università Cattolica del Sacro Cuore, Italy
[b] CEPR, UK
[c] CESifo, Germany
[d] Department of Economics and Statistics; and CSEF, Università Federico II di Napoli, Italy
[e] Department of Management and Economics; and CSEF, Università di Bergamo, Italy

ARTICLE INFO

Article history:
Received 11 March 2019
Revised 17 July 2019
Accepted 4 August 2019

JEL classification:
K14
K40

Keywords:
Enforcement policies
Maximum punishment
Monitoring cost
Individual-Level data

ABSTRACT

The marginal deterrence principle of law enforcement implies that penalties must be scaled according to the severity of crimes, in order to deter individuals from committing more severe offenses. In this paper, we test whether the US legal system is consistent with the rational economic model of marginal deterrence. To this purpose, we use novel and unique data on sentence length for a large sample of inmates in US correctional facilities, combined with an official ranking of crimes by severity and with proxies for the maximum possible punishment and for the cost of monitoring criminals (specifically, the cost of policing) in each US state, over a period spanning up to 50 years. We find that sentences are on average longer in states where maximum punishment is higher and monitoring cost is lower. We document that these relations are systematically stronger in states where the private benefits from crime are more heterogeneous. Finally, we show that sanctions increase relatively faster with the severity of crimes in states with higher maximum punishment and lower monitoring cost. Overall, these results point to the rational economic model of marginal deterrence as providing a reasonable description of the actual enforcement policies chosen by regulators.

1. Introduction

Since the pioneering work by Becker (1968), law enforcement has attracted considerable attention by economists and other social scientists. Notably, designing good laws might not suffice to guarantee economic growth, social cohesion, and the rise of modern democracies, in the absence of enforcement systems that deter people from breaking these laws (see, e.g., Alesina and Giuliano, 2010).

Over the past decades, several prominent scholars have 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

☆ We thank Dimitris Christelis, Giuseppe Dari-Mattiacci, Giacomo De Giorgi, Elisa Facchetti, Francesco Fasani, Giovanni Mastrobuoni, Dilip Mookherjee, Arieda Muço, Laura Ogliari, Tommaso Oliviero, Marco Pagano, Paolo Pinotti, I.P.L. Png, Jesper Roine, Francesco Russo, Annalisa Scognamiglio, and participants at numerous venues for useful comments and discussions. 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.

* Corresponding author.
E-mail addresses: rosario.crino@unicatt.it (R. Crinò), giovanni.immordino@unina.it (G. Immordino), salvatore.piccolo@unibg.it (S. Piccolo).

https://doi.org/10.1016/j.jebo.2019.08.003
0167-2681/

enough to deter people from infringing the law: the maximum punishment principle (Becker, 1968).[1] Yet, in more complex environments where agents can also choose the severity of the offense, 'hanging people for a sheep' might not be a good idea (Friedman and Sjostrom, 1993). The reason is that setting a harsher punishment for a specific offense may have the undesirable effect of enticing people to commit more severe crimes. In order to crowd out this incentive, therefore, an efficient enforcement system must scale penalties according to the severity of crimes, in such a way that individuals are deterred from committing more severe crimes by the difference (or margin) between the sanctions on these crimes and those on less severe crimes: the so called *marginal deterrence principle* (Stigler, 1970).

The design of enforcement systems based on the principle of marginal deterrence is socially desirable, because these systems induce criminals to choose relatively less severe acts, thereby lowering the overall amount of harm that criminal activities impose on the society (Polinsky and Shavell, 1998). Despite the relevance of the topic, the empirical literature surprisingly lacks a systematic analysis of the practical relevance of the marginal deterrence principle. Specifically, are regulators inspired by this principle when designing an enforcement system? In order to fill this gap, we test whether the US legal system is consistent with the rational economic model of marginal deterrence of law enforcement. Using novel and unique micro-data on US inmates over 50 years, we find that this model provides a reasonable description of the actual policies chosen by regulators and law enforcers.

To motivate the analysis, in Fig. 1, we show a number of facts about the behavior of sentences and crime rates across US states at the turn of this century.[2] States are divided into two groups, depending on whether the overall crime rate at the end of the 1990s was above or below the cross-state median. The top left graph shows that crime rates vary markedly across states. Moreover, states with high crime rates exhibit a relatively greater incidence of violent crimes compared to the other states. Explaining this large variation in crime rates is of paramount importance for economists and other social scientists. While the determinants can be numerous (see below for a review of the empirical literature), the bottom left graph suggests that the enforcement system could play a decisive role. Indeed, note that crimes with high levels of offense are punished less severely in high crime rate states than in other states, while the opposite pattern holds for crimes with low offense levels. In the right graphs, we repeat these exercises looking at the first years of this century. The classification of states is still based on the crime rates observed in the 1990s (i.e., it is the same as in the left graphs). Interestingly, US states with initially high crime rates have subsequently punished the most severe crimes more harshly than the other states, while becoming more lenient on the least severe crimes. At the same time, these states have experienced a relatively stronger reduction in the incidence of crimes, especially violent crimes.[3] These patterns are broadly consistent with the marginal deterrence principle, according to which, by graduating penalties to the severity of crimes, law enforcers could deter individuals from committing more serious offenses, thereby lowering the average level of harm inflicted by crime to the society. Moving from these facts, in this paper, we provide more direct evidence in support of the marginal deterrence principle.

For the sake of exposition, the empirical strategy will be centered around the general framework of marginal deterrence developed by Mookherjee and Png (1994), where individuals are heterogeneous with respect to the private benefit of breaking the law and thus choose different crimes — criminals with higher private benefit choose more harmful acts. Hence, like earlier contributions in the marginal deterrence literature, this model predicts that, other things being equal, penalties and severity of crimes should be positively correlated.[4] In addition, the model offers specific comparative-statics predictions concerning the relationship between the penalties received by individual law breakers and some characteristics of the enforcement system. First, the model predicts a positive correlation between individual penalties and the maximum punishment that a regulator can possibly inflict. Second, the model implies a negative correlation between individual sanctions and the cost of monitoring (i.e., detecting) criminals.[5] Finally, these relations are predicted to be stronger for more severe crimes, as well as in contexts in which criminals are more heterogeneous.

Testing these predictions requires three pieces of information. First, we need detailed data on the penalties received by individual law breakers and on the characteristics of each criminal and his crime. Second, we need proxies for the maximum possible punishment and for the cost of monitoring criminals in different enforcement systems. Third, we need an objective ranking of crimes according to their severity. A key contribution of this paper is to assemble a new data set with these characteristics. We draw individual-level data on a large sample of inmates in US prisons from the 2004 'Survey of Inmates

[1] That increasing (maximizing) expected punishment should increase (maximize) enforcement through general deterrence is something that has been debated (and contested) centuries before Becker (e.g., by Beccaria and Bentham, among others). Becker formalized part of the complex pre-existing debate on optimal deterrence. See also Landes and Posner (1975), Polinsky and Shavell (1984) and Friedman (1981), among others.

[2] Data on crime rates (number of incidences in percentage of the population) across US states come from the 'USA Counties Database'. Data on sentences (number of days of conviction per inmate) come from the 2004 'Survey of Inmates in State and Federal Correctional Facilities' of the US Bureau of Justice Statistics. This survey and the other databases used in this paper are described in grater detail both below and in Section 3.

[3] The crime rates reported at the top of each bar in Fig. 1 show that the incidence of crime has dropped in both groups of states and for all types of crimes. However, the decline has been relatively stronger in high crime rate states, especially in the case of violent crimes.

[4] See Friedman and Sjostrom (1993), Mookherjee and Png (1992), Reinganum and Wilde (1986), Shavell (1991, 1992), and Wilde (1992) for other models of marginal deterrence yielding a similar prediction.

[5] Throughout the paper, we use the same terminology as in Mookherjee and Png (1994), and refer to 'monitoring' as 'detection'. Accordingly, the expression 'cost of monitoring criminals', or 'monitoring cost', indicates the cost of detecting criminals; similarly, 'monitoring rate' stands for the rate at which crimes are detected. In the empirical analysis, we proxy for monitoring cost using measures of the cost of police activities. Accordingly, we somewhat abuse of terminology and use 'monitoring' for referring to detection of criminals by the police, i.e., as a synonym for 'policing'.

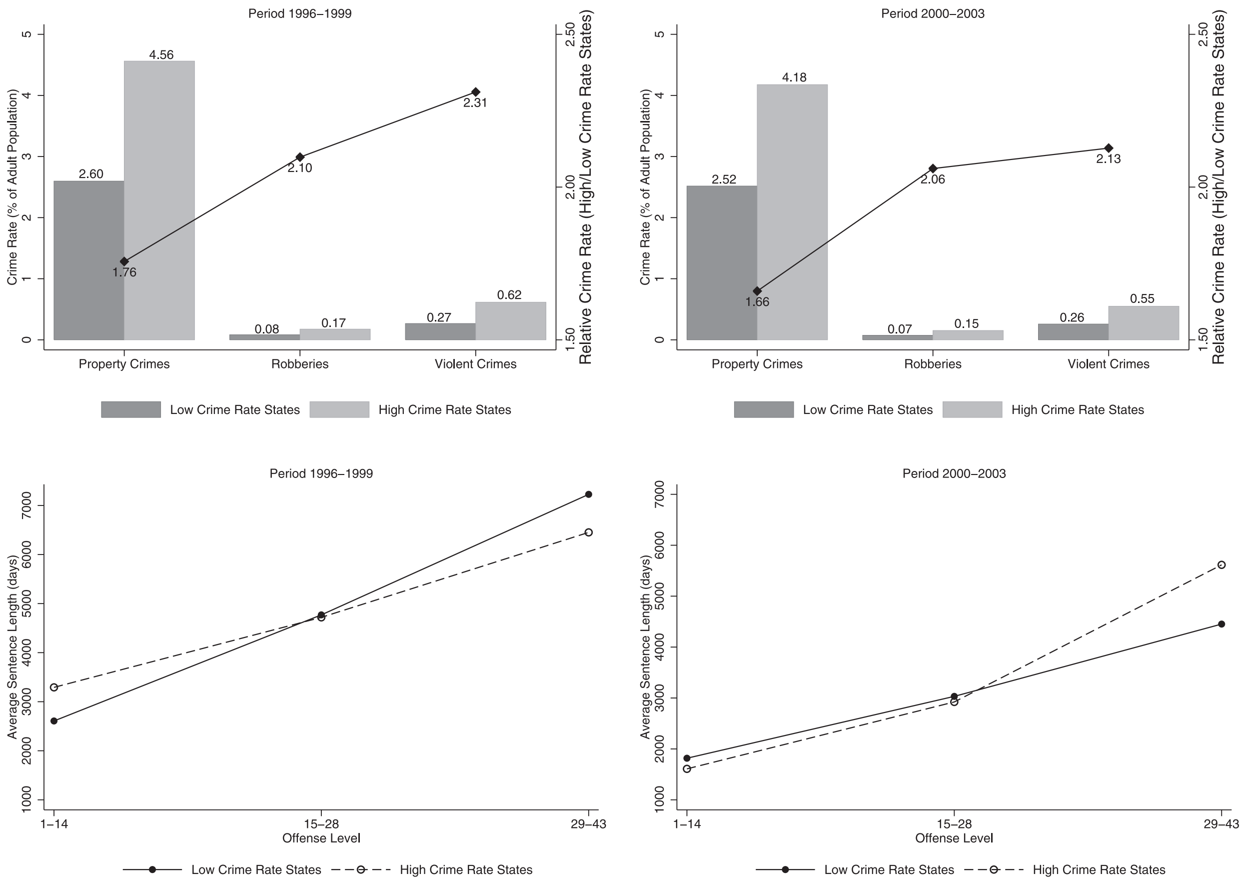

Fig. 1. Sentences and Crime Rates across US states. *Source:* 'USA Counties Database'; 'Survey of Inmates in State and Federal Correctional Facilities', 2004. The top two graphs plot the average crime rate (number of crimes in percentage of the population) over the periods 1996–1999 and 2000–2003, separately for three groups of offenses (property crimes, robberies, and violent crimes) and for US states whose overall crime rate in the 1996–1999 period was either above or below the cross-state median (high and low crime rate states, respectively). The relative crime rate indicated below a given diamond may be slightly different from the ratio between the two crime rates reported at the top of the corresponding bars due to rounding. The bottom two graphs plot the average sentence length (number of days of conviction per inmate) in high and low crime rate states, separately for three groups of crimes of increasing level of offense.

in State and Federal Correctional Facilities'. To the best of our knowledge, we are the first to use these data in economics.[6] For each individual, the survey contains information on his offense, sentence, demographic characteristics, criminal history, and other aggravating circumstances. We observe inmates who have committed crimes over a period of 50 years, from 1953 to 2003. We combine these data with information on the existence and use of the death penalty in each US state over time to build proxies for maximum punishment. To proxy for monitoring cost in different states, we use data on police wage and on the cost of gathering weapons and ammunition across police departments. Finally, we use the US 'Federal Sentencing Guidelines Manual' to obtain an official ranking of crimes by severity.

Our first step is to document how sanctions vary with the severity of crimes. The marginal deterrence principle implies that the punishment-severity schedule should be upward sloping. This prediction should be largely uncontroversial, given the general perception that penalties are designed in such a way that more severe offenses receive harsher punishments. Our results show that the punishment-severity schedule for the US is not only upward sloping but also quite steep. Specifically, the average sentence for the most severe offense ('first-degree murder') is 30 times longer than that for the least severe offense ('attempted trespassing against property'). Other characteristics of the schedule are less well understood and more interesting. In particular, the Mookherjee and Png (1994) model implies that the steepness of the punishment-severity schedule should vary across enforcement systems with different characteristics. In a first exploration of the data, we find that the steepness of the schedule does vary considerably across US states, with a difference of 23 times between the state with the steepest schedule (Oregon) and the state with the flattest schedule (Minnesota).

[6] Recent studies using these data in other disciplines, e.g., criminology, include Felson and Lane (2010), Thompson and Uggen (2012), and Porter et al. (2016).

Next, we test the main predictions of Mookherjee and Png (1994) by documenting robust patterns of correlation between individual penalties, maximum punishment, and monitoring cost in accordance with the theory.[7] We use two complementary empirical strategies exploiting different sources of variation in the data: a cross-sectional strategy, which exploits between-state variation (the main source of variability in our data), and a panel strategy, which exploits within-state variation over time and thus controls for time-invariant state characteristics. We find that sentences for the same crime type are on average longer in US states where the death penalty is in place than in other states. Our most conservative estimate implies a difference of 19% between the two groups of states, corresponding to approximately 2.4 extra years of conviction for the average inmate. Moreover, we find that sentences for the same crime type are on average longer in US states where monitoring criminals is less costly. We conservatively estimate that a one standard deviation reduction in police wage is associated with an 8% increase in sentence length, corresponding to 1 extra year of incarceration for the average inmate.

These results are obtained using highly demanding specifications, which control for: demographic characteristics of the inmates; aggravating circumstances of the offenses; time-varying proxies for economic and social conditions of the states; time-varying proxies for the demand for police force and for the quality and effectiveness of the police system in each state; year fixed effects; and both fixed effects and time trends for each crime type. While all of this largely reassures about compositional effects and obvious confounds, we also submit the baseline results to an extensive sensitivity analysis, in order to address remaining concerns with measurement, sample, and specification. We find that our main conclusions continue to hold when using additional proxies for maximum punishment and monitoring cost, as well as when employing alternative estimation samples to accommodate specific features of the data or to bring out specific aspects of the theory. Furthermore, we show that our baseline estimates are robust to controlling for heterogeneous trends in sentence length across states. Overall, this sensitivity analysis suggests that the relations between sentence length, maximum punishment, and monitoring cost detected in our data are not coincidental, but unveil robust features of the enforcement systems of US states consistent with Mookherjee and Png (1994).

We complement the baseline results with evidence on additional implications of the model. First, since the framework of Mookherjee and Png (1994) 'converges' to the single-act framework of Becker (1968) if criminals are all identical, we empirically verify that the relations between sentence length, maximum punishment, and monitoring cost are relatively stronger in US states characterized by more heterogeneous crime returns, as proxied for using income inequality. Interestingly, we find that individual penalties are uncorrelated with maximum punishment and monitoring cost when inequality is at the minimum level in our sample (corresponding to West Virginia in 2003).

Second, we study how the relations between individual penalties, maximum punishment, and monitoring cost vary with the level of severity of crimes. We find the relations to be stronger for relatively more severe crimes. This implies that the cross-state variation mentioned before in the steepness of the punishment-severity schedule is not random, but correlates with maximum punishment and monitoring cost in accordance with Mookherjee and Png (1994). Overall, our novel and varied body of evidence therefore implies 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.

Besides the work cited above, our paper connects with the literature on the determinants of crime (see Chalfin and McCrary, 2017, for a recent review). This literature has studied the role of direct policies such as the size of the police force, the incarceration rate, and capital punishment,[8] as well as the effect of more indirect factors such as abortion and gun or drug laws.[9] Unlike all these papers, we do not investigate the determinants of crime, but the characteristics of the enforcement policies meant to fight it.

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 and illustrates the punishment-severity schedules for the US. Section 4 studies the main relations between maximum punishment, monitoring cost, and sentence length. Section 5 provides evidence on additional implications of the Mookherjee and Png (1994) model. Section 6 concludes.

2. Theoretical background and empirical predictions

In order to gain the main theoretical intuitions and guide our empirical analysis, we shortly summarize the basic logic of the Mookherjee and Png (1994) model, which provides the most general environment for studying marginal deterrence of law enforcement. The model yields predictions both on the optimal pattern of sanctions that a regulator should implement for creating marginal deterrence and on how this pattern correlates with other characteristics of the enforcement system. In what follows, we provide an informal discussion of the model (Section 2.1) and highlight the main theoretical results and the empirical implications that we test in our paper (Section 2.2). A formal sketch of the model is presented in the Appendix.

[7] Regulators normally choose the overall design of the enforcement system, jointly deciding upon its different characteristics. The individual aspects of an enforcement system are therefore intertwined but their relations do not have a causal interpretation.

[8] See, among others, Cameron (1988), Cornwell and Trumbell (1994), DiTella and Schargrodsky (2004), Moody and Marvell (1996), Levitt (1996), and McCrary (2002) on the size of the police force; 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 et al. (2006) on the incarceration rate; and Alesina and La Ferrara (2014), Cohen-Cole et al. (2009), Donohue and Wolfers (2005), Ehrlich (1975, 1977), Katz et al. (2003), and Passell and Taylor (1977) on capital punishment.

[9] See, e.g., Dills and Miron (2006), Donohue and Levitt (2001, 2004, 2008), Foote and Goetz (2008), Joyce (2003, 2009), and Lott and Mustard (1997) on abortion; 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) on gun law; and Dragone et al. (2019) on drug laws.

2.1. Set-up

The economy is populated by a continuum of agents who derive heterogeneous private benefits (types) from committing illegal activities (crimes). Each agent can choose among crimes of different severity. More severe crimes produce larger private benefits to every agent. However, for given level of severity, agents with higher types derive larger total and marginal benefit than agents with lower types, implying that higher types have a relatively stronger incentive to commit more severe crimes. The level of harm imposed on the society is increasing in the severity of crimes.

The regulator is not informed about the agents' types and chooses both the rate at which the illegal acts are detected (monitoring or detection rate) and the penalties that are inflicted on crimes of different severity. The objective of the regulator is to maximize an additive utilitarian welfare function attributing equal weight to private benefits, external harms, and monitoring costs.[10] The monitoring technology is such that all acts are detected at the same rate regardless of their severity, implying that expected and actual penalties behave similarly (see, e.g., also Shavell, 1992).[11]

The set of possible penalties has an upper bound. This maximum punishment makes the problem of the regulator non-trivial because, otherwise, any desired pattern of deterrence could be achieved at minimal cost by combining an arbitrarily low monitoring rate with sufficiently large penalties. The combination of monitoring rate and penalties defines the 'enforcement policy' of the regulator. For a given enforcement policy, each individual selects the crime that maximizes his own utility, which is equal to the difference between the private benefit from committing the illegal act and the corresponding penalty.

2.2. Results and empirical implications

In the first-best benchmark, the optimal policy induces each individual to choose the crime that equalizes the marginal private benefit corresponding to his type with the marginal harm for the society. This objective could be achieved if the regulator knew the agents' types. However, when the regulator is uninformed, he cannot implement the first-best allocation because sanctions cannot be tailored to types: a standard adverse selection problem. As a result, in the second best, every type chooses a more severe crime than in the first best. The size of this distortion depends on three characteristics of the environment — i.e., the cost of monitoring, the heterogeneity in the profitability of crime across individuals, and the maximum punishment — yielding two key comparative-statics predictions that we test in the next section (see Empirical implications 2 and 3 below).

Before presenting the key predictions of the model and to gain insights on them, it is useful to first describe the economic forces behind the Mookherjee and Png (1994) model. The structure of penalties chosen by the uninformed regulator must be such that every agent is enticed to choose the crime that the society deems optimal given his type. For this purpose, the optimal penalty for each crime must be equal to the difference (margin) between the private benefit that the corresponding type obtains from committing that crime and the private benefit that he would obtain from committing less severe crimes — i.e., the standard second-best rent. Then, because higher types choose more severe crimes than lower types and derive greater private benefits from any crime, optimal penalties are increasing in the severity of crimes: the policy features marginal deterrence. It follows that:

Empirical implication 1. *Other things being equal, penalties and severity of crimes should be positively correlated (i.e., the punishment-severity schedule should be upward sloping).*

This implication is largely uncontroversial in the law and economics literature and conforms well with the general perception that penalties should be designed in such a way that more serious offenses receive harsher punishments. Earlier models of marginal deterrence (e.g., Mookherjee and Png, 1992; Friedman and Sjostrom, 1993; Shavell, 1991; Shavell, 1992; Wilde, 1992) also deliver this implication. This is not surprising, given that an upward-sloping punishment-severity schedule is necessary to create marginal deterrence. In non-economic theories of justice, such as the retributive or the incapacitation principle (Perry, 2006), increasing sanctions also help achieve goals unrelated to deterrence, such as some form of moral justice (whereby whoever commits a crime should be punished accordingly) or the prevention of future crimes (through the temporary or permanent removal of the offender from the society). In this sense, documenting a positive correlation between penalties and severity of crimes is only a necessary condition to argue that an enforcement system is coherent with the principle of marginal deterrence.[12]

[10] Enforcement normally consists of detection (monitoring), prosecution, and punishment. In the baseline version of the model, which is considered here, prosecution and punishment are treated as costless, so that the overall cost of enforcement coincides with the monitoring cost. Mookherjee and Png (1994) find that, when prosecution and punishment are also costly, the main insights of the theory are strengthened, albeit at the cost of dealing with a more complicated model.

[11] Assuming that all crimes are detected at the same rate is common in the law enforcement literature (see, e.g., Reinganum and Wilde, 1986; Shavell, 1992; Wilde, 1992). This assumption implies that the probability of apprehension (monitoring rate in Mookherjee and Png, 1994) is the same across alternative crimes. Hence, the only way of changing the expected penalty for one crime without changing the expected penalty for another crime is by altering the actual penalty. More recently, Friehe and Miceli (2014) have shown that expected and actual penalties behave similarly also when the monitoring rate is specific to the severity of each crime, if individuals choose sequentially.

[12] A positive correlation between punishment and crime is documented in most ancient legal systems, including Sharia, ancient Hebrew law, ancient Roman law, etc., and may thus not necessarily reflect marginal deterrence. A similar pattern would indeed be observed also when the sanction is chosen according to ethical, philosophical and religious norms of justice; retribution; considerations of costs of punishment for society; etc.

In order to sharpen the analysis and provide a more direct test of the marginal deterrence principle, we now discuss additional comparative-statics results specific to the model of Mookherjee and Png (1994). These results pertain to the relation between penalties and two characteristics of the legal environment: the maximum punishment that the regulator can inflict and the cost of monitoring criminals.

Recall that costly monitoring and the existence of a maximum punishment create a distortion, which leads the regulator to compel acts of greater severity compared to the first best. To understand how the maximum punishment influences the size of this distortion, consider what the regulator must do in order to reduce the severity of the crime optimally chosen by any type: the regulator must raise penalties not only for that crime, but necessarily also for all more severe crimes. However, penalties cannot be raised beyond the maximum punishment. Hence, a higher maximum punishment ensures a greater scope for deterrence (i.e., it allows the regulator to compel crimes of lower severity) through the means of higher sanctions. It follows that:

Empirical implication 2. *Other things being equal, the correlation between the maximum punishment available in an enforcement system and the penalties received by individual law breakers should be positive.*

The effect of an increase in monitoring cost is less intuitive. At a first glance, one might conjecture that by reducing the detection rate, a higher monitoring cost should induce the regulator to increase sanctions for all illegal acts, in order to compensate the lower probability of apprehension. As shown by Mookherjee and Png (1994), however, this argument fails when there is an upper bound on the feasible sanctions. Hence, in the presence of marginal deterrence, there is a second prevailing effect, whose logic is as follows: when detection becomes more costly, the regulator legalizes a larger set of actions (relative to the status quo), in order to economize on the monitoring cost; in turn, this leads to a reduction in the sanctions for all remaining illegal acts, in order to induce agents to switch from more to less severe crimes. To fix ideas, consider three acts, A (high severity), B(moderate severity) and C (low severity). Assume that all acts are illegal for a given monitoring cost, and that A is always punished with the maximum punishment. Next, consider an increase in monitoring cost, which leads the regulator to legalize C in order to economize on the cost of detection. Clearly, some criminals will switch from B to C, which benefits society. However, since the detection rate is lower, some criminals that were initially committing crime B will now find crime A more appealing and eventually switch to it, thereby harming society. What can the regulator then do? The only way to prevent the switching from B to A is to reduce the sanction associated with B (in order to make this crime more attractive), since A is already punished with the maximum penalty.[13] Hence, as a response to a higher monitoring cost, crimes C and B are punished less severely, while the sanction associated with A remains constant. In other words, a higher monitoring cost induces a downward shift in the sanction schedule (see the Appendix for a formal analysis). It follows that:

Empirical implication 3. *Other things being equal, the correlation between the cost of monitoring criminals in an enforcement system and the penalties received by individual law breakers should be negative.*

Finally, we mention two further implications of the model that our data allow to test. First, if all individuals were identical and derived the same benefits from infringing the law, the model would 'converge' to the single-act framework of Becker (1968). In this extreme case, it would be optimal for the regulator to impose the same act on every individual. As a result, the optimal structure of penalties would be to legalize all crimes up to this optimal level and to set maximum penalties for all more severe crimes. Hence, the greater is the heterogeneity in private benefits from crime, the more the optimal penalty structure deviates from that of the single-act model and fits with the general predictions of Mookherjee and Png (1994). Accordingly:

Empirical implication 4. *Other things being equal, the correlations between individual penalties, maximum punishment, and monitoring cost (as per Empirical implications 2 and 3) should be stronger in enforcement systems characterized by greater heterogeneity in private benefits from crime.*

Second, when the regulator can set penalties closer to the first best — e.g., because the maximum punishment is higher or the monitoring cost is lower — more severe crimes should be punished relatively more harshly, in order to preserve marginal deterrence. Hence, a higher maximum punishment or a lower monitoring cost are associated with higher penalties not only in absolute terms but also in marginal terms.[14] It follows that:

Empirical implication 5. *Other things being equal, the punishment-severity schedule should be relatively steeper in enforcement systems characterized by a higher maximum punishment or a lower monitoring cost.*

[13] Clearly, the reduction of the sanction associated with crime B must take into account the fact that this may also cause a switch from C to B, which is however less harmful compared to a switch from B to A.

[14] The Mookherjee and Png (1994) model also has implications for the relation of maximum punishment and monitoring cost with the monitoring rate chosen by the regulator. While our data are better suited for testing the implications of the model regarding penalties (Empirical implications 1–5), in unreported results (available upon request) we found that, consistent with the model, US states characterized by higher a maximum punishment or a lower monitoring cost also display a higher monitoring rate, as proxied by the share of policemen in total employment.

Table 1
Descriptive statistics on inmate characteristics.

	Full sample			Baseline sample		
	Mean	S.D.	Obs.	Mean	S.D.	Obs.
Length of sentence (days)	4465	6755	9327	4654	7091	8071
Age	36	11	18,185	36	11	8071
Male	0.79	0.41	18,185	0.80	0.40	8071
White	0.50	0.50	18,185	0.50	0.50	8071
Black	0.43	0.49	18,185	0.42	0.49	8071
Asian	0.01	0.10	18,185	0.01	0.11	8071
Other race	0.08	0.27	18,185	0.08	0.27	8071
Married	0.19	0.39	18,140	0.20	0.40	8058
Widowed	0.02	0.15	18,140	0.02	0.15	8058
Divorced	0.20	0.40	18,140	0.21	0.41	8058
Separated	0.06	0.23	18,140	0.05	0.22	8058
Never married	0.53	0.50	18,140	0.52	0.50	8058
Elementary school	0.12	0.32	18,013	0.12	0.32	8028
High school	0.71	0.45	18,013	0.68	0.47	8028
College	0.15	0.36	18,013	0.17	0.37	8028
Graduate school	0.02	0.15	18,013	0.03	0.18	8028
Sentenced parent	0.20	0.40	17,792	0.19	0.39	7938
US born	0.90	0.30	18,173	0.88	0.33	8070
Federal prison	0.20	0.40	18,185	0.26	0.44	8071
Served US Armed Forces	0.09	0.29	18,163	0.10	0.30	8066
Used weapon	0.21	0.41	17,756	0.24	0.42	8007
Previously in prison	0.17	0.38	17,556	0.14	0.35	7857
Used drugs	0.18	0.39	17,939	0.15	0.36	7993

Source: 'Survey of Inmates in State and Federal Correctional Facilities', 2004. The full sample consists of 18,185 inmates who were held in custody in US State or Federal prisons in the year 2004. The number of observations for a given variable may be lower than 18,185 due to missing values for that variable. The baseline sample consists of 8071 inmates who were sentenced to serve time on the date of the interview and for whom there is complete information on sentence length, state of offense, year of arrest, and type of crime. The number of observations for any other variable in the baseline sample may be lower than 8071 due to missing values for that variable.

3. Data description

We now describe the data used in the empirical analysis (Section 3.1) and document some important features of the punishment-severity schedule (Section 3.2).

3.1. Main variables

We draw information on a large sample of inmates in US prisons from the latest wave of the 'Survey of Inmates in State and Federal Correctional Facilities'. The survey was run by the Bureau of Justice Statistics on a nationally-representative sample of 18,185 inmates who were held in custody in State and Federal prisons in the year 2004.[15] For each inmate, the survey contains information on his current offense, sentence, demographic characteristics, family background, criminal history, and other aggravating circumstances.

As discussed in Section 2, the Mookherjee and Png (1994) model implies precise patterns of variation in sentence length across states, years, and crime types. To test the model predictions, we therefore need complete information on these variables. Accordingly, we focus on the 10,764 inmates who were sentenced to serve time on the date of the interview. Out of these inmates, 8071 individuals have complete information on sentence length, year of arrest, crime type, and state of offense. These inmates constitute our baseline estimation sample.[16]

Table 1 reports descriptive statistics on the individual-level variables used in our analysis. The composition of the baseline estimation sample is very similar to that of the original sample, suggesting that sentenced inmates closely resemble the population of US inmates in terms of the characteristics considered in this paper.[17] The average inmate is 36-year old and its sentence length is 4654 days (roughly 13 years). The vast majority of inmates are males (80%). The fraction of white inmates is slightly larger than that of black inmates (50% vs. 42%), whereas married and divorced individuals are equally

[15] The survey covers regular prisons and does not include special correctional facilities, such as juvenile detention centers.

[16] Out of the 10,764 individuals who were sentenced to serve time on the date of the interview, the information on sentence length is clearly unavailable for the 697 inmates charged with a death or a life sentence, and for other 740 inmates due to item non response. Of the remaining inmates, 1256 have missing information on the year of arrest, the offense level of their crimes, or the state in which the offense was committed.

[17] When regressing a dummy equal to 1 for inmates with available sentence length on our main proxies for maximum punishment and monitoring cost (introduced below), we found very small and imprecisely estimated coefficients, which further suggests that the composition of the baseline estimation sample is statistically unrelated to our main variables of interest.

Table 2
Death penalty, executions, and police wage across US states.

State	Years with Death Penalty in Place	Cumulated N. of Executions	Real Police Wage Mean	Real Police Wage S.D.
Alabama (AL)	1953–1971, 1976–2003	28	1871.1	149.6
Alaska (AK)	1953–1956	0	3192.3	437.3
Arizona (AZ)	1953–1971, 1973–2003	22	2087.8	83.6
Arkansas (AR)	1953–1971, 1973–2003	25	1620.7	73.6
California (CA)	1953–1971, 1974–2003	10	2413.8	266.4
Colorado (CO)	1953–1971, 1975–2003	1	2211.7	309.6
Connecticut (CT)	1953–1971, 1973–2003	0	2418.1	239.4
District of Columbia (DC)	1953–1971	0	–	–
Delaware (DE)	1953–1957, 1962–1971, 1975–2003	13	2237.7	157.6
Florida (FL)	1953–1971, 1973–2003	57	1824.1	175.9
Georgia (GA)	1953–1971, 1973–2003	34	1779.2	99.0
Hawaii (HI)	1953–1956	0	–	–
Idaho (ID)	1953–1971, 1977–2003	1	1968.0	329.9
Illinois (IL)	1953–1971, 1977–2003	12	2334.9	153.9
Indiana (IN)	1953–1971, 1973–2003	11	1819.9	206.3
Iowa (IA)	1953–1964	0	2289.4	244.3
Kansas (KS)	1953–1971, 1994–2003	0	1748.3	60.7
Kentucky (KY)	1953–1971, 1975–2003	2	1733.7	97.4
Louisiana (LA)	1953–1971, 1973–2003	27	1599.9	112.7
Maine (ME)	–	0	1920.0	164.2
Maryland (MD)	1953–1971, 1978–2003	3	2180.9	223.7
Massachusetts (MA)	1953–1971, 1983–1984	0	2419.1	322.5
Michigan (MI)	–	0	2393.7	121.2
Minnesota (MN)	–	0	2358.3	139.6
Mississippi (MS)	1953–1971, 1974–2003	6	1500.5	64.5
Missouri (MO)	1953–1971, 1976–2003	61	1783.5	135.9
Montana (MT)	1953–1971, 1974–2003	2	1725.5	150.1
Nebraska (NE)	1953–1971, 1973–2003	3	1694.2	83.2
Nevada (NV)	1953–1971, 1973–2003	9	2049.9	279.6
New Hampshire (NH)	1953–1971, 1991–2003	0	2022.0	210.5
New Jersey (NJ)	1953–1971, 1982–2003	0	2577.3	404.5
New Mexico (NM)	1953–1971, 1979–2003	1	1743.0	200.2
New York (NY)	1953–1971, 1995–2003	0	2542.8	211.4
North Carolina (NC)	1953–1971, 1977–2003	30	1906.7	142.9
North Dakota (ND)	1953–1971	0	1647.8	75.9
Ohio (OH)	1953–1971, 1974–2003	8	2060.2	113.1
Oklahoma (OK)	1953–1971, 1973–2003	69	1612.9	113.6
Oregon (OR)	1953–1971, 1979–2003	2	2288.2	183.8
Pennsylvania (PA)	1953–1971, 1974–2003	3	2194.2	153.2
Rhode Island (RI)	1953–1971, 1973–1983	0	2873.2	245.3
South Carolina (SC)	1953–1971, 1974–2003	28	1577.5	74.6
South Dakota (SD)	1979–2003	0	1600.5	104.8
Tennessee (TN)	1953–1971, 1974–2003	1	1680.2	134.8
Texas (TX)	1953–1971, 1974–2003	313	1825.1	100.0
Utah (UT)	1953–1971, 1973–2003	6	1884.4	106.5
Vermont (VT)	1953–1971	0	2283.9	249.3
Virginia (VA)	1953–1971, 1976–2003	89	1971.0	201.0
Washington (WA)	1953–1971, 1976–2003	4	2173.3	99.0
West Virginia (WV)	1953–1964	0	1546.4	87.4
Wisconsin (WI)	–	0	1948.2	102.4
Wyoming (WY)	1953–1971, 1977–2003	1	1739.7	206.5
United States		882	2017.4	405.1

Sources: Death Penalty Information Center; Census Bureau's 'Annual Government Finance Survey' and 'Annual Survey of Public Employment'. The cumulated numbers of capital executions refer to the period 1953–2003; the mean and standard deviation of police wage refer to the period 1982–2003. All real figures are expressed in constant 1982 US$, using the US Consumer Price Index as deflator.

numerous (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%). Finally, one-fifth of inmates have a parent who was sentenced in the past, 14% of inmates had spent some time in jail before the current sentence, 24% of inmates have used a weapon during the offense, and 15% of inmates used drugs.

Using the information on the state in which the offense was committed and on the year of arrest of each inmate, we merge the individual-level data with a number of state characteristics that are relevant to test the theory of marginal deterrence. We use information on the existence of a death penalty law (sourced from the 'Death Penalty Information Center') to construct a proxy for the maximum possible punishment in each state and year. As shown in Table 2, there is substantial

Table 3
Correlates of police wage across US states.

	(1)	(2)	(3)	(4)
Private wage	0.905***	0.940***	0.943***	0.947***
	[0.057]	[0.075]	[0.084]	[0.103]
Number of policemen		−0.034	−0.035	−0.034
		[0.023]	[0.028]	[0.027]
Crime rate			0.011	−0.005
			[0.046]	[0.042]
Relative education of policemen				0.408
				[0.367]
Arrests/offenses				0.050
				[0.152]
Felons/prisoners				0.102
				[0.104]
Detected crimes/total crimes				0.108
				[0.224]
(Detected crimes/total crimes) × (Relative education of policemen)				0.399
				[0.356]
(Detected crimes/total crimes) × (Arrests/offenses)				0.080
				[0.131]
(Detected crimes/total crimes) × (Felons/prisoners)				0.014
				[0.089]
Observations	1026	1026	1021	1021
R2	0.75	0.76	0.76	0.77

All regressions are estimated across US states over 1982–2003. The dependent variable is the log monthly wage of policemen in each state and year. The ratio of detected crimes to total crimes is the average for the US in each year. Each variable is expressed in logs. Standard errors corrected for clustering by state are reported in square brackets. ***, **, *: indicate significance at the 1, 5, and 10% level, respectively. See also notes to previous tables.

variation in death penalty across states in any given year. Specifically, four states never had the death penalty in place between 1953 and 2003; eleven states maintained the death penalty in all of these years except one; and the remaining states fall in between these two extremes. Within-state variation over time is instead more limited, because the introduction or removal of the death penalty are infrequent events in most states. For robustness, we complement the information on the existence of a death penalty law with data on the number of capital executions in each state and year (sourced as well from the 'Death Penalty Information Center'). As shown in Table 2, the cross-state variation in this variable is large, with the total number of executions between 1953 and 2003 ranging from 0 (in 19 states) to 313 (in Texas).

Our main proxy for monitoring cost is 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's 'Annual Government Finance Survey' and 'Annual Survey of Public Employment'. This variable is meant to proxy for the direct cost faced by the state for hiring an extra person in charge of monitoring criminals. As shown in Table 2, the average police wage equals US$ 2017 (in constant 1982 prices), with an overall standard deviation of US$ 405. Most of the variation is across states: Average state-level wages range from US$ 1500 in Mississipi to US$ 3192 in Alaska, and have a standard deviation of US$ 363; the latter figure exceeds most of the within-state standard deviations reported in the last column of the table.

Table 3 analyzes possible determinants of the observed variation in police wage. Column (1) reports the results of a regression of log police wage on log private wage (a proxy for overall labor costs) across US states over 1982–2003. The coefficient is close to 1 and the R^2 is equal to 0.75. Columns (2) and (3) add the log number of policemen and the log crime rate as proxies for the demand for police force in each state and year. These variables enter with very small and imprecisely estimated coefficients, and their inclusion add essentially nothing in terms of the R^2. Finally, column (4) adds a comprehensive set of controls for the quality and effectiveness of the police system in each state and year: (i) The log education of policemen relative to workers in other occupations, to account for the fact that police wage may be higher when policemen are more educated; (ii) the log ratio between the number of arrests and the number of detected crimes, since police wage may reflect the effectiveness of police officers at solving criminal cases; (iii) the log ratio between the number of sentenced criminals (people convicted of felonies) and the number of arrested criminals (people held in prison), to control for the fact that police wage may reflect the ability of police officers at collecting criminal evidence or at writing solid police reports that could lead to a sentence of conviction; and (iv) the log ratio of detected to total crimes in the US, plus its interaction with the previous three variables, since police wage may be higher in years in which detection rates are higher, especially in states where the police force is relatively more effective at fighting crime.[18] The coefficients on these variables are all small

[18] To construct the relative education of policemen in each state and year, we use individual-level data from the 'Merged Outgoing Rotation Groups' of the 'Current Population Survey' from 1982 to 2003. Following Jaeger (1997), we construct a consistent measure of education over time, by recoding the information on years of schooling and grade attended into a variable that ranges between 1 and 5, with higher values denoting higher education levels. Next, we focus on working-age individuals (aged 18–64), identify policemen using the occupation of employment of each worker, and compute average education of policemen and workers in other occupations using full-time equivalent hours of labor supply as weights (as in Autor et al., 2003). Finally, we

and statistically not significant, and the R^2 only marginally increases compared to the previous columns. Overall, these correlations suggest that the variation in police wage observed in our data mostly reflects differences in the cost of labor across US states, rather than differences in crime rates, demand for police force, and quality and effectiveness of the police system.

In some robustness checks, we complement the information on police wage with an additional variable that proxies for the cost of capital used in policing activities. Following Bove and Gavrilova (2017) and Masera (2016), we use the physical cost that police departments must face to gather military equipment from specific disposition centers of the US Government Defense Logistics Agency. As explained below, this variable varies across states but not over time.

Finally, to obtain an objective ranking of crimes according to their severity, we use the crime offense levels obtained from Chapter 2 of the 'US Federal Sentencing Guidelines Manual'. The Guidelines set rules for a uniform sentencing of individuals who are convicted of felonies and serious misdemeanors in the US Federal court system. Each crime is assigned one of 43 offense levels, with higher numbers indicating more severe offenses. We manually map the description of each inmate's offense reported in the survey (based on a classification featuring 149 crime types) to one of the 43 offense levels of the Guidelines. To illustrate the resulting mapping, 'attempted trespassing against property' receives an offense level of 1, 'marijuana or hashish smuggling' of 13, 'attempted sexual assault' of 17, 'unarmed robbery' of 21, and 'first-degree murder' of 43. The average offense level in the baseline sample is 22, corresponding to 'causing death by driving under the influence of drugs or alcohol'.

While the Guidelines apply to the Federal court system, they encompass the entire spectrum of crimes, including small offenses that typically occur in violation of State laws. The reason is that these crimes could fall under the Federal jurisdiction in some circumstances; the typical example is the violation of a State law in which all plaintiffs and all defendants are from different states ('diversity jurisdiction'). Moreover, while a system of Guidelines exists at the Federal level, less than half of the US states have their own guidelines for sanctioning violations of State laws. These state-specific guidelines are known to be vastly heterogeneous across states. The large variation in sentencing rules across states is what allows us to identify our key parameters of interest in the following sections.[19] Finally, while the Guidelines rank crimes according to their severity, they do not impose any functional relation (e.g., linearity) between severity and offense levels. This feature is convenient for us, since for testing the main predictions of the Mookherjee and Png (1994) model, we need a way of ordering crimes in increasing order of severity, but we do not need assumptions regarding how severity changes along the ranking.

3.2. Punishment-severity schedules

We now use these data to document some important features of the punishment-severity schedule for the US. As mentioned in Section 2, the principle of marginal deterrence requires that this schedule be upward sloping (Empirical implication 1). Other characteristics of the schedule, such as its steepness and heterogeneity across states, are less well understood. As discussed in Section 2, these properties are useful for testing the additional predictions of the Mookherjee and Png (1994) model. Hence, besides providing support for the first empirical implication, the patterns documented in this section will serve as building blocks for the subsequent empirical analysis, especially the one discussed in Section 5.2.

To start off, we provide a graphical illustration of the aggregate punishment-severity schedule for the US. The left graph in Fig. 2 shows the unconditional schedule, obtained by plotting the average sentence, computed across all inmates in our sample over the period 1953–2003, against the 43 offense levels. To ease the interpretation of the results, the average sentence for each offense level is divided by the average sentence for the first offense level. The size of the circles is proportional to the number of inmates in the corresponding offense levels. As expected, the schedule is positively sloped. Perhaps more interestingly, the schedule is quite steep, with crimes of offense level 43 receiving sentences that are 30 times longer than those received by crimes of offense level 1.

The right graph plots a conditional schedule to control for compositional effects. To construct this schedule, we first regress log sentence length (number of days) on: Controls for demographic characteristics of the inmates and for aggravating circumstances of the offenses; time-varying proxies for economic and social conditions of the states; time-varying proxies for the demand for police force and for the quality and effectiveness of the police system in each state; and year effects. The inclusion of these controls shortens the sample period to the years 1990–2003, as some of these variables are not available for earlier years.[20] Then, we compute the residuals from this regression, take their exponential, and plot the average of the

take the ratio between the average education of policemen and of workers in other occupations to obtain the relative education of policemen in each state and year. To construct the ratio between the number of arrests and the number of detected crimes, we aggregate at the state-year level the county-level data on arrests and offenses contained in the 'Uniform Crime Reports' (UCR) for the period 1982–2003. The data on the number of new people convicted of felonies and entering prison in each state and year are sourced from Shannon et al. (2017). Finally, we use information on victimization from the 'National Crime Victimization Survey' (NCVS), together with data on the total number of detected crimes from the UCR, to construct the ratio of detected to total crimes in the US. We construct this variable at the national level because the NCVS does not contain information on individual state of residence.

[19] When considering separate samples for State and Federal inmates, we find precise estimates for the former sample but not for the latter (see Table 7). This is consistent with the Guidelines ensuring a uniform sentencing of individuals across US states in the case of violations of Federal but not State laws.

[20] The demographic controls are: age and age squared, 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 controls for aggravating circumstances are: a dummy for use or possession of weapons during the offense, an indicator for whether the inmate has been in jail in the past, and a dummy for whether the inmate used drugs. The controls for economic and social conditions of the states are: the shares of Catholics and Muslims in the adult population, the unemployment rate, log population, and GDP; these controls are sourced from the 'USA

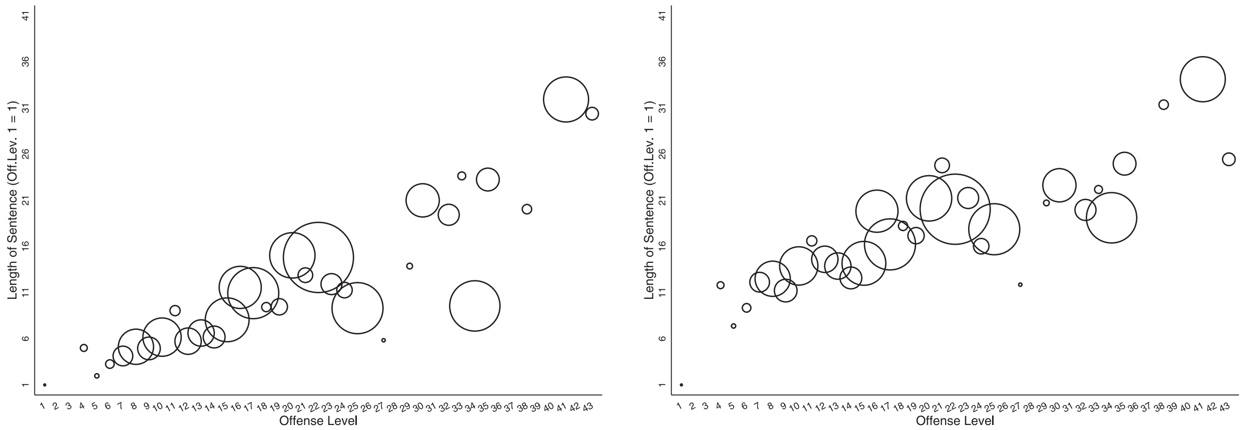

Fig. 2. Punishment-Severity Schedule for the US. The figure plots the average sentence length across all inmates against the base offense level assigned to their crime. The left graph reports the unconditional relation whereas the right graph plots the conditional schedule, obtained by controlling for: demographic characteristics of the inmates and aggravating circumstances of the offenses; economic and social conditions of the state; proxies for the demand for police force and for the quality and effectiveness of the police system in each state and year; and year fixed effects (see footnote 20 for details). The sample period is 1953–2003 in the left graph and 1990–2003 in the right graph. The size of each circle is proportional to the number of inmates in the corresponding offense level.

resulting variable against the 43 offense levels. The resulting schedule is upward sloping also in this case, and it is actually steeper than the unconditional schedule.

Next, we illustrate how the punishment-severity schedule varies across US states. We estimate state-specific, conditional, schedules by regressing, separately for each state, log sentence length on the offense level variable, controlling for demographic characteristics of the inmates, aggravating circumstances of the offenses, and a time trend. We restrict to states with at least 40 inmates to have enough degrees of freedom. The coefficients on the offense level variable indicate by how much sentence length changes in a given state for each additional level of offense, other things being equal. Hence, these coefficients provide a synthetic measure of the steepness of the schedule in each state.

Fig. 3 plots the distribution of the coefficients across states. The average coefficient equals 0.021. The coefficients are positive in all but two states, and in the vast majority of cases (29 out of 40 states) they are statistically significant despite a moderate sample size.[21] These results confirm that the punishment-severity schedule is upward sloping even if one looks at individual states. A more interesting implication of Fig. 3 is that the aggregate evidence emerging from Fig. 2 masks substantial heterogeneity across states. The estimated steepness coefficients have a standard deviation of 0.016, and range from a minimum of −0.048 to a maximum of 0.047. Even if one focuses on the positive estimates, the difference between the highest steepness (for Oregon) and the lowest steepness (for Minnesota) is a factor of 23. In Section 5.2, we will document that this variation is not random, but correlates systematically with maximum punishment and monitoring cost in accordance with Empirical implication 5.

4. Maximum penalty, monitoring cost, and sentence length

We now test the two key predictions of the Mookherjee and Png (1994) model: Empirical implications 2 and 3. These predictions pertain to the relations between sentence length, maximum punishment, and monitoring cost. As discussed in Section 2, the model predicts sentences to be longer in environments characterized by higher maximum punishment and lower monitoring cost. We start by illustrating our empirical strategies for testing these predictions (Section 4.1). Then, we present the baseline results (Section 4.2). Finally, we study the sensitivity of the baseline evidence to the use of additional proxies, alternative specifications, and various sub-samples (Section 4.3).

Counties Database', with the exception of GDP ('BEA Regional Accounts', available since 1990) and religious composition ('US Religious Landscape Survey'). The controls for the demand for police force in each state and year are: indicators for the incidence of different types of crimes (i.e., the number of violent crimes, robberies, and property crimes per inhabitant) and the log number of policemen; the crime rates are sourced from the 'USA Counties Database', while police employment is sourced from the 'Criminal Justice Expenditure and Employment Extracts'. Finally, the controls for the quality and effectiveness of the police system in each state and year are: the log relative education of policemen, the share of detected offenses cleared by arrest, the log ratio between the number of people convicted of felonies and the number of people held in prison, and the interaction of these three variables with the share of detected crimes in total committed crimes in the US (the linear term of the latter variable is absorbed by the year fixed effects); these controls are explained in details in footnote 18.

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

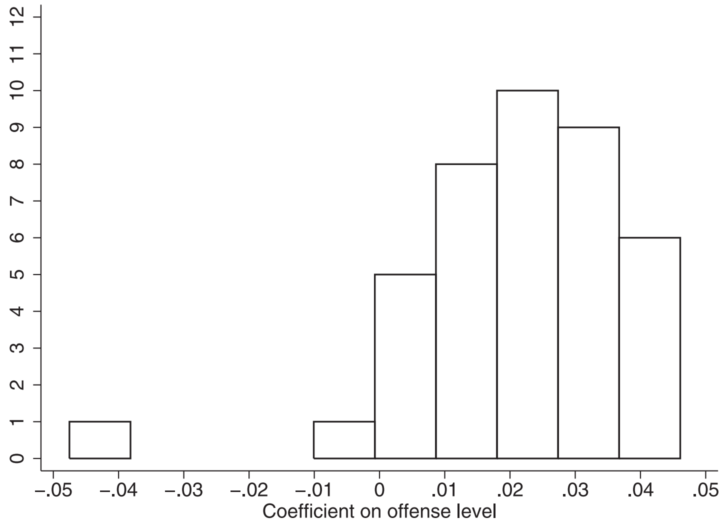

Fig. 3. Distribution of Schedule Steepness across US states. The figure plots the distribution of the steepness of the punishment-severity schedules across US states. The steepness is estimated by running, separately for each state, a regression of log sentence length on offense level, controlling for demographic characteristics of the inmates, aggravating circumstances of the offenses, and a time trend. The steepness for each state is the coefficient on the offense level variable in the corresponding regression. The sample period is 1953–2003. Only states with at least 40 inmates are considered.

4.1. Empirical strategies

The Mookherjee and Png (1994) model can be interpreted in either a cross-sectional or a time-series sense. In the cross-sectional sense, the model applies to the comparison of enforcement systems featuring different levels of maximum punishment and monitoring cost at a given point in time. In the time-series sense, the model applies to the comparison of the same enforcement system across periods characterized by different levels of maximum punishment and monitoring cost. Accordingly, the first interpretation suggests using between-state variation within a year for testing the model's predictions. The second interpretation suggests instead using within-state variation over time.

Both strategies have strengths and weaknesses. The first approach exploits cross-sectional variation, the main source of variability in our data. However, it does not control for time-invariant state characteristics, which could influence the results. By exploiting within-state variation over time, the second approach absorbs time-invariant state characteristics and cleans the results from their influence. However, time variation is relatively limited in our data, so the coefficients would be identified from changes around few specific years and could be less precise. In light of these considerations, we use both strategies for robustness.

To implement the first strategy, we estimate specifications of the following form:

$$\ln Days_{icst} = \alpha_c + \alpha_t + \beta \cdot X_{st} + \gamma \cdot Controls_{icst} + \varepsilon_{icst}. \tag{1}$$

$Days_{icst}$ is sentence length (in number of days) of inmate i, who committed a crime of offense level c in state s and was arrested in year t.[22] $Controls_{icst}$ is a vector of controls for demographic characteristics, aggravating circumstances, and state-level time-varying characteristics (details below). α_c and α_t are offense level and year fixed effects, respectively. X_{st} is a vector containing our proxies for maximum punishment and monitoring cost. Finally, ε_{icst} is an error term. In this specification, the coefficients in β are identified from the variation in sentence length across inmates who have similar observed characteristics (as captured by $Controls_{icst}$), have committed crimes of the same offense level (as captured by α_c) in the same year (as captured by α_t), and have acted in states with similar observable attributes except for maximum punishment and monitoring cost.

To implement the second strategy, we modify the specification as follows:

$$\ln Days_{icst} = \alpha_s + \alpha_c + \alpha_{dt} + \beta \cdot X_{st} + \gamma \cdot Controls_{icst} + \varepsilon_{icst}. \tag{2}$$

α_s are state fixed effects, which absorb all time-invariant state characteristics, including those potentially correlated with maximum punishment, monitoring cost, and sentence length. For instance, more conservative states may favor the death penalty and be harsher on crime more in general, which would generate a positive correlation between sentence length and death penalty even if sanctions did not follow the marginal deterrence principle. α_{dt} are Census divisions $\times$ year fixed ef-

[22] We have also experimented with specifications in which sentence length enters in levels, estimated by Poisson. The results (available upon request) were unchanged.

fects, where Census divisions are nine small groups of neighboring states defined by the US Census Bureau.[23] α_{dt} absorb all time-varying shocks hitting each cluster of neighboring states, including changes in economic conditions, social and political attitudes potentially correlated with our variables of interest. For instance, the profound structural transformation occurred in the second half of the twentieth century, by causing the dismantling of entire sectors of economic activity that were key employers in some US regions, may have influenced the level of wages and changed attitudes against crime, thereby generating a correlation between police wage and sentence length independent of marginal deterrence. Hence, the combination of α_s and α_{dt} implies that the coefficients in β are identified from deviations of maximum punishment, monitoring cost, and sentence length from their long-run averages within each state, conditioning on time-varying shocks hitting all states in a small neighbor. For both specifications, we correct the standard errors for clustering within state-year pairs and weight the regressions by the number of inmates in each state as emerging from the survey to give more weight to states with a larger number of inmates. We expect $\beta > 0$ for maximum punishment and $\beta < 0$ for monitoring cost, implying that sentences are ceteris paribus longer the higher is maximum punishment and the lower is monitoring cost.

4.2. Baseline results

The baseline estimates of Eq. (1) are reported in Table 4 and those of Eq. (2) in Table 5. The two tables have the same structure. Panel a) contains specifications in which the proxy for maximum punishment (a dummy for the existence of a death penalty law) is included alone; panel b) refers to specifications is which the proxy for monitoring cost (log police wage) is included alone; and panel c) refers to specifications in which the two proxies are jointly included. The set of control variables becomes larger in each subsequent column, as indicated at the bottom of the table and as explained below.

Column (1) reports the results of baseline specifications controlling just for the relevant fixed effects appearing in Eqs. (1) and (2). These specifications maximize sample size, as they use all relevant inmates who have committed crimes over the period 1953–2003 (panel a) or 1982–2003 (panels b and c).[24] In keeping with the key 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 police wage. The point estimates barely change when the two proxies are used jointly compared to when they are included separately, consistent with maximum punishment and monitoring cost capturing distinct aspects of the enforcement systems of US states.[25] The coefficients in Table 4 are slightly more precisely estimated than those in Table 5, in line with between-state variation being the main source of variability in our data. In terms of size, the coefficient on police wage is similar in the two tables. The death penalty coefficient is instead larger in Table 5 than in Table 4, as the former coefficient is identified by the subset of states in which the death penalty was introduced or removed during the sample period, and only from variation around those years. These differences aside, the results in the two tables are strongly consistent with one another, suggesting that our main conclusions are largely insensitive to the empirical strategy.

In the remaining columns, we include further controls for observable characteristics of inmates and states. We start, in column (2), by adding demographic controls: 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 aggravating circumstances: a dummy for use or possession of weapons during the offense; a dummy for whether the inmate used drugs; and a dummy for whether the inmate was in jail in the past. Adding these controls does not imply noteworthy changes in the main results, suggesting that the latter do not reflect differences in the composition of inmates across states.

Next, we control for a number of time-varying state-level characteristics that could be correlated with different aspects of the enforcement system of a state. In column (4), we add controls for economic and social conditions of the states: the shares of Catholics and Muslims in the adult population, to control for differences in religious composition across states; GDP and unemployment rate, to control for differences in average income across states and in economic conditions potentially correlated with criminal activities; and log population, to account for differences in state size. Adding these controls limits the sample period to the years 1990–2003. Yet, our coefficients of interest maintain their sign, remain precisely estimated, and do not exhibit significant changes in their magnitude compared to the previous column.

In column (5), we add a wealth of time-varying controls for the demand for police force and for the quality and effectiveness of the police system in each state. The controls for police demand are: the log number of policemen; and three crime rates (defined as number of offenses in percentage of the state population) for violent crimes, robberies, and property crimes, respectively. These variables account for differences in the size of the police force and in the incidence of different types of offenses across states. Both factors could drive differences in the demand for police force, and thus in police wage,

[23] In particular, the nine Census divisions are 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.

[24] Recall that information on police wage is unavailable prior to 1982.

[25] Death penalty and log police wage have a correlation of −0.12 in our data.

Table 4
Sentence length, maximum punishment, and monitoring cost: Between-state variation.

	(1)	(2)	(3)	(4)	(5)	(6)
	a) Maximum punishment					
Death penalty	0.275***	0.300***	0.299***	0.261***	0.249***	0.239***
	[0.067]	[0.065]	[0.067]	[0.062]	[0.065]	[0.062]
Observations	8071	7919	7668	7275	7080	7080
R2	0.49	0.51	0.51	0.50	0.50	0.52
	b) Monitoring cost					
Police wage	−0.512***	−0.543***	−0.560***	−0.528***	−0.407***	−0.487***
	[0.093]	[0.091]	[0.094]	[0.126]	[0.140]	[0.137]
Observations	7788	7646	7407	7080	7080	7080
R2	0.49	0.50	0.51	0.49	0.50	0.52
	c) Both variables					
Death penalty	0.187***	0.213***	0.209***	0.191**	0.214***	0.193***
	[0.068]	[0.067]	[0.068]	[0.074]	[0.067]	[0.064]
Police wage	−0.477***	−0.502***	−0.520***	−0.452***	−0.301**	−0.389***
	[0.092]	[0.089]	[0.092]	[0.132]	[0.139]	[0.137]
Observations	7788	7646	7407	7080	7080	7080
R2	0.49	0.50	0.51	0.50	0.50	0.52
Offense level fixed effects	✓	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓	✓
Demographic controls		✓	✓	✓	✓	✓
Aggravating circumstances controls			✓	✓	✓	✓
State controls (economic & social conditions)				✓	✓	✓
State controls (police demand & effectiveness)					✓	✓
Offense level-specific time trends						✓

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 the death penalty 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). *Year fixed effect* are fixed effects for the year of arrest. *Offense level fixed effect* are fixed effects for 43 categories of crimes with different offense levels. *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. *Aggravating circumstances controls* include: a dummy for use or possession of weapons during the offense; a dummy for whether the inmate spent any time in a correctional facility in the past; and a dummy for whether the inmate used drugs. *State controls (economic & social conditions)* include: the shares of Catholics and Muslims in the state adult population; the state unemployment rate; the log population of the state; and the state GDP. *State controls (police demand & effectiveness)* include: the number of violent crimes, robberies, and property crimes per state inhabitant; the log number of policemen in the state; the log relative education of policemen in the state; the share of total offenses that are cleared by arrests in the state; the log ratio between the number of people convicted of felonies and the number of people held in prison in the state; and the interaction between the latter three variables and the share of detected crimes in total crimes committed in the US in each year. *Offense level-specific time trends* are linear and quadratic time trends interacted with a full set of offense level dummies. Standard errors corrected for clustering by state-year are reported in square brackets. The regressions are weighted by the number of inmates in each state. In columns (1)-(3), the sample period is 1953–2003 (panel a) or 1982–2003 (panels b and c); in the remaining columns, the sample period is 1990–2003 in all panels. ***, **, *: indicate significance at the 1, 5, and 10% level, respectively. See also notes to previous tables.

across states. The controls for the quality and effectiveness of the police system are: the log relative education of policemen; the share of detected offenses that are cleared by arrest; the log ratio between the number of people convicted of felonies and the number of people held in prison; and the interaction between each of these variables and the share of detected crimes in total crimes committed in the US. These variables account for the fact that differences in police wage across states and over time could reflect differences in the quality and effectiveness of the police systems, for the reasons explained in Section 3.1. If regulators systematically adjusted penalties based on the strength of the police force, this would generate a correlation between police wage and individual sentences independent of marginal deterrence. Yet, we find that the inclusion of these controls has little bearing on our coefficients of interest.

Finally, in column (6), we add a linear and a quadratic time trend for each of the 43 offense levels, in order to control for heterogeneity in the time evolution of sentences across crime types. The main evidence is preserved also in this case, and the point estimates remain largely stable compared to the previous column.

Besides being precisely estimated and qualitatively consistent with the framework of marginal deterrence, the coefficients reported in Tables 4 and 5 are also quantitatively sizable. For concreteness, consider the estimates of the most complete specification reported in Table 4 (column 6, panel c), which yields the most conservative results. The coefficient on log police wage is -0.39, implying that a reduction by one standard deviation in this variable (0.21) is associated with an 8% increase in sentence length for the same type of crime. For the average inmate in our sample, this would correspond to an extra 372 days of conviction. Similarly, the death penalty coefficient equals 0.19, implying that sentences are 19% longer for

Table 5
Sentence length, maximum punishment, and monitoring cost: Within-state variation.

	(1)	(2)	(3)	(4)	(5)	(6)
	a) Maximum punishment					
Death penalty	0.384***	0.375***	0.452***	0.536***	0.481**	0.497**
	[0.121]	[0.127]	[0.134]	[0.204]	[0.218]	[0.247]
Observations	8071	7919	7668	7275	7080	7080
R2	0.54	0.56	0.56	0.53	0.52	0.55
	b) Monitoring cost					
Police wage	−0.353**	−0.347**	−0.381**	−0.402**	−0.438**	−0.475**
	[0.170]	[0.164]	[0.170]	[0.192]	[0.192]	[0.193]
Observations	7788	7646	7407	7080	7080	7080
R2	0.53	0.54	0.55	0.52	0.52	0.55
	c) Both variables					
Death penalty	0.384***	0.396***	0.491***	0.674***	0.437*	0.447*
	[0.127]	[0.135]	[0.136]	[0.207]	[0.223]	[0.254]
Police wage	−0.311*	−0.305*	−0.330*	−0.332*	−0.405**	−0.441**
	[0.171]	[0.165]	[0.171]	[0.194]	[0.199]	[0.200]
Observations	7788	7646	7407	7080	7080	7080
R2	0.53	0.54	0.55	0.52	0.52	0.55
Offense level fixed effects	✓	✓	✓	✓	✓	✓
State fixed effects	✓	✓	✓	✓	✓	✓
Census division × year fixed effects	✓	✓	✓	✓	✓	✓
Demographic controls		✓	✓	✓	✓	✓
Aggravating circumstances controls			✓	✓	✓	✓
State controls (economic & social conditions)				✓	✓	✓
State controls (police demand & effectiveness)					✓	✓
Offense level-specific time trends						✓

The dependent variable is the length of sentence, expressed in number of days and in logs. *State fixed effects* are fixed effects for the state in which the crime was committed. *Census divisions* are nine groups of neighboring states defined by the US Census Bureau (see footenote [23] for details). Standard errors corrected for clustering by state-year are reported in square brackets. The regressions are weighted by the number of inmates in each state. In columns (1)-(3), the sample period is 1953–2003 (panel a) or 1982–2003 (panels b and c); in the remaining columns, the sample period is 1990–2003 in all panels. ***, **, *: indicate significance at the 1, 5, and 10% level, respectively. See also notes to previous tables.

the same type of crime when the death penalty is in place than when it is not. For the average inmate in our sample, this would correspond to an extra 884 days, or slightly more than 2.4 years, of conviction.

4.3. Sensitivity analysis

We now assess the robustness of the previous results to the use of additional proxies, various sub-samples, and alternative specifications. As previously discussed, some of the variables used in this section have no time variation and cannot be used in the context of Eq. (2). Hence, we temporarily focus on Eq. (1), returning to Eq. (2) in the following sections.

4.3.1. Additional proxies

We start by considering other proxies for maximum punishment and monitoring cost. Regarding the former, one concern is that the death penalty dummy may not fully account for the actual differences in maximum punishment across states, because some of the states where the death penalty is in place resort only occasionally to the capital sentence. Hence, we use the number of executions in each state and year (see, e.g., Donohue and Wolfers, 2005; Katz et al., 2003) as an additional proxy for maximum punishment. While the death penalty dummy captures differences across states in the extent of the maximum possible penalty, the number of executions captures differences in the frequency with which the maximum penalty is imposed by courts.

As for monitoring cost, police wage proxies for the cost of the labor input used in policing activities. Since policing also requires capital, in the form of vehicles, weapons, ammunition, clothing, etc., we construct a second proxy that is meant to capture differences across states in the cost of gathering military equipment. Following Bove and Gavrilova (2017) and Masera (2016), we exploit a program created by the National Defense Authorization Act — the 1033 Program — which gave US police departments the possibility of obtaining military equipment from a number of disposition centers of the US Government Defense Logistics Agency (DLA). The 1033 Program was signed into law in 1996; prior to that, in 1995, a dedicated DLA office (the Law Enforcement Support Office) was created to work exclusively with law enforcement agencies under the smaller and narrower predecessor of the 1033 Program, known as the 1028 Program.[26] Under the 1033 Program, the costs

[26] The 1028 Program allowed the Secretary of Defense to transfer to Federal and State agencies military hardware that was deemed suitable for use in counter-drug activities.

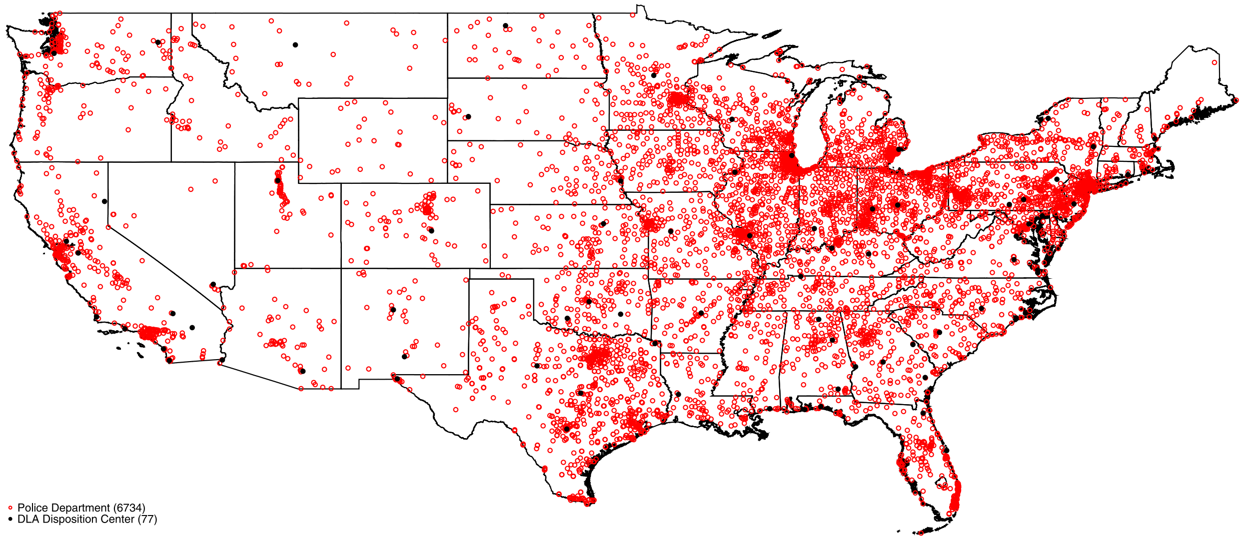

Fig. 4. Police Departments and DLA Disposition Centers. The figure plots the location of police departments (red hollow dots) and DLA disposition centers (black dots). Data for Alaska and Hawaii are available to us and used in the regressions, but are not displayed in the figure to make it more readable. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

of transporting military equipment from a DLA disposition center to a police department are borne by the department. It follows that obtaining military equipment should be ceteris paribus more expensive in states where police departments are located further away from the disposition centers. Accordingly, we start by geo-coding all the police departments in each US state and all the DLA disposition centers (see Fig. 4). Then, we compute the distance between each department and its closest disposition center. Finally, we calculate the mean or median distance across all the departments in each state. The resulting proxies vary only across states and capture differences in monitoring costs associated with the sourcing of military equipment, a main component of the capital input used in policing activities.

At this point, it is worth pausing to acknowledge a number of possible limitations of the above variables, as these limitations may influence the interpretation of the results. Regarding the number of executions, the decision to impose the death penalty is taken by a court through a sentence. Accordingly, one concern is that a positive correlation between executions and sentence length may arise mechanically, as far as states in which courts are tougher on crime both apply longer sentences on average and resort more frequently to the death penalty. Regarding the distance variables, one concern is that the distribution of DLA disposition centers across the US territory could be the result of a deliberate choice by the US Federal Government, which could have placed these centers closer to police departments in states characterized by a weaker police system. If regulators also tried to compensate this weakness by dictating tougher sanctions on all crimes in these states, this would induce a negative correlation between distance and sentence length independent of marginal deterrence. For these reasons, we limit the use of these proxies to the present sensitivity analysis.

Table 6 reports the results obtained by estimating Eq. (1) using executions and distance as proxies for maximum punishment and monitoring cost, respectively. In panel a), executions are expressed per 100,000 state inhabitants and distance is computed as the mean across all police departments in a state. In panel b), executions are expressed per 1000 state prisoners and distance is computed as the median across all police departments in a state. We start by introducing each of these variables alone (columns 1 and 2) and then use them together (column 3). In all cases, we find sentences to be longer in states characterized by a higher number of executions and by a shorter distance between police departments and DLA disposition centers. The estimates in column (3) imply that a one s.d. increase in per-capita executions (0.06) is associated with a 4.6% longer sentence, which would correspond to 216 extra days of conviction for the average inmate. A commensurate increase in per-prisoner executions (0.10) is associated with a 5.6% longer sentence, or 261 extra days of conviction relative to the average sentence. The point estimates also imply that a one s.d. decrease in average distance (0.32) or median distance (0.38) is associated with a 9.3% longer sentence, i.e., slightly more than 1 extra year of conviction for the average inmate.

Next, we use the fact that the 1033 Program was signed into law in 1996, which suggests that distance should matter for monitoring cost only afterward. Accordingly, in column (4) of Table 6, we interact the distance variables with a dummy equal to 1 in 1996 and later years, and zero otherwise. As expected, the interaction terms are negative and precisely estimated, whereas the linear coefficients, which capture the effect of distance prior to 1996, are small and not significant. A possible explanation for this result is that the significance of the interaction coefficients could be driven by the mechanical increase in the number of observations over time, due to the fact that inmates entering jail in the past are less likely than more recent inmates to be still in jail in 2004. In this respect, our most complete specification is estimated on the period 1990–2003, and therefore excludes earlier years for which the number of inmates is relatively more limited. Moreover, in Fig. 5, we estimate the same specification as in column (4) of Table 6, but this time we interact average distance (left graph) or median

Table 6
Sentence length, maximum punishment, and monitoring cost: Additional proxies.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	a) Per Capita Executions and Average Distance							
Per capita executions	0.879***		0.773***	0.799***	1.148***	0.607***	0.614***	0.904**
	[0.216]		[0.216]	[0.221]	[0.384]	[0.213]	[0.215]	[0.364]
Police dept. distance (av.)		−0.321***	−0.291***	0.029	−0.185	−0.288***	0.084	−0.094
		[0.061]	[0.060]	[0.135]	[0.159]	[0.060]	[0.141]	[0.155]
Police dept. distance (av.) * Post 1996				−0.363**	−0.292*		−0.428***	−0.362**
				[0.143]	[0.168]		[0.149]	[0.163]
Death penalty						0.148**	0.138**	0.256***
						[0.062]	[0.062]	[0.084]
Police wage						−0.376**	−0.436***	−0.497**
						[0.148]	[0.143]	[0.239]
Observations	7080	7080	7080	7080	2511	7080	7080	2511
R2	0.52	0.52	0.52	0.52	0.32	0.52	0.52	0.32
	b) Per Prisoner Executions and Median Distance							
Per prisoner executions	0.650***		0.560***	0.560***	0.760***	0.420***	0.403***	0.597***
	[0.140]		[0.136]	[0.136]	[0.199]	[0.137]	[0.136]	[0.194]
Police dept. distance (med.)		−0.283***	−0.246***	−0.025	−0.156	−0.278***	−0.001	−0.102
		[0.050]	[0.049]	[0.107]	[0.129]	[0.051]	[0.113]	[0.129]
Police dept. distance (med.) * Post 1996				−0.249**	−0.190		−0.323***	−0.258*
				[0.112]	[0.135]		[0.118]	[0.133]
Death penalty						0.142**	0.126**	0.249***
						[0.063]	[0.062]	[0.084]
Police wage						−0.440***	−0.516***	−0.542**
						[0.152]	[0.147]	[0.237]
Observations	7076	7080	7076	7076	2511	7076	7076	2511
R2	0.52	0.52	0.52	0.52	0.32	0.52	0.53	0.32
Sample period	1990-2003	1990-2003	1990-2003	1990-2003	1993-1999	1990-2003	1990-2003	1993-1999

The dependent variable is the length of sentence, expressed in number of days and in logs. *Per capita executions* is the number of capital executions in each state and year per 100,000 state inhabitants. *Per prisoner executions* is the number of capital executions in each state and year per 1000 state prisoners. *Police dept. distance* is the distance between each police department and its closest DLA disposition center: panel a) uses the log average of this measure across all police departments in a given state, whereas panel b) uses the log median. *Post 1996* is a dummy equal to one in 1996 and all subsequent years. All regressions include the same controls as in column (6) of Table 4. Standard errors corrected for clustering by state-year are reported in square brackets. The regressions are weighted by the number of inmates in each state. The sample period is 1990–2003, except in columns (5) and (8) where it is 1993–1999. ***, **, *: indicate significance at the 1, 5, and 10% level, respectively. See also notes to previous tables.

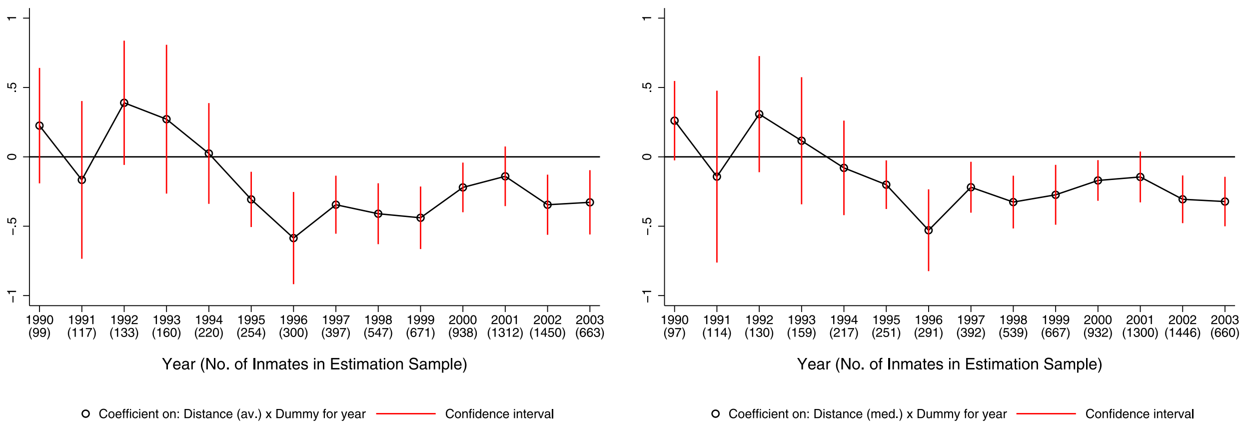

Fig. 5. Sentence Length and Distance from DLA Disposition Centers: Heterogeneity over Time. The figure plots the coefficients $\beta_{2,t}$ obtained from the following regressions: $\ln Days_{icst} = \alpha_c + \alpha_t + \beta_1 \cdot Per.\ Cap.\ Executions_{st} + \sum_{t=1990}^{2003} \beta_{2,t} \cdot Distance\ (av.)_s \cdot Y_t + \gamma \cdot Controls_{icst} + \varepsilon_{icst}$ (left graph) and $\ln Days_{icst} = \alpha_c + \alpha_t + \beta_1 \cdot Per.\ Pris.\ Executions_{st} + \sum_{t=1990}^{2003} \beta_{2,t} \cdot Distance\ (med.)_s \cdot Y_t + \gamma \cdot Controls_{icst} + \varepsilon_{icst}$ (right graph), where $Per.Cap.Executions_{st}$ and $Per.Pris.Executions_{st}$ indicate the number of per capita and per prisoner executions, respectively, in state s and year t; $Distance(av.)_s$ and $Distance(med.)_s$ are the average and median distance, respectively, between police departments in state s and their closest DLA disposition center; Y_t denotes a dummy equal to 1 in year t and zero otherwise; and $Controls_{icst}$ is a vector including the same control variables as in column (6) of Table 4. The regressions are weighted by the number of inmates in each state. The 90% confidence intervals are based on standard errors corrected for clustering by state-year. The sample period is 1990–2003.

distance (right graph) with a full set of year dummies, rather than with a single post-1996 dummy. This exercise allows us to study how the correlation between sentence length and distance varies over time, and to compare this correlation across contiguous years for which differences in the number of observations are moderate.[27] The results show that the correlation is highly imprecisely estimated and volatile around zero in the first part of the period, but then it drops and plateaus around a negative and precisely estimated value after 1996.[28] In addition, in column (5) of Table 6, we estimate the same specification as in column (4), but this time we further restrict the estimation sample to a narrow window of three years around 1996. This exercise excludes the initial and final sample periods, among which differences in the number of observations are relatively more pronounced. While estimates are somewhat less precise due to the large drop in sample size, the qualitative pattern of results is preserved.

Finally, in columns (6)-(8), we estimate the same specifications as in columns (3)–(5), but we now add death penalty and police wage among the regressors. Reassuringly, we find no noteworthy change in the coefficients on our main proxies compared to the baseline estimates. At the same time, the coefficients on the additional proxies are also largely unaffected. Aside from the limitations of the additional proxies discussed above, this pattern suggests that the aspects of the enforcement system that these variables capture tend as well to correlate with sentence length in accordance with the predictions of the marginal deterrence framework.

4.3.2. Alternative samples

We now study the robustness of the baseline results to the use of alternative samples. The results are reported in Table 7. In panel a), we use the main proxies for maximum punishment and monitoring cost, in panel b) we use the additional proxies introduced in Section 4.3.1, and in panel c) we include both sets of variables in the same specification.[29] In column (1), we exclude the 1% of inmates with the longest sentence, exceeding 75 years. One may be concerned that sentences in this range result from reporting errors or exceptional aggravating circumstances, which might not be well captured by our control variables. Yet, excluding these sentences leaves the point estimates essentially unchanged. In column (2), we restrict the sample to inmates who committed crimes in their own state of residence (78% of inmates in our sample). It might be the case that the information on the state of offense is imprecisely recorded for individuals who migrated to perform their illegal activities. Yet, excluding these inmates from the sample does not cause any noteworthy change in the results.

In column (3), we restrict to inmates who are sentenced for a single crime (75% of inmates in our sample). In the case of inmates with more than one count, we have information on the overall sentence, so the sentence length of these inmates might not be fully comparable with that of single-count inmates. However, our main coefficients barely change when excluding individuals with multiple counts. In column (4), we account for recidivism by limiting the sample to the 85% of individuals who have never been in jail before. This also addresses the concern that our criminal record control (a dummy for having been in jail in the past) might be coarse. The point estimates obtained on this sub-sample are close to those obtained on the whole sample. In column (5), we instead exclude inmates who have committed crimes of the highest level of offense (level 43). This exercise accounts for the possibility that the baseline sample might include individuals who could alternatively have been sentenced to life or death. We find no substantial difference from the baseline estimates also in this case.[30]

The Mookherjee and Png (1994) model is a rational choice model, in which individuals decide which illegal act to commit to maximize utility. Accordingly, one concern with our results so far is that they could be driven by crimes that do not reflect a rational choice by the inmate, that is, crimes that the inmate may have committed without a prior assessment of their private costs and benefits. To allay this concern, in column (6), we exclude inmates committing non-premeditated crimes,

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[27] In particular, the left graph plots the coefficients $\beta_{2,t}$ obtained from the following regression:

$$\ln Days_{icst} = \alpha_c + \alpha_t + \beta_1 \cdot Per.\ Cap.\ Executions_{st} + \sum_{t=1990}^{2003} \beta_{2,t} \cdot Distance\ (av.)_s \cdot Y_t + \gamma \cdot Controls_{icst} + \varepsilon_{icst}.$$

Similarly, the right graph plots the coefficients $\beta_{2,t}$ obtained from the following specification:

$$\ln Days_{icst} = \alpha_c + \alpha_t + \beta_1 \cdot Per.\ Pris.\ Executions_{st} + \sum_{t=1990}^{2003} \beta_{2,t} \cdot Distance\ (med.)_s \cdot Y_t + \gamma \cdot Controls_{icst} + \varepsilon_{icst}.$$

$Per.Cap.Executions_{st}$ and $Per.Pris.Executions_{st}$ indicate the number of per capita and per prisoner executions, respectively, in state s and year t; $Distance(av.)_s$ and $Distance(med.)_s$ are the average and median distance, respectively, between police departments in state s and their closest DLA disposition center; Y_t denotes a dummy equal to 1 in year t and zero otherwise; and $Controls_{icst}$ include the same set of control variables as in previous specifications (see, e.g., column 6 of Table 4). The regressions are weighted by the number of inmates in each state, and the confidence intervals are based on standard errors corrected for clustering by state-year.

[28] The marginally significant coefficient for 1995 is consistent with the fact that this year coincides with the extension of the 1028 Program to all law enforcement agencies. Accordingly, using a post-1995 dummy rather than a post-1996 dummy in column (4) of Table 6 would yield an even starker difference in the correlations between the first and the second part of the sample period (available upon request).

[29] In the interest of space, in panels b) and c), we use per capita executions and the average distance of police departments from the DLA disposition centers. Using per prisoner executions and the median distance yields similar results (available upon request).

[30] In untabulated regressions, we found similar results when excluding the following states: (1) Alaska and Hawaii, the two states in which the death penalty was in place for the smallest number of years; (2) Texas and Virginia, the two states with the highest number of cumulated executions over the sample period; (3) Maine and North Dakota, the two states with the smallest number of inmates; and (4) Texas and Florida, the two states with the largest number of prisoners.

Table 7
Sentence length, maximum punishment, and monitoring cost: Alternative samples.

	No Large Sentences (1)	Off. in State of Residence (2)	Single Count (3)	No Jail Before (4)	No Murder (5)	No Non-Prem. Crimes (Strict) (6)	No Non-Prem. Crimes (Broad) (7)	Economic Crimes (8)	Federal Inmates (9)	State Inmates (10)
	a) Main variables									
Death penalty	0.177***	0.168**	0.242***	0.165**	0.198***	0.208***	0.190***	0.089	−0.093	0.228***
	[0.063]	[0.066]	[0.072]	[0.067]	[0.065]	[0.065]	[0.069]	[0.076]	[0.103]	[0.076]
Police wage	−0.401***	−0.445***	−0.494***	−0.408***	−0.379***	−0.366***	−0.350**	−0.294*	−0.199	−0.590***
	[0.138]	[0.145]	[0.159]	[0.133]	[0.137]	[0.140]	[0.148]	[0.161]	[0.198]	[0.163]
Observations	7036	5536	5275	6107	7050	6706	5247	3841	1823	5257
R2	0.52	0.53	0.54	0.52	0.52	0.52	0.50	0.54	0.62	0.50
	b) Additional proxies									
Per capita executions	0.710***	1.012***	0.804***	0.738***	0.776***	0.785***	0.882***	0.784***	0.097	1.104***
	[0.209]	[0.231]	[0.238]	[0.226]	[0.218]	[0.226]	[0.252]	[0.280]	[0.298]	[0.306]
Police dept. distance (av.)	−0.278***	−0.254***	−0.351***	−0.304***	−0.296***	−0.296***	−0.300***	−0.270***	0.028	−0.383***
	[0.058]	[0.064]	[0.071]	[0.062]	[0.061]	[0.063]	[0.065]	[0.079]	[0.076]	[0.075]
Observations	7036	5536	5275	6107	7050	6706	5247	3841	1823	5257
R2	0.52	0.53	0.54	0.53	0.52	0.52	0.50	0.54	0.62	0.50
	c) All variables									
Death penalty	0.134**	0.128**	0.187***	0.114*	0.152**	0.161**	0.143**	0.047	−0.094	0.167**
	[0.062]	[0.064]	[0.070]	[0.064]	[0.062]	[0.063]	[0.067]	[0.074]	[0.102]	[0.073]
Police wage	−0.394***	−0.392**	−0.508***	−0.407***	−0.365**	−0.354**	−0.332**	−0.283*	−0.195	−0.554***
	[0.150]	[0.161]	[0.165]	[0.146]	[0.147]	[0.152]	[0.156]	[0.160]	[0.206]	[0.166]
Per capita executions	0.538***	0.839***	0.579**	0.561**	0.615***	0.624***	0.723***	0.663**	0.043	0.845***
	[0.202]	[0.231]	[0.228]	[0.223]	[0.215]	[0.225]	[0.255]	[0.280]	[0.306]	[0.294]
Police dept. distance (av.)	−0.279***	−0.256***	−0.355***	−0.308***	−0.291***	−0.291***	−0.301***	−0.287***	−0.002	−0.384***
	[0.058]	[0.063]	[0.072]	[0.061]	[0.060]	[0.063]	[0.065]	[0.079]	[0.073]	[0.074]
Observations	7036	5536	5275	6107	7050	6706	5247	3841	1823	5257
R2	0.52	0.53	0.55	0.53	0.52	0.52	0.51	0.54	0.62	0.51

The dependent variable is the length of sentence, expressed in number of days and in logs. Column (1) excludes inmates with sentences longer than 75 years. Column (2) restricts to inmates who have committed the offense in their state of residence. Column (3) restricts to inmates with a single count. Column (4) excludes inmates who have been in jail before. Column (5) excludes inmates who have committed crimes of offense level 43. Columns (6) and (7) excludes inmates who have committed crimes that may have been non-premeditated, using a strict and a broad definition of such crimes, respectively (see footnotes [31] and [32]). Column (8) restricts to inmates who have committed economic crimes. Column (9) restricts to inmates of Federal prisons. Column (10) restricts to inmates of State prisons. All regressions include the same controls as in column (6) of Table 4. Standard errors corrected for clustering by state-year are reported in square brackets. The regressions are weighted by the number of inmates in each state. The sample period is 1990–2003. ***, **, *: indicate significance at the 1, 5, and 10% level, respectively. See also notes to previous tables.

using a strict definition of non-premeditation that mostly encompasses accidental law violations.[31] In column (7), we use a broader definition of non-premeditated crimes, and further exclude inmates committing offenses that may sometimes be driven by irrational impulses (such as anger, addiction, etc.) or the result of negligence.[32] Excluding these inmates has virtually no bearing on the coefficients. In column (8), we perform a third, complementary, exercise, by restricting the sample to inmates who are sentenced for economic crimes, that is, illegal acts that are perpetrated with the aim of obtaining a financial, property, or personal advantage. These crimes are more likely than other offenses to reflect a rational choice by the individual. Moreover, the assumption of the Mookherjee and Png (1994) model, according to which more severe crimes yield higher private benefits than less severe crimes (see Section 2.1), is more likely to hold for economic crimes than for other offenses. At the same time, restricting the sample to economic crimes may be problematic for an empirical test of the marginal deterrence principle. Indeed, some of the inmates who are sentenced for a non-economic crime (e.g., a homicide), and are thus excluded from the sample in column (8), may have committed such an offense during the perpetration of an economic crime (e.g., a robbery), if the enforcement system was unable to achieve marginal deterrence. Hence, by focusing on economic crimes, we are forced to exclude offenses that individuals may have deemed rational to perpetrate in the light of marginal deterrence. Despite this caveat, and even though the number of observations almost halves, our evidence is qualitatively preserved. The point estimates also remain in the same ballpark as our baseline coefficients, with statistical significance been somewhat reduced in line with the above considerations.

Finally, in columns (9) and (10), we show that our results hold strong in the sample of inmates of State prisons, but are not present in the sample of inmates of Federal prisons. This pattern is consistent with the fact that, for Federal of-

[31] These crimes are: manslaughter; hit and run driving; and non-premeditated aggression, reception of stolen property, and drug or alcohol related offenses.

[32] The additional crimes excluded in column (7) are: murder; sexual assault or misconduct; property destruction; possession of drugs; driving under the influence of drugs or alcohol; and obstruction of justice.

Table 8
Sentence length, maximum punishment, and monitoring cost: Underlying trends.

	Unempl. Rate (1)	Pop. Size (2)	Violent Crime Rate (3)	Police. Educ. (4)	Arrests/ Offenses (5)	Felons/ Prisoners (6)
	a) Main variables					
Death penalty	0.226***	0.210***	0.188***	0.187***	0.181***	0.169***
	[0.068]	[0.065]	[0.061]	[0.066]	[0.062]	[0.065]
Police wage	−0.344**	−0.445***	−0.368**	−0.432***	−0.482***	−0.555***
	[0.138]	[0.132]	[0.145]	[0.143]	[0.136]	[0.141]
Observations	7080	7080	7080	7080	7080	7080
R2	0.52	0.52	0.53	0.52	0.52	0.53
	b) Additional proxies					
Per capita executions	0.730***	0.863***	0.648***	0.806***	0.787***	1.012***
	[0.222]	[0.215]	[0.249]	[0.222]	[0.248]	[0.277]
Police dept. distance (av.)	−0.298***	−0.267***	−0.231***	−0.269***	−0.289***	−0.268***
	[0.058]	[0.056]	[0.064]	[0.058]	[0.058]	[0.060]
Observations	7080	7080	7080	7080	7080	7080
R2	0.52	0.53	0.53	0.52	0.52	0.53
	c) All variables					
Death penalty	0.180***	0.165***	0.153**	0.136**	0.135**	0.109*
	[0.066]	[0.063]	[0.060]	[0.064]	[0.060]	[0.063]
Police wage	−0.347**	−0.404***	−0.379**	−0.420***	−0.500***	−0.553***
	[0.149]	[0.146]	[0.150]	[0.153]	[0.143]	[0.145]
Per capita executions	0.568***	0.678***	0.476**	0.590***	0.616***	0.657***
	[0.218]	[0.216]	[0.240]	[0.218]	[0.230]	[0.245]
Police dept. distance (av.)	−0.291***	−0.253***	−0.226***	−0.274***	−0.296***	−0.284***
	[0.058]	[0.056]	[0.064]	[0.056]	[0.057]	[0.060]
Observations	7080	7080	7080	7080	7080	7080
R2	0.53	0.53	0.53	0.53	0.53	0.53

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 first year value of the characteristic indicated in the column's heading. All regressions include the same controls as in column (6) of Table 4. Standard errors corrected for clustering by state-year are reported in square brackets. The regressions are weighted by the number of inmates in each state. The sample period is 1990–2003. ***, **, *: indicate significance at the 1, 5, and 10% level, respectively. See also notes to previous tables.

fenses, sentences are set by the Federal Guidelines and are therefore largely uniform across states, leaving little variation for identification.

4.3.3. Alternative specifications

Our baseline specifications control for the most likely potential confounders, such as inmate characteristics, aggravating circumstances of the offenses, heterogeneous trends across crime types, and both time-varying and time-invariant state characteristics. As a final sensitivity check, we expand the set of controls to account for heterogeneous trends across states. To this purpose, in Table 8, we add interactions between the year dummies and the initial value of the state characteristic named in each column's heading: unemployment rate (column 1), population size (column 2), violent crime rate (column 3), relative education of policemen (column 4), share of detected offenses that are cleared by arrest (column 5), and ratio between the number of people convicted of felonies and the number of people held in prison (column 6). This exercise aims at ruling out the possibility that our results be driven by sentences following a different evolution across states characterized by different initial conditions, independent of the enforcement policies enacted by regulators during the sample period. The coefficients are similar to the baseline estimates, suggesting that the latter do not reflect heterogeneous trends across states.

5. Additional evidence

In this section, we corroborate the previous results by providing evidence on the additional predictions of the Mookherjee and Png (1994) model. First, the relations documented above should vary in size across states characterized by different degrees of individuals' heterogeneity: Empirical implication 4 (Section 5.1). Second, the relations should also vary across crimes of different offense levels: Empirical implication 5 (Section 5.2).

5.1. Heterogeneity across states

As mentioned in Section 2, the marginal deterrence framework of Mookherjee and Png (1994) converges to the single-act framework of Becker (1968) if criminals are all equal. Instead, a greater heterogeneity in the private benefits from crime

Table 9
Heterogeneity across states.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	a) Between-State variation				b) Within-State variation			
Death penalty	0.309***		0.279***	0.305***	0.265		0.226	0.481
	[0.064]		[0.065]	[0.066]	[0.308]		[0.314]	[0.332]
Death penalty * Theil	0.894**		1.131***	1.205***	1.646**		1.496*	1.655**
	[0.354]		[0.345]	[0.360]	[0.797]		[0.801]	[0.817]
Police wage		−0.464***	−0.412***	−0.387***		−0.317	−0.320	−0.337
		[0.135]	[0.142]	[0.144]		[0.209]	[0.213]	[0.218]
Police wage * Theil		−1.080**	−1.175**	−1.232**		−2.302***	−2.103**	−2.411***
		[0.521]	[0.532]	[0.581]		[0.890]	[0.897]	[0.921]
Theil	−0.938**	9.141**	8.669*	9.046*	−1.504*	19.156***	16.013**	18.377**
	[0.388]	[4.374]	[4.504]	[4.914]	[0.910]	[7.376]	[7.567]	[7.764]
Observations	7080	7080	7080	6706	7080	7080	7080	6706
R2	0.52	0.52	0.52	0.52	0.55	0.55	0.55	0.55
Sample	All Inmates	All Inmates	All Inmates	No Non-Prem. Crimes (Strict)	All Inmates	All Inmates	All Inmates	No Non-Prem. Crimes (Strict)

The dependent variable is the length of sentence, expressed in number of days and in logs. *Theil* is the Theil index of income inequality in each state and year. All regressions include the same controls as in column (6) of Table 4. Standard errors corrected for clustering by state-year are reported in square brackets. The regressions are weighted by the number of inmates in each state. The sample period is 1990–2003. Columns (4) and (8) exclude inmates who committed crimes that may have been non-premeditated. ***, **, *: indicate significance at the 1, 5, and 10% level, respectively. See also notes to previous tables.

implies that the optimal penalty structure deviates from that of the single-act model and fits with the general predictions of Mookherjee and Png (1994). Accordingly, the relations between sentence length, maximum punishment, and monitoring cost should be stronger the more heterogeneous are the private benefits from crime.

To test this prediction, we augment the baseline specifications as follows:

$$\ln Days_{icst} = \alpha_c + \alpha_t + \beta_1 \cdot X_{st} + \beta_2 \cdot Inequal_{st} + \beta_3 \cdot X_{st} \cdot Inequal_{st} + \gamma \cdot Controls_{icst} + \varepsilon_{icst} \tag{3}$$

and

$$\ln Days_{icst} = \alpha_s + \alpha_c + \alpha_{dt} + \beta_1 \cdot X_{st} + \beta_2 \cdot Inequal_{st} + \beta_3 \cdot X_{st} \cdot Inequal_{st} + \gamma \cdot Controls_{icst} + \varepsilon_{icst}, \tag{4}$$

where $Inequal_{st}$ is income inequality, which proxies for heterogeneity in the private benefits from crime in each state and year. 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. Panel a) refers to the estimates of Eq. (3) using between-state variation, whereas panel b) refers to the estimates of Eq. (4) using within-state variation. In columns (1)–(3) and (5)–(7), we consider all inmates, whereas in columns (4) and (8) we restrict to inmates who have committed non-premeditated crimes. The sample period is 1990–2003 in all columns. Income inequality is proxied by the Theil index computed by Frank (2009) and Frank et al. (2015) for each state and year. We normalize the index by subtracting its sample mean, so that β_1 measures the relations for the average level of inequality in the sample.

Note that the interaction coefficients are always correctly signed, precisely estimated, and large, implying that the relations between sentence length, death penalty, and police wage become stronger as income inequality increases. Quantitatively, the overall coefficient on police wage (computed as $\beta_1 + \beta_3 \cdot Inequal_{st}$) ranges from 0.157 (s.e. 0.304) at the lowest level of the Theil index in the sample (West Virginia, in 2003) to −1.299 (s.e. 0.415) at the highest level (Connecticut, in 2000). The death penalty coefficient varies instead from −0.269 (s.e. 0.168) to 0.893 (s.e. 0.209). Interestingly, the coefficients are small and statistically not significant at the lowest level of inequality, consistent with the Mookherjee and Png (1994) model converging to the single-act framework as heterogeneity becomes small. More generally, the coefficients increase rapidly with income inequality, suggesting that heterogeneity in private benefits from crime is indeed an important mediator of the relations between penalties, maximum punishment, and monitoring cost, as predicted by the theory of marginal deterrence.

5.2. Heterogeneity across offense levels

In the marginal deterrence setting, when the regulator increases penalties, she must do so relatively more for more severe crimes, in order to deter individuals from committing relatively more serious offenses. Accordingly, the relations between sentence length, maximum punishment, and monitoring cost should hold not only on average but also in marginal terms, i.e., the relations should be stronger for crimes of higher offense levels. In this section, we use two complementary strategies for testing this prediction. First, we employ a descriptive approach and document that sentences increase faster with the offense level (i.e., the punishment-severity schedule is steeper) in states where the death penalty is in place or police wage is lower. Second, we turn to regression analysis and show that the coefficients on death penalty and police wage in a regression for sentence length are systematically larger for crimes of higher offense levels.

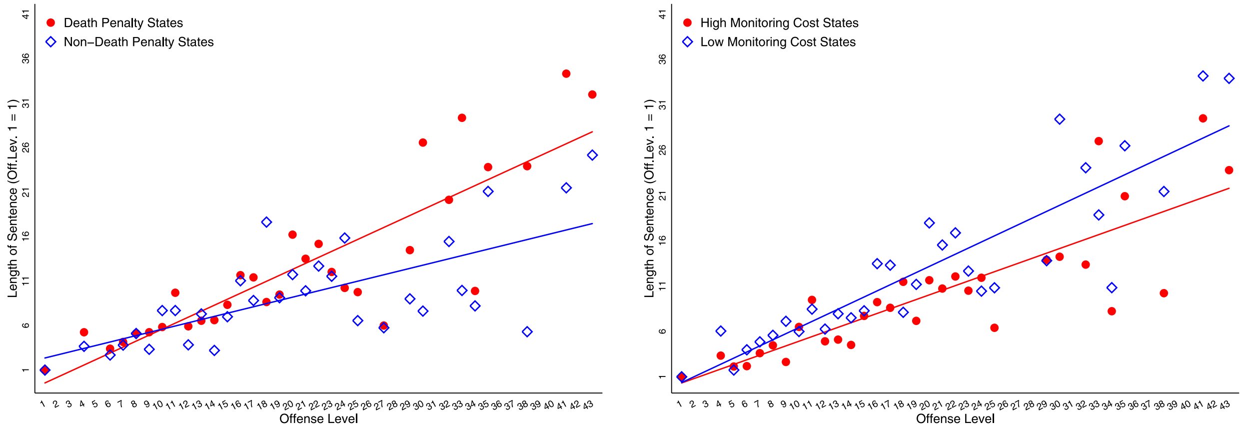

Fig. 6. Heterogeneity across Offense Levels: Punishment-Severity Schedules. The figure plots the average sentence length across all inmates against the base offense level assigned to their crime, separately for death penalty and non-death penalty states (left graph) and for high monitoring cost and low monitoring cost states (right graph). Death penalty states are states in which the death penalty was in place for a number of years greater than the cross-state mean. High monitoring cost states are states in which the average police wage is above the cross-state mean.

As shown in Section 3.2, the steepness of the punishment-severity schedules varies markedly across US states. To study the correlates of this variation non parametrically, we plot separate punishment-severity schedules for states with different levels of maximum punishment and monitoring cost. In particular, we classify each state according to whether the number of years in which it maintained the death penalty is above or below the sample mean (death penalty vs. non-death penalty states) and according to whether average police wage in the state is above or below the sample mean (high monitoring cost vs. low monitoring cost states).

Fig. 6 plots the punishment-severity schedules for death penalty vs. non-death penalty states (left graph) and for low vs high monitoring cost states (right graph). The figure shows that sentence length increases faster with the offense level in death penalty states than in non-death penalty states, as well as in low monitoring cost states compared to high monitoring cost states. In death penalty states, average sentences for crimes of offense level 43 are 32 times higher than those for crimes of offense level 1; the difference drops to 25 in non-death penalty states. Similarly, the difference in average sentences between crimes of offense level 43 and crimes of offense level 1 is 34 times in low monitoring cost states and 24 times in high monitoring cost states. Hence, consistent with the theory of marginal deterrence, the punishment-severity schedule is substantially steeper in states where maximum punishment is higher and monitoring cost is lower.

Next, we turn to regression analysis and study how the relations between sentence length, maximum punishment, and monitoring cost vary across crimes of different offense levels. To this purpose, we modify the baseline specifications as follows:

$$\ln Days_{icst} = \alpha_c + \alpha_t + \beta_1 \cdot X_{st} + \beta_2 \cdot X_{st} \cdot OffLev_c + \gamma \cdot Controls_{icst} + \varepsilon_{icst} \tag{5}$$

and

$$\ln Days_{icst} = \alpha_s + \alpha_c + \alpha_{dt} + \beta_1 \cdot X_{st} + \beta_2 \cdot X_{st} \cdot OffLev_c + \gamma \cdot Controls_{icst} + \varepsilon_{icst}, \tag{6}$$

where $OffLev_c$ ranges from 1 to 43 across crimes.[33] If $\beta_2 > 0$ when X_{st} is the death penalty dummy, then a higher maximum punishment is associated with longer 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 if $\beta_2 < 0$ when X_{st} is police wage.

The results are reported in Table 10, where panel a) shows the estimates of Eq. (5) and panel b) the estimates of Eq. (6). In columns (1)-(3) and (5)-(7), we consider all inmates, whereas in columns (4) and (8) we restrict to inmates who have committed non-premeditated crimes. The sample period is 1990–2003 in all columns. Note that the interaction coefficients are all correctly signed and precisely estimated, implying that the relations between sentence length, maximum punishment, and monitoring cost are stronger for crimes of higher offense levels. Using the point estimates reported in columns (3) and (7), Fig. 7 plots the overall coefficients on the death penalty dummy and police wage against the offense level (solid red line), together with confidence intervals (dashed black lines). The size of the coefficients grows sharply with the offense level of the crime. For death penalty, the coefficient is generally small and imprecisely estimated for crimes of offense levels below 20, but reaches a precisely estimated 0.45 for crimes of offense level 43. Similarly, the coefficient on police wage is generally small and statistically not significant for offense levels below 15, but reaches a precisely estimated −0.86 for crimes

[33] The linear term in $OffLev_c$ is subsumed in the offense level fixed effects (α_c). We have also estimated richer specifications including the interactions between X_{st} and higher-order terms in $OffLev_c$. The coefficients on these interactions (available upon request) turned out to be small and imprecisely estimated.

Table 10
Heterogeneity across Offense Levels: Estimation results.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	a) Between-State variation				b) Within-State variation			
Death penalty	−0.108		−0.111	−0.146	0.146		0.194	0.371
	[0.123]		[0.130]	[0.137]	[0.273]		[0.284]	[0.340]
Death penalty * Offense lev.	0.015***		0.013***	0.015***	0.012***		0.009*	0.011**
	[0.005]		[0.005]	[0.005]	[0.005]		[0.005]	[0.005]
Police wage		0.030	0.044	0.035		0.117	0.119	0.057
		[0.222]	[0.228]	[0.248]		[0.270]	[0.279]	[0.295]
Police wage * Offense lev.		−0.025***	−0.021**	−0.020**		−0.027***	−0.025***	−0.024**
		[0.008]	[0.009]	[0.010]		[0.009]	[0.009]	[0.010]
Observations	7080	7080	7080	6706	7080	7080	7080	6706
R2	0.52	0.52	0.52	0.52	0.55	0.55	0.55	0.55
Sample	All Inmates	All Inmates	All Inmates	No Non-Prem. Crimes (Strict)	All Inmates	All Inmates	All Inmates	No Non-Prem. Crimes (Strict)

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. All regressions include the same controls as in column (6) of Table 4. Standard errors corrected for clustering by state-year are reported in square brackets. The regressions are weighted by the number of inmates in each state. The sample period is 1990–2003. Columns (4) and (8) exclude inmates who committed crimes that may have been non-premeditated. ***, **, *: indicate significance at the 1, 5, and 10% level, respectively. See also notes to previous tables.

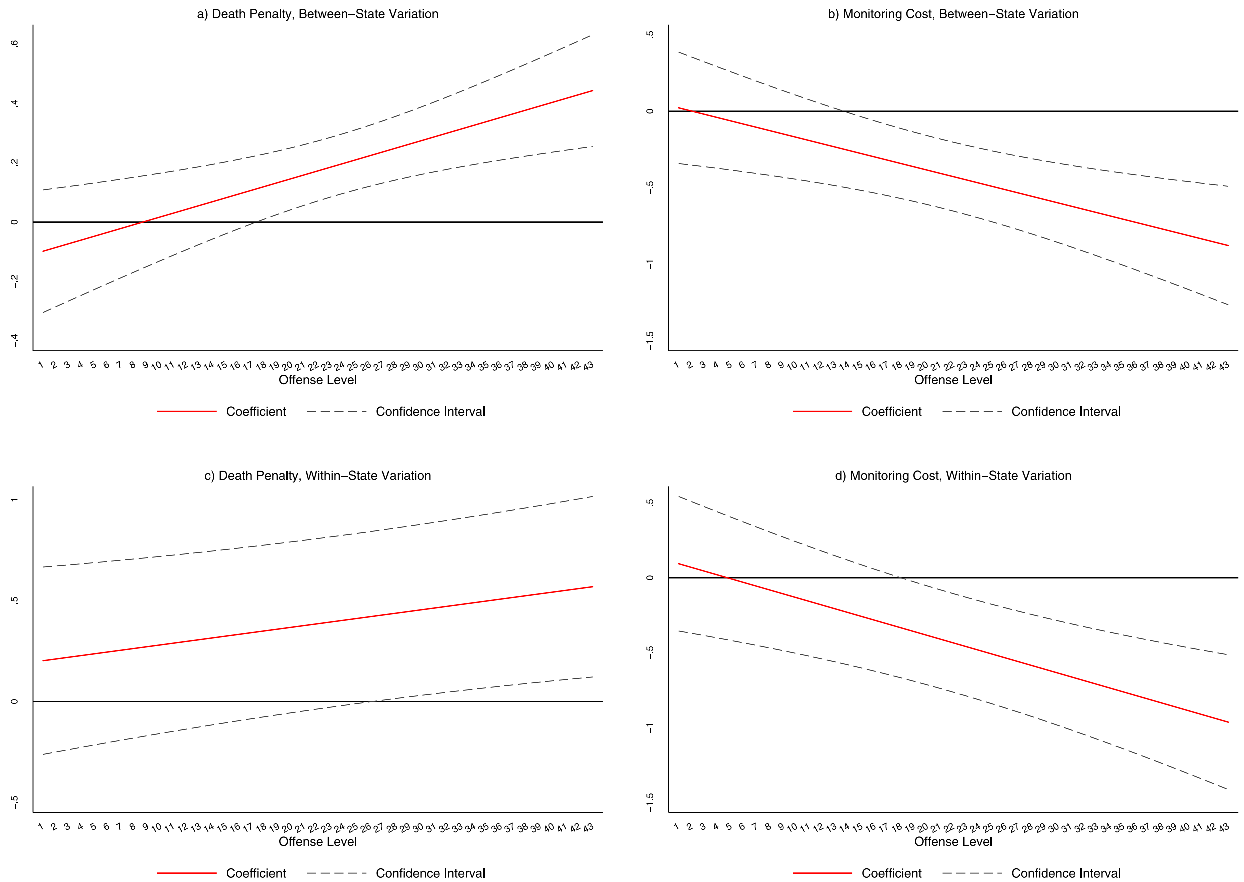

Fig. 7. Heterogeneity across Offense Levels: Predicted Coefficients. The figure plots the overall coefficients on death penalty (graphs a and c) and police wage (graphs b and d) across different offense levels. The results in graphs a) and b) are based on the estimates reported in column (3) of Table 10. The results in graphs c) and d) are based on the estimates reported in column (7) of Table 10. The regressions are weighted by the number of inmates in each state. The 90% confidence intervals are based on standard errors corrected for clustering by state-year. The sample period is 1990–2003.

of offense level 43. Taken together, these results suggest that the cross-state variation in the steepness of the punishment-severity schedules is not random, but correlates with maximum punishment and monitoring cost in accordance with the predictions of the marginal deterrence framework.

6. Conclusion

We have assembled a novel and unique data set, which contains individual-level data for a large sample of US inmates, an official ranking of crimes by severity, and proxies for maximum punishment and monitoring cost in each US state, over a period spanning up to 50 years. In line with the predictions of the most general model of marginal deterrence (Mookherjee and Png, 1994), we have found robust evidence that sentences are on average longer in states where maximum punishment is higher and monitoring cost is lower. We have also found that these relations are systematically stronger in states where the private benefits from crime are more heterogeneous, as well as for offenses of greater severity. The main contribution of this paper is to show that the US legal system is consistent with the rational economic model of marginal deterrence. Some of our results also seem to exclude theories of justice different from marginal deterrence, such as ethical, philosophical, or religious norms of justice and retribution.

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 of the abolition of capital punishment across offenses, estimate 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.

Appendix

In order to better understand the logic of Mookherjee and Png (1994), we now provide a simplified version of their model. They study a game in which the level of the criminal activity is a continuous variable, and individuals derive heterogeneous private benefits from infringing the law. They consider an environment in which, although the monitoring system detects all acts (regardless of their harmfulness) at a common rate, acts of different severity may be penalized at different rates. The considered policy (mechanism) specifies a monitoring rate and a penalty schedule. For a given policy, each individual chooses a 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. As a consequence higher types — i.e., those who benefit more from a harmful act — cannot choose less harmful acts.

Formally, the private benefit of a type-t individual from committing an act a is $t \times b(a)$, with $b(\cdot)$ being differentiable, strictly increasing, and such that $\lim_{a\to+\infty} b(a) = \bar{b} > 0$. The harm related to act a is $h(a)$, differentiable and strictly increasing. The cost of monitoring is c, while w denotes the maximum possible penalty. The monitoring rate μ is assumed to be non-contingent on the severity of the harm produced by criminals, whereas penalties are contingent on it. Finally, types are distributed on the support [0, 1] with density function $g(t)$.

In this environment, following Mirrlees (1986), the authors characterize the optimal policy for a regulator that maximizes the following utilitarian welfare function

$$W = \int_0^1 [tb(a_t) - h(a_t)]g(t)dt - c\mu \int_0^1 g(t)dt, \tag{A7}$$

where the function (schedule) a_t denotes the optimal act chosen by a type-t individual who solves

$$\max_{a\geq 0} \{tb(a) - \mu f(a)\} \quad \Rightarrow \quad tb'(a) = \mu f'(a), \tag{A8}$$

with $f(a)$ being the sanction associated with a generic act a.

By standard techniques — i.e., by operating a change of variables, such that the regulator chooses the schedule a_t rather than the sanction function $f(\cdot)$— the optimal policy maximizes (A7), subject to a_t being non-decreasing (as standard in screening models) and to the following two constraints

$$w \geq \bar{b} - \int_0^1 b(a_t)dt, \tag{A9}$$

$$\mu = \frac{1}{w}\left[\bar{b} - \int_0^1 b(a_t)dt\right]. \tag{A10}$$

The first constraint (which can be obtained by applying the Envelope Theorem to (A8)) reflects the fact that the most severe act (which will be chosen by the highest type, $t = 1$) cannot be punished more than w. The second constraint reflects the minimum (and thus optimal) detection rate that the regulator has to enforce if she wants to induce a given set of actions a_t. Note that, the larger is w (i.e., the more severe is the maximum punishment), the less monitoring is needed to implement a desired action schedule a_t.

Substituting (A10) into (A7), the first-order condition with respect to a_t characterizing the regulator's optimal policy is

$$\underbrace{tb'(a_t) - h'(a_t)}_{\text{First-best rule}} + \underbrace{\frac{c+\lambda}{wg(t)} b'(a_t)}_{\text{Distortion }+} = 0 \tag{A11}$$

where λ is the Lagrange multiplier associated to (A9). This condition implies immediately that the regulator allows agents to choose more severe acts in the second best than in the first best. Allowing a type-t agent to undertake a more severe act yields the agent a higher private benefit but is also more harmful to society. Yet, the marginal benefit causing the distortion is related to the fact that monitoring is costly and types are unknown: the regulator is forced to allow agents to commit more harmful acts because monitoring is costly and because the regulator cannot increase the penalty above w (as reflected by the constraint (A9)).

The actual penalties, which can be obtained directly from (A8), satisfy

$$f(a) = w\frac{t(a)b(a) - \int_0^{t(a)} b(a_\tau)d\tau}{\bar{b} - \int_0^1 b(a_\tau)d\tau}, \tag{A12}$$

where $t(a)$ defines the maximum type t that chooses an action a — i.e., the solution with respect to t of $a_t = a$.

Having described the general setting, it is useful to develop a closed-form example of the model, in order to emphasize some of the comparative statics results we have tested. Note that Mookherjee and Png (1994) already develop an example of their general analysis showing Empirical implications 2 and 3 in a transparent way (see Mookherjee and Png, 1994, p. 1051). Here, we propose a different example and show that the results are the same. Specifically, suppose that t is uniformly distributed, that $b(a) = \bar{b} - \frac{\gamma}{a}$, and that $h(a) = a$. Following the logic of Mookherjee and Png (1994), we assume $\lambda = 0$ for simplicity.[34] Hence, the solution of (A11) is

$$a_t^* = \sqrt{\gamma\left(t + \frac{c}{w}\right)},$$

which is clearly increasing in c and t, and decreasing in w.

Note that the most harmful legal action is determined by $a_0^* = \sqrt{\frac{\gamma c}{w}}$, which is also increasing in c and decreasing in w. Substituting into (A10) we obtain

$$\mu^* = \frac{\gamma}{w}\int_0^1 \frac{1}{a_t}dt = 2\sqrt{\gamma w}\left(\sqrt{w+c} - \sqrt{c}\right),$$

which is increasing in w and decreasing in c.

Turning to (A12) we have

$$f^*(a) = \begin{cases} 0 & \Leftrightarrow a \le a_0^* \\ \frac{-a + \frac{\gamma c}{aw} + 2w(wa - \sqrt{\gamma wc})}{2\sqrt{\gamma w}\left(\sqrt{w+c} - \sqrt{c}\right)} & \Leftrightarrow a > a_0^* \end{cases}.$$

Of course, a_1^* denotes the most harmful act committed in the society — i.e., no crime above a_1^* is committed in equilibrium. In order to highlight the comparative statics with respect to c and w in the clearest possible way, we now know plot this function for different parameter configurations. The left graph in Fig. A1 shows how the optimal sanction varies

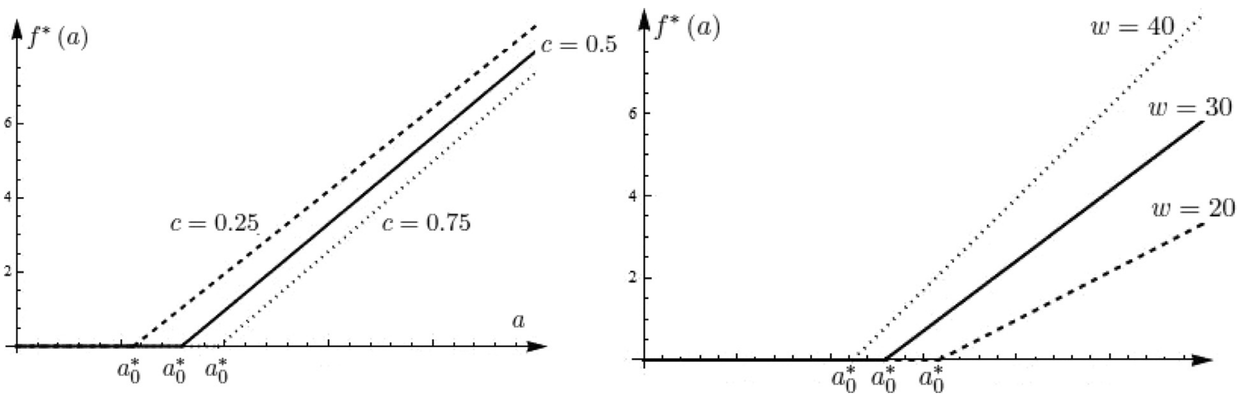

Fig. A1. Optimal Sanction, Maximum Punishment, and Monitoring Cost: Comparative Statics. The figure shows comparative statics for the optimal sanction $f^*(a)$ with respect to monitoring cost c (left graph) and maximum punishment w (right graph) for different parameter configurations.

[34] It can be checked that a sufficient condition for this to happen is that $\frac{c}{w}$ is high enough. However, since the distortion is increasing in λ, results would be qualitatively the same when considering the case $\lambda > 0$.

when the cost of monitoring c increases. It can be seen that a_0^* moves to the right (more crimes are legalized), and that the sanction schedule drops. The right graph in Fig. A1 shows how the optimal sanction varies when the maximum punishment w increases. It can be seen that a_0^* moves to the left (less crimes are legalized), and that the sanction schedule increases.[35]

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[35] Mookherjee and Png (1994) find the same patterns by assuming a different function for the private benefit — i.e., $b(a) = \bar{b} - e^{-\beta a}$ (see Mookherjee and Png, 1994, p. 1055).

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