Mandatory Minimum Reforms, Sentencing, and Racial-Ethnic Disparities

Terry-Ann Craigie* Mariyana Zapryanova†

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Abstract

Over the last twenty years, numerous states and the federal government enacted mandatory minimum reforms to reduce sentencing disparities between Blacks and Whites. Yet little is known about how effective these reforms have been at reducing racial-ethnic bias in the criminal justice system at the state level. Using quasi-experimental methods and rich administrative data on defendants, this study evaluates the impact of mandatory minimum reforms on racial-ethnic disparities in sentencing at the state level. We find that mandatory minimum reforms reduce sentences in general, but do not lower racial-ethnic disparities.

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Keywords: sentencing, mandatory minimum laws, racial-ethnic disparities

*Department of Economics, Connecticut College. Contact Email: tcraigie@conncoll.edu.

†Department of Economics, Smith College. Contact Email: mzapryanova@smith.edu. We thank participants at the 2020 Eastern Economic Association Conference in Boston, MA, for their valuable comments and suggestions.

1 Introduction

As a direct result of stringent criminal justice policies, the United States now has the highest incarceration rates in the world (Walmsley et al., 2018; Carson, 2018; Tonry, 2013). "Tough on crime" policies are designed to increase admissions to prison and to lengthen time served behind bars (Pfaff, 2017). Foremost among these policies are mandatory minimum sentencing laws. Mandatory minimum sentences (or mandatory minimums) are statutes that require judges to sentence defendants to a specified minimum prison term for a specific crime. These laws mandate a minimum sentence or prison time for certain offenses (for example, drug, violent, or sex offenses) or for specific triggering events (for example, offenses involving use of a firearm, against a minor, or in proximity to a school). Since the 1980s, mandatory minimum sentences have become one of the central features of US federal and state criminal justice systems, ballooning prison populations and exacerbating racial disparities in the criminal justice system as a result (Tonry, 2013).

In light of this pattern, civil rights activists have called for urgent reforms to mandatory minimums. The Fair Sentencing Act of 2010 aimed to reduce the racial gap in federal mandatory minimum sentences for powder and crack cocaine offenses. Bjerk (2017a) finds that this reform generally reduced Black-White disparities in drug sentences at the federal level. However, we know very little about the efficacy of these reforms at the state level. This study aims to address this conspicuous gap in the literature by evaluating the impact of state-level mandatory minimum reforms on drug sentences and consequent racial-ethnic disparities.

To do this, we exploit administrative data from the National Corrections Reporting Program (NCRP) (1985-2016), which provides prisoner-level data on offenses, demographics, admission and release dates, and judicially imposed sentences. Because state-level mandatory minimums are predominantly applied to drug crimes, we restrict our analyses to prisoners convicted of drug offenses only. We establish causality by using a generalized difference-in-difference (DD) strategy to evaluate how exogenous variations in state mandatory minimum reforms change the sentences of prisoners relative to their counterparts in states without any reforms to mandatory minimum sentences. In addition, we use a generalized triple-difference (DDD) strategy to evaluate how sentence disparities between Blacks and Whites and between Hispanic and Whites change in response to these reforms.

We find that, in general, mandatory minimum reforms reduce sentences by up to 46 months ($p < 0.01$). In addition, the effect increases over time. However, we find no significant evidence that mandatory minimum reforms reduce racial-ethnic disparities in drug sentences. The sentences of Black prisoners increase by up to 33 months ($p < 0.05$) relative to White counterparts, suggesting that reforms to mandatory minimums worsen the relative sentencing outcomes of Blacks. Although state-level mandatory minimum reforms appear to reduce the sentences of Hispanic prisoners relative to White counterparts, non-parallel trends suggest that this result is to be interpreted with caution. The findings from our study strongly support prior studies that confirm latent judicial biases against Black defendants (Nutting, 2017, 2013; Sorensen et al., 2014), even when sentencing reforms are enacted (Didwania, 2020).

The remainder of the paper proceeds as follows. Section 2 describes the institutional details of reforms to state mandatory minimum laws. Section 3 provides an overview of the existing literature. Section 4 introduces the data and presents summary statistics. Section 5 provides an overview of the empirical strategies used in the analysis. Section 6 presents the main findings and results. Section 7 summarizes our conclusions.

2 Institutional Background

Mandatory minimum sentencing laws are statutes that require judges to sentence defendants to minimum prison terms for certain crimes. These laws constrain sentencing or release decisions for various offenses (for example, drugs) or triggering events (for example, offenses involving use of a firearm). At the federal level, mandatory minimums were enacted primarily for drug crimes and chiefly some weight threshold of the drug. For instance, the 1986 Anti-Drug Abuse Act imposes a minimum five-year sentence for drug offenses involving, for example, 5 grams of crack, 500 grams of cocaine, or 1 kilogram of heroin. (21 U.S.C. 841(b)(1)(B), P.L. 99-570). Meanwhile, at the state level, mandatory minimums are used as a blunt tool for crime deterrence. They may broadly target certain crimes (for example, drugs) or certain drug quantity thresholds (for example, crack or cocaine), or be triggered by a particular benchmark (for example, school zones or repeat reoffenses). Mandatory minimums helped generate not only the highest incarceration rate in the world, but stark racial-ethnic disparities in the prison population. The race-neutrality of mandatory minimums failed to account for the fact that certain offenses are highly correlated with race and ethnicity, leading to disparate impact (Schlesinger, 2011; Bonilla-Silva, 2006).

Under mandatory minimum laws, if a prosecutor presents charges and a defendant is found guilty, judges must impose the mandatory minimum sentence even when there are mitigating factors at work. Absence of this option, it is unclear a priori whether judges will be more willing to hand in a lower or higher sentence. Prosecutors can also leverage mandatory minimums to coerce plea bargains from defendants. Plea bargains essentially strong-arm defendants into pleading guilty to obtain a more favorable sentence; alternatively, they could go to trial and face the credible threat of a mandatory minimum sentence (Bjerk, 2005; Fellner, 2014; Oppel Jr, 2011). Prosecutors use sentencing guidelines and mandatory minimums to secure guilty pleas or harsh sentences (Stuntz, 2004). In theory, abolishing mandatory minimums could significantly limit prosecutors in designing the attributes of the plea deal and sentencing request to the judge, which in turn could lead to defendants receiving lower sentences. Further, the prosecutorial discretion that mandatory minimums afford, likely exacerbates racial-ethnic disparities in sentencing, with more favorable deals going to White defendants compared to their minority counterparts, who disproportionately comprise the correctional population.[1] Therefore, mandatory minimums could worsen sentencing disparities inside and outside the courtroom, prompting widespread activism for reform. An important question, therefore, is whether repealing mandatory minimums could correct any racial disparities in sentencing.

At the federal level, the Fair Sentencing Act of 2010 directly targeted the sentencing gap between crack and powder cocaine offenses. However, reforms to mandatory minimum sentencing at the state level are diverse in their form and impact. There are four main types of mandatory minimum reforms: expansion of judicial discretion, "second look" judicial review, repeal or revision of automatic sentence enhancements, and repeal or revision of mandatory sentences (Families Against Mandatory Minimums, (FAMM), 2019).

Reforms that increase judicial discretion (also known as "safety valves"), involve provisions that keep a mandatory minimum penalty in place, but allow judges to sentence defendants below that minimum if certain factors apply. These policies do not repeal or eliminate mandatory minimum sentences but rather allow courts to give shorter, more appropriate prison sentences to offenders who pose less of a public safety threat. Second look sentencing is a process by which courts review, or take another "look" at a lengthy sentence (after a significant portion of the sentence has been served), and authorizes a judge to modify

[1]Sloan (2019) examines racial bias in convictions by prosecutors and suggests that prosecutors may seek harsh punishments for some offenders and lenient punishments for others.

the sentence. Some states go a step further, by repealing or revising the automatic sentence enhancements that trigger longer sentences if certain statutory conditions or thresholds are met, such as speeding in a construction zone, selling drugs in a school zone, committing a crime in the presence of a minor, using a handgun in the commission of a crime, or having a certain number of previous criminal convictions.

Because judicial discretion, second look, and automatic sentence enhancements are all predicated on conditions we cannot observe in our data, the primary focus of our study is mandatory minimum sentence repeals and revisions. These reforms represent full or partial modifications to existing mandatory minimum sentence laws, typically issued for drug offenses. These reforms give judges full discretion over what sentence to hand down while mitigating the prosecutors' leverage to negotiate potentially biased sentences or wrongful convictions.

Because repeals or revisions to mandatory minimum sentences primarily target drug offenses, our analyses evaluate how drug-, state-, and year-specific repeals or revisions of mandatory minimum sentences impact judicially imposed sentences, *ex post*.[2] Figure 1 shows that by 2015, nineteen states had either repealed or revised mandatory minimum guidelines for drug offenses. We report these states along with the effective date of these laws in Appendix Table A1. We focus on reforms that either revise or fully repeal mandatory minimums. Therefore, our results present a lower bound of the the effect of eliminating mandatory minimums altogether. One important caveat is that although we know the reforms partially or fully modify mandatory minimum sentences, we cannot always observe precisely how judges execute these modifications. Therefore, the results are best characterized as *intent-to-treat* effects.

[2]In our sensitivity checks, we control for the other three types of mandatory minimum reforms: judicial discretion, sentence enhancements, and second look; the results are consistent with the general findings.

3 Literature Review

There are three main strands of the burgeoning literature on mandatory minimum sentences. The first (and most extensive) strand examines the effect of mandatory minimum guidelines and policies on criminal justice outcomes. Dominguez-Rivera et al. (2019) and Bartos and Kubrin (2018) explore the effect of California's Proposition 47 (Prop 47), which reduced drug possession offenses and certain lower-level property offenses to misdemeanors. While these studies find a decrease in jail and state-prison populations and in property- and drug-crime arrests, they find little to no evidence that Prop 47 affects violent-, property-, or drug-crime rates. Helland and Tabarrok (2007) find that California's three-strikes laws reduced felony arrest rates by 17 to 20 percent among individuals who already had two strikes. On the other hand, Marvell and Moody (2001) find that three-strikes laws increase homicides, but little to no evidence of other crime reduction. Moreover, Abrams (2012) finds that sentence enhancements, rather than mandatory minimums, have a deterrent effect on armed robberies.

The second strand focuses on the sentencing disparities induced by mandatory minimums – a critical source of bias in the criminal justice system. Rehavi and Starr (2014) find that mandatory minimums explain a significant portion of Black-White sentencing gap. Similarly, Fischman and Schanzenbach (2012) report that sentencing disparities increased after the *Booker* decision mainly because of the increased use of mandatory minimums. Yang (2015) and Starr and Rehavi (2013) echo this finding and argue that mandatory minimums have been more likely to be used against black defendants since *Booker*. More recently, Tuttle (2019) extends Bjerk (2017b) and uses time variation in the mandatory minimum threshold for federal drug crimes. He finds that there is a significant bunching of cases sentenced at the drug threshold that triggers a ten-year mandatory minimum. This bunching is disproportionately large for Blacks and Hispanics. Sorensen et al. (2014) find that under federal guidelines, judicial preferences tend to disadvantage Black males relative to White counterparts. Fischman and Schanzenbach (2012) also show that racial disparities are either reduced or little changed and federal mandatory minimums are less likely to be binding for a Black defendant when the judge is given more discretion over the sentence. In addition, female defendants subject to harsher sentences under federal mandatory minimum guidelines receive more lenient sentences compared to male counterparts (Nutting, 2017, 2013).

Our study contributes to the third (and most scant) strand of the literature, which evaluates how reforms to mandatory minimum policies change various criminal justice outcomes. The Fair Sentencing Act of 2010 aimed to reduce the racial gap in federal mandatory minimum sentences for powder and crack cocaine offenses. Bjerk (2017a) confirms that this legislation reduced Black-White disparities in drug sentences. However, Didwania (2020) finds that an August 2013 memo – disseminated by then attorney general Eric Holder advising federal prosecutors to end the use of mandatory minimums for low-level non-violent offenses – did not have an impact on sentencing disparities. Our study adds to this strand of the literature, by using rich administrative data to evaluate the impact of *state-level* mandatory minimum reforms on judicially imposed sentences for drug offenses. We focus on states that either repeal or revise their mandatory minimum sentences and how this in turn affects the sentencing outcomes of individuals charged with drug offenses. Our study also contributes to the second strand of the literature, by evaluating the extent to which the sentencing disparities change in response to mandatory minimum reforms.

4 Data

We use data on prison admissions from the (1985-2016) National Corrections Reporting Program (NCRP) compiled by the Bureau of Justice Statistics. The NCRP is a prisoner-level data set in which participating states voluntarily submit data on prisoners entering and leaving the custody of state authorities. During our sample period, forty-four states provided prisoner-level data on admissions to prison at some point. For each prison spell, we observe the admission and release date for each offender and the judicially imposed sentence. Additionally, the NCRP contains rich information on prisoners' demographic characteristics, such as age, race, Hispanic ethnicity, highest grade completed, gender, whether the offender has previously been convicted of and incarcerated for a felony, and the type of entry (for example, new conviction or probation/parole revocation). We also observe up to three crimes for which the offender was convicted and the combined sentence length. We restrict our analysis to individuals who have been convicted of a drug-related offense for at least one of the three crimes we observe.[3]

Several studies using the older versions of the NCRP data have identified issues with data reliability and have used a subset of states to ensure consistency (Neal and Rick, 2016; Pfaff, 2011). For instance, using (1983-2002) NCRP data, Pfaff (2011) compares counts of individuals entering and exiting state prisons to other official counts such as the National Prisoner Statistics. He concludes that only eleven states consistently reported prisoner-level data to the NCRP: California, Colorado, Illinois, Kentucky, Michigan, Minnesota, Nebraska, New Jersey, South Dakota, Virginia, and Washington. Further, Neal and Rick (2016) use (1983-2009) NCRP data to conduct several checks and confirm that these eleven states consistently report prison admissions. Therefore, we restrict our sample to these eleven

[3]In addition, we drop individuals who have not yet been released or who died in custody.

states to evaluate the robustness of our results.

Figure 2 shows the average sentence length of prisoners in the NCRP data.[4] The average sentence is 63 months, or approximately five years. For Blacks, the average sentence is about 70 months, whereas White and Hispanic prisoners have similar average sentences of 56 and 55 months, respectively. This suggests that the average Black-White sentence gap is about 15 months and that Blacks receive about 27 percent longer sentence than Whites; on the other hand, the Hispanic-White sentence gap is negligible.

Table 1 shows the summary statistics for all the control variables in our empirical analysis. Unsurprisingly, the analysis sample is largely comprises minority males with low education. Almost half of the prisoners are Black, compared with about 17 percent who are Hispanic. The majority of prisoners are male. About 30 percent of prisoners are high school dropouts, and the average age at prison admission is 34.5 years old. Twenty-eight percent of prisoners had felony convictions prior to their current incarceration episode, and close to 25 percent of prisoners were incarcerated for violating parole or probation.[5]

Prisoner characteristics and offenses vary significantly by race and ethnicity. Twenty-three percent of Black prisoners have high school diplomas. Compared with White and Hispanic prisoners, a higher percentage of Blacks (roughly 32 percent) had felony convictions prior to the current sentence. In addition, a larger percentage of Blacks (27 percent) had their probation or parole revoked. Hispanic prisoners have the lowest level of education, with 28 percent having less than a high school diploma. They also have the youngest age of admission, at 33.6 years old and the lowest incidence of probation or parole revocation, at 22 percent.

[4]We topcoded the data to 80 years. For even longer sentences, our main results remain unchanged when we exclude these individuals from our estimation sample.

[5]Although the sample seems to consist of fewer repeat offenders, it is not completely surprising given that the data are missing information on prior felony incarceration completely for some states (Yang, 2017)

5 Empirical Strategy

5.1 Difference-in-Difference Estimation (DD)

To estimate the effect of mandatory minimum reforms on judicially imposed sentencing, we exploit variation in the staggered timing of state sentencing laws that repeal or revise mandatory minimums. We use the effective dates of these sentencing reforms as exogenous shocks to sentence length in a difference-in-difference framework. Exploiting the panel nature of our data and the fact that states reform their sentencing practices at different times, we set our baseline specification as follows:

$$Sentence_{ist} = \alpha + \beta MML_{st} + \gamma X_{it} + \delta_t + \eta_s + \epsilon_{ist} \tag{1}$$

where $Sentence_{ist}$ is sentence length, measured in months, of prisoner i, imprisoned for any drug offense in state s and admitted in year-month t. MML_{st} is the DD indicator for whether state s has reformed (repealed or revised) its mandatory minimum sentencing laws by the year-month t in which the offender was admitted.

X_{it} is a vector of characteristics about the individual imprisoned. These characteristics are both time-invariant (race/ethnicity, gender, highest grade completed at entry) and specific to the particular prison spell (age at admission and prior-felony incarceration). We also include indicators for missing data on each of these time-invariant and prison-spell characteristics. δ_t and η_s are prison-admission year and state fixed effects, respectively. We cluster the standard errors ϵ_{ist} at the state level.

Our identification of the impact of state sentencing reforms for incarcerated individuals compares observably similar individuals admitted to prison in the same state, who happen to be sentenced either under the old "get tough" sentencing policies or under the repealed or revised mandatory minimums. The coefficient of interest, β, is identified using random variation in the month of admission, whether that admission occurred before or after the passage of the mandatory minimum sentencing reforms, and how an individual's sentence compares with the sentences of other prisoners with similar characteristics. We show pre-trends in coefficient plots as evidence that our controls are adequately absorbing pre-existing trends. We explain this strategy in the subsequent subsection.

5.2 Event-Study Design

We extend our differences-in-differences framework to an event study by including leads and lags of treatment as regressors. The event-study specification can be written as follows:[6]:

$$Sentence_{ist} = \alpha + \sum_{L \in K} \beta_L MML_{st}^{L} + \gamma X_{it} + \delta_t + \eta_s + \epsilon_{ist} \tag{2}$$

where $K = \{-4, -3, -2, -1, 0, 1, 2, 3, 4\}$, with -4 denoting four or more years before and 4 denoting four or more years after the state mandatory minimum sentencing reform took effect. [7] Similar to the variable of interest in the difference-in-differences framework, our variable of interest, $Sentence_{iost}$, is sentence length, measured in months; X_{it} is a vector of characteristics about the individual imprisoned; δ_t are year fixed effects; and η_s are state fixed effects.

The set of MML_{st}^{L} dummies represents the year, L, relative to the enactment of the mandatory minimum sentencing reform. (L = 0 denotes the year of implementation of

[6]See Jacobson et al. (1993) for more detail on the event-study specification.

[7]We experimented with different leads and lags, but results are robust to the event-window definition. Also, note that we bin up the event dummies at the endpoints of the event window (that is, $K = -4$ and $K = 4$), and thus the dummy MML_{st}^{-4} accounts for all reforms of mandatory minimums occurring four or more years, *ex ante*.

the mandatory minimum sentencing reform and is the excluded category.) For example, MML^1_{st} is an indicator that equals to 1 if prisoner i is admitted a year after the reform and 0 otherwise. Each of the β_L coefficients is measured relative to the omitted category – the year of implementation. The validity of this research design relies on the assumption that the outcome in treatment and comparison states would have behaved similarly in post-reform years without mandatory minimum sentencing reforms. Finding β_L coefficients in the pre-reform years that are not statistically different from the excluded category (that is, parallel trends), indicates that other policies or events do not conflate post-year impact estimates. As we show in the Results section, the parallel pre-trends suggest that the states that did not reform their mandatory minimum sentencing practices are a valid comparison group for this quasi-experimental exercise.

5.3 Triple-Difference Estimation (DDD)

The DD analysis allows us to estimate intent-to-treat effects of repealing or revising states' mandatory minimum laws on sentencing. We next expand on this analysis to explore whether these effects are most pronounced for Black and Hispanic prisoners. To evaluate whether mandatory minimum reforms change Black and Hispanic sentences relative to Whites, we adopt the following triple-difference model:

$$\begin{aligned} Sentence_{ist} =& \alpha + \delta MML * Min_i + \beta_1 MML_{st} + \beta_2 Min_i + \beta_3 Min_{ist} * \lambda_s \\ &+ \beta_5 \gamma_t * Min_{it} + \beta_6 X_{it} + \gamma_t + \lambda_s + \epsilon_{ist} \end{aligned} \tag{3}$$

where $Sentence_{ist}$ is sentence length, measured in months, of prisoner i, imprisoned for a drug offense in state s at month t. As in equation 1, MML_{st} is the DD indicator for whether state s has reformed (repealed or revised) its mandatory minimum sentencing laws for the year-month t in which the offender was admitted. Min is an indicator equal to 1 if the prisoner is Black or Hispanic and 0 if the prisoner is White. The coefficient of interest on the interaction $MML * Min_i$, δ, measures the net impact of mandatory minimum reforms on the sentences of minority prisoners relative to White prisoners, *ex post*. As with the event-study DD design, we extend this triple-difference model into an event-study specification.[8]

6 Results

Table 2 presents DD impact estimates from equation 1, DDD impact estimates from equation 3, and estimates from their corresponding event studies. Column (1) shows that mandatory minimum reforms reduce sentences by about 12 months in general (nearly 20 percent of the outcome mean), though this estimate is not statistically significant. To evaluate the dynamic effects of these reforms and test the parallel-trend assumption, Column (2) presents the event-study impact estimates. Pre-reform estimates are positive, but not statistically different from zero, confirming that outcome trends of treatment and comparison groups are parallel. Post-reform impact estimates, however, are negative and range from 2 to 28 months. Figure 3 illustrates these event-study DD estimates along with their 95 percent confidence intervals. While we use the graphical representation of the event-study specifications mainly to establish flat pre-trends, we can also observe that rather than dissipating, the treatment effect grows over time to a reduction in sentence length by up to 28 months

[8]More specifically, the corresponding event-study equation can be written as follows:

$$\begin{aligned} Sentence_{ist} = & \alpha + \sum_{L \in K} \delta_L MML^L_{st} * Min_i + \beta_1 MML_{st} + \beta_2 Min_i + \beta_3 Min_{ist} * \lambda_s \\ & + \beta_5 \gamma_t * Min_{it} + \beta_6 X_{it} + \gamma_t + \lambda_s + \epsilon_{ist} \end{aligned} \tag{4}$$

where $K = \{-4, -3, -2, -1, 0, 1, 2, 3, 4\}$ with -4 denoting four or more years before and 4 denoting four or more years after the state mandatory minimum sentencing reform. Identification of causal effects in the event-study DDD design also requires common trends before treatment.

($p < 0.05$), *ex post*.

To measure the impact of mandatory minimum reforms on racial-ethnic disparities in sentencing, we employ the triple-difference model outlined in equation 3. Given that the literature has already established that minorities are more likely to have mandatory minimum sentences (Rehavi and Starr, 2014; Fischman and Schanzenbach, 2012; Yang, 2015; Starr and Rehavi, 2013), it is highly relevant to determine how the Black-White and Hispanic-White sentence disparities change in response to these reforms. The DDD results reported in Table 2 Column (5) show that Hispanic prisoners receive sentences 35 months ($p < 0.05$) lower than White counterparts, *ex post*. This estimate is approximately two-thirds of the outcome mean. For Blacks on the other hand, DDD analyses provide no evidence of a statistically significant change in the Black-White sentence disparity for drug offenses. DDD results therefore confirm that revisions and repeals of mandatory minimum sentences significantly shorten sentences for Hispanic prisoners relative to White prisoners, but not for Black prisoners – the population faced with the most stringent sentencing outcomes.

It is important to underscore that the validity of the DDD results hinges on parallel pre-reform outcome trends. Pre-reform sentencing trends in both treatment and comparison states should be parallel if post-reform estimates are not driven by other factors than mandatory minimum reforms. To illustrate this, we present event-study DDD estimates for the Black-White sentence gap in Table 2 Column (4) and graphically in Figure 4. Pre-reform estimates are generally flat and not statistically different from zero, reinforcing that parallel trends hold and DDD impact estimates, albeit not statistically significant, are unbiased. We do not uncover similar pre-reform trends for the Hispanic-White sentence gap. Table 2 Column (6) and Figure 5 reveal that the identifying assumption of parallel trends is violated, with two of the four pre-reform indicators statistically significant at the 5 percent level. This finding suggests that while mandatory minimum reforms appear to lower drug sentences for Hispanic prisoners relative to their White counterparts, the result is likely to be biased. Accordingly, while sentencing disparities grew under mandatory minimum sentencing guidelines, we do not find evidence that the disparities diminish when these guidelines are relaxed (Mustard, 2001; Fischman and Schanzenbach, 2012).

We run a set of sensitivity checks to test whether our estimates are driven by modeling choices or estimation sample. Table 3 provides additional results that explore the sensitivity of our estimates to general and state-specific time trends.[9] For DD and DDD models, the inclusion of a general and quadratic linear time trend does not change estimates significantly. Additionally, standard errors remain quite stable in each specification.

Repealing or revising mandatory minimum laws might be a part of a sweeping overhaul of a state's sentencing structure, which could conflate the findings. To test this possibility, we construct an indicator that is equal to 1 if a state has passed any other of the three types of mandatory minimum reforms (that is, judicial discretion, sentence enhancements, or second look). We re-estimate equations 1 and 3 controlling for this indicator and present the results in Table 4. The findings indicate that estimated DD and DDD impact estimates are not sensitive to the inclusion of other sentencing reforms in the model.

A key limitation of the NCRP data is that some states do not consistently report admissions and release information. Neal and Rick (2016) show that only eleven states report data consistently from 1985 to 2014. We restrict our sample to these eleven states, to determine the extent to which inconsistent state reports could account for the lack of precision in the general model. The results from this restricted sample are presented in Table 3. These results are more robust, and bolster the impact estimates from the general model. From

[9] Time is defined as prison admission year.

the DD specification, mandatory minimum reforms reduce sentences in general by approximately 46 months ($p < 0.01$). The event-study estimates presented in Figure 3 show not only that parallel trends hold, but also that the negative impacts of mandatory minimum reforms get larger over time. One year after the reform, mandatory minimum reforms reduce drug sentences by about 15 months ($p < 0.10$); however, four years later, these reforms lower drug sentences by 42 months ($p < 0.05$).

The results from the DDD specification are less encouraging. Figure 4 illustrate that Black prisoners receive longer sentences relative to White counterparts, *ex post*; however, the positive impact declines over time from about 44 months ($p < 0.05$) to 28 months *(n.s.)*. Figure 5 illustrates that Hispanic prisoners receive shorter sentences than White counterparts *ex post*, although this estimate is only marginally significant and is rendered biased by nonparallel trends.

Note that within the eleven-state consistent sample, only two states (Michigan and New York) are treated states. These two states repealed – rather than revised – their mandatory minimum sentencing guidelines, making it less surprising that their impact estimates are larger and more robust than the general estimates. Associated with the few-treated-states problem, are incorrect standard errors, which require wild-cluster-bootstrapped standard errors for unbiased inference (Cameron et al., 2008). Using one thousand wild-cluster-bootstrap iterations, the significance levels of the DD and DDD impact estimates remain statistically similar.[10] We can therefore conclude from these analyses that mandatory minimum reforms reduce drug sentences in general, but increase Black-White sentencing disparities.

[10]We also run the same sensitivity checks as presented in Table 3, and the results remain statistically similar (results available upon request).

7 Conclusions

An extensive literature has already explored the effect of mandatory minimum sentencing guidelines on criminal justice outcomes and corresponding racial-ethnic disparities. However, most of this literature focuses on the federal level by evaluating the impact of federal guidelines (Bjerk, 2017a; Fischman and Schanzenbach, 2012; Rehavi and Starr, 2014; Yang, 2015; Starr and Rehavi, 2013; Tuttle, 2019) or reforms to them (Bjerk, 2017a; Didwania, 2020). This paper investigates the effect of mandatory minimum reforms but at the state level, where more than 60 percent of US prisoners are housed.

Our study evaluates whether repealing or revising state-level mandatory minimums can reduce judicially imposed sentences and corresponding racial-ethnic disparities. We used longitudinal prisoner-level data from the National Corrections Reporting Program and quasi-experimental methods to identify the impact of these reforms. We find that revisions and repeals to mandatory minimum sentences reduce overall sentence durations for drug offenses by up to 46 months ($p < 0.01$). In addition, this impact of mandatory minimum reforms grows larger over time.

We find no evidence that mandatory minimum sentence reforms reduce sentencing disparities by race or ethnicity. In fact, the sentences of Black prisoners lengthen by up to 33 months ($p < 0.05$) relative to the sentences of White counterparts. Although state-level mandatory minimum reforms appear to reduce the sentences of Hispanic prisoners relative to their White counterparts, nonparallel pre-reform trends render this result biased.

Accordingly, the study uncovers that the disparate impact of early race-neutral mandatory minimum policies (Schlesinger, 2011; Bonilla-Silva, 2006) is not ameliorated by subsequent revisions or repeals. Although often touted as a solution to disparate sentencing outcomes (Fellner, 2014; Oppel Jr, 2011), transferring the power of sentencing from prosecutors to judges does not appear to succeed in that regard. This finding bolsters prior evidence that reforms to federal mandatory guidelines actually worked to increase Black-White sentencing disparities (Didwania, 2020) because of latent judicial bias against Black defendants (Nutting, 2017, 2013; Sorensen et al., 2014). Therefore, in lieu of adjustments to sentencing capabilities of judges and prosecutors, the study underscores the need for policies that directly address racial-ethnic disparities in sentencing.

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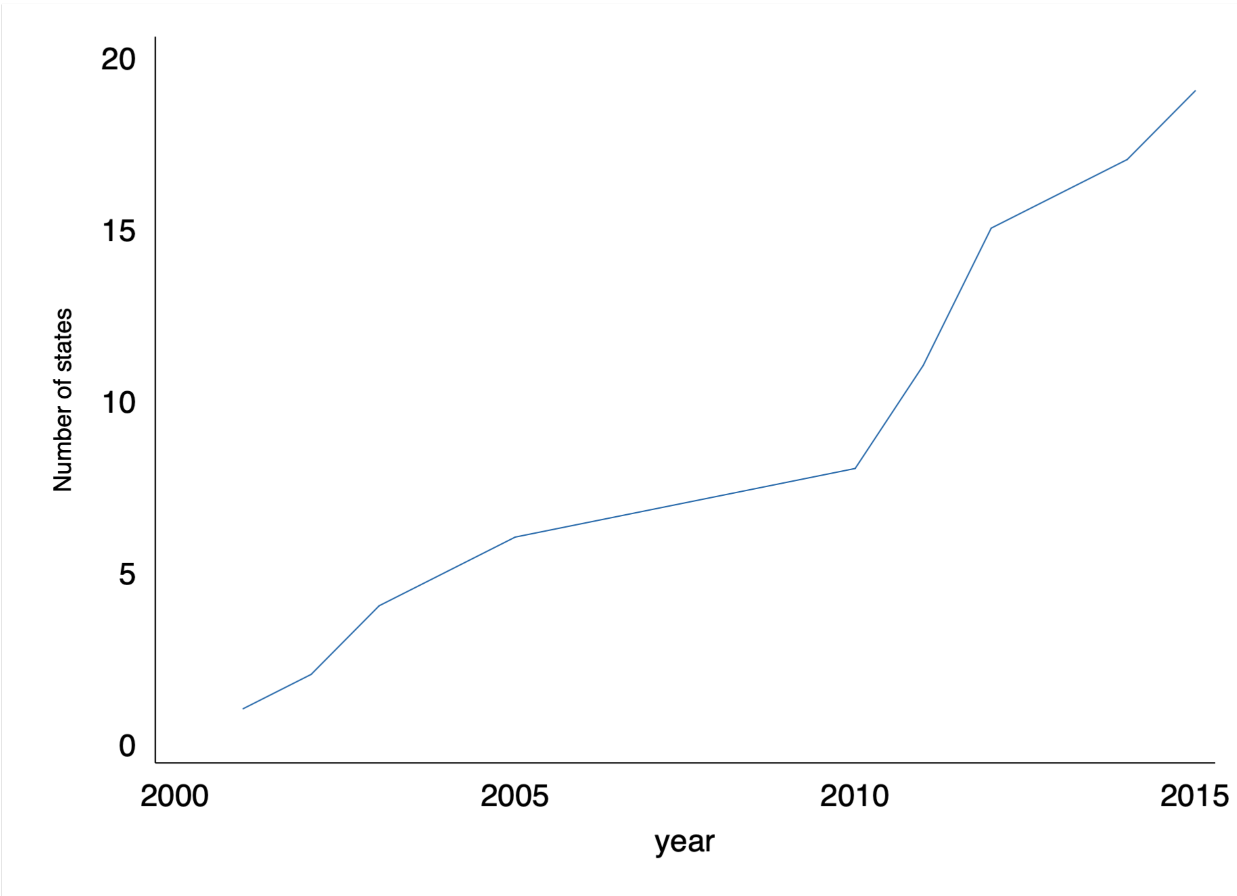
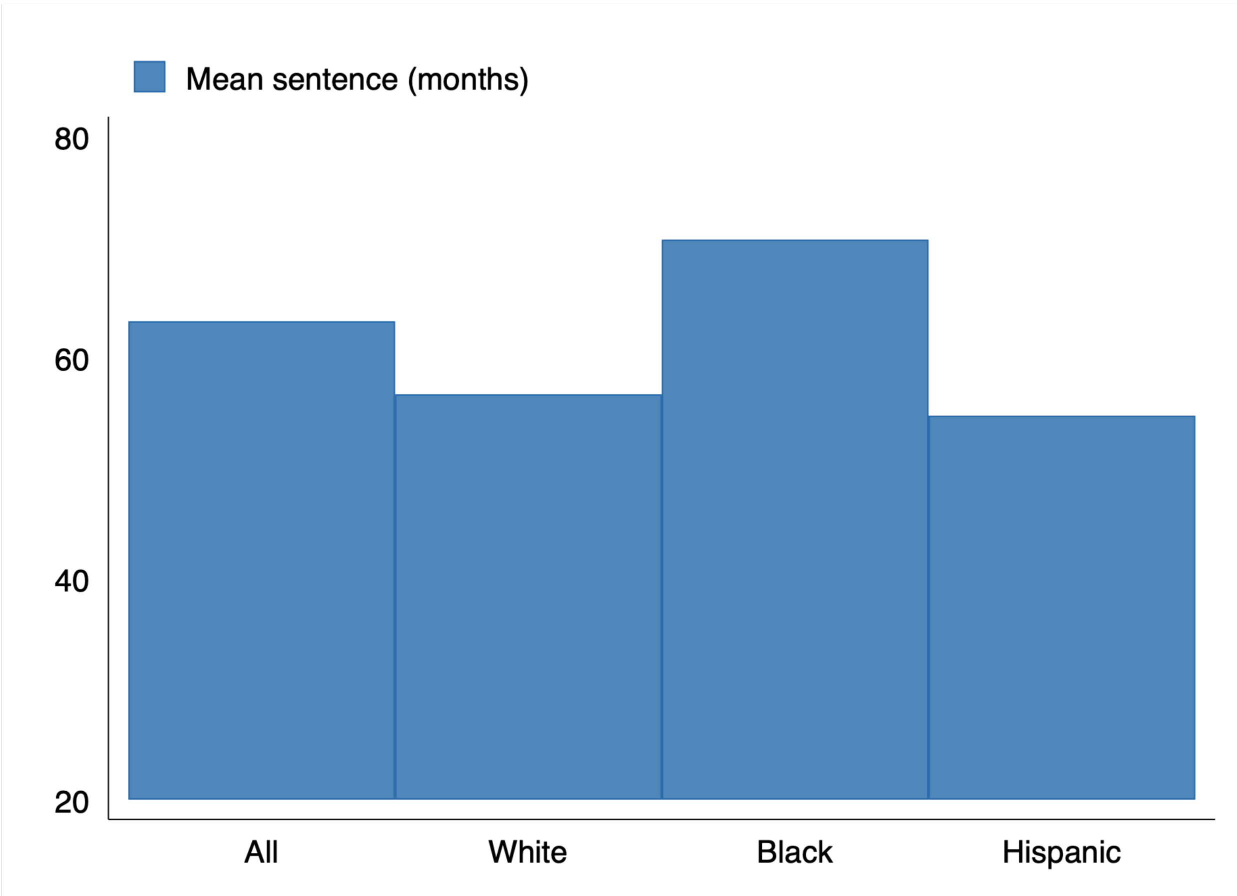
8 Figures and Tables

Figure 1: Number of States that Reform their Mandatory Minimum Laws



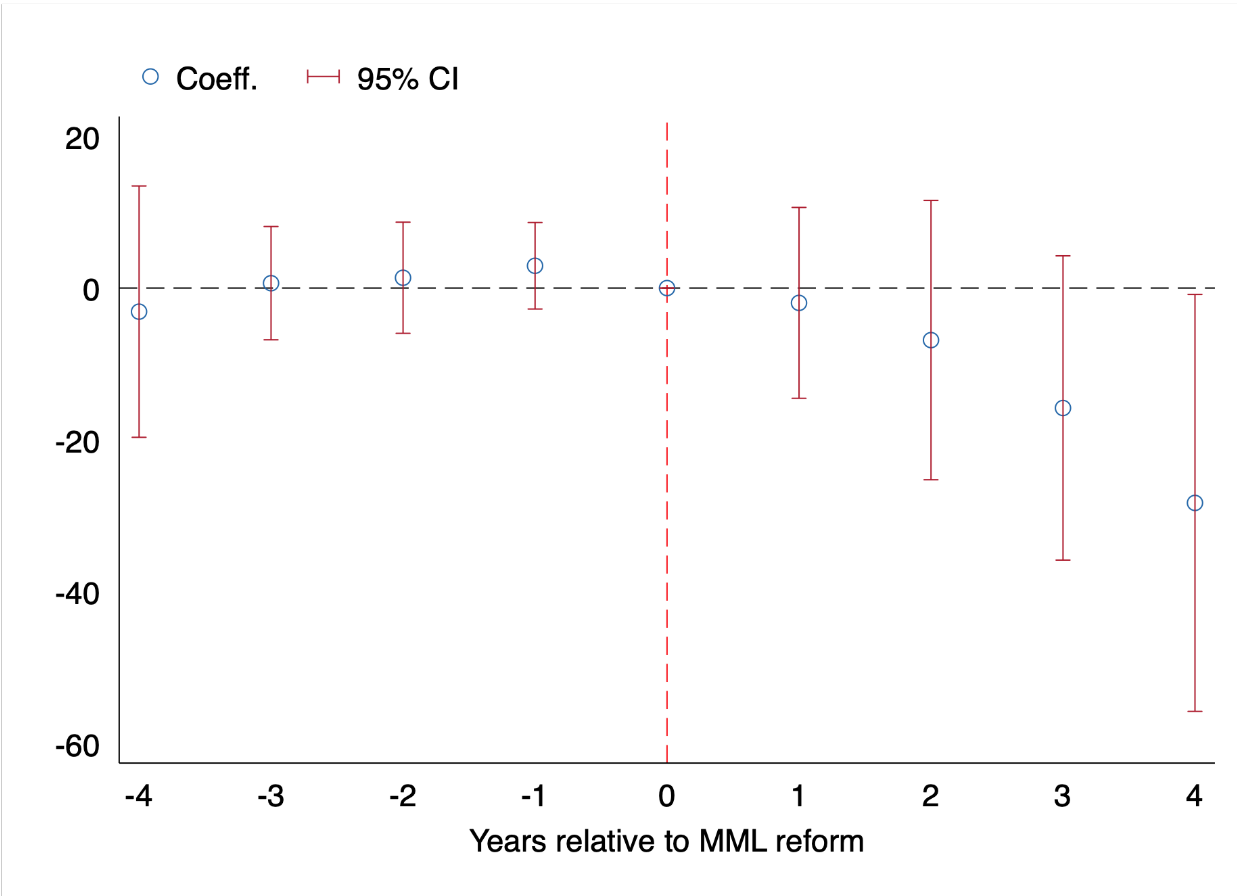
Notes: This graph reports the number of states that reform (repeal or revise) their mandatory minimum laws over time.

Figure 2: Average Sentence Length by Race-Ethnicity



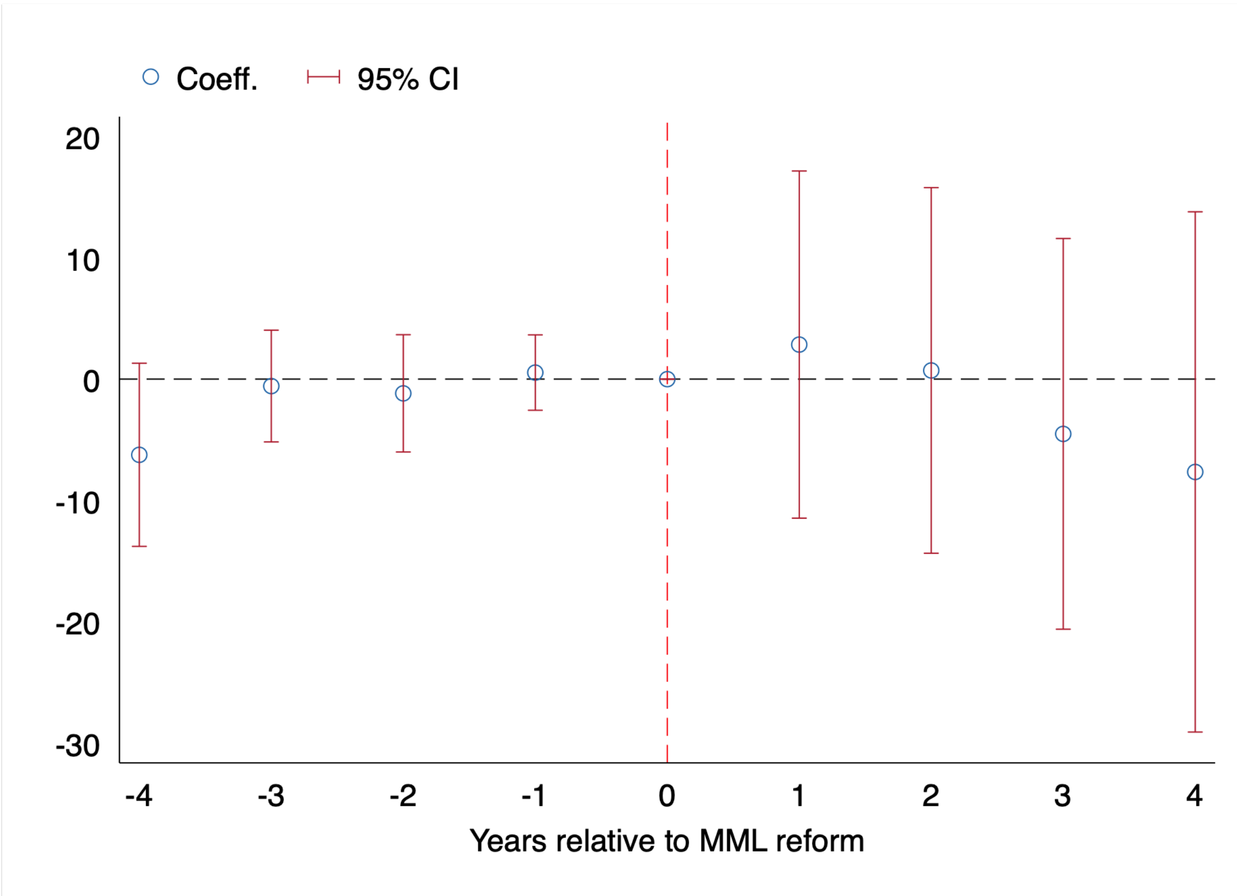
Notes: This figure plots mean sentence length (in months) by race and ethnicity. Data are from the National Corrections Reporting Program (NCRP).

Figure 3: Event-Study DD Estimates



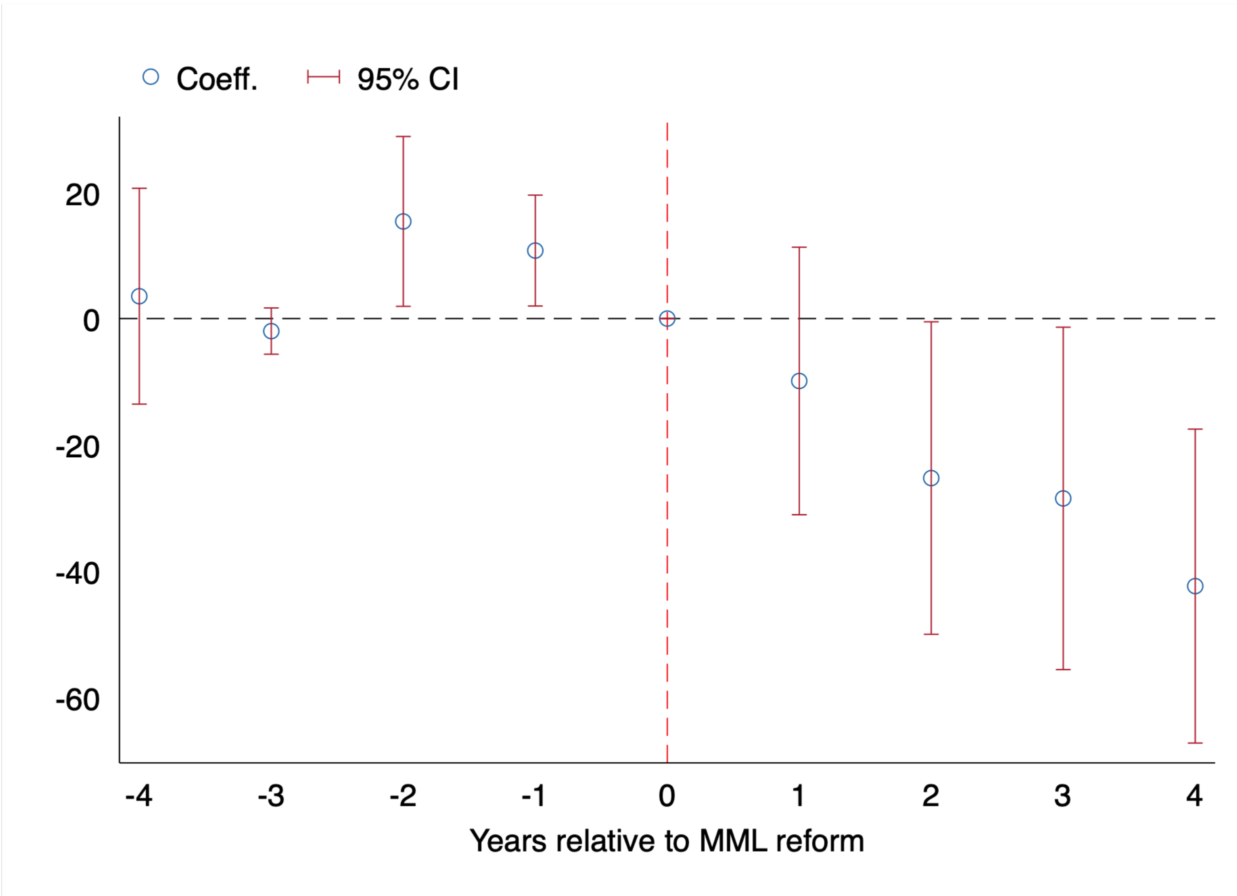
Notes: This figure plots event-study estimates ($\beta_L, L = \{-4, -3, -2, -1, 0, 1, 2, 3, 4\}$) and corresponding 95 percent confidence bands of equation 2. The dependent variable is individual sentence length, measured in months. The omitted dummy is year of implementation, so the coefficient β_0 is set to zero. We control for individual demographic characteristics (race, ethnicity, gender, age, age squared, highest grade completed, prior felony incarceration) and reason for prison admission. We also include indicators for missing data for each of these control variables. State and admission year fixed effects are included in all specifications. Data are from the National Corrections Reporting Program (1985-2016).

Figure 4: Event-Study DDD Estimates of the Black-White Sentence Gap



Notes: This figure plots event-study estimates ($\delta_L, L = \{-4, -3, -2, -1, 0, 1, 2, 3, 4\}$) and corresponding 95 percent confidence bands of equation 4. The dependent variable is individual sentence length, measured in months. The omitted dummy is year of implementation, so the coefficient δ_0 is set to zero. We control for individual demographic characteristics (race, ethnicity, gender, age, age squared, highest grade completed, prior felony incarceration) and reason for prison admission. We also include indicators for missing data for each of these control variables. State and admission year fixed effects are included in all specifications. Data are from the National Corrections Reporting Program (1985-2016).

Figure 5: Event-Study DDD Estimates of the Hispanic-White Sentence Gap



Notes: This figure plots event-study estimates ($\delta_L, L = \{-4, -3, -2, -1, 0, 1, 2, 3, 4\}$) and corresponding 95 percent confidence bands of equation 4. The dependent variable is individual sentence length, measured in months. The omitted dummy is year of implementation, so the coefficient δ_0 is set to zero. We control for individual demographic characteristics (race, ethnicity, gender, age, age squared, highest graded complete, prior felony incarceration) and reason for prison admission. We also include indicators for missing data for each of these control variables. State and admission year fixed effects are included in all specifications. Data are from the National Corrections Reporting Program (1985-2016).

Table 1: Summary Statistics

	All		White		Black		Hispanic	
	Mean	S.D.	Mean	S.D.	Mean	S.D.	Mean	S.D.
Sentence length (in months)	63.279	102.409	56.669	85.139	70.671	118.349	54.747	92.741
Black	0.472	0.499	0.000	0.000	1.000	0.000	0.022	0.147
White	0.370	0.483	0.701	0.458	0.000	0.000	0.165	0.371
female	0.140	0.347	0.184	0.387	0.090	0.287	0.096	0.295
Male	0.860	0.347	0.816	0.387	0.909	0.287	0.904	0.295
Hispanic	0.172	0.377	0.318	0.466	0.008	0.089	1.000	0.000
Less than HS Degree	0.293	0.455	0.261	0.439	0.328	0.469	0.284	0.451
HS Degree	0.239	0.426	0.248	0.432	0.229	0.420	0.174	0.379
Some college	0.035	0.184	0.036	0.186	0.034	0.181	0.014	0.118
College Degree	0.004	0.064	0.005	0.070	0.003	0.058	0.002	0.042
Age at prison admission	34.526	9.850	34.808	9.578	34.211	10.137	33.651	9.437
Prior Felony Incarceration	0.280	0.449	0.247	0.431	0.318	0.466	0.184	0.387
New court commitment	0.584	0.493	0.579	0.494	0.590	0.492	0.598	0.490
Parole revocation	0.185	0.389	0.156	0.363	0.218	0.413	0.191	0.393
Probation revocation	0.073	0.261	0.087	0.282	0.058	0.234	0.034	0.181
N	2,599,631		1,372,493		1,227,138		446,831	

Notes: This table contains summary statistics by race and ethnicity for all variables used in the analysis. We restrict the sample to drug offenses. New court commitment, probation and parole revocation refer to the reason for prison admittance. Data are from the National Corrections Reporting Program (1985-2016).

Table 2: Difference-in-Difference, Triple-Difference, and Event-Study Estimates

	DD		DDD-Black		DDD-Hispanic	
	(1)	(2)	(3)	(4)	(5)	(6)
MML	-12.49		0.855		-36.57**	
	(14.34)		(8.764)		(16.78)	
MML(-4)		-3.110		-6.239		3.541
		(8.453)		(3.851)		(8.726)
MML(-3)		0.646		-0.572		-1.988
		(3.811)		(2.350)		(1.871)
MML(-2)		1.360		-1.174		15.39**
		(3.738)		(2.467)		(6.865)
MML(-1)		2.939		0.540		10.77**
		(2.908)		(1.586)		(4.482)
MML(1)		-1.953		2.847		-9.887
		(6.420)		(7.300)		(10.81)
MML(2)		-6.858		0.716		-25.27*
		(9.395)		(7.689)		(12.63)
MML(3)		-15.81		-4.513		-28.50**
		(10.23)		(8.215)		(13.84)
MML(4)		-28.32**		-7.651		-42.39***
		(14.02)		(10.94)		(12.69)
Mean Sentence	63.28	63.28	65.95	65.95	57.06	57.06
R-squared	0.477	0.478	0.511	0.511	0.219	0.220
N	2599631	2599631	2189282	2189282	1335286	1335286

Notes: The dependent variable is individual sentence length, measured in months. Standard errors clustered at the state level are shown in parentheses (forty-three clusters). In all regressions, we control for individual demographic characteristics (race, ethnicity, gender, age, age squared, highest grade complete, prior felony incarceration) and reason for prison admission. We also include indicators for missing data for each of these control variables. State and admission year fixed effects are included in all specifications. Column (1) reports the coefficient estimate on MML, a DD indicator that equals to 1 if a state has reformed (repealed or revised) its mandatory minimum sentencing laws for the year-month in which the offender was admitted to prison. Columns (3) and (5) report the coefficient estimate on the same MML variable interacted with an indicator for whether the offender is Black or Hispanic, respectively. Columns (2), (4), and (6) present the corresponding event-study estimates.
* $p < .10$, ** $p < .05$, *** $p < .01$
Data source: NCRP 1985-2016.

Table 3: Sensitivity Analysis

	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Difference-in-Difference					
	(1)	(2)	(3)	(4)	(5)	(6)
MML	-12.49	-10.56	-12.48	-12.55	-12.55	-12.54
	(14.34)	(14.17)	(14.42)	(14.42)	(14.42)	(14.42)
	Panel B: Triple-Difference–Black					
	(1)	(2)	(3)	(4)	(5)	(6)
MML	0.855	1.309	0.740	0.645	0.647	0.646
	(8.764)	(8.698)	(8.792)	(8.804)	(8.805)	(8.804)
	Panel C: Triple-Difference–Hispanic					
	(1)	(2)	(3)	(4)	(5)	(6)
MML	-36.57**	-33.64**	-36.74**	-36.73**	-36.73**	-36.73**
	(16.78)	(16.14)	(16.84)	(16.90)	(16.90)	(16.89)
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Admission year FE	Yes	Yes	Yes	Yes	Yes	Yes
State-specific time trend	No	Yes	No	No	No	No
General state trend	No	No	Yes	Yes	Yes	Yes
General state trend squared	No	No	No	Yes	Yes	Yes
Time trend	No	No	No	No	Yes	Yes
Time trend squared	No	No	No	No	No	Yes

Notes: The dependent variable is individual sentence length, measured in months. Standard errors clustered at the state level are shown in parentheses (forty-three clusters). In all regressions, we control for individual demographic characteristics (race, ethnicity, gender, age, age squared, highest grade completed, prior felony incarceration) and reason for prison admission. We also include indicators for missing data for each of these control variables.
* p < .10, ** p < .05, *** p < .01

Table 4: Difference-in-Difference and Triple-Difference Estimates: Controlling for Other Sentencing Reforms

	DD	DDD-Black	DDD-Hispanic
	(1)	(2)	(3)
MML	-7.167	5.758	-34.95**
	(15.17)	(8.833)	(15.92)
Mean Sentence	63.28	65.95	57.06
R-squared	0.478	0.511	0.220
N	2599631	2189282	1335286

Notes: The dependent variable is individual sentence length, measured in months. Standard errors clustered at the state level are shown in parentheses (forty-three clusters). In all regressions, we control for individual demographic characteristics (race, ethnicity, gender, age, age squared, highest grade completed, prior felony incarceration) and reason for prison admission. We also include indicators for missing data for each of these control variables. State and admission-year fixed effects are included in all specifications. Column (1) reports the coefficient estimate on MML, a DD indicator that equals 1 if a state has reformed (repealed or revised) its mandatory minimum sentencing laws after the offender was admitted to prison. Columns (2) and (3) report the coefficient estimate on the same MML variable interacted with an indicator for whether the offender is Black or Hispanic, respectively. Data are from the National Corrections Reporting Program (1985-2016).
* $p < .10$, ** $p < .05$, *** $p < .01$

Table 5: Difference-in-Difference and Triple-Difference Estimates: Consistent Sample

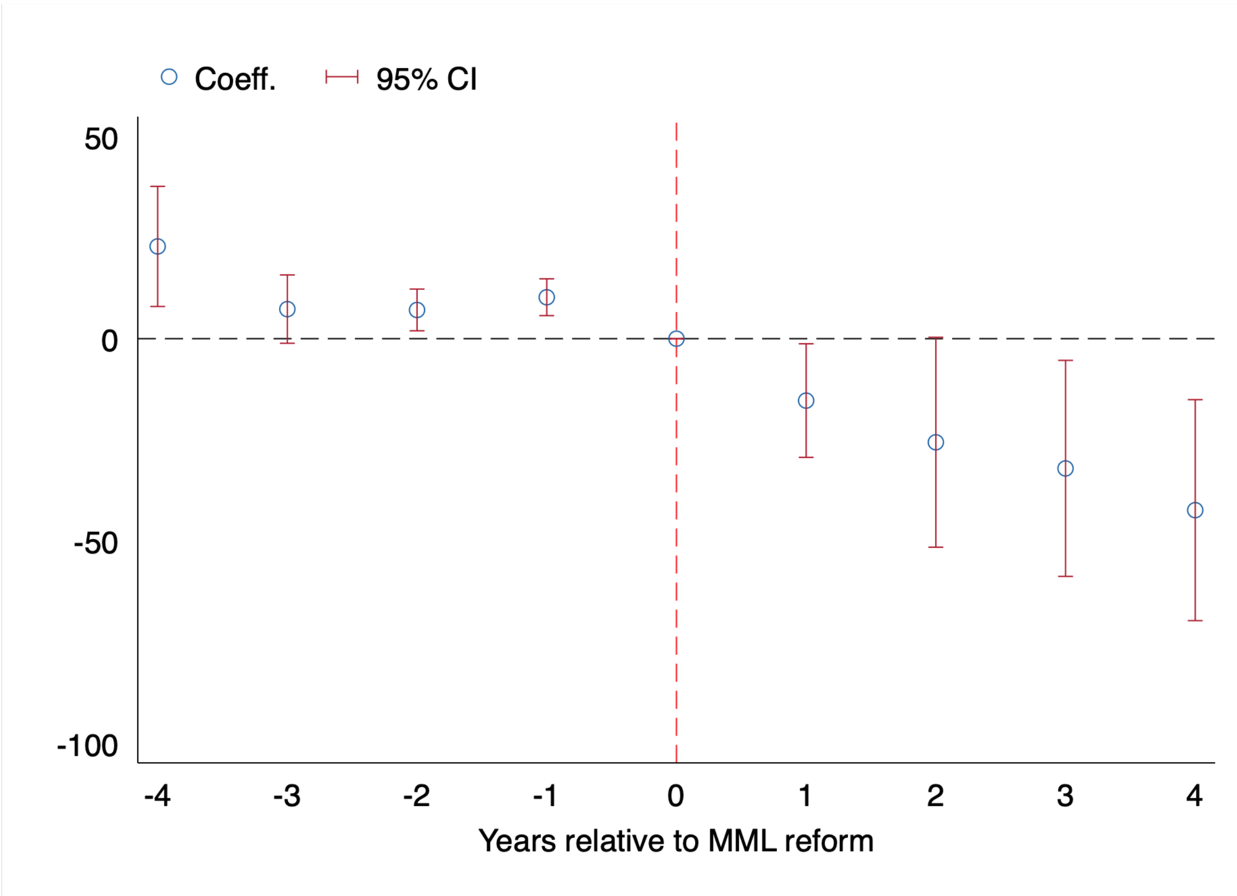
	DD	DDD-Black	DDD-Hispanic
	(1)	(2)	(3)
MML	-46.35***	32.79**	-28.42
	(12.78)	(11.40)	(19.66)
Wild bootstrap t-stat	-5.33	4.47	-1.69
Mean Sentence	59.43	61.93	56.55
R-squared	0.305	0.342	0.244
N	1118240	869007	563702

Notes: The dependent variable is individual sentence length, measured in months. The sample is limited to the eleven states that consistently reported data, as identified by Neal and Rick (2016): California, Colorado, Illinois, Kentucky, Michigan, Minnesota, Nebraska, New Jersey, South Dakota, Virginia, and Washington. As suggested by Cameron et al. (2008) in cases with a small number of clusters, we computed t-stat from one thousand wild-cluster bootstrap iterations for the MML coefficient of both the DD and DDD regressions. We cluster the standard errors at the county level. In all regressions, we control for individual demographic characteristics (race, ethnicity, gender, age, age squared, highest grade completed, prior felony incarceration) and reason for prison admission. We also include indicators for missing data for each of these control variables. State and admission-year fixed effects are included in all specifications. Column (1) reports the coefficient estimate on MML, a DD indicator that equals 1 if a state has reformed (repealed or revised) its mandatory minimum sentencing laws after the offender was admitted to prison. Columns (2) and (3) report the coefficient estimate on the same MML variable interacted with an indicator whether the offender is Black or Hispanic, respectively. Data are from the National Corrections Reporting Program (1985-2016).

* $p < .10$, ** $p < .05$, *** $p < .01$

A Appendix

Figure A1: Event-Study DD Estimates: Consistent Sample



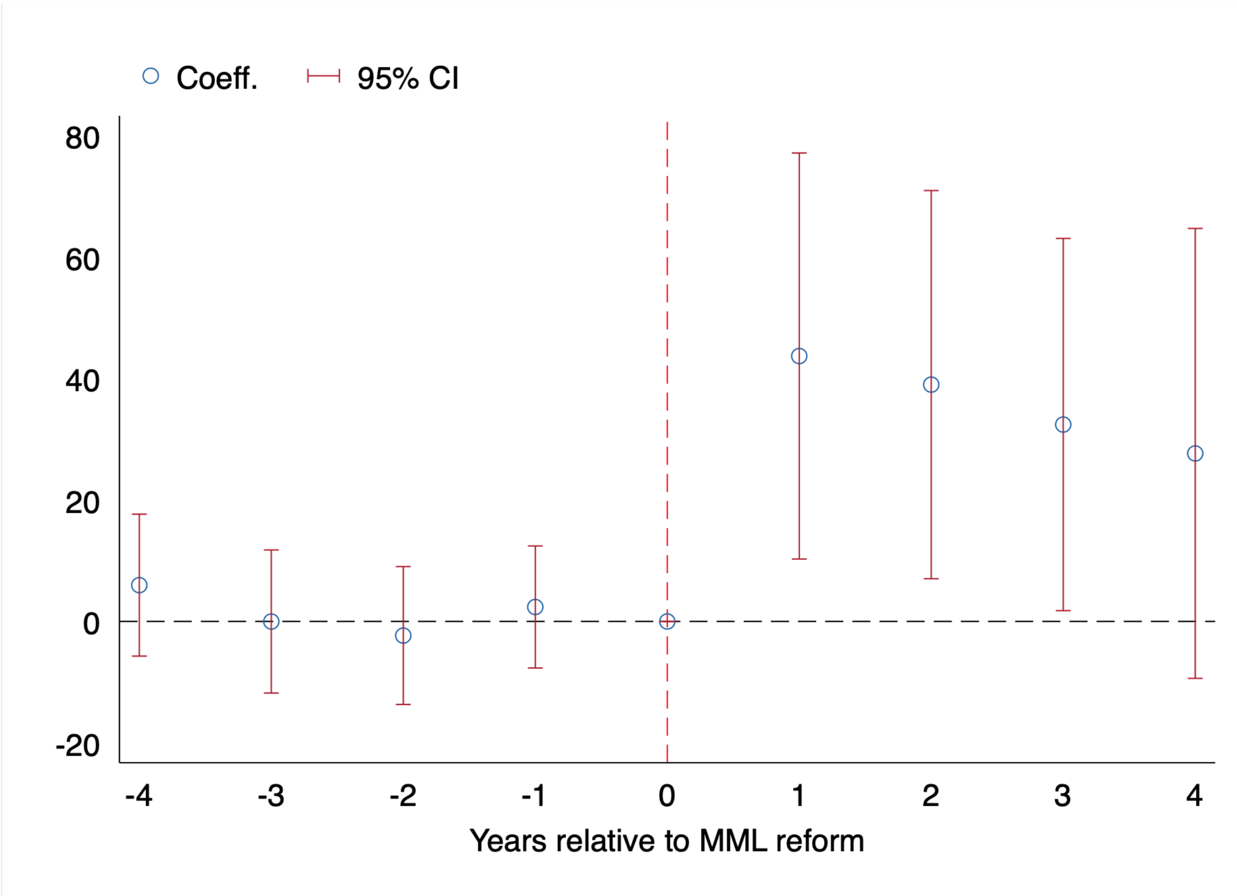
Notes: This figure plots event-study estimates ($\beta_L, L = \{-4, -3, -2, -1, 0, 1, 2, 3, 4\}$) and corresponding 95 percent confidence bands of equation 2. The dependent variable is individual sentence length, measured in months. The omitted dummy is year of implementation, so the coefficient β_0 is set to zero. We control for individual demographic characteristics (race, ethnicity, gender, age, age squared, highest grade completed, prior felony incarceration) and reason for prison admission. We also include indicators for missing data for each of these control variables. State and admission-year fixed effects are included in all specifications. The sample is limited to the eleven states that consistently reported data, as identified by Neal and Rick (2016): California, Colorado, Illinois, Kentucky, Michigan, Minnesota, Nebraska, New Jersey, South Dakota, Virginia, and Washington. Data are from the National Corrections Reporting Program (1985-2016).

Table A1: State Criminal-Law Changes

State	Date (1)	Type of Drug Crimes (2)	Consistent (4)	Repeal (5)
Alabama				
Alaska				
Arizona				
Arkansas	3/22/2011	Drug Possession		
California			Yes	
Colorado			Yes	
Connecticut	7/11/2005	Drug (Non-Violent)		
Delaware	6/3/2003	All		Yes
D.C.				
Florida	7/1/2014	Drug Trafficking		
Georgia	7/1/2012	Drug Possession		
Hawaii				
Idaho				
Illinois			Yes	
Indiana	1/1/2001	Drug Possession		
Iowa				
Kansas				
Kentucky				
Louisiana	6/29/2015	Drug(Non-Violent)		
Maine				
Maryland				
Massachusetts	8/06/2010	All		
Michigan	1/1/2002	All	Yes	Yes
Minnesota				
Mississippi	7/1/2014	All		
Missouri	8/28/2012	All		
Montana				
Nebraska			Yes	
Nevada				
New Hampshire				
New Jersey			Yes	
New Mexico				
New York	1/1/2004	All	Yes	Yes
North Carolina				
North Dakota				
Ohio	9/30/2011	All		
Oklahoma	5/9/2012	All		
Oregon				
Pennsylvania	1/1/2011	All		
Rhode Island	11/13/2009	All		Yes
South Carolina	6/2/2010	Drug Possession	Yes	Yes
South Dakota				
Tennessee				
Texas				
Utah	10/1/2015	All	Yes	
Vermont				
Virginia				
Washington			Yes	
West Virginia				
Wisconsin			Yes	
Wyoming				

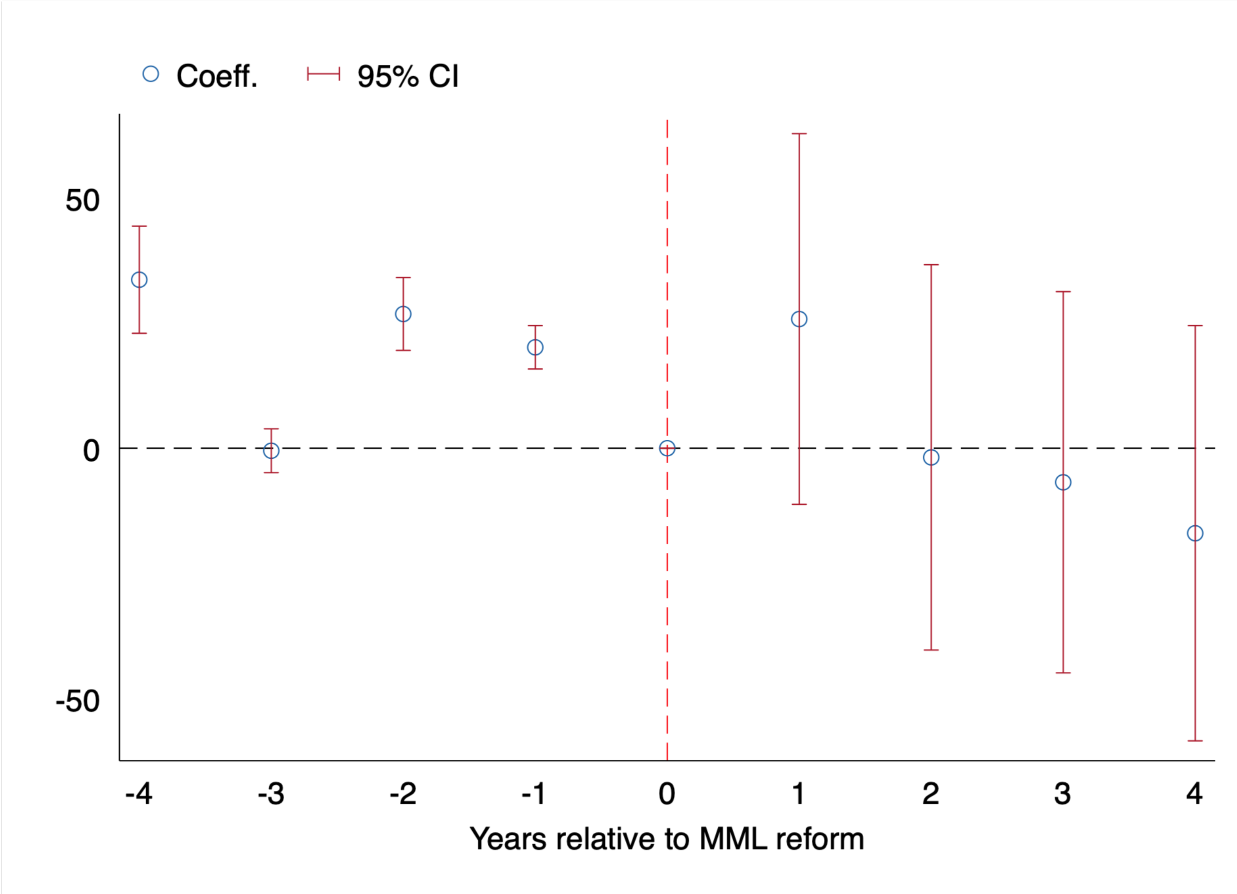
Notes: Column (1) reports the exact implementation date for states that modified or abolished mandatory minimum sentencing laws (MMLs). We were unable to find the day and month of MML repeals for Indiana, Michigan, New York, and Pennsylvania, and thus we assume the laws were passed on January 1. Column (2) lists the crimes for which MMLs were modified or lifted. Column (3) lists all states that consistently report to National Corrections Reporting Program, as identified by Neal and Rick (2016). These are the states we use for some of our robustness checks. Column (4) indicates whether the mandatory minimum were fully repealed.
Data sources: Sentencing Project, The State of Sentencing: Developments in Policy and Practice, various years, retrieved from `https://www.sentencingproject.org/issues/sentencing-policy/`; Subramanian and Delaney (2013); Austin (2010); `https://famm.org/`; and authors' own research on state statutes and legislative histories.

Figure A2: Event-Study DDD Estimates of the Black-White Sentence Gap: Consistent Sample



Notes: This figure plots event-study estimates ($\delta_L, L = \{-4, -3, -2, -1, 0, 1, 2, 3, 4\}$) and corresponding 95 percent confidence bands of equation 2. The dependent variable is individual sentence length, measured in months. The omitted dummy is year of implementation, so the coefficient δ_0 is set to zero. We control for individual demographic characteristics (race, ethnicity, gender, age, age squared, highest grade completed, prior felony incarceration) and reason for prison admission. We also include indicators for missing data for each of these control variables. State and admission-year fixed effects are included in all specifications. The sample is limited to the eleven states that consistently reported data, as identified by Neal and Rick (2016): California, Colorado, Illinois, Kentucky, Michigan, Minnesota, Nebraska, New Jersey, South Dakota, Virginia, and Washington. Data are from the National Corrections Reporting Program (1985-2016).

Figure A3: Event-Study DDD Estimates of the Hispanic-White Sentence Gap: Consistent Sample



Notes: This figure plots event-study estimates ($\delta_L, L = \{-4, -3, -2, -1, 0, 1, 2, 3, 4\}$) and corresponding 95 percent confidence bands of Equation 2. The dependent variable is individual sentence length, measured in months. The omitted dummy is year of implementation, so the coefficient δ_0 is set to zero. We control for individual demographic characteristics (race, ethnicity, gender, age, age squared, highest grade completed, prior felony incarceration) and reason for prison admission. We also include indicators for missing data for each of these control variables. State and admission-year fixed effects are included in all specifications. The sample is limited to the eleven states that consistently reported data, as identified by Neal and Rick (2016): California, Colorado, Illinois, Kentucky, Michigan, Minnesota, Nebraska, New Jersey, South Dakota, Virginia, and Washington. Data are from the National Corrections Reporting Program (1985-2016).