Mandatory Minimum Reforms, Sentencing, and Racial-Ethnic Disparities

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Abstract

Over the last twenty years, numerous states and the federal government enacted mandatory minimum reforms, especially for drug offenses. Yet little is known about how effective these reforms have been at the state-level in lowering drug sentences. Using quasi-experimental methods and administrative data, this study evaluates the impact of state-level mandatory minimum reforms on drug sentences and their concomitant racial-ethnic disparities. We find that state-level mandatory minimum reforms do not lower drug sentences in general or change racial-ethnic disparities statistically significantly. These findings suggest that the profound racial-ethnic bias sparked by state-level mandatory minimums are not fully ameliorated by subsequent state-level reforms.

JEL codes: K14, K42.

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

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1 Introduction

As a direct result of stringent criminal justice policies, the United States now has the highest incarceration rate in the world (Walmsley et al., 2018; Carson, 2018; Tonry, 2013). "Tough on crime" policies are designed to increase prison admissions 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 a central feature of U.S. federal and state criminal justice systems, ballooning prison populations and exacerbating racial disparities 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 (FSA) of 2010 aimed to reduce the racial gap in federal mandatory minimum sentences for powder and crack cocaine offenses. However, Bjerk (2017a) finds that the FSA did little to reverse patterns in excessive sentencing for crack cocaine defendants, but rather sustained *ex ante* downward sentencing trends. Moreover, we know very little about the efficacy of these reforms at the state level, and whether they help mitigate racial-ethnic disparities in sentencing outcomes. 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) (1997-2016), which provides prisoner-level data on offenses, demographics, admission and release dates, and judicially imposed sentences. Yet, it is important to acknowledge the limitations of the NCRP data, with numerous states reporting inconsistent administrative prison records (Neal and Rick, 2016; Pfaff, 2011). As such, the study follows Neal and Rick (2016) to define a restricted analysis sample comprised of the thirteen states[1] that consistently report prison admissions data. Since state-level mandatory minimums are predominantly applied to drug crimes, we also restrict our analyses to prisoners convicted of drug offenses. Next, to establish causality, we use a generalized difference-in-difference (DD) strategy to evaluate how exogenous variations in state mandatory minimum reforms change the drug sentences of prisoners relative to counterparts in states without any such reforms. In addition, we use a generalized triple-difference (DDD) strategy to evaluate how Black-White and Hispanic-White sentence disparities change in response to these reforms.

Using generalized DD estimation, we find that in general, mandatory minimum reforms do not change sentence durations for drug offenses statistically significantly. However, these reforms appear to have a delayed effect (largely driven by the state of New York), reducing drug sentences four or more years, *ex post.* In our heterogeneous analyses by race-ethnicity, we also observe a decline in drug sentences ($p < 0.10$) for Hispanic defendants; however, we are unable to determine whether this decline occurs in response to changes in judicial discretion (Fischman and Schanzenbach, 2012) or shifts in prosecutorial behavior (Didwania, 2020). Further research investigating changes in judicial and prosecutorial behaviors in response to state-level mandatory minimum reforms is imperative.

Our DDD estimates do not provide robust evidence that mandatory minimum sentence reforms reduce sentencing disparities between minorities and Whites. Although state-level mandatory minimum reforms appear to reduce the sentences of Hispanic pris-

[1]These states are California, Colorado, Georgia, Illinois, Michigan, North Dakota, New Jersey, New York, Ohio, South Carolina, Utah, Washington, and Wisconsin. We discuss the rational for selecting these states in more detail in Section 4.

oners relative to their White counterparts, nonparallel pre-reform trends render this finding biased.

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 sensitivity checks. Section 8 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, drug-related offenses) or triggering events (for example, offenses involving use of a firearm). At the federal level, mandatory minimums were enacted primarily for drug crimes and were chiefly based on some weight threshold of the drug. For instance, the 1986 Anti-Drug Abuse Act imposed a minimum five-year sentence for drug offenses involving 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).

At the state level, mandatory minimums are used as a blunt tool for crime deterrence. They broadly target certain crimes (for example, drug-related crimes), or certain drug quantity thresholds (for example, crack or cocaine), or are triggered by a particular benchmark (for example, school zones or repeat reoffenses). Albeit race-neutral, 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). As such, mandatory minimums not only helped generate the highest incarceration rate in the world, but also stark racial-ethnic disparities in the prison population.

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. 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). Plea bargains may appear to produce better sentencing outcomes for defendants, but they are not costless. The prosecutorial power of the plea bargain endangers the defendants' right to have their day in court and the opportunity to be acquitted. Further, the prosecutorial discretion that mandatory minimums afford can exacerbate racial-ethnic disparities in sentencing, with more favorable deals going to White defendants compared to their minority counterparts, who disproportionately comprise the correctional population (Starr and Rehavi, 2013; Ulmer et al., 2007). Therefore, mandatory minimums tend to worsen sentencing disparities inside and outside the courtroom, prompting widespread activism for reform.

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: repeal or revision of mandatory minimums, expansion of judicial discretion, "second look" judicial review, and repeal or revision of automatic sentence enhancements (Families Against Mandatory Minimums, 2019).

The primary focus of our study is mandatory minimum sentence repeals and revisions given that judicial discretion under "safety valves", second look, and automatic sentence enhancements is all predicated on conditions we cannot observe in our data.[2] Repeals and

[2] "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 individuals who pose less of a public safety threat. Second look sentencing is a process

revisions represent full or partial modifications to existing mandatory minimum sentence laws, typically issued for drug offenses. For instance, Arkansas, revised their mandatory minimum laws by narrowing the crimes to which these laws apply. In 2012, Missouri simply lowered the mandatory minimum sentence for some crack cocaine trafficking offenses. Therefore, mandatory minimum reforms give judges more discretion over what sentences to hand down, while mitigating the prosecutors' leverage to negotiate potentially biased sentences or wrongful convictions.

Because repeals or revisions to state mandatory minimum laws primarily target drug offenses, our analyses evaluate how such drug-, state-, and year-specific reforms impact judicially imposed sentences, *ex post*.[3] Figure OA1 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. Because the study focuses on reforms that either revise or fully repeal mandatory minimums, the results represent a lower bound of 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.

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 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.

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

3 Literature Review

There are three main strands of the growing literature on mandatory minimum sentences. The first strand exploits state-level crime data to examine changes to sentencing 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.[4] Helland and Tabarrok (2007) find that California's three-strikes laws reduce felony arrest rates by 17 to 20 percent among individuals who already have two strikes. On the other hand, Marvell and Moody (2001) find that three-strikes laws increase homicides, with 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 of the literature examines racial disparities in the prosecutorial application of mandatory minimum sentences and judicial adherence to sentencing guidelines at the federal level. Rehavi and Starr (2014) find that mandatory minimums explain a significant portion of the Black-White sentencing gap. In addition, Yang (2015) and Starr and Rehavi (2013) show that mandatory minimums were more likely to be used against Black defendants after the *Booker* decision.[5] *Rita, Gall,* and *Kimbrough* promoted additional departures from *Booker*, which ultimately did not benefit Black defendants (Fischman and Schanzenbach, 2012). Insofar as sentences for White and Hispanic defendants declined post-*Rita*, sentences for Black defendants flatlined, except under binding

[4] In addition, Lofstrom et al. (2020) argue that Prop 47 lowered arrests, bookings, and pretrial detention quickly and substantially.

[5] This refers to the *United States v. Booker* majority decision that made federal sentencing guidelines advisory rather than mandatory.

mandatory minimum constraints. Multiple reports from the U.S. Sentencing Commission also found that racial disparities increased after *Booker* (USSC, 2006, 2011, 2010), though the racial gap appears to narrow in more recent years (USSC, 2017).[6]

More recently, Tuttle (2019) uses irregular bunching around drug thresholds to assess the extent to which the FSA helped change racial-ethnic disparities in crack-cocaine sentences, and highlights the key roles of prosecutorial discretion and racial discrimination in explaining this bunching.[7] He finds that Black and Hispanic defendants tend to cluster above the mandatory minimum threshold that triggers a ten-year mandatory minimum, while White defendants disproportionately appear below the threshold. Sorensen et al. (2014) find that under federal mandatory minimum guidelines, judicial preferences tend to disadvantage Black males relative to White counterparts. In addition, defendants that are high school graduates or female – initially subject to harsher sentences under these guidelines – receive more lenient sentences *ex post*, although the latter reduction is attributed to changes in the methodology of sentence application rather than judge's leniency (Nutting, 2013).

The third (and most scant) strand of the literature evaluates how *reforms* to mandatory minimum policies change sentencing patterns and whether these changes differentially impact minority defendants. The Fair Sentencing Act of 2010 aimed to reduce the racial-ethnic gap in federal mandatory minimum sentences for powder and crack cocaine offenses. In two related papers, Bjerk (2017a,b) evaluate the impact of the FSA on federal sentencing, by examining the implementation of mandatory minimums and their differential impact on sentencing for individuals convicted of federal crack and powder cocaine crimes. Bjerk (2017a) concludes the FSA was not primarily responsible for the decline

[6] Racial disparities in federal sentencing has also been documented in multiple reports from various federal agencies. For example, USSC (2018) reports that penalty enhancement for federal drug trafficking with a prior felony drug conviction is disproportionally used against Blacks.

[7] At the state level, Sloan (2019) finds prosecutors do not generally exhibit racial bias, except against individuals charged with property offenses.

in sentences for crack cocaine defendants, but continued the existing downward trend in sentences. Bjerk (2017b) shows that while eligibility for mandatory minimum sentences raises sentences on average, this increase is not uniform across individuals. For example, first-time drug defendants are likely to avoid prosecution because of "safety valve" provisions. Additionally, 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 – modestly reduced sentence length for those eligible for mandatory minimums, but did not reduce the corresponding racial-ethnic disparities.

We complement the first strand of the literature by examining whether reforms to state mandatory minimum policies impact criminal sentencing, as well as the second strand, by evaluating the extent to which racial-ethnic sentencing disparities change in response to these reforms. However, our study makes the most significant contribution to the third strand, by complementing this literature in three ways. First, we directly examine racial-ethnic disparities in sentencing after mandatory minimum reforms. This lies in contrast to Bjerk (2017a,b), which only examine sentencing disparities between crack and powder cocaine defendants. Second, we add to this literature by evaluating 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. Understanding the effect of these reforms at the state-level is of particular policy interest, given that majority of people incarcerated for drug offenses are charged at the state level.[8]

[8] In 2019, Carson (2020) estimated that roughly 73,200 individuals were incarcerated for a drug crime at the federal level compared to 176,300 at the state level.

4 Data

We use data on prison admissions from the National Corrections Reporting Program (NCRP) (1997-2016) 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. Over the 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 prisoner along with the corresponding 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 individual 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 prisoner was convicted and the total 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. Since the study evaluates the effect of mandatory minimums on sentencing, we consider prisoners who have either been released from prison or are currently incarcerated.

Figure A1 shows the average sentence length of prisoners in the NCRP data.[9] The average sentence is 62 months, or approximately five years. For Blacks, the average sentence is about 68.5 months, whereas White and Hispanic prisoners have similar average sentences of 59.4 and 54.5 months, respectively. This suggests that the average Black-White sentence gap is about nine months, while the Hispanic-White sentence gap is significantly lower at about five months.

Table 1 shows the summary statistics for all the control variables in our empirical

[9] We topcoded sentences at 720 months or 60 years. We tested a variety of sentence topcodes or upper bounds, and find that the results are qualitatively similar to the main findings. Our results are also robust to winsorizing the outcome at the 99^{th} percentile.

analysis using the full NCRP sample of states.[10] Unsurprisingly, the analysis sample is largely comprised of minority males with low education. Almost half of the prisoners are Black and about 17 percent are Hispanic. Thirty percent of prisoners are high school dropouts, and the average age at prison admission is 34.5 years old. Roughly 27 percent of prisoners had felony convictions prior to their current incarceration episode, and close to 25.7 percent of prisoners were incarcerated for violating parole or probation.[11]

Prisoner characteristics and offenses vary significantly by race and ethnicity. Twenty-four percent of Black prisoners have high school diplomas, and a higher percentage of Blacks (roughly 30.6 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 youngest age of admission, at 33.6 years old and the lowest incidence of probation or parole revocation, at 22 percent. About 4 percent of White and 3 percent of Black prisoners have more than a high school diploma compared to 1.6 percent of Hispanic prisoners. [12] White prisoners are the oldest at the age of admission, at nearly 35.3 years old.

However, the NCRP has a key limitation in that it relies on potentially inconsistent voluntary state reporting. Several studies using 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 the purposes of this study, we follow Neal and Rick (2016) to identify states that provide consistent NCRP reports. Neal and Rick (2016) use an extract of the NCRP that better matches the time frame of our sample compared to Pfaff (2011), and performs both an external and internal vetting of the prison admissions

[10] We present the summary statistics for the thirteen-state consistent sample in Table A2. Note that the time-invariant personal and prison-spell characteristics in the NCRP have missing data. Thus, we report the fraction of prisoners with missing race, ethnicity, and educational level in our summary statistics.

[11] Although the sample seems to consist of fewer recidivists, this is not completely surprising given that prior felony incarceration data are missing for some states (Yang, 2017).

[12] These statistics should be interpreted with caution given that a large percentage of the sample has missing values in educational level.

data, imperative for accurately measuring our outcome of interest.

Therefore, in addition to using the full NCRP sample, we also restrict our analyses to the thirteen states — California, Colorado, Georgia, Illinois, Michigan, North Dakota, New Jersey, New York, Ohio, South Carolina, Utah, Washington, and Wisconsin — that Neal and Rick (2016) show consistently report prison inflows and outflows from 1983 through 2009.[13] In particular, we restrict our sample to states that pass the first and third consistency tests in Neal and Rick (2016),[14] which concludes that for these states, the NCRP records on admissions and releases are internally consistent (i.e., for a given year, the total number of prisoners released with recorded admission dates do not exceed the number of prisoners recorded in the admissions files) and externally consistent (i.e., there are no large deviations in terms of admissions and releases of prisoners between the NCRP data and the National Prisoner Statistics data). Table A2 presents the summary statistics for this consistent sample. In general, they are statistically similar to those presented in Table 1.

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:

[13] Of these states, Michigan, New York, and South Carolina revised or repealed their MMLs.

[14] We also want to point out that neither Pfaff (2011) nor Neal and Rick (2016) vet the NCRP data for specific years. As such, it is possible that for specific years, some of the states excluded from the consistent sample might pass internal and external consistency tests.

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

where $Sentence_{ist}$ is total 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 individual 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. ϵ_{ist} is serially-correlated, and thus we cluster standard errors 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 following subsection.

5.2 Event-Study Design

We extend our difference-in-difference framework to an event study by including leads and lags of treatment as regressors. The event-study specification can be written as follows:[15]

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

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.[16] Similar to the variable of interest in the difference-in-difference 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^L_{st} dummies represents the year, L, relative to the enactment of the mandatory minimum sentencing reform. (L = 0 denotes the year of implementation of 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 one 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 Section 6, 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.

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

[16]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^{-4}_{st} accounts for all reforms of mandatory minimums occurring four or more years, *ex ante*.

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. Next, we expand on this analysis to explore whether these effects are more pronounced among Black and Hispanic prisoners relative to White counterparts. To evaluate whether mandatory minimum reforms change Black and Hispanic sentences statistically significantly relative to Whites, we adopt the following triple-difference (DDD) model:

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

where BH_i is a binary indicator equal to 1 if the prisoner is Black(Hispanic) and 0 if the prisoner is White. 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 individual was admitted. The coefficient of interest on the interaction $MML * BH_i$, δ_1, measures the net impact of mandatory minimum reforms on the sentences of Black(Hispanic) 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. [17] Identification of causal effects in the event-study DDD design also requires common trends before treatment.

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

$$\begin{aligned} Sentence_{ist} =& \alpha + \sum_{L \in K} \delta_L MML^L_{st} * BH_i + \beta_1 MML_{st} + \beta_2 BHi + \beta_3 BH_{ist} * \lambda_s \\ & + \beta_4 \gamma_t * BH_{it} + \beta_5 X_{it} + \gamma_t + \lambda_s + \epsilon_{ist} \end{aligned} \tag{4}$$

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.

6 Results

Table 2 presents general and event-study DD impact estimates from equations 1 and 2 using the full and restricted NCRP samples, respectively. Columns (1) and (3) show that mandatory minimum reforms reduce sentences by about 11 and 25 months in general, although these estimates are not statistically different from zero. To evaluate the dynamic effects of these reforms and test the parallel-trend assumption, Columns (2) and (4) present the event-study impact estimates from equation 2. Pre-reform estimates are positive, but are not statistically different from zero, confirming that outcome trends of treatment and comparison groups are parallel. Figure 1 illustrates these event-study DD estimates along with their confidence intervals. While we use the graphical representation of the event-study specifications to illustrate flat pre-trends, we also observe that rather than dissipating, the treatment effect grows over time to reduce sentence length by up to 25 months ($p < 0.10$) in the full sample and 36 months in the consistent sample ($p < 0.05$), *ex post*.[18]

We also explore treatment effect heterogeneity by race and ethnicity in Figure 2. We find evidence that lower drug sentences have a downward trend, *ex post*; however, the impact is only statistically significant for Hispanic defendants. Figure 2 depicts parallel pre-reform trends for Hispanics, boosting our confidence in these heterogeneous effects.

Given that the literature has already established that minorities are more likely to be affected by mandatory minimums (Rehavi and Starr, 2014; Yang, 2015; Starr and

[18] We also want to note that our generalized difference-in-difference model uses staggered adoption of mandatory minimum reforms. Given that our results suggest that the impact of mandatory minimum reforms vary over time, we follow (De Chaisemartin and d'Haultfoeuille, 2020) to test for the presence of negative weights in our estimation. We find no negative weights, but instead all positive weights. De Chaisemartin and d'Haultfoeuille (2020)'s explanation for the lack of negative weights is: the random assignment of treated units to each group, treatment effects do not differ statistically significantly between periods with many versus few treated groups, and between groups are treated for many versus few periods. De Chaisemartin and d'Haultfoeuille (2020) also note that "[o]verall, negative weights are much more prevalent in the "more early adopters" than in the later adopters case." In our study, there are very few early adopters, which suggests that early treatment effects might not differ substantially from later treatment effects.

Rehavi, 2013), it is highly relevant to determine how the Black-White and Hispanic-White sentence disparities change in response to these reforms. The study measures the impact of mandatory minimum reforms on racial-ethnic disparities in sentencing using DDD estimation. These findings are presented in equation 3 in Table 3. For example, Table 3 Columns (3) and (7) show that Hispanic prisoners receive sentences more than 30 months ($p < 0.05$) lower than White counterparts, *ex post*. On the other hand, DDD analyses show no statistically significant changes in the Black-White sentence disparity for drug offenses in response to the reforms.[19]

However, we must underscore that the validity of these DDD results hinges on parallel pre-reform outcome trends. As such, we present event-study DDD estimates for the Black-White and Hispanic-White sentence disparities in Table 3 and in Figures 3 and 4. The findings reveal that the identifying assumption of parallel trends is violated in both samples, with pre-reform indicators that are statistically significant. 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. Therefore, these DDD results indicate that although sentencing disparities grew under mandatory minimum sentencing guidelines, there is no evidence that these disparites diminish when such guidelines are relaxed (Mustard, 2001; Fischman and Schanzenbach, 2012).

One limitation of the thirteen-state consistent sample is that only three states (Michigan, New York, and South Carolina) are treated. Although the majority of these states repealed (rather than revised) their mandatory minimum sentencing guidelines, the corresponding impact estimates are not statistically different from the general estimates. Additionally, the few-treated-states problem produces incorrect standard errors, which

[19] To gain more efficiency, we also pool all observations in the full sample and re-estimate the DDD model as one regression equation. The results from the pooled DDD model – presented in Online Appendix Table OA1 – are qualitatively similar to the non-pooled DDD estimates in Table 3.

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 are statistically similar to the main findings.

7 Sensitivity Checks

We run numerous checks to test the sensitivity of our estimates. First, we explore the sensitivity of our estimates to the inclusion of different time trends.[20] For DD and DDD models, Online Appendix Table OA2 shows that the inclusion of state-specific linear and quadratic time trends do not change the full or consistent sample estimates statistically significantly. Additionally, standard errors remain quite stable in each specification. We also estimate the robustness of results to the inclusion of state-by-admission-year fixed effects. We present these estimates in Online Appendix Table OA3. The general DD results are smaller in magnitude, but remain statistically equivalent to zero. However, the event-study analysis shows that state-level mandatory minimum reforms lower drug sentences statistically significantly, four or more years, *ex post*. We also observe that post-reform, the sentence gap between Blacks and Whites is exacerbated.

We also test the robustness of the findings to outliers and alternative functional form assumptions. Online Appendix Tables OA4 and OA5 indicate the results remain statistically similar to the main findings when we winsorize (at the 99^{th} percentile) and log-transform sentence length, respectively. Moreover, prior felony incarceration has information missing for some states; however, excluding this control variable does not change our findings statistically significantly (Neal and Rick, 2016) (see Online Appendix Table OA6).

It is important to note that mandatory minimum reforms – measured as repeals or revisions to mandatory minimums – may be a part of sweeping overhauls to state-level

[20] Time is defined as prison admission year.

sentencing guidelines. To test this possibility, we construct a binary indicator 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 Panels A and B of Online Appendix Table OA7. The results remain statistically similar to the main findings.

We also examine whether the general findings are driven by specific states, by excluding each state from the full and consistent sample analyses in succession. Online Appendix Figure OA2 shows that when Indiana and Michigan are excluded from the analyses, the general estimate falls to just below zero; meanwhile, excluding Missouri and New York raises the general estimate to just above zero. However, standard errors remain large for these estimates, suggesting that despite the deviation from zero, the general finding that mandatory minimum reforms do not statistically significantly change drug sentences holds. We also exploit this "leave-one-out' method for the DDD analyses used to evaluate Black-White and Hispanic-White sentencing disparities, respectively. Online Appendix Figures OA3 and OA4 follow a similar pattern to Online Appendix Figure OA2, where a few states deviate from the general zero estimate, but retain large standard errors.

Still, we must acknowledge that in all of these figures, New York appears to have the strongest influence on the findings – the estimates all shift statistically significantly when it is excluded from the analysis samples.[21] The results in Online Appendix Table OA8 Columns (1)-(2) suggest that the decline in sentences, as a result of mandatory minimums reforms, is likely driven by the state of New York because the point estimate is no longer statistically significant, four or more years, *ex post*. Yet, this result is not surprising. In 2004, the state of New York passed a major reform that overhauled the criminal justice system, including mandatory minimum sentences. Subsequently, the 2009 Drug

[21] Note that when we exclude Missouri – another state that our estimates are highly sensitive to – we find statistically significant effects; however, Missouri does not consistently report data to the NCRP and likely accounts for the biased results (see Columns (3)-(4) in Online Appendix Table OA8).

Law Reform Act eliminated all mandatory minimum sentences and expanded judicial discretion over drug treatment and rehabilitation as an alternative to incarceration.[22] As such, this may explain why we observe the largest decrease four or more years after the first mandatory minimum reform in 2004.

Because of the guidelines of mandatory minimum reforms, people who are charged with a drug offense, *ex ante*, may not receive the same charge, *ex post*. As such, our evaluation of drug sentences as the outcome of interest may understate the role of mandatory minimum reforms. To test this possibility, we evaluate the number and fraction of sentences by type of offense as outcome variables. Online Appendix Figure OA5 shows that changes in the number of sentences for violent, property, drug trafficking, drug possession, and any drug offense in the years preceding and proceeding mandatory minimum reforms are statistically similar to zero. Meanwhile, the number of sentences for other offenses, broadly defined, increases statistically significantly over time. When we evaluate the composition of crimes – as measured by the fraction of total sentences for each offense – Online Appendix Figure OA6 shows that the estimates pre- and post- reform are in general statistically equivalent to zero, except four or more years, *ex post*. Therefore, along with the role of New York in the analysis samples, the composition of crimes may help explain why drug sentences decline four or more years after mandatory minimum reforms. However, we cannot ignore that for this sensitivity check, the estimates on four or more years, *ex post*, are small in magnitude; as such, crime composition is unlikely to fully explain the decline in drug sentences four or more years after mandatory minimum reforms took effect.

[22]In fact, we observe a relatively large decrease in drug-crime-related prison admissions in New York after this reform (see Online Appendix Table OA9).

8 Conclusions

The existing literature explores the effect of mandatory minimum sentencing guidelines on criminal justice outcomes. However, most of this literature evaluates the impact of changes in federal sentencing guidelines and practices (Fischman and Schanzenbach, 2012; Rehavi and Starr, 2014; Yang, 2015; Starr and Rehavi, 2013; Didwania, 2020) or examine the effect of the Fair Sentencing Act of 2010 on federal sentencing patterns (Bjerk, 2017a,b; Tuttle, 2019). This study, on the other hand, investigates the effect of mandatory minimum reforms at the state level, where more than 60 percent of US prisoners convicted of a drug offense are housed (Carson, 2020).

Specifically, our study evaluates whether repealing or revising state-level mandatory minimums can reduce judicially imposed sentences and concomitant racial-ethnic disparities. We use prisoner-level data from the National Corrections Reporting Program (1997 - 2016) along with generalized difference-in-difference and triple-difference estimation to identify the impact of these reforms. We uncover that in general, state reforms to mandatory minimums do not change overall drug sentences statistically significantly. However, these reforms reduce drug sentences four or more years after the state mandatory minimum reform took effect (although this impact is largely driven by New York). We also observe that drug sentence declines are most pronounced for Hispanic defendants.

Our study acknowledges that the NCRP does not consistently report prison admissions data for all states. As such, we restrict the analysis sample to the thirteen states that have been identified by Neal and Rick (2016) to provide internally and externally consistent reporting of these data. Using this restricted analysis sample, the general finding holds: state-level mandatory minimum reforms do not lower drug sentences statistically significantly.

We also explore whether the disparate impact of early race-neutral mandatory minimum policies (Schlesinger, 2011; Bonilla-Silva, 2006) is ameliorated by subsequent mandatory minimum revisions or repeals. To do this, we use DDD estimation to evaluate changes in Black-White and Hispanic-White disparities in drug sentencing, *ex post*. We do not find statistically significant evidence that revisions or repeals of mandatory minimum laws decrease sentencing disparities. This is consistent with the prior literature that finds little (Fischman and Schanzenbach, 2012) to modest reductions (Didwania, 2020) in racial disparities in sentencing in response to sentencing reforms.

Our findings may be driven by changes in judicial treatment of minority defendants (Fischman and Schanzenbach, 2012), or changes in prosecutorial behavior (Didwania, 2020). While this goes beyond the scope of our paper, future research dedicated to disentangling these mechanisms is exceedingly important.

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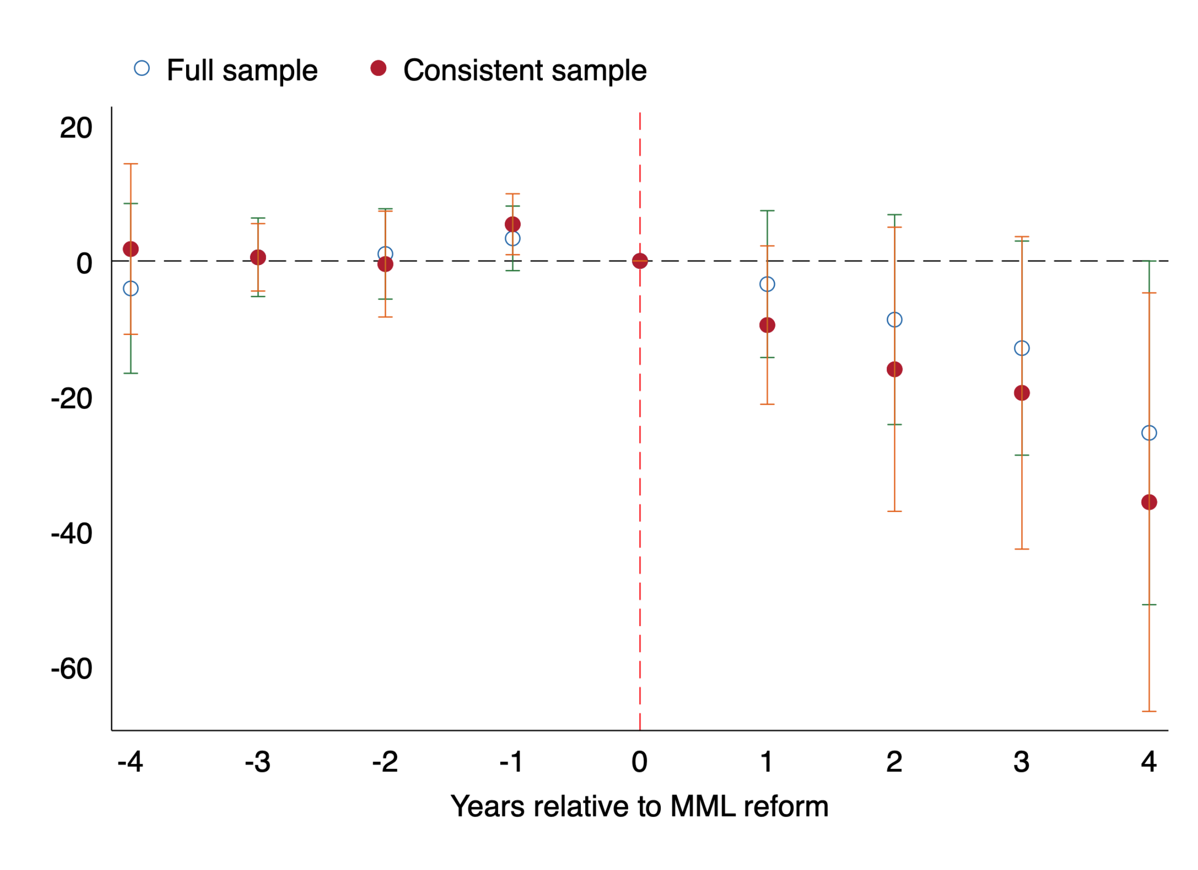
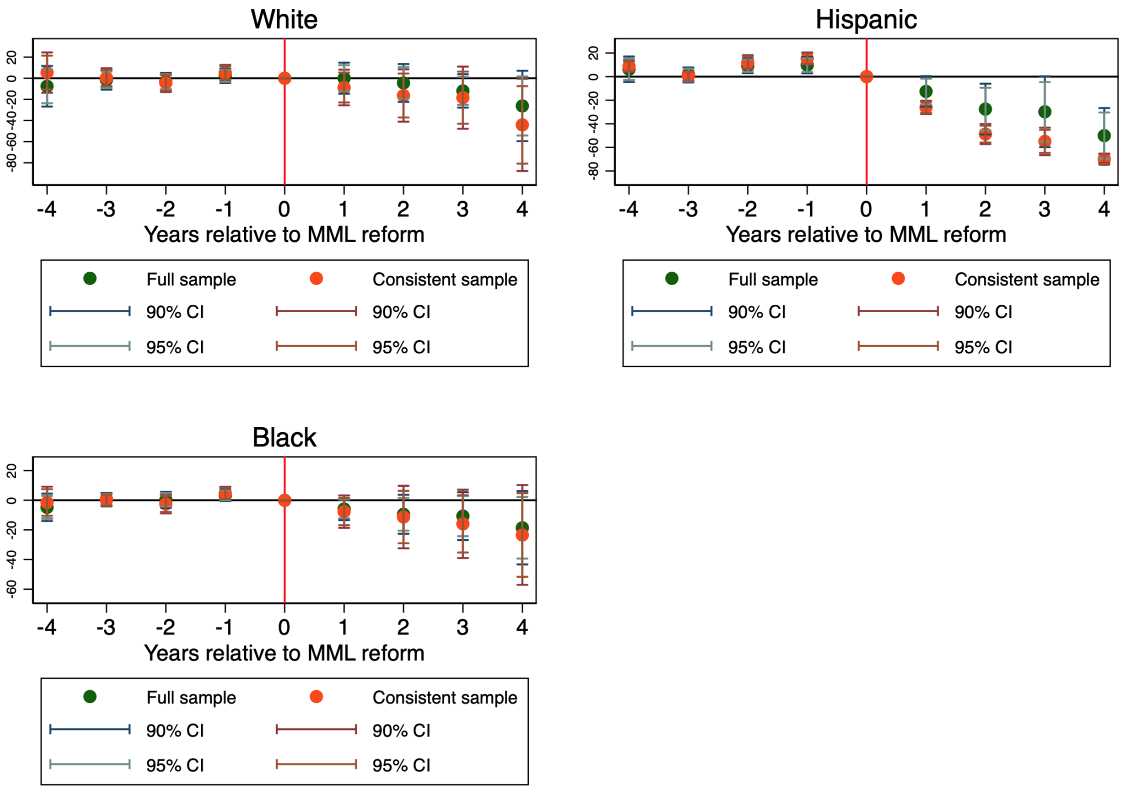
9 Figures and Tables

Figure 1: Event-Study DD Estimates



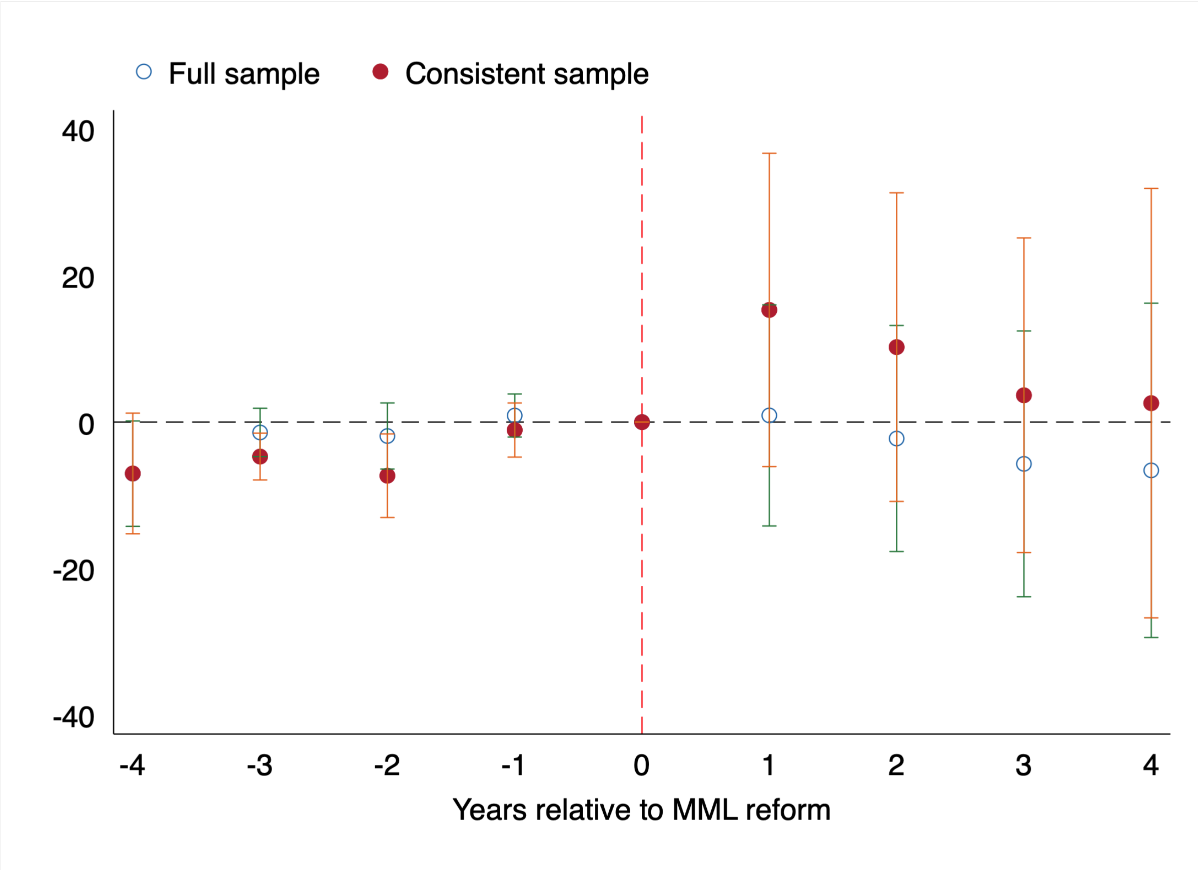
Notes: This figure plots event-study estimates ($\beta_L, L = \{-4,-3,-2,-1,0,1,2,3,4\}$) and corresponding 95 percent confidence bands of equation 2. The full sample consist of all states reporting to NCRP (1997-2016) while the consistent sample is restricted to the thirteen states that consistently reported data, as identified by Neal and Rick (2016): California, Colorado, Georgia, Illinois, Michigan, North Dakota, New Jersey, New York, Ohio, South Carolina, Utah, Washington, and Wisconsin. 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.

Figure 2: Event-Study DD Estimates by Race-Ethnicity



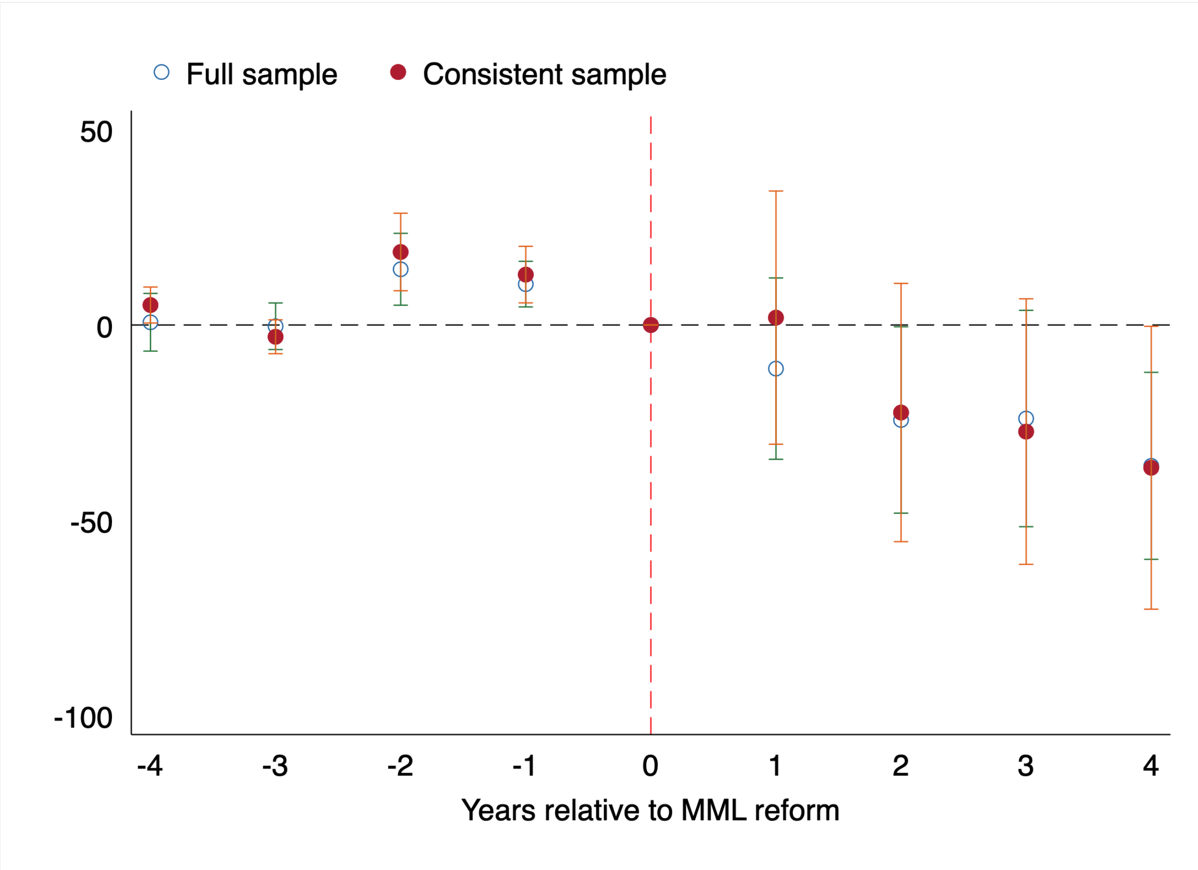
Notes: This figure plots event-study estimates ($\delta_L, L = \{-4, -3, -2, -1, 0, 1, 2, 3, 4\}$) and corresponding 90 and 95 percent confidence bands. These estimates are computed separately subsamples of NCRP based on race or ethnicity. The full sample consist of all states reporting to NCRP (1997-2016) while the consistent sample is restricted to the thirteen states that consistently reported data, as identified by Neal and Rick (2016): California, Colorado, Georgia, Illinois, Michigan, North Dakota, New Jersey, New York, Ohio, South Carolina, Utah, Washington, and Wisconsin. 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 (1997-2016).

Figure 3: 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 estimation sample consist of all states reporting to NCRP (1997-2016). 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.

Figure 4: 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 full sample consist of all states reporting to NCRP (1997-2016) while the consistent sample is restricted to the thirteen states that consistently reported data, as identified by Neal and Rick (2016): California, Colorado, Georgia, Illinois, Michigan, North Dakota, New Jersey, New York, Ohio, South Carolina, Utah, Washington, and Wisconsin. 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.

Table 1: Summary Statistics

	All		White		Black		Hispanic	
	Mean	S.D.	Mean	S.D.	Mean	S.D.	Mean	S.D.
Sentence length (in months)	62.147	91.977	59.410	80.246	68.466	105.320	54.494	82.553
Black	0.470	0.499	0.000	0.000	1.000	0.000	0.023	0.149
White	0.374	0.484	1.000	0.000	0.000	0.000	0.203	0.402
Missing/other race	0.156	0.363	0.000	0.000	0.000	0.000	0.774	0.418
Female	0.138	0.345	0.209	0.407	0.093	0.290	0.094	0.291
Male	0.862	0.345	0.791	0.407	0.907	0.290	0.906	0.291
Hispanic	0.172	0.378	0.094	0.291	0.008	0.091	1.000	0.000
Not Hispanic	0.641	0.480	0.721	0.449	0.760	0.427	0.000	0.000
Missing/other ethnicity	0.186	0.389	0.185	0.389	0.231	0.422	0.000	0.000
Less than HS Degree	0.300	0.458	0.267	0.442	0.339	0.473	0.304	0.460
HS Degree	0.246	0.431	0.282	0.450	0.241	0.428	0.179	0.383
Some college	0.034	0.181	0.042	0.201	0.034	0.180	0.014	0.118
College Degree	0.004	0.064	0.006	0.078	0.003	0.058	0.002	0.042
Missing education	0.413	0.492	0.398	0.489	0.380	0.485	0.500	0.500
Age at prison admission	34.549	9.811	35.323	9.596	34.221	10.083	33.619	9.430
Prior Felony Incarceration	0.269	0.443	0.253	0.435	0.306	0.461	0.181	0.385
New court commitment	0.588	0.492	0.579	0.494	0.598	0.490	0.607	0.488
Parole revocation	0.186	0.389	0.152	0.359	0.215	0.411	0.187	0.390
Probation revocation	0.071	0.256	0.103	0.304	0.055	0.228	0.035	0.185
N	2,788,102		1,042,456		1,311,636		480,109	

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. We report percent of the data with missing or other race, ethnicity, and educational attainment. Data are from the National Corrections Reporting Program (1997-2016).

Table 2: Main Results: Difference-in-Difference Estimates

	Full Sample		Consistent Sample	
	General	Event Study	General	Event Study
	(1)	(2)	(3)	(4)
MML	-11.21		-24.69	
	(11.58)		(14.20)	
MML(-4)		-4.064		1.762
		(6.409)		(6.438)
MML(-3)		0.544		0.537
		(2.964)		(2.546)
MML(-2)		1.038		-0.462
		(3.410)		(3.996)
MML(-1)		3.341		5.430**
		(2.438)		(2.300)
MML(1)		-3.424		-9.487
		(5.542)		(5.979)
MML(2)		-8.683		-16.03
		(7.923)		(10.73)
MML(3)		-12.90		-19.52
		(8.083)		(11.79)
MML(4)		-25.43*		-35.69**
		(12.97)		(15.80)
Mean Sentence	62.15	62.15	58.81	58.81
R-squared	0.445	0.446	0.300	0.302
N	2788102	2788102	1392894	1392894

Notes: Column (1)-(2) are the full sample estimates while Columns (3)-(4) represents estimates from the sample restricted to the thirteen states that consistently reported data, as identified by Neal and Rick (2016): California, Colorado, Georgia, Illinois, Michigan, North Dakota, New Jersey, New York, Ohio, South Carolina, Utah, Washington, and Wisconsin. The dependent variable is individual sentence length, measured in months. Column (1) and (3) report 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 individual was admitted to prison. Columns (2) and (4) present the corresponding event-study estimates, with the number in brackets on the MML variable indicating years prior or post mandatory minimum reforms. 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.

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

Data source: NCRP 1997-2016.

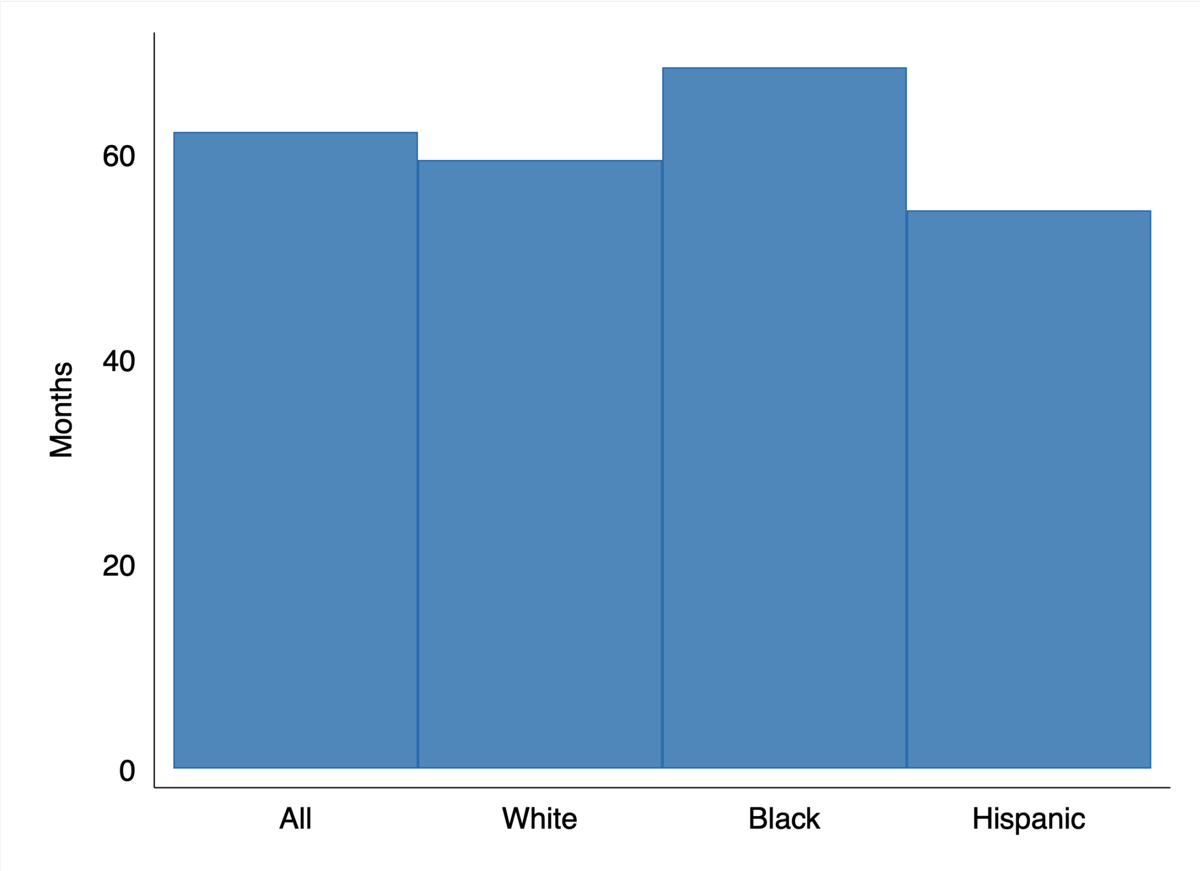
Table 3: Main Results: Triple-Difference Estimates

	Full Sample				Consistent Sample			
	Black		Hispanic		Black		Hispanic	
	General	Event Study	General	Event Study	General	Event Study	General	Event Study
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
MML	1.557		-30.70**		15.38		-33.11**	
	(9.211)		(12.75)		(9.651)		(15.20)	
MML(-4)		-7.042*		0.695		-7.016		5.113*
		(3.672)		(3.769)		(4.204)		(2.352)
MML(-3)		-1.414		-0.326		-4.707**		-3.015
		(1.685)		(3.044)		(1.632)		(2.211)
MML(-2)		-1.899		14.27***		-7.327**		18.70***
		(2.303)		(4.697)		(2.912)		(5.065)
MML(-1)		0.899		10.46***		-1.088		12.90***
		(1.500)		(2.980)		(1.885)		(3.696)
MML(1)		0.913		-11.17		15.32		1.882
		(7.706)		(11.85)		(10.92)		(16.55)
MML(2)		-2.242		-24.33*		10.24		-22.42
		(7.878)		(12.18)		(10.76)		(16.88)
MML(3)		-5.706		-23.96*		3.667		-27.31
		(9.263)		(14.14)		(10.96)		(17.35)
MML(4)		-6.591		-36.08***		2.588		-36.56*
		(11.65)		(12.22)		(14.97)		(18.48)
Mean Sentence	61.23	61.23	53.90	53.90	58.87	58.87	53.28	53.28
R-squared	0.487	0.487	0.204	0.204	0.319	0.319	0.216	0.217
N	2204154	2204154	1322084	1322084	1072415	1072415	655580	655580

Notes: Column (1)-(4) are the full sample estimates while Columns (5)-(8) represents estimates from the sample restricted to the thirteen states that consistently reported data, as identified by Neal and Rick (2016): California, Colorado, Georgia, Illinois, Michigan, North Dakota, New Jersey, New York, Ohio, South Carolina, Utah, Washington, and Wisconsin. The odd columns report 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 individual was admitted to prison, interacted with an indicator for whether the individual is Black or Hispanic, as indicted by the column headers. The even columns present the corresponding event-study estimates, with the number in brackets the MML variable indication years prior or post mandatory minimum reforms. 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.
* $p < .10$, ** $p < .05$, *** $p < .01$
Data source: NCRP 1997-2016.

A Appendix

Figure A1: Average Sentence Length by Race and Ethnicity



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

Table A1: State Criminal-Law Changes

State	Date (1)	Type of Drug Crimes (2)	Consistent (3)	Repeal (4)
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	Yes	
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				
Nevada				
New Hampshire				
New Jersey			Yes	
New Mexico				
New York	1/1/2004	All	Yes	
North Carolina				
North Dakota			Yes	
Ohio	9/30/2011	All	Yes	
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			Yes	
Washington			Yes	
West Virginia				
Wisconsin			Yes	
Wyoming				

Notes: Column (1) reports the exact implementation date for states that modified or repealed mandatory minimum sentencing laws (MMLs). We were unable to find the day and month of MML laws for Indiana, Michigan, New York, and Pennsylvania, and thus we assume they were implemented on January 1. Column (2) lists the crimes for which MMLs were modified or lifted. Column (3) lists all states that consistently report data to NCRP (see Section 4 for more detail). Column (4) indicates whether the MMLs were fully repealed.
Data sources: Sentencing Project, "The State of Sentencing: Developments in Policy and Practice," `https://www.sentencingproject.org/issues/sentencing-policy/`, various years; Subramanian and Delaney (2013); Austin (2010); `https://famm.org/`; and authors' own research on state statutes and legislative histories.

Table A2: Summary Statistics: Consistent Sample

	All		White		Black		Hispanic	
	Mean	S.D.	Mean	S.D.	Mean	S.D.	Mean	S.D.
Sentence length (in days)	58.807	80.254	56.679	77.358	63.703	84.108	55.768	88.377
Black	0.487	0.500	0.000	0.000	1.000	0.000	0.030	0.170
White	0.317	0.465	1.000	0.000	0.000	0.000	0.183	0.387
Missing/other race	0.195	0.396	0.000	0.000	0.000	0.000	0.787	0.409
Female	0.119	0.324	0.175	0.380	0.094	0.292	0.086	0.281
Male	0.881	0.324	0.825	0.380	0.906	0.292	0.914	0.281
Hispanic	0.213	0.409	0.123	0.328	0.013	0.113	1.000	0.000
Not Hispanic	0.669	0.471	0.752	0.432	0.845	0.362	0.000	0.000
Missing ethnicity	0.119	0.323	0.126	0.332	0.142	0.349	0.000	0.000
Less than HS Degree	0.238	0.426	0.187	0.390	0.313	0.464	0.184	0.387
HS Degree	0.165	0.371	0.165	0.372	0.199	0.400	0.096	0.294
Some College	0.026	0.159	0.032	0.175	0.030	0.170	0.007	0.084
College Degree	0.004	0.061	0.005	0.073	0.004	0.059	0.001	0.037
Missing education	0.565	0.496	0.604	0.489	0.452	0.498	0.710	0.454
Age at prison admission	34.782	9.663	35.684	9.334	34.435	9.996	34.006	9.282
Prior Felony Incarceration	0.231	0.422	0.210	0.407	0.281	0.449	0.127	0.333
New court commitment	0.523	0.499	0.543	0.498	0.515	0.500	0.535	0.499
Parole revocation	0.266	0.442	0.217	0.412	0.312	0.463	0.244	0.430
Probation revocation	0.028	0.166	0.041	0.198	0.029	0.169	0.007	0.082
N	1,392,894		442,046		678,924		296,075	

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. We report percent of the data with missing or other race, ethnicity, and educational attainment. The sample is restricted to the thirteen states that consistently reported data, as identified by Neal and Rick (2016): California, Colorado, Georgia, Illinois, Michigan, North Dakota, New Jersey, New York, Ohio, South Carolina, Utah, Washington, and Wisconsin. Data are from the National Corrections Reporting Program (1997-2016).