Mass Incarceration and Stopped Convergence in Black-White Educational Attainment

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Abstract

The Black-White gap in educational attainment in the U.S. narrowed between the 1960s and the mid-1980s. Nevertheless, it began to widen since the late 1980s, with Black men experiencing a greater decline than Black women in their educational attainment. This paper studies how childhood exposure to mass incarceration affects high school completion for young Black adults. To establish causality, I construct an instrumental variable for the incarceration rate, which exploits sentencing harshness across states and over years. I find that a 1 percentage point increase in the Black male incarceration rate that a young Black man was exposed to between 13 and 18 years decreases his likelihood of completing high school by 2.3 percentage points. Moreover, the negative effect is mainly driven by exposure to higher incarceration rates at the extensive margin (i.e., higher risks of incarceration conditional on arrest), rather than at the intensive margin (i.e., increased length of time served).

JEL Codes: I21, J13, K14.

Keywords: Mass incarceration, criminal justice, sentencing, education, high school completion, Black-White education gap.

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1 Introduction

In the United States, the educational achievement of Blacks has lagged behind that of Whites: Black students have lower scores and Black adults have less completed schooling than their White counterparts. Between the 1960s and the mid-1980s, the Black-White gap in educational attainment continued to narrow. Nevertheless, the convergence stopped in the late 1980s and the gap began to widen again, with Black men experiencing a greater decline than Black women in their educational attainment (Evans et al., 2016; Neal, 2006). When this convergence halted, the United States has also experienced a dramatic growth in incarceration, which has affected Black men disproportionately: 13.4% of Black men born in 1974 are expected to go to prison during their lifetime, and the number increases to 29.4% for those born in 1991 (Bonczar et al., 2003).

A substantial body of literature investigates how the educational gap is driven by children's unequal access to educational resources (e.g., school quality) and parental characteristics (e.g., parental education, income, and incarceration). Nevertheless, the role of the criminal system is not considered. This paper aims to fill these gaps and studies how childhood exposure to mass incarceration affects Black children's future educational attainment.

Mass incarceration could be a plausible explanation, since rising incarceration rates have coincided with the stopped convergence in Black-White educational attainment and have affected Black men disproportionately. In addition, multiple mechanisms could explain a negative effect of incarceration on children's educational attainment. First, mass incarceration could affect children through family or neighborhood circumstances.[1] Children with a parent in prison may face credit constraints or family instability, which could adversely affect their outcomes. For Black children growing up in disadvantaged neighborhoods that were disproportionately affected by mass incarceration, even if their parents were present, such a negative childhood "shock" may hamper their human capital accumulation (Chetty et al., 2020; Bezin et al., forthcoming).[2] Second, higher risks of incarceration due to harsher sentencing policies could lower the expected return to education, and thus decrease investments in human capital (Becker, 1964). Specifically, for a juvenile with

[1]The literature has shown the importance of parenting for children's future outcomes (Heckman, 2007, 2013; Almond et al., 2018). Moreover, Chetty and Hendren (2018a,b) show that the neighborhoods where children grow up affect their educational attainment.

[2]Bezin et al. (forthcoming) use a two-period overlapping generations model to illustrate how the absence of fathers makes children more vulnerable to crime influence of the neighborhood. Chetty et al. (2020) empirically show that the share of single-mother households can predict intergenerational mobility for Blacks.

a positive probability of committing a crime in his lifetime, a higher risk of incarceration could decrease his expected return to education by reducing the probability of employment:[3] A person cannot be employed while serving time in prison, and even after release, former prisoners encounter obstacles to reentering the workforce.[4] Lastly, harsher sentencing policies that lead to the growth of adult incarceration could be accompanied with harsher school discipline policies, which could also lower high school completion (Wald and Losen, 2003; Aizer and Doyle Jr, 2015).

However, assessing the causal effects of the aggregate incarceration rate that individuals were exposed to in childhood on their educational attainment is difficult, because of omitted variables or measurement error. To establish causality, I construct an instrumental variable (IV) for the incarceration rate. In the spirit of simulated and shift-share IVs, the IV parameterizes plausibly exogenous changes in sentencing policies across states and over years.[5] It exploits variation in sentencing harshness at both the extensive margin (i.e., whether to incarcerate an arrestee) and the intensive margin (i.e., how long to imprison an inmate) within each crime category. Precisely, the IV predicts the incarceration rate of Black men sentenced from a metropolitan area using (leave-one-out) estimates of state-level sentencing outcomes within each crime category, based on a simulation procedure of the prison population. Intuitively, the method compares two similar juveniles with some positive probability of being arrested for a similar offense (e.g., theft), but one juvenile is in a lenient state and the other is in a harsh state—while the lenient state tends to punish the offense with a fine, the harsh state tends to punish the offense with imprisonment. The method estimates how different risks of incarceration (attributed to the harshness of their state-level sentencing policies) faced by these juveniles affect their educational outcomes. With this method, I address the issue that different incarceration prospects could be driven by unobservable factors—e.g., economic returns to crime or discrimination—which may affect educational outcomes independently.

To implement the strategy, I use offender-level administrative data on prisoners entering and leaving prisons between 1983 and 2009 from the National Corrections Reporting Program (NCRP) and arrest data from the Uniform Crime Reporting Program (UCR). The data allow me to estimate

[3] Having contact with the criminal justice system is not a low-chance event since 1 out of 3 Black men are likely to spend some time in prison in their lifetime (Bonczar et al., 2003). Nevertheless, for those who will not commit a crime, higher incarceration rates could also increase their expected return to education by lowering the labor supply.

[4] Agan and Starr (2018) find that having a criminal conviction decreases the likelihood of being called back for a (fictitious) job applicant, and whether the person has a high school diploma does not affect the callback rate.

[5] Simulated IVs are commonly used in public economics. The method can be viewed as extracting the exogenous component of policy variation.

the variables used to construct the IV: (i) the probability of incarceration conditional on arrest and (ii) average length of time served in prison within each crime category by gender, race, year, and metropolitan statistical area (MSA) of sentencing. I also estimate the incarceration rate by gender and race at the MSA level. To investigate the impact of childhood exposure to incarceration on the educational attainment of young Black adults, I match each young adult in the 5% U.S. census or the American Community Survey to the Black male incarceration rate in the MSA where the individuals lives for the years when he/she was under age 18.

I find that childhood exposure to mass incarceration negatively affects young Black adults' high school completion, in particular for Black men. For instance, a 1 percentage point (pp) increase in the Black male incarceration rate that a young Black man (aged from 19–23) was exposed to between 13 and 18 years decreases his likelihood of having a high school diploma or GED by 2.3 pp.[6] The estimated effect is smaller and statistically insignificant for Black women. Moreover, the IV results are considerably larger in magnitude than the OLS results. This could result from omitted variables or measurement error. Another reason could be heterogeneous treatment effects. Specifically, incarceration could have a greater impact on schooling decisions for juveniles who are inclined to commit some minor crimes and face higher risks of incarceration due to harsher sentencing policies (compliers). In contrast, for juveniles who are inclined to commit more serious crimes and be locked up regardless of the harshness of sentencing policies (always-takers), or for those who are not inclined to commit crimes (never-takers), their schooling decisions are unlikely to be affected by changes in sentencing policies.[7]

In addition, I find that the negative effect of childhood exposure to mass incarceration on high school completion is mostly driven by higher risks of incarceration at the extensive margin—i.e., higher incarceration rates due to harsher sentences that increase the likelihood of incarceration conditional on arrest, rather than sentences that increase sentencing lengths. The finding is reasonable, because people facing higher risks of incarceration at the *extensive* margin are more likely to commit minor crimes, and they may complete high school if they were not incarcerated (under less

[6]The effect is mainly driven by individuals at the lower tails of the education distribution. On the other hand, it is possible that individuals at the upper tails of the distribution may choose to complete more education—they may do so to signal that they are unlikely to have contact with the criminal justice system. I do not find such a positive effect.

[7]The compliers may complete high school if they are not incarcerated. However, a higher risk of incarceration could lower their expected return to education and incentivize them to drop out from high school. In contrast, the alway-takers are unlikely to complete high school and the never-takers are likely to complete high school regardless of the harshness of sentencing policies.

harsh sentencing policies). In contrast, people facing higher risks of incarceration at the *intensive* margin are more likely to commit more serious crimes, and they probably would not complete high school even if they could be released earlier (under less harsh sentencing policies).

Lastly, I explore the potential mechanisms. The negative effect of childhood exposure to mass incarceration on high school completion is likely to be driven by reduced expected returns to education, because the negative effect is mainly driven by harsher sentences at the extensive margin and mainly for men—women are unlikely to be affected through this channel since the female incarceration rate is fairly low. Worsened family or neighborhood circumstances could also play a role. Lastly, I cannot rule out the possibility that harsher sentencing policies for adults could be accompanied with harsher school discipline policies, which may directly affect high school completion. I provide evidence to show that this is unlikely the main pathway by analyzing individuals who migrated during childhood and by controlling for the juvenile incarceration rate.

This paper contributes to two strands of literature. First, while there is a rich body of literature on Black-White educational gaps, much less is known about the reversed convergence. Many studies focus on understanding the convergence between the 1960s and the 1980s; hypotheses to explain the convergence include higher parental education, lower levels of racial segregation, increased quality of schools, and improved infant health (Cook and Evans, 2000; Chay et al., 2009; Guryan, 2004). Less attention is paid to what stalled the progress (Murnane, 2013; Neal, 2006). A few studies shed light on the possible explanations, but they do not explain why Black men experienced greater decline than Black women in their educational attainment (Jacob, 2001; Dee and Jacob, 2006; Lutz, 2011).[8] The most closely related study is by Evans et al. (2016), who propose the introduction of crack cocaine markets as a key explanation for the stalled progress in Black high school completion. I contribute to the literature by proposing another explanation—mass incarceration of adults resulting from harsher sentencing policies.

More broadly, this paper contributes to the literature that examines the spillover effects of incarceration.[9] Many studies investigate the intergenerational effects of parental incarceration on

[8] Lutz (2011) shows that the termination of court-mandated desegregation plans in the early 1990s increased the dropout rate for Black students outside the South. Jacob (2001) and Dee and Jacob (2006) show that increased high school graduation requirements since the 1970s reduced high school completion rates, in particular for Black students.

[9] A large body of literature focuses on the direct effect of incarceration on crime rates (Levitt, 1996; Liedka et al., 2006; Owens, 2009; Johnson and Raphael, 2012; Buonanno and Raphael, 2013; Kuziemko, 2013). Criminology literature has studied the effects of incarceration on inmates' outcomes after release. Aizer and Doyle Jr (2015) provide a detailed review. More recent studies provide causal evidence by exploiting the random assignment of judges

children's outcomes (Johnson, 2009; Hjalmarsson and Lindquist, 2012; Bhuller et al., 2018a,b; Billings, 2018; Dobbie et al., 2018; Bald et al., 2019; Arteaga, 2020; Wildeman, 2020). Some studies examine the effect of male incarceration on female outcomes (Myers and Wilkins, 2000; Charles and Luoh, 2010; Mechoulan, 2011; Caucutt et al., 2018; O'Keefe, 2019; Liu, 2021). This paper focuses on how mass incarceration of adults could affect the educational attainment of the next generation who grew up in neighborhoods that were disproportionately affected by mass incarceration—the mechanism is not necessarily through the incarceration of one's own parents. This paper also contributes to the literature by building a novel IV that exploits state-level sentencing harshness to evaluate the causal effect of incarceration.[10]

The rest of the paper is organized as follows. Section 2 describes the data and measurement. Section 3 shows the empirical strategy. Section 4 presents and discusses the results. Section 5 concludes.

2 Data

2.1 Education Data

Educational outcomes are from the U.S. Census 5% samples for 1990 and 2000 and the American Community Survey (ACS) 2006–2010 from the Integrated Public Use Microdata Series (IPUMS) (Ruggles et al., 2017). I focus on U.S.-born, Black or White non-Hispanic respondents born between 1965 and 1990, who turned age 18 between 1983 and 2008. I restrict the cohort because the incarceration rate is not available prior to 1983. The data provide the educational attainment of each respondent, and I mainly focus on whether a respondent has a high school diploma or GED. The data do not distinguish between a high school diploma and a GED, which may cause an issue—since individuals can acquire a GED over time, older cohorts had longer to obtain a GED in a given survey year. To mitigate the issue, I restrict the sample to young adults aged from 19 to 23 in the main specification. The advantage of the data is that they provide various geographic information, such as the current MSA of residence, and MSA of residence 5 years ago (in the 5%

(Kling, 2006; Green and Winik, 2010; Di Tella and Schargrodsky, 2013; Aizer and Doyle Jr, 2015; Mueller-Smith, 2015; Bhuller et al., 2018b).

[10] I also employ the method to study the causal effect of Black male incarceration on Black women's marriage and labor market outcomes, and Black children's family structure (Liu, 2021).

Census) or 1 year ago (in the ACS). Information on migration is useful to distinguish the potential mechanisms, which is discussed in Section 4.4.

2.2 Descriptive Facts on High School Completion

Figure 1 replicates an analysis of Evans et al. (2016) by showing the high school completion rates for cohorts who turned age 18 between 1965 and 2005, using the the ACS data.[11] Panels A and B present race-specific high school completion rates for men and women, respectively. Panel C presents White-Black differences in high school completion rates by gender. Completion rates for both Black men and women increased dramatically before the late 1980s, by around 10 pp from 1965–1986. In contrast, completion rates for Whites did not change significantly. Thus, the Black-White educational convergence before the late 1980s is mainly driven by the higher educational attainment of Blacks. However, in the mid- to late-1980s, the convergence stopped and the gap started to increase. This is again mainly driven by the performance of Blacks. Moreover, Black men experienced a greater decline than Black women between 1986 and 2005, with a decline of 10 pp for Black men and 4 pp for Black women.

2.3 Crime and Prisoner Data

I use data on crimes and prisoners from two sources. First, the Uniform Crime Reporting Program (UCR) collects data on crimes and arrests through reporting by law enforcement agencies. I use the yearly summary data on the number of arrests by state, year, offense, and gender (or race).

Second, I use data on prisoners from the National Corrections Reporting Program (NCRP), which has collected offender-level administrative data annually since 1983. It consists of data for the universe of prisoners who were admitted to or released from prison from 1983–2009, and data on stocks of prisoners in custody at year end from 2004–2009. The NCRP provides demographic information, including date of birth, gender, race, and educational attainment. The data also provide sentencing information, including date of admission to prison, date of release from prison (for released prisoners only), conviction offense, length of the longest sentence, state of custody, and

[11] Figure 1 is comparable to Figure 1 in Evans et al. (2016). The estimated Black-White convergence before the late 1980s is slightly greater than what they find. This is likely because I include more recent data and therefore each cohort had more time to acquire a GED.

county where sentence was imposed.

2.4 Measurement of the Incarceration Rate

Public-use data on prisoners usually provide prisoner counts at the state level. However, there is substantial variation in incarceration rates across areas within states.[12] This section summarizes the approach from my previous paper that estimates the prison population and the incarceration rate at the level of MSA where offenders were sentenced (Liu, 2021).[13]

The NCRP does not provide information on stocks of prisoners in custody prior to 2004. Thus, I use the perpetual inventory method to estimate the year-end prison population by year, MSA of sentencing, race, and gender between 1983 and 2009. First, I use the NCRP data on stocks of prisoners in custody between 2004 and 2009 to estimate the year-end prison population of Black men by year and MSA.[14] Second, I use the NCRP data on admissions and releases between 1983 and 2009 to estimate yearly changes in prison population—namely the number of admissions minus the number of releases within each year and MSA.[15] Last, I back out the year-end prison population before 2004. Due to data limitations, I can only estimate the MSA-level prison population for 20 states that provide data on stocks of prisoners, but the overall pattern of Black male incarceration for the 20 states is similar to that for all states (Figure A1).[16] Appendix A1 presents evidence on the reliability of the estimated MSA-level prison population—e.g., I aggregate the MSA-level prison population to the state level, and show that the resulting state-level prison population is comparable to the state-level counts by an alternative source.

The incarceration rate ($IncRate$) of Black men in year t and MSA m is

[12] For instance, in California, while on average 9.4% of Black men aged from 20–54 were imprisoned in 2000, the rate is lower than 2% in San Luis Obispo County and higher than 20% in Shasta County.

[13] Using the MSA of sentencing has several advantages. First, the location of sentencing is different from the location of custody. The location of custody depends on many factors, such as risk level and the capacity of institutions, so it can be far from the location where the crime was committed. Also, prisoners could be held in other states since states can lease their prison space. Importantly, by using the MSA of sentencing, transfers of prisoners between locations do not affect the estimation.

[14] I match the county where sentence was imposed (from the NCRP) to a MSA based on the 1999 delineation of the Office of Management and Budget.

[15] I assume that prisoners return to the MSA where they were sentenced after release. The assumption is reasonable because their social networks are likely to remain where they used to live.

[16] The 20 states include Arkansas, California, Colorado, Florida, Louisiana, Michigan, Minnesota, Missouri, New Jersey, New York, North Carolina, Oklahoma, Oregon, Pennsylvania, South Carolina, Tennessee, Texas, Virginia, Washington, and Wisconsin. Liu (2021) provides more details on the availability of the NCRP data across states, the data cleansing procedures for the estimation of the prison population, and the perpetual inventory method.

$$\text{Fraction of Black men in prison}_{mt} = \frac{\text{\# of Black men in prison in year } t \text{ sentenced from MSA } m}{\text{residential population of Black men aged from 20–54 in year } t \text{ MSA } m}. \quad (1)$$

The numerator is estimated as described above, and the denominator is estimated using U.S. Census Intercensal County Population Data. I restrict the age range because more than 90% of Black male inmates are between the ages of 20 and 54.[17]

3 Empirical Strategy

This section describes the identification strategy. First, I set up the baseline specification and discuss the identification issues. Second, I provide an overview of the IV and some intuition. Third, I show the formal construction of the IV. Lastly, I discuss the validity of the IV.

3.1 Setup

As is suggested in Figure 1, the exacerbation of racial disparities in educational attainment since the late 1980s is mainly driven by worsened educational outcomes of Blacks, especially among Black men. Therefore, I focus on the impact of mass incarceration on high school completion for Blacks. Specifically, I estimate the effect of the Black male incarceration rate that a Black young adult was exposed to in adolescence on whether he/she has a high school diploma or GED, using the following specification:

$$y_{icm} = \beta_0 + \beta_1 IncRate_{cm} + X_{icm}\delta + \mu_c + \gamma_m + \epsilon_{icm}, \quad (2)$$

where y_{icm} is an indicator that individual i from cohort c living in MSA m has a high school diploma or GED. $IncRate_{cm}$ is the Black male incarceration rate that individuals from cohort c living in MSA m faced in adolescence. μ_c includes cohort fixed effects and γ_m includes MSA fixed effects. X_{icm} includes age dummies of individual i in the survey year, because older cohorts had more time to acquire a GED in a specific survey year. ϵ_{icm} is an error term.

Assessing the causal impact of incarceration on high school completion is difficult. First, there

[17] The estimation is based on statistics from Prison and Jail Inmates at Midyear 2000–2009, which provide the number of inmates in prisons and jails by gender, race, and age group.

can be omitted variables. To illustrate the potential sources of omitted variables, consider the probability of *admission* to prison $\Pr(Inc)$, which can be written as

$$\Pr(Inc) = \Pr(Inc \mid Arrest) \cdot \Pr(Arrest \mid C) \cdot \Pr(C),$$

where $\Pr(Inc \mid Arrest)$ is the probability of prison admission conditional on arrest, $\Pr(Arrest \mid C)$ is the probability of arrest conditional on crime, and $\Pr(C)$ is the probability of committing a crime. $\Pr(Inc)$ contributes to the incarceration rate $IncRate$ at the extensive margin. At the intensive margin, $IncRate$ also depends on the average length of time that prisoners serve in prison conditional on receiving a sentence of incarceration. Omitted variables can be associated with criminal or police behaviors, which likely affect $IncRate$ through $\Pr(C)$ and $\Pr(Arrest \mid C)$.[18] For instance, higher economic returns of crime could lead to higher incarceration rates by increasing $\Pr(C)$. In the meantime, higher economic returns to crime could also lower human capital investment by increasing the opportunity cost of education.[19] For another instance, higher levels of discrimination could increase Black incarceration rates by increasing $\Pr(Arrest \mid C)$ due to potential racial bias of police (Hoekstra and Sloan, 2022); labor market discrimination could decrease the educational attainment of Blacks by lowering their expected returns to education.

Moreover, measurement error could lead to attenuation bias. In particular, the incarceration rate that an individual perceives could differ from the incarceration rate that I measure. For instance, a person could be disproportionately influenced by the incarceration rate in his neighborhood or the incarceration rate of his economic background, but I do not observe this incarceration rate and I can only observe a rough proxy for it.

3.2 Identification: Sentencing Reforms

To establish causality, I exploit plausibly exogenous changes in sentencing policies across states and over years. The surge in incarceration rates since the mid-1970s was associated with notable changes in sentencing regimes—many of them increased the tendency to incarcerate or the length

[18]In contrast, the sentencing outcomes, i.e., $\Pr(Inc \mid Arrest)$ and average length of time served in prison, are likely to be affected by the punitiveness of sentencing policies. More details are discussed in Section 3.2 and Appendix A2.

[19]Higher economic returns to certain types of crime could also increase human capital investment since certain White collar crimes may require higher levels of education.

of time served. Appendix A2 provides a brief background on the sentencing reforms.[20]

However, it is difficult to identify the effect of specific sentencing policy changes on the incarceration rate using a difference-in-differences framework because of the complexity of the reforms. Some studies use panel regressions to estimate the effect of a particular sentencing reform on the prison population, but the results are mixed (Nicholson-Crotty, 2004; Stemen et al., 2006; Zhang et al., 2009; Stemen and Rengifo, 2011). This is likely because different states have different requirements for similar laws—e.g., California's and Pennsylvania's three-strikes laws are essentially different laws that share the same name.[21] Moreover, many policy changes were implemented simultaneously. The variation in and complexity of these policies provide supporting evidence of the exogeneity of the policy changes, but in the meantime make it difficult to identify their effects on the incarceration rate.

To tackle the difficulty of exploiting the complex sentencing reforms, I construct an IV that predicts the incarceration rate using (leave-one-out) estimates of state-level sentencing outcomes within each crime category, based on a simulation procedure of the prison population. Raphael and Stoll (2013) and Neal and Rick (2016) present simulation models that aim to match the simulated prison population to the actual prison population. I construct the IV by extracting the exogenous components of the models that characterize the punitiveness of sentencing outcomes: (i) the probability of incarceration conditional on arrest and (ii) the average length of time served for each type of offense. The leave-one-out estimates of sentencing outcomes allow me to eliminate the effect of potentially endogenous local factors—e.g., the sentencing harshness of local judges or the severity of crimes, and capture only the effect of *state-level* sentencing policies. The key identifying assumption is that the sentencing outcomes used to construct the IV are orthogonal to factors other than childhood exposure to incarceration that influence individuals' educational outcomes. Section 3.4 discusses the validity of the IV.

[20] Appendix A2 presents several major sentencing reforms since the mid-1970s. However, I cannot identify all sentencing policy changes, such as states' amendments to their sentencing guidelines. Overall, I show that there is considerable variation across states in terms of the type of policies implemented, the timing of policy changes, and the requirements (Table A1).

[21] Three-strikes laws impose more severe mandatory sentences for repeat offenders, but states vary in terms of the number and type of convictions to trigger the laws and the sentences imposed under them. For instance, in California, a "second striker" (i.e., someone with a prior violent offense convicted of a second felony) receives a sentence equal to twice the sentence and a "third striker" receives an indeterminate sentence of 25 years to life. Pennsylvania's three-strikes law is triggered only when an offender of two prior felonies is convicted of one of eight specified offenses, and the judge has the discretion to increase the sentence by up to 25 years (Raphael and Stoll, 2013).

The IV resembles the classical simulated IVs commonly used in public economics (e.g., Currie and Gruber, 1996; Gruber and Saez, 2002).[22] The advantage of the IV is that it provides a continuous measure of sentencing harshness, and thus increases the power of the first stage.

3.3 Construction of the IV

This section describes a simple simulation procedure of the prison population and the construction of the IV based on the simulation procedure.

Simulation Procedure Let I_{mt} be the year-end prison population sentenced from MSA m of year t and I_{mt}^c be that of crime c ($c = 1, ..., N$). Let A_{mt}^c be the number of newly admitted prisoners and C_{mt}^c be the population of criminals. Let α_{mt}^c be the probability of arrest conditional on committing crime c, γ_{mt}^c be the probability of prison admission conditional on arrest, and $\bar{S}_{mt}^c$ be the average number of years served in prison for crime c.

For simplicity, assume that the prison population is zero in year $t = 0$, so that there are not prisoners released from $t = 0$. Then the prison population sentenced from MSA m at year end 1 is equal to the number of newly admitted prisoners during the year:

$$I_{m1} = \sum_{c=1}^{N} I_{m1}^c = \sum_{c=1}^{N} A_{m1}^c = \sum_{c=1}^{N} C_{m1}^c \alpha_{m1}^c \gamma_{m1}^c.$$

The prison population sentenced from MSA m at year end 2 is

$$\begin{aligned} I_{m2} &= \sum_{c=1}^{N} A_{m2}^c + \sum_{c=1}^{N} A_{m1}^c \mathbb{1}\{\bar{S}_{m1}^c > 1\} \\ &= \sum_{c=1}^{N} C_{m2}^c \alpha_{m2}^c \gamma_{m2}^c + \sum_{c=1}^{N} C_{m1}^c \alpha_{m1}^c \gamma_{m1}^c \mathbb{1}\{\bar{S}_{m1}^c > 1\}, \end{aligned}$$

where $\mathbb{1}\{\bar{S}_{m1}^c > 1\}$ is an indicator that the average time served for prisoners of crime c sentenced from MSA m in year 1 is greater than 1 year.[23] In other words, prisoners in custody at year end 2

[22] Currie and Gruber (1996) study the effect of expansions of medical eligibility on health outcomes. They develop a simulated IV that holds individual sources of variation in eligibility constant and only exploits differences in eligibility across states and over years. Gruber and Saez (2002) study the impact of changes in tax schedules on individual income. They develop simulated IVs—so-called behavior-constant tax rates—for the actual tax rates faced by each individual, and the simulated IVs serve as sufficient statistics for complex tax law changes.

[23] The correct formula should be $I_{m2} = \sum_{c=1}^{N} A_{m2}^c + \sum_{c=1}^{N} A_{m1}^c \Pr\{S_{m1}^c > 1\}$. I approximate $\Pr\{S_{m1}^c > 1\}$ using $\mathbb{1}\{\bar{S}_{m1}^c > 1\}$ because of data restrictions—estimating the distribution of S_{m1}^c needs more accurate data than

consists of the newly admitted prisoners during year 2 and those from year 1 who are not released during year 2. In general, the prison population sentenced from MSA m at year end t is

$$\begin{aligned} I_{mt} &= \sum_{c=1}^{N} A_{mt}^{c} + \sum_{c=1}^{N} \sum_{j=1}^{t-1} A_{mj}^{c} \mathbb{1}\{\bar{S}_{mj}^{c} > t-j\} \\ &= \underbrace{\sum_{c=1}^{N} C_{mt}^{c} \alpha_{mt}^{c} \gamma_{mt}^{c}}_{\text{(i)}} + \underbrace{\sum_{c=1}^{N} \sum_{j=1}^{t-1} C_{mj}^{c} \alpha_{mj}^{c} \gamma_{mj}^{c} \mathbb{1}\{\bar{S}_{mj}^{c} > t-j\}}_{\text{(ii)}} . \end{aligned} \tag{3}$$

The equation reflects that the year-end prison population depends on (i) the number of newly admitted prisoners during the current year and (ii) the accumulation of past flows in and out of prison. The number of current admissions can be influenced by the prevalence of crime (C_{mt}^{c}), police effectiveness (α_{mt}^{c}), or the punitiveness of sentencing decisions to incarcerate (γ_{mt}^{c}). The past flows of prisoners further depend on the length of time served in prison ($\bar{S}_{mt}^{c}$).

IV Construction I construct the IV based on the simulation procedure described by Equation (3). Specifically, since C_{mt}^{c} and α_{mt}^{c} are likely influenced by endogenous criminal and police behaviors, I hold them constant to construct the IV: $C_{mt}^{c}\alpha_{mt}^{c} = C\alpha$. This ensures that variation in the IV only stems from the post-arrest sentencing outcomes.[24] Accordingly, the simulated prison population with constant arrests $C\alpha$ is

$$\sum_{c=1}^{N} C\alpha\gamma_{mt}^{c} + \sum_{c=1}^{N} \sum_{j=1}^{t-1} C\alpha\gamma_{mj}^{c} \mathbb{1}\{\bar{S}_{mj}^{c} > t-j\}.$$

Next, let $\bar{S}_{-mt}^{c}$ be the average number of years served for offenders of crime c sentenced in year t from the state that contains MSA m, leaving out MSA m itself. I substitute $\bar{S}_{-mt}^{c}$ for $\bar{S}_{mt}^{c}$ to construct the IV, because the leave-one-out estimates can exclude the impact of local endogenous factors—e.g., local criminal activity and sentencing harshness of local judges. Due to data limitations discussed below, the probability of incarceration conditional on arrest can be only estimated

estimating $\bar{S}_{m1}^{c}$. The approximation does not cause an issue since the purpose is to construct an IV, not accurately predicting the actual prison population.

[24]Harsher sentencing policies may have deterrence effects. At the extensive margin, it is possible that fewer people commit crimes due to deterrence. The IV does not characterize this variation, since it is hard to distinguish whether declining crime rates are caused by deterrence or other confounds. This may decrease the power of the IV, but is not a threat to identification. At the intensive margin, it is possible that people commit less serious crimes for a given type of offense because of deterrence. This is not likely to be a concern because if so, the first-stage estimate would be weak or go in the other direction.

at the state level, denoted by $\gamma^c_{s(m)t}$. Thus, the simulated prison population with constant arrests $C\alpha$ and state-level variation in sentencing harshness is

$$I^*_{mt} = \sum_{c=1}^{N} C\alpha\gamma^c_{s(m)t} + \sum_{c=1}^{N}\sum_{j=1}^{t-1} C\alpha\gamma^c_{s(m)j}\mathbb{1}\{\bar{S}^c_{-mj} > t-j\}. \tag{4}$$

The IV for the incarceration rate of MSA m in year t is

$$IV_{mt} = \frac{I^*_{mt}}{P_{mt}} = \frac{\sum_{c=1}^{N} C\alpha\gamma^c_{s(m)t} + \sum_{c=1}^{N}\sum_{j=1}^{t-1} C\alpha\gamma^c_{s(m)j}\mathbb{1}\{\bar{S}^c_{-mj} > t-j\}}{P_{mt}}. \tag{5}$$

where P_{mt} is the residential population of MSA m in year t.

Intuitively, the IV is a *behavior-constant* incarceration rate, which holds the number of arrests constant and only reflects variation in sentencing harshness. It is noteworthy that letting $C^c_{mt}\alpha^c_{mt} = C\alpha$ is a specific normalization to ensure that variation in the IV is not driven by endogenous pre-arrest behaviors. The way of normalization (or the *level* of the IV) is not important, because the purpose is to construct an IV, not accurately predicting the actual prison population.

IV Estimation I estimate $\gamma^c_{s(m)t}$, $\bar{S}^c_{-mt}$, and $C\alpha$ using the NCRP and UCR data on adult Black male offenders.[25] First, I estimate $\gamma^c_{s(m)t}$—the probability of incarceration conditional on arrest—using the number of Black adults admitted to prison (from the NCRP) divided by the number of Black adults arrested (from the UCR) for each category of offense, state, and year. The variable is estimated at the state level, because the county-level UCR data are heavily flawed (Maltz and Targonski, 2002; Kaplan, 2020).

Second, I estimate $\bar{S}^c_{-mt}$—the leave-one-out mean of the number of years served in prison—using admission and release data from the NCRP. Specifically, I match the two datasets by year of prison admission, MSA where the sentence was imposed, offense, race, and gender. For the matched offenders, $\bar{S}^c_{mt}$ is estimated with the year of release (from release data) subtracting the year of admission. For offenders found in admission data only, it is likely that they were still in prison by 2009, so I approximate $\bar{S}^c_{mt}$ using the average length of sentence.[26]

Last, I let $C\alpha = 1$. This is only a way of normalization, which does not affect the first-stage

[25]The denominator of the IV is the residential population of Black men aged from 20–54, estimated using U.S. Census Intercensal County Population Data.

[26]For offenders with life sentences or sentences longer than 30 years, I apply an upper bound of 30 years. Using higher upper bounds, such as 60 or 80 years, does not affect the results.

interpretation in standard deviation or the second-stage results.

3.4 IV Validity

The identifying assumption is that both components of the IV—tendency to incarcerate an arrestee and the average length of sentence served by offenders—are orthogonal to factors other than childhood exposure to mass incarceration that influence individuals' educational outcomes.

MSA- or state-level confounds The main threat to identification is that sentencing reforms may not be exogenous. There could be MSA- or state-level unobservables that affect both education and sentencing harshness. To mitigate the concern, I use leave-one-out means of state-level sentencing outcomes when possible to construct the IV, which could partially purge the effect of MSA-level confounds. Below, I provide additional robustness checks.

First, I show that the IV is not correlated with several potential confounding variables (Table 1). Feigenberg and Miller (2021) show that local racial heterogeneity affects local punishment severity because localities have discretion in how they enforce state-level sentencing laws. Column 1 shows that the IV is not statistically significantly correlated with the share of the Black population of the MSA. Another concern is that a state may implement harsher sentencing policies to fight against increasing crime rates of the state. Columns 2–5 show that the IV is not statistically significantly correlated with either the current crime rates or the growth rates of the lagged crime rates.[27] Column 6 shows that the IV is not correlated with the prevalence of crack cocaine.[28] Lastly, although the IV is constructed by holding the number of arrests constant, it can still be correlated with the arrest rates because sentencing policies may affect police arrest decisions. Columns 7–12 show that the IV is not statistically significantly correlated with either the current arrest rates or the growth rates of the lagged arrest rates.[29]

[27] I use state-level crime rates from the FBI UCR. The lagged crime growth rate is calculated as $(\text{crime rate}_{t-1} - \text{crime rate}_{t-2})/\text{crime rate}_{t-2}$. There is a negative correlation between the IV and the property crime rate, which could be a result of the deterrence effect of harsher sentencing policies. Also, there is a positive correlation between the IV and the growth of the lagged crime rates. However, all of the estimates are not statistically significant.

[28] The prevalence of crack cocaine is measured with the crack index proposed by Fryer Jr et al. (2013). I match their city-level index to the MSA level. However, the index is only available for 1985, 1989, 1993, 1997, and 2000.

[29] There is a negative correlation between the IV and the arrest rates: Police officers may be less likely to arrest if they expect harsher sentencing policies would cause prison overcrowding. However, the estimates are not statistically significant, suggesting that the effect of sentencing policies on police behaviors is not likely to be a major concern.

Second, I find no evidence of pre-treatment trends. Specifically, I control for the lagging and leading IVs in the first stage regression (Table 2 column 2). In the presence of pre-treatment trend, changes in the current incarceration rate should be able to predict future sentencing reforms. However, the F-test fails to reject the null that the leading coefficients are jointly equal to zero, indicating that pre-treatment trend is less likely to be a concern.

Section 4.6 presents more robustness checks. For instance, I add various control variables to mitigate several concerns—e.g., (i) increasing criminal justice expenditures may crowd out government investment in education; (ii) there could be contemporaneous economic shocks to MSAs that disproportionally affect the employment opportunities of less-educated Black workers; or (iii) changes in political views could be an omitted variable. I also show that it is not a concern that large MSAs may dominate their states' policymaking.

Criminal activity Another potential concern is that variation in the IV could be partially driven by changes in the severity of crimes, in addition to sentencing harshness.

First, I show that sentencing outcomes have become more punitive at both the extensive and the intensive margin, for almost all types of offenses. Figure A4 shows that more people per 1,000 arrests were sent to prison and they were likely to spend longer time in prison for those who were arrested in 2000, compared with those arrested in 1988.[30] The finding provides indirect support to the hypothesis that sentencing policies have become more punitive toward almost all types of offenses—it is unlikely that people nowadays tend to commit more serious crimes for all types of offenses compared with what they did in the past.

Second, I exploit the Anti-Drug Abuse Acts of 1986 and 1988, which mandated various minimum sentences for drug possession.[31] Figure A5 shows a sizable increase in the probability of incarceration conditional arrest for Black offenders of drug possession after the law changes.

Third, although I cannot conduct event-study analysis in this research design—because, similar to shift-share IVs, the IV averages various events (i.e., sentencing reforms)—I present evidence

[30]Figure A4 presents the number of persons *per 1,000 arrests* who served time in prison for those arrested in 1988 (in dotted blue lines) and in 2000 (in solid red lines) for each type of offense. x-axis represents years served in prison. Solid red lines are higher for all types of offenses, which implies people arrested in 2000 were more likely to enter prison and they also tend to spend longer time in prison than people arrested in 1988.

[31]The act became effective on October 27, 1986, which mandated a minimum sentence of 5 years without parole for possession of 5 grams of crack cocaine. The amended act became effective on November 18, 1988, which made crack cocaine the only drug with a mandatory minimum penalty for a first offense of possession.

that variation in the IV is likely driven by sentencing reforms. Specifically, I take two states as examples—Arkansas and Colorado—where sentencing policy changes were enacted in a relatively discrete way. Figure 2 shows how the simulated prison population I^*_{mt} (aggregated to the state level in SD) evolves over time for these two states.[32] The figure shows little evidence of pre-policy-change trends in I^*_{mt}. After the 1994 and 1995 reforms, I^*_{mt} increases dramatically. In particular, I^*_{mt} grows gradually instead of jumping immediately after the reforms. This is reasonable because, first, a new reform may only apply to a small group of offenders initially, who were convicted of specific offenses after a specific date. The prison population is built up gradually as more relevant offenders are sentenced under the new law. Second, it takes time for harsher sentences to be reflected in greater prison population. For instance, suppose a reform increases the average time served by some offenders from 3 to 5 years. Then it would take 3 years for the relevant offenders who were sentenced right after the reform to contribute to the higher prison population. Given these features, it is unlikely that other shocks, rather than the sentencing reforms, that affected I^*_{mt} in the way shown in Figure 2.[33]

4 Results

4.1 First-Stage Results

To consider the first-stage relationship, I estimate the following equation:

$$IncRate_{icm} = \alpha_0 + \alpha_1 IV_{cm} + X_{icm}\pi + \theta_c + \lambda_m + v_{icm},$$

where $Incar_{icm}$ is measured by the Black male incarceration rate that individual i of cohort c living in MSA m faced at the age of 18. IV_{cm} is the IV for the Black male incarceration rate of the corresponding year and MSA; it is normalized so that the mean is 0 and the SD is 1. Other variables are defined as in Equation (2).

[32] I aggregate the simulated prison population I^*_{mt} to the state level, and standardize the aggregate simulated prison population by state and year, so that the mean is 0 and the SD is 1.

[33] I do not find clear evidence for all of the states. However, this does not indicate that the exogeneity assumption of the IV fails. This is because, first, some reforms prior to the period of analysis may have a long-lasting effect, which could create a pattern of pre-treatment trends for the later reforms. Second, not all sentencing reforms are identified in the paper—e.g., states' amendments to their sentencing guidelines cannot be identified. However, such amendments to the sentencing guidelines can be reflected in the IV.

Table 2 presents the estimates of the first-stage equation.[34] The result in column 1 indicates that a 1 SD increase in the IV increases the incarceration rate of Black men by 0.9 pp. Column 2 further controls for the lagging and leading IVs and presents evidence that pre-treatment trend is less likely to be a concern.

4.2 Impact of Childhood Exposure to Incarceration on Education

I estimate the impact of the Black male incarceration rate (i.e., the fraction of Black men aged from 20–54 who are in prison) that young Black adults were exposed to in childhood on their high school completion, according to Equation (2):

$$y_{icm} = \beta_0 + \beta_1 IncRate_{cm} + X_{icm}\delta + \mu_c + \gamma_m + \epsilon_{icm}.$$

I focus on the impact of Black male incarceration because the female incarceration rate is fairly low—on average, 0.67% of Black women aged from 20–54 were in prison between 1983 and 2009, compared with 7.5% for Black men.

Figure 3 presents the IV estimates of β_1 with $IncRate_{cm}$ measured by the Black male incarceration rate that an individual faced at age t, shown on the x-axis, with 95% confidence intervals. I only present the estimates for $13 \leq t \leq 18$, because the first stage is less powerful and the estimates are mostly small and statistically insignificant for $t < 13$. First, I present the estimates for all young Black adults aged from 19–23. A potential concern of the specification is that individuals in the sample may reside in a different MSA in their childhood, since I only observe the current MSA of residence. Thus, I also present the estimates, restricting the sample to those who resided in the same MSA 5 years ago (in the 5% Census) or 1 year ago (in the ACS).

The IV estimates of β_1 for Black men in both samples are statistically significant (except for $t = 15$). The estimates are slightly larger in magnitude in the restricted sample. Interestingly, the estimated effect is greater when $IncRate_{cm}$ is measured at a younger age, although the differences are statistically insignificant. In contrast, the estimates for Black women are much smaller in magnitude and mostly statistically insignificant—the estimates are significant only for $t = 17$ or 18 in the restricted sample. Overall, the results suggest greater and more robust estimated effect of

[34] Figure A6 presents a bin scattered plot of the first-stage relationship.

childhood exposure to incarceration on educational attainment for Black men than Black women.

Table 3 present both the OLS and IV estimates of β_1. The sample comprises U.S.-born, Black non-Hispanic respondents aged from 19–23 from cohorts that turned age 18 between 1983 and 2008. I restrict the sample to young adults because individuals can acquire a GED over time. In particular, people can acquire a GED while serving time in prison. This may dilute the negative effect on education, in particular for Black men. For robustness, in columns 3 and 6, I remove the age restriction.

In Panel A, I consider the effect of the Black male incarceration rate that a Black adult faced at age 18. The OLS estimates for both men and women are small and statistically insignificant (columns 1 and 4). The IV estimate in column 2 suggests that a 1 pp increase in the Black male incarceration rate that a young Black man was exposed to at age 18 decreases the probability of having a high school diploma or GED by 1.7 pp in the early adulthood. The IV estimate for all Black male adults is smaller but statistically significant (column 3). In contrast, the IV estimates for Black women are smaller in magnitude and statistically insignificant (columns 5–6).

In Panel B of Table 3, I consider the effect of the average Black male incarceration rate that a Black adult faced between the age of 13 and 18. Interestingly, I find positive and statistically significant OLS estimates for both Black men and women, although the magnitudes are small. The IV estimates are still negative and become greater in magnitude compared with Panel A for Black men; the IV estimates are much smaller and statistically insignificant for Black women.

Local Average Treatment Effect Table 3 shows that the IV estimates have the opposite sign in some specifications compared with the OLS estimates, and IV estimates are much greater in magnitude and more statistically significant. The differences between the IV and OLS estimates could be owing to omitted variables or measurement error, discussed in Section 3.1. Another reason could be that the IV identifies the local average treatment effect. First, there could be a positive correlation between the incarceration rate that an individual faced in childhood and his educational attainment—Arteaga (2020) shows a positive effect of parental incarceration on children's educational outcomes in Colombia.[35] However, the IV identifies the local average treatment effect of compliers—compliers are MSAs that experienced higher incarceration rates due to the imple-

[35] Arteaga (2020) shows that the positive effect of parental incarceration is mainly driven by parents who commit violent offenses in Colombia.

mentation of harsher sentencing policies. Incarcerated men in such MSAs tend to commit less serious crimes, and therefore, their incarceration could impose a negative effect on the educational attainment of the next generation.[36]

Changes in expected returns to education can be another pathway underlying the sizable IV estimates. In the complier subpopulation, juveniles may expect a higher risk of incarceration during their lifetime due to harsher sentences. Thus, it is reasonable for them to reduce the investment in schooling in the face of harsher sentences, which could be seen as a negative shock to their expected returns to education. In contrast, in the always-taker subpopulation, juveniles would probably expect themselves to be incarcerated in the future and would probably not complete high school regardless of the harshness of sentencing policies. In the never-taker subpopulation, juveniles would not expect themselves to commit crimes, and therefore would not expect to be incarcerated no matter how harsh sentencing policies are. Their educational decisions are less likely to be affected by the incarceration rate. For the never-takers, their expected returns to education could even be positively affected, expecting a higher wage driven by lower labor supply.

Table A3 in the Appendix presents evidence on the effect of childhood exposure to mass incarceration on the education distribution. The results in Panel A suggest that the negative effect on high school completion is mainly driven by individuals at the lower tails of the distribution. Nevertheless, individuals on the upper tails of the distribution are less likely to have at least 1 year of college education. The results suggest that it is unlikely that in the face of higher incarceration rates during childhood, some individuals may choose to receive more education, for instance to signal that they would not have contact with the criminal justice system.

4.3 Impact of Incarceration at Different Margins

In this section, I distinguish different effects of incarceration at different margins. Specifically, the growth in incarceration rates could be driven by greater tendencies to incarcerate arrestees (extensive margin) or increasing length of time served (intensive margin). To investigate heterogeneity in the effects of incarceration at different margins, I construct two IVs, denoted by IV^{ex} and IV^{in}.

[36] Table A2 in the Appendix presents the average characteristics of Black offenders in the complier MSAs and all Black offenders. The results suggest that the complier Black offenders are less likely to have committed violent or property offenses, and more likely to have committed drug offenses or offenses against public order.

IV^{ex} exploits variation in the tendency to incarcerate, $\gamma^{c}_{s(m)t}$, holding the average time served at the 1983 level, $\bar{S}^{c}_{-m0}$; IV^{in} exploits variation in the average time served, $\bar{S}^{c}_{-mt}$, holding the tendency to incarcerate at the 1983 level, $\gamma^{c}_{s(m)0}$.

Columns 1 and 3 of Table 4 report the IV estimates with IV^{ex}; columns 2 and 4 report the IV estimates with IV^{in}. The results suggest that the negative effect of childhood exposure to incarceration on high school completion is mainly driven by higher Black male incarceration rates at the extensive margin (columns 1 and 3), not at the intensive margin (columns 2 and 4).[37]

It is not surprising that harsher sentencing policies at the extensive margin play a more important role than those at the intensive margin. The reasons are twofold. First, children whose fathers were incarcerated at the extensive margin are more likely to be negatively affected, because these fathers were more likely to have committed less serious crimes. These children's educational attainment could be negatively affected because of lower household income or father absence. Parental incarceration could be a potential mechanism since I find a negative effect for both young Black men and women.

Second, the greater estimated effect of exposure to incarceration at the extensive margin could be because a higher risk of incarceration at the extensive margin could lower the expected return to education more than that at the intensive margin. Specifically, a short contact with the criminal justice system can foster stigmatization and make potential employers reluctant to hire people with a criminal record, which could lower the expected return to education. In contrast, the length of time that a person is imprisoned may have less impact on potential employers' decisions.

4.4 The School-to-Prison Pipeline

A challenge of the study is to distinguish between the effect of mass incarceration for adults and the school-to-prison pipeline (SPP). The phenomenon of SPP is that students from racial minorities are disproportionately affected by increasingly harsh school discipline policies and police in schools, which push them out of the classroom and into prison. On the one hand, it is possible that harsher sentencing policies reinforce the implementation of tough school policies, which could

[37] Table A4 shows estimates of the reduced-form regressions. The results suggest that harsher sentencing policies that an individual was exposed to in adolescence have a negative effect on his or her high school completion. Consistent with the results in Table 4, the negative effect is mainly driven by harsher sentencing policies at the extensive margin.

drive the divergence in the Black-White educational attainment. On the other hand, it could be mass incarceration of adults, not school expulsion, that leads to less high school completion due to worsened family circumstances or their rational responses to lower economic returns to education.

Previous studies have examined the effect of school discipline or school police on students' criminal outcomes (e.g., arrests or incarceration) and educational outcomes (Kinsler, 2013; Owens, 2017; Bacher-Hicks et al., 2019; Sorensen et al., 2019; Weisburst, 2019). However, the literature mainly exploits natural experiments or uses data in a specific state or school districts, and there is not a measure of the harshness of school discipline policies across all states. As a result, I cannot control for the harshness of discipline policies that individuals faced in adolescence. Although I cannot rule out a potential effect of SPP, I provide evidence that the negative effect of childhood exposure to mass incarceration is unlikely to be entirely driven by an effect of SPP.

First, I focus on the sample of individuals whose MSA of residence 1 or 5 years ago is different from the current MSA of residence. Then I evaluate the impact of the Black male incarceration rate that an individual faced *before* migration when he was between 13 and 18 years.[38] This is because the incarceration rate or the sentencing harshness that an individual faced before migration should be uncorrelated with the school discipline policies that he faced right before completing or dropping out from high school. The results are presented in Table 5. The results suggest that a 1 pp increase in the Black male incarceration rate that a young Black man faced when he was at age 13 (before he migrated to his current MSA of residence) decreases the likelihood of completing high school by 3.48 pp in early adulthood (column 1).[39] Again, the estimated effect is much smaller and statistically insignificant for young Black women. Interestingly, the magnitude of the estimate (when the incarceration rate is measured as faced at age 13) based on the population who migrated in the past is almost identical compared to the estimate based on the population who did not migrate in the past (i.e., the estimate in Figure 3 based on second sample, x-axis $t = 13$). These comparable results further suggest that it is unlikely a concern that dividing the sample based on individuals' migration status would lead to selection.

[38]For instance, consider a respondent of age 21 in the 5% Census for 2000. His MSA of residence 5 years ago was X and his current MSA of residence is Y. I use either the incarceration rate of MSA X in 1992 (when the respondent was age 13) or the average incarceration rate between 1992 and 1995 in MSA X (when the respondent was between the age of 13 and 16). He was likely to have migrated to MSA Y after age 16.

[39]The estimated effect is greater than that in Table 3. This is unsurprising since Figure 3 suggests that the incarceration rate that an individual was exposed to at a younger age has a greater effect on his educational attainment.

Second, I directly control for the Black juvenile incarceration rate that an individual faced at age 18, as a rough approximate of the effect of SPP. I discuss this robustness check in Appendix A3 given that the regression could be problematic—the juvenile incarceration rate can be endogenous. The results in Table A5 suggest that the estimates remain exactly the same after controlling for the Black juvenile incarceration rate. Despite the potential endogeneity issue, the robust finding provides some evidence that SPP is not likely to be a major concern.

4.5 Discussion of the Mechanisms

Multiple mechanisms could explain the negative effect of childhood exposure to mass incarceration on high school completion in early adulthood. First, the incarceration of a parent can be a negative childhood "shock," which may lead to financial hardship or family instability. Even if one's parents were present, growing up in a disadvantaged neighborhood that was disproportionately affected by mass incarceration could have negative effects on children's wellbeing. Second, exposure to higher incarceration rates in adolescence could lower young men's expected returns to education, and therefore reduce their investment in education. Lastly, it is possible that harsher sentencing policies reinforce the implementation of tough school policies, which could directly affect students' educational attainment.

The results suggest that family or neighborhood circumstances are likely to be an underlying mechanism, because I find a negative impact of childhood exposure to mass incarceration on high school completion for both young Black men and young Black women—women are unlikely to be affected through the channel of reduced expected returns to education or SPP. Nevertheless, the estimates for women are mostly small in magnitude and statistically insignificant, indicating that family or neighborhood circumstances may not be the main driving force.[40]

Lower expected returns to education caused by higher chances of incarceration are likely to play an important role in lowering the educational attainment for Black men. The reasons are twofold. First, I find that the effect of childhood exposure to mass incarceration is much greater for young Black men, and the results for men are more robust to different specifications. Second, I find that the effect is mainly driven by higher risks of incarceration at the extensive margin. A

[40] It is possible that neighborhood circumstances could have a greater effect for boys than for girls. In this sense, family or neighborhood circumstances could still be an underlying channel.

short contact with the criminal justice system can make potential employers reluctant to hire the person, while the length of time that a person was imprisoned may have less impact on potential employers' decisions. Therefore, it is reasonable that higher risks of incarceration at the extensive margin would reduce the expected return to education more than that at the intensive margin.

Lastly, I cannot rule out the possibility that the SPP can be a potential mechanism, but the different effects of incarceration at different margins (discussed above) suggest that the SPP may not be the main explanation. In addition, I focus on individuals who migrated 1 or 5 years ago and examine the effect of the incarceration rate that an individual faced before migration—the sentencing harshness faced before migration is unlikely to be correlated with the school discipline policies faced right before completing or dropping out from high school. I find that this estimate is comparable to the estimate based on individuals who did not migrate, suggesting that school discipline may not be a driving force.

4.6 Robustness Checks

This section provides several robustness checks, which present evidence that the main findings are not likely to be driven by omitted variables.

First, I show that the main results are robust to including additional control variables (Table 6). It could be a concern that increasing criminal justice expenditures may crowd out public investments in education, which could have a long-run impact on children's educational attainment. To address the concern, I control for the log state government spending on elementary and secondary education per capita from the Historical Finances of State Governments Data.[41] Columns 2 and 5 of Table 6 suggest that adding this control variable does not affect the results significantly. Another concern is that there could be contemporaneous economic shocks to MSAs that disproportionally affect the employment opportunities of less-educated Black workers. To mitigate the concern, I construct a Bartik instrument, formed by interacting local industry shares and national industry em-

[41] For each individual, I control for the log spending for his state of residence in the year when he was at age 18. The results do not change if I use the average log spending during the years when an individual was in an elementary or secondary school.

ployment growth rates for Blacks without 4-year college education.[42] Moreover, since sentencing reforms can be influenced by political parties, I also control for the share of votes for Democrats, using data from the American National Election Studies (ANES).[43] Columns 3 and 6 of Table 6 presents the results controlling for state government spending on education, the Bartik instrument, share of votes for Democrats, and share of Blacks. The estimate for young Black men becomes even greater in magnitude after adding the control variables. The estimate for young Black women does not change much and remains statistically insignificant.

In the second robustness check, I address the concern that large MSAs may dominate their states' policymaking. If so, the leave-one-out approach cannot address the resulting endogeneity. As a robustness check, I calculate the average Herfindahl-Hirschman index (HHI) for each state from 1983–2008. The index measures the concentration of Black population within states: It is small if a state consists of many MSAs of relatively equal sizes of Black population and reaches the maximum of 10,000 if a single MSA contains all the Black population of a state.[44] I restrict the sample to states where the HHI is relatively small, so that the exclusion restriction is more likely to be satisfied.[45] Table 7 shows that the estimated effect does not change significantly when restricting the sample to states where Black population are less concentrated towards specific MSAs. Since the exclusion restriction is more likely to be satisfied in the restricted sample, the robust finding suggests that the baseline results are not likely to be driven by omitted variables.

[42]Specifically, the Bartik instrument is constructed as follows:

$$B_{mt} = \sum_i \left(\frac{E_{im,1986}}{E_{m,1986}}\right) \cdot \left(\frac{E_{it}/P_t}{E_{i,1986}/P_{1986}} - 1\right),$$

where $E_{im,1986}$ is the level of employment for Blacks aged 22–56 without 4-year college education in industry i, MSA m and year 1986, and $E_{m,1986}$ is the level of employment for Blacks of the age and educational group in MSA m and year 1986. Similarly, E_{it} is the level of employment for Blacks of the age and educational group in industry i and year t. P_t represents the population of Blacks aged 22–56 in year t.

[43]The ANES provides biennial data on voting. I approximate the values for the years when the data are not available using the average of values of the two closest years.

[44]The HHI for each state and year is calculated by squaring the Black population share of each MSA in the state and then summing the resulting numbers.

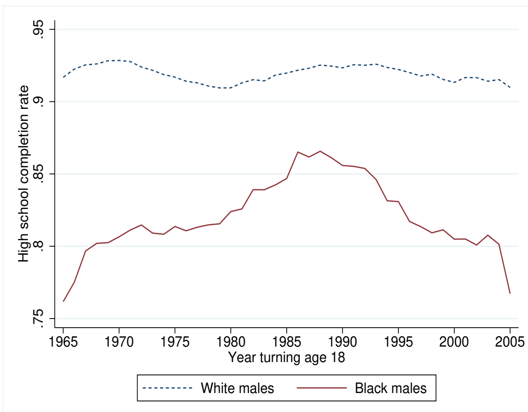
[45]In my sample, the HHI ranges from 1611 in Florida to 8577 in Minnesota, with the mean of 4112 and the median of 3656. I restrict the sample to states with an average HHI smaller than 4112, including Arkansas, California, Florida, Louisiana, Oklahoman, Tennessee, Texas, and Virginia.

5 Conclusion

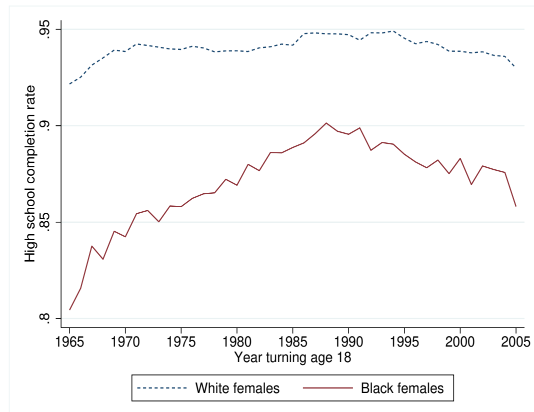
This paper aims to provide an explanation for the stalled progress in Black educational attainment since the late 1980s, especially among Black men, which has resulted in a reversed convergence in the Black-White educational attainment. Specifically, I study whether childhood exposure to mass incarceration lowers young Black adults' high school completion. By employing an IV that exploits sentencing harshness across states and over years, I estimate the causal effect of higher Black male incarceration rates faced by individuals in their childhood due to the implementation of harsher sentencing policies. I find that higher Black male incarceration rates that young Black adults were exposed between 13 and 18 years decrease the likelihood of completing high school, and the effect is greater for Black men than for Black women. The results also suggest that the negative effect is mostly driven by exposure to higher incarceration rates at the extensive margin (i.e., higher likelihood of incarceration conditional on arrest). One potential mechanism underlying the negative effect could be worsened family or neighborhood circumstances due to the incarceration of parents or other Black men from the community. Another mechanism could be that higher risks of incarceration lower young Black men's expected returns to education. Although I present evidence that the school-to-prison pipeline is unlikely to be the main driving force, future research is needed to further distinguish between the role of adult mass incarceration and school expulsion due to harsher school discipline policies.

Figure 1: High School Completion Rates

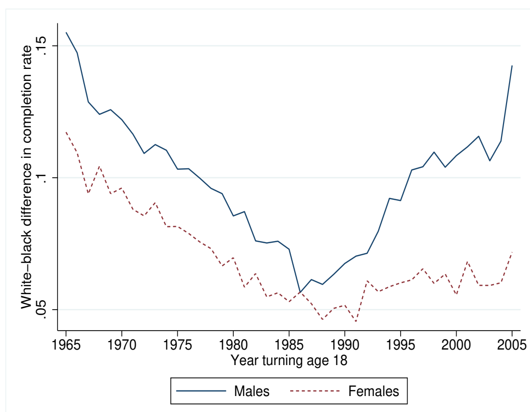
(A) By Race and Cohort, Males



(B) By Race and Cohort, Females



(C) White-Black Differences, By Sex

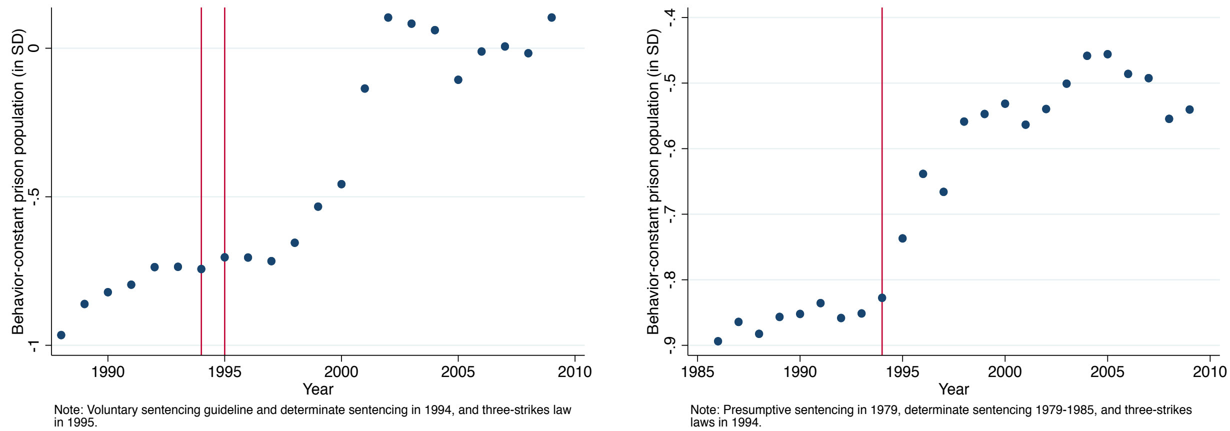


Note: These figures show the high school completion rates by year that each cohort turns age 18. High school completion rates are defined as the fraction of respondents with a high school diploma or GED from the 2006–2010 ACS. The sample comprises all U.S.-born, Black or White non-Hispanic respondents aged 18 or older.

Figure 2: Sentencing Policy Changes and Behavior-Constant Prison Population

(A) Arkansas

(B) Colorado

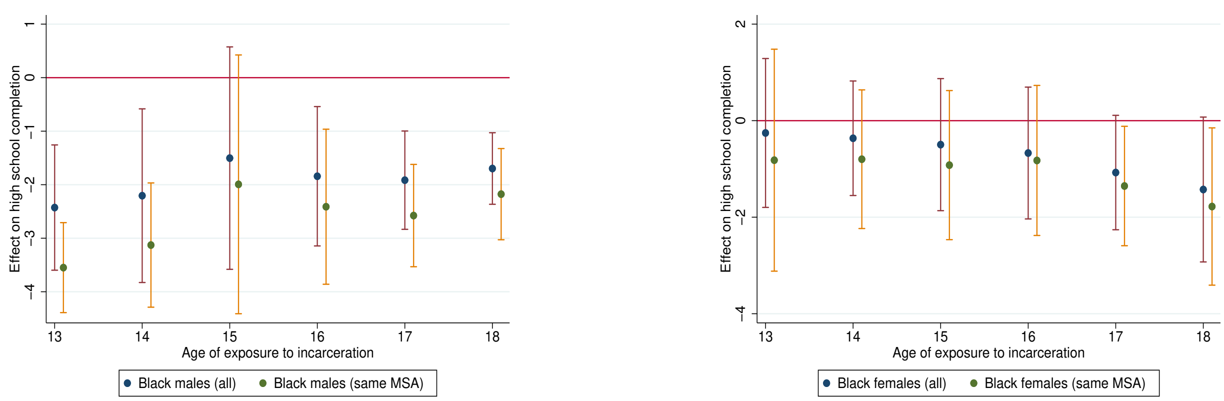


Note: These figures show how the simulated behavior-constant prison population evolved over time in Arkansas and Colorado, where sentencing policy changes were implemented in a relatively discrete way. The y-axis denotes the state-level simulated prison population in SD. Specifically, I aggregate the simulated prison population by MSA and year (I^*_{mt} in equation (4)) to the state level, and standardize the aggregate the state-year simulated prison population so that the mean is 0 and the SD is 1. The values on the y-axes are mostly negative because these states are relatively small.

Figure 3: Impact of Childhood Exposure to Incarceration on High School Completion

(A) Black Males

(B) Black Females



Note: These figures show the impacts of the Black male incarceration rate that individuals faced at different ages (shown on the x-axis) on high school completion rates (IV estimates of β_1 in Equation (2)) and 95% confidence intervals. The blue points represent the estimates for all U.S.-born, Black non-Hispanic respondents aged from 19–23 who turned 18 between 1983 and 2008. The green points represent the estimates for those who were in the same MSA 5 years (5% census) or 1 year (ACS) ago.

Table 1: Correlation between the IV and Potential Confounds

Dependent variable	Share of black population (1)	Violent crime rate (2)	Violent crime growth rate (3)	Property crime rate (4)	Property crime growth rate (5)	Crack Index (6)
IV	0.00404	0.0132	0.0125	-0.136	0.0143	-0.0103
	(0.00749)	(0.0807)	(0.0116)	(0.297)	(0.0108)	(0.0199)
Observations	2,105	2,105	2,105	2,105	2,105	196
R-squared	0.993	0.822	0.592	0.892	0.461	0.867
Dependent variable	**Violent arrest rate (7)**	**Violent arrest growth rate (8)**	**Property arrest rate (9)**	**Property arrest growth rate (10)**	**Drug arrest rate (11)**	**Drug arrest growth rate (12)**
IV	-0.605	-0.0141	-0.675	-0.00160	-0.792	0.0349
	(0.396)	(0.0240)	(0.418)	(0.0167)	(0.493)	(0.0527)
Observations	2,105	2,105	2,105	2,105	2,091	2,078
R-squared	0.743	0.194	0.824	0.233	0.682	0.321

Note: Each observation is a year-MSA cell. The observations are weighted by the size of MSA black population. In Column 1, the dependent variable is the share of black population at the MSA level. In Columns 2–5, the crime rates (i.e., the number of crimes per 100 residents) are measured at the state level using data from the FBI UCR; the lagged crime growth rates are calculated as (crime rate$_{t-1}-$crime rate$_{t-2}$)/crime rate$_{t-2}$. In Column 6, the dependent variable is the crack index proposed by Fryer Jr et al. (2013). I merge their city-level index to the MSA level. The index is only available for 1985, 1989, 1993, 1997, and 2000. The 1985 index is assigned to the 1986 observations to increase the power. In Columns 7–12, the arrest rates are measured at the state level using data from the UCR; the lagged arrest growth rates are calculated as (arrest rate$_{t-1}-$arrest rate$_{t-2}$)/arrest rate$_{t-2}$. All regressions control for year and MSA fixed effects. Standard errors are clustered at the state level. Robust standard errors in parentheses: ***p <0.01, **p <0.05, *p <0.1.

Table 2: First-stage Estimates

Dependent variable	Incarceration rate (age 18)	
	OLS	OLS
	(1)	(2)
IV (age 18)	0.00903***	0.00638***
	(0.00189)	(0.00190)
IV (age 17)		0.00321*
		(0.00178)
IV (age 16)		0.000160
		(0.000988)
IV (age 15)		-0.00241
		(0.00226)
IV (age 19)		0.00123
		(0.00119)
IV (age 20)		0.00132
		(0.000968)
IV (age 21)		-0.00106
		(0.00136)
Observations	121,231	89,727
R-squared	0.863	0.895
Mean of dep. var.	0.064	0.066
SD of dep. var.	0.026	0.026
F-statistic	22.81	NA
Leads = 0	NA	0.237

Note: The sample comprises U.S.-born, Black non-Hispanic respondents aged from 19–23 who turned 18 between 1983 and 2008. The sample excludes MSAs where the Black population (15–24 years) is below the 5th percentile. The dependent variable is the incarceration rate of Black men that an individual was exposed to at age 18. The independent variable in column 1 is the IV (in standard deviation) of the corresponding year and MSA. Column 2 further controls for the lags and leads of the IV. "Leads=0" shows the p-value of the hypothesis test that the leading coefficients are jointly zero. All regressions control for age, cohort, and MSA fixed effects. The standard errors are clustered at the state level. Robust standard errors in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 3: Impact of Black Male Incarceration on High School Completion

Dependent variable	Have high school diploma or GED					
	Black males			Black females		
	OLS	IV	IV	OLS	IV	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A						
Incarceration rate	0.102	-1.697***	-0.871**	-0.273	-1.426	-0.424
(faced at age 18)	(0.289)	(0.406)	(0.420)	(0.269)	(0.912)	(0.488)
Observations	56,968	56,968	143,610	64,263	64,263	178,058
Panel B						
Incarceration rate	0.786*	-2.299***	-1.315**	0.866*	-0.228	-0.0363
(faced at ages 13–18)	(0.445)	(0.421)	(0.520)	(0.451)	(2.120)	(1.284)
Observations	42,159	42,159	90,468	47,479	47,479	110,886
Mean of dep. var.	0.747	0.747	0.809	0.804	0.804	0.845
SD of dep. var.	0.435	0.435	0.393	0.397	0.397	0.362
Respondent age range	19–23	19–23	19+	19–23	19–23	19+

Note: The sample comprises U.S.-born, Black non-Hispanic respondents aged from 19–23 who turned 18 between 1983 and 2008. Columns 3 and 6 do not impose the age restriction and are for all respondents aged 19 or older. The sample excludes MSAs where the Black population (15–24 years) is below the 5th percentile. The dependent variable is an indicator for having a high school diploma or GED. The independent variable in Panel A is the incarceration rate of Black men that an individual was exposed to at age 18, and the independent variable in Panel B is the average incarceration rate of Black men that an individual was exposed to at ages 13–18. All regressions control for age, cohort, and MSA fixed effects. The standard errors are clustered at the state level. Robust standard errors in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 4: Impact of Black Male Incarceration at Different Margins on High School Completion

Dependent variable	Have high school diploma or GED			
	Black males		Black females	
	Extensive IV (1)	Intensive IV (2)	Extensive IV (3)	Intensive IV (4)
Incarceration rate (faced at age 18)	-3.261** (1.303)	-0.309 (0.783)	-1.969** (0.921)	-0.670 (1.184)
Observations	56,968	56,968	64,263	64,263

Note: The sample comprises U.S.-born, Black non-Hispanic respondents aged from 19–23 who turned 18 between 1983 and 2008. The sample excludes MSAs where the Black population (15–24 years) is below the 5th percentile. The independent variable is the incarceration rate of Black men that an individual was exposed to at age 18. Columns 1 and 3 presents the estimates in which the IV exploits variation in sentencing at the extensive margin only (i.e., probability of incarceration given arrest). Columns 2 and 4 presents the estimates in which the IV exploits variation in sentencing at the intensive margin only (i.e., average length of time served in prison). All regressions control for age, cohort, and MSA fixed effects. Standard errors are clustered at the state level. Robust standard errors in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 5: Impact of Black Male Incarceration on High School Completion: Migrated Individuals

Dependent variable	Have high school diploma or GED			
	Black males		Black females	
	IV	IV	IV	IV
	(1)	(2)	(3)	(4)
Incarceration rate	-3.480**		-1.592	
(faced at age 13)	(1.687)		(1.282)	
Incarceration rate		-2.742*		-1.332
(faced at ages 13–18)		(1.510)		(1.078)
Observations	14,297	14,153	19,149	18,929
Mean of dep. var.	0.755	0.755	0.789	0.789
SD of dep. var.	0.430	0.430	0.408	0.408

Note: The sample comprises U.S.-born, Black non-Hispanic respondents aged from 19–23 who turned 18 between 1983 and 2008, and resided in a MSA 1 or 5 years ago that is different from the current MSA of residence. The sample excludes MSAs where the Black population (15–24 years) is below the 5th percentile. The independent variable in columns 1 and 3 is the incarceration rate of Black men of the MSA where an individual resided before migration in the year when the individual was at age 13. The independent variable in columns 2 and 4 is the average incarceration rate of Black men of the MSA where an individual resided before migration during the years when the individual was between 13 and 18 years old prior to migration. (Suppose an individual migrated at age 17. The independent variable is the average incarceration rate that he/she faced between 13 and 16 years.) All regressions control for age, cohort, and MSA fixed effects. Standard errors are clustered at the state level. Robust standard errors in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 6: Impact of Black Male Incarceration on High School Completion:
Adding Control Variables

Dependent variable	Have high school diploma or GED					
	Black males			Black females		
	IV (1)	IV (2)	IV (3)	IV (4)	IV (5)	IV (6)
Incarceration rate (faced at age 18)	-1.697*** (0.406)	-1.735*** (0.425)	-2.604*** (0.590)	-1.426 (0.912)	-1.420 (0.910)	-1.330 (1.049)
Observations	56,968	56,968	49,134	64,263	64,263	55,357
Age, Cohort, & MSA FEs	Yes	Yes	Yes	Yes	Yes	Yes
Education spending	No	Yes	Yes	No	Yes	Yes
Share of votes for Democrats, share of Blacks & Bartik	No	No	Yes	No	No	Yes

Note: The sample comprises U.S.-born, Black non-Hispanic respondents aged from 19–23 who turned 18 between 1983 and 2008. The sample excludes MSAs where the Black population (15–24 years) is below the 5th percentile. All regressions control for age, cohort, and MSA fixed effects. Columns 2 and 5 further controls for log state spending on elementary and secondary education per capita. Columns 3 and 6 further controls for the share of votes to Democrats of the state, the share of Black adult population of the MSA, and labor demand for less-educated Black workers of the MSA. Labor demand is measured with a Bartik instrument, formed by interacting local industry shares and national industry employment growth rates for Black workers aged from 22–56 without 4-year college education. The standard errors are clustered at the state level. Robust standard errors in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 7: Impact of Black Male Incarceration on High School Completion: States with Less Concentrated Black Population

Dependent variable	Have high school diploma or GED			
	Black males		Black females	
	IV	IV	IV	IV
	(1)	(2)	(3)	(4)
Panel A: Baseline (all states)				
Incarceration rate	-1.697***		-1.426	
(faced at age 18)	(0.406)		(0.912)	
Incarceration rate		-2.299***		-0.228
(faced at ages 13–18)		(0.421)		(2.120)
Observations	56,968	42,159	64,263	47,479
Mean of dep. var.	0.747	0.747	0.804	0.804
SD of dep. var.	0.435	0.435	0.397	0.397
Panel A: States with less concentrated Black population				
Incarceration rate	-1.994*		-1.121	
(faced at age 18)	(1.024)		(1.327)	
Incarceration rate		-2.583***		0.798
(faced at ages 13–18)		(0.769)		(2.290)
Observations	37,309	28,078	41,958	31,558
Mean of dep. var.	0.752	0.752	0.808	0.808
SD of dep. var.	0.432	0.432	0.494	0.494

Note: In Panel A, the sample comprises U.S.-born, Black non-Hispanic respondents aged from 19–23 who turned 18 between 1983 and 2008. The sample excludes MSAs where the Black population (15–24 years) is below the 5th percentile. Panel B further restricts the sample to states where the Black population are less concentrated toward specific MSAs. Specifically, the sample comprises states with HHI ≤ 4112 (the mean HHI across states in the sample), including Arkansas, California, Florida, Louisiana, Oklahoman, Tennessee, Texas, and Virginia. All regressions control for age, cohort, and MSA fixed effects. The standard errors are clustered at the state level. Robust standard errors in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

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Appendix

A1 MSA-level Prison Population

I check whether the estimated MSA-level prison population is reliable in two ways. First, Figure A2 compares the MSA-level prison population estimated directly from the NCRP year-end prison population data (x-axes) to the MSA-level prison population backed out using the 2009 year-end prison population and yearly changes in prison population estimated using the 2005–2009 admission and release files (y-axes) from 2003 to 2008. This exercise checks whether data on stocks of prisoners are consistent with data on admissions and releases in the NCRP. Figure A2 shows that the two estimates are almost the same.

Second, I aggregate the estimated MSA-level prison population to the state level, and compare the resulting state-level prison population to the state-level prison population provided by the National Prisoners Statistics (NPS). The NPS produces an enumeration of persons in state and federal prisons by year, state, gender, and race. These two data series should not match exactly.[46] However, large deviations in the trend between the two estimates can cause some concern. Figure A3 shows that the NCRP estimates are comparable to the NPS estimates for several large states. The average correlation between two estimates for all states in my sample is 0.94.

A2 Background on Sentencing Reforms

This section provides a brief background on changes in sentencing policies between the 1970s and the 1990s, how they differ across states, and how they could have contributed to the growth of incarceration. Table A1 compares the policy changes across states.

Determinate sentencing Between the late 1970s and the 1990s, some states adopted determinate sentencing by abolishing or curtailing the discretionary power of parole boards, to ensure that time

[46]The NCRP and NPS differ in several respects. First, the NCRP includes prisoners sentenced to state or federal prisons. The NPS includes prisoners in federal and state prisons prior to 1999, and has been expanded to include inmates in local jails since then. Second, the NPS separates race and Hispanic origin prior to 1999. Since then, it has combined race and Hispanic origin into a single item, including non-Hispanic White, non-Hispanic Black, Hispanic, and other races. To be consistent, I only consider races without distinguishing Hispanic origin.

served by offenders would be determined by the length of the sentence.[47] Determinate sentencing may have contributed to the growth of the prison population by eliminating the possibility that parole boards could adjust prison populations through selective release, which could increase the average length of time served (Raphael and Stoll, 2013).

Sentencing guidelines In the 1970s, states started to adopt various forms of sentencing guidelines for consideration in judicial sentencing decisions (Stemen et al., 2006). Among states with sentencing guidelines, there is substantial variation. For instance, while in some states guidelines are legally binding, in other states guidelines are voluntary.[48] These guidelines may have contributed to the growth of incarceration because they include many mandatory minimum sentences, which curtail judges' discretion to impose alternatives to incarceration and can lead to longer sentences (Raphael and Stoll, 2013).[49]

Truth-in-sentencing laws From 1984 through the late 1990s, many policy changes made sentences more stringent (Tonry, 2013). Some states sought to ensure that offenders serve a substantial portion of their sentences through truth-in-sentencing laws.[50] The requirements of the laws vary considerably across states, in terms of the type of offenders covered under the laws and the proportion of sentences to be served.[51] Such laws may have contributed to the growth of incarceration through longer time served in prison (Ditton and Wilson, 1999).

Three-strikes laws Since 1994, some states have adopted three-strikes laws, which impose more severe mandatory sentences for repeat offenders. States vary in terms of the number and type of convictions to trigger the laws and the sentences imposed under them (Clark et al., 1997; Stemen

[47] Throughout the early 1970s, indeterminate sentencing was implemented in all states in which parole boards maintained their authority to release inmates at their discretion.

[48] For a detailed review of state sentencing guidelines, see Frase (2005).

[49] For instance, after federal sentencing guidelines were implemented in 1987, the share of convicted federal offenders to whom probation could be applied at the discretion of judges dropped from more than 60% to less than 15% (Champion, 2008).

[50] In 1994, the federal government established the Truth-in-Sentencing Incentive Grants Program, which provided grants for prison construction and expansion to states that adopted policies requiring some offenders to serve large portions of their sentences.

[51] For instance, although most truth-in-sentencing states require specific offenders to serve 85% of the prison sentence, some states have a 50% requirement or a 100% requirement. In addition, while most states apply the requirements to violent offenders or certain other offenders, some states apply the requirements to all sentenced offenders (Ditton and Wilson, 1999; Sabol et al., 2002).

et al., 2006).[52] Such laws may have contributed to the growth of incarceration through a higher tendency to incarcerate arrestees and longer sentences.

The war on drugs The Anti-Drug Abuse Acts of 1986 and 1988 were major federal laws that paid special attention to crack cocaine.[53] It has been argued that the crack cocaine provisions of the act targeted Black drug offenders (Alexander, 2012; Neal and Rick, 2016).

Summary This section presents several major sentencing reforms since the mid-1970s. There is considerable variation across states in terms of the type of policies implemented, the timing of policy changes, and the requirements. The variation in and complexity of these policies provide supporting evidence of the exogeneity of the policy changes. Despite the complexity, the discussion in this section indicates that sentencing reforms are likely to have contributed to the increasing prison population through (i) a higher tendency to incarcerate arrestees and (ii) longer time served in prison. Nevertheless, it is noteworthy that this is not a complete review of changes in sentencing harshness across states. For instance, states' amendments to their sentencing guidelines are not documented in the paper, but such amendments to the sentencing guidelines can also affect the level of the IV.

A3 Juvenile Incarceration Rates

As a robustness check, I control for the Black juvenile incarceration rate that an individual faced at age 18. I use the Black juvenile incarceration rate to approximate the harshness of school discipline policies. However, it should be noted that this specification could be problematic because the juvenile incarceration rate can be endogenous.

I obtain the juvenile incarceration rate by state and race using the Easy Access to the Census of Juveniles in Residential Placement: 1997–2017, provided by the Office of Juvenile Justice and

[52] For instance, in California, a "second striker" (i.e., someone with a prior violent offense convicted of a second felony) receives a sentence equal to twice the sentence for the second offense and a "third striker" receives an indeterminate sentence of 25 years to life. Pennsylvania's three-strikes law is triggered only when an offender of two prior felonies is convicted of one of eight specified offenses, and the judge has the discretion to increase the sentence by up to 25 years (Raphael and Stoll, 2013).

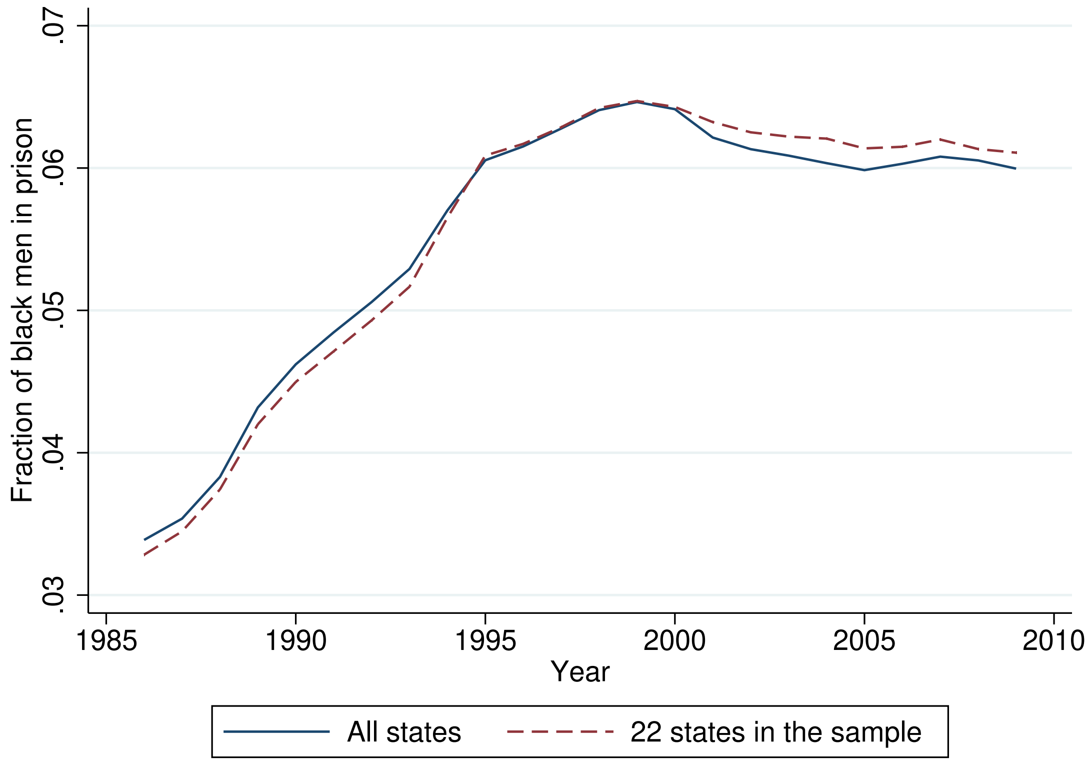
[53] For instance, a minimum sentence of 5 years without parole was mandated for possession of 5 grams of crack cocaine, while the same sentence was mandated for a possession of 500 grams of powder cocaine—the so-called 100:1 disparity.

Delinquency Prevention (Sickmund et al., 2019). The data analysis tool provides the number of juvenile offenders in residential placement per 100,000 juveniles aged 10 through the upper age of the juvenile court jurisdiction in each state, by race and state, for 1997, 1999, 2001, 2003, 2006, 2007, 2010, 2011, 2013, 2015, and 2017.

I match each individual (aged 19–23) in my sample with the state-level juvenile incarceration rate of the state and year when the individual was at age 18. Since the juvenile incarceration rates are not available prior to 1997, I exclude data from the 5% Census for 1990. If the rate of the year of interest is not available, I use the closest available year. For instance, consider a respondent aged 20 in the 5% Census for 2000. I need the juvenile incarceration rate for his state in 1998 (when he was at age 18). Since the data are not available for 1998, I instead use the juvenile incarceration rate in 1997 (when he was at age 17).

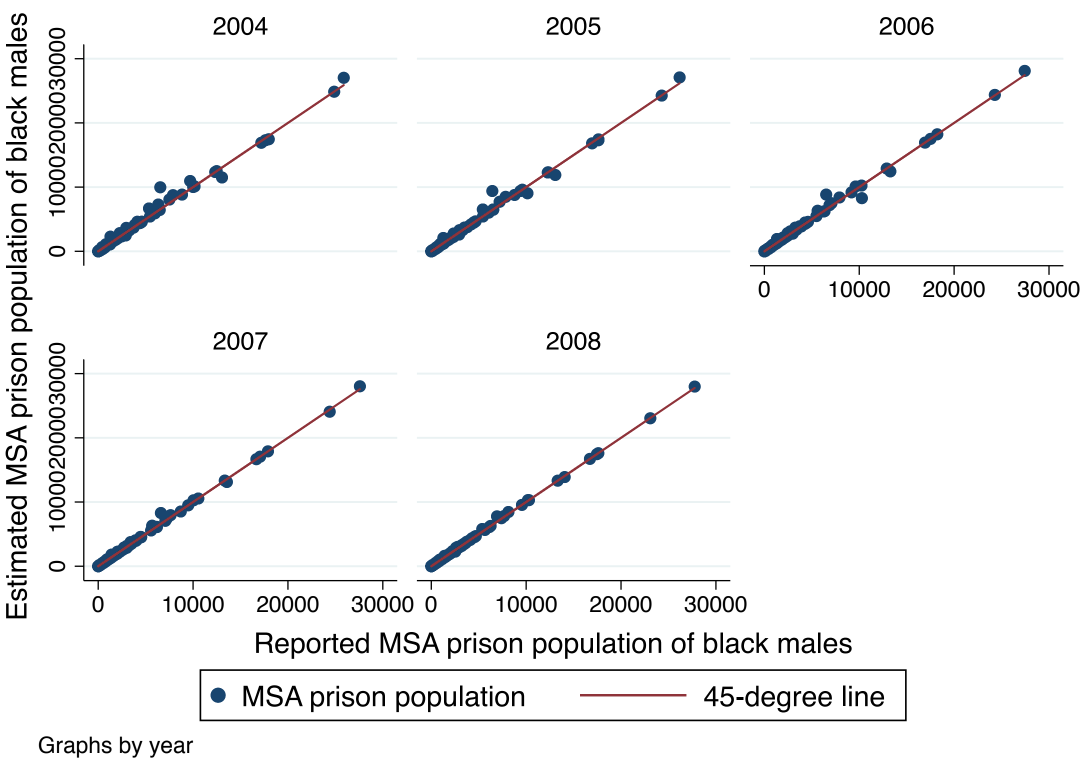
Table A5 presents the results. Columns 1 and 4 present the baseline estimates for comparison (i.e., columns 2 and 5 of Table 3 Panel A). Columns 2 and 5 present the estimates using data from the 5 % Census for 2000 and the ACS 2006–2010. The estimates without observations in the 1990 data are slightly smaller in magnitude, but the estimate is still statistically significant for Black men and insignificant for Black women. Lastly, columns 3 and 6 show that the estimates remain the same after controlling for the Black juvenile incarceration rate faced at age 18. The results should be interpreted with caution since the juvenile incarceration rate could be endogenous. However, the robust finding could still provide some evidence that the school-to-prison pipeline is not likely to be the main mechanism behind the negative effect of childhood exposure to mass incarceration on high school completion.

Figure A1: Black Male Incarceration Rates for Two Samples



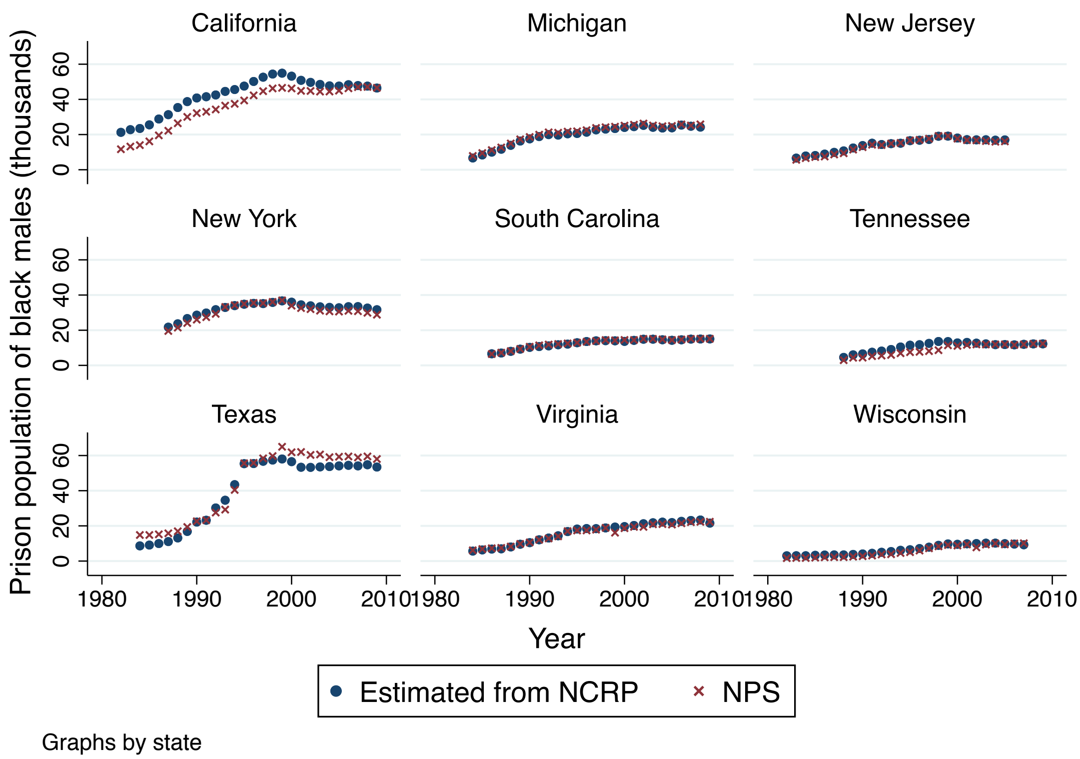
Note: The figure shows the fractions of Black men in state or federal prisons for the 22 states in my sample and for all states using data from the NPS and U.S. Census Intercensal County Population Data. The denominators of the incarceration rates are the residential population of Black men aged from 20 to 54.

Figure A2: Estimated and Reported Prison Population from the NCRP
(MSA Level, Black Men)



Note: The figure checks the consistency between the NCRP year-end prison population data and the NCRP data on admissions and releases. The x-axis shows the year-end prison population directly estimated from NCRP year-end prison population data. The y-axis shows the MSA-level prison population backed out using the 2009 year-end prison population and yearly changes in prison population estimated using the 2005–2009 admission and release files. Each point represents a MSA.

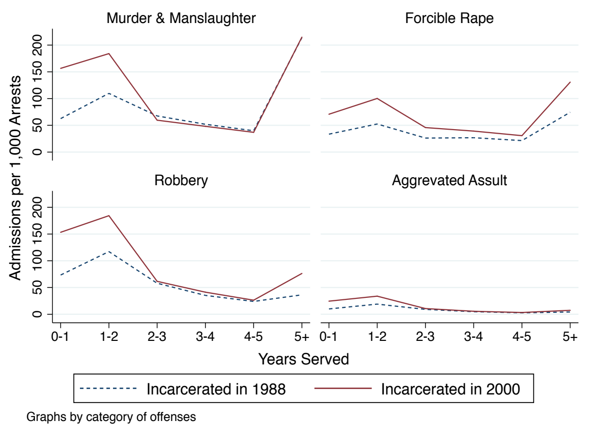
Figure A3: Prison Population from the NCRP and NPS
(State Level, Black Men)



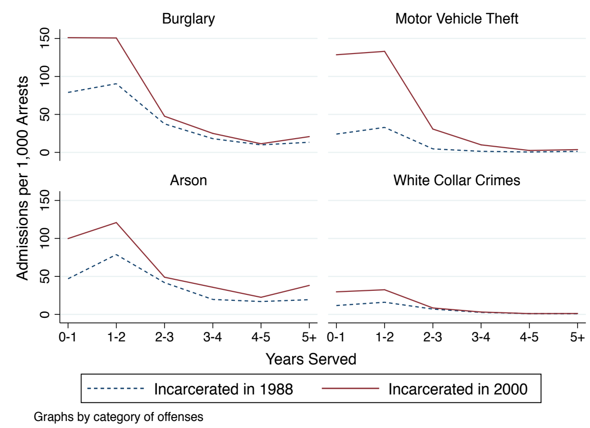
Note: The figures checks the reliability of the MSA-level prison population estimated from the NCRP. The blue circles represent the state prison population obtained by aggregating the MSA-level prison population estimated from the NCRP. The red x-markers represent the state-level prison population provided by the NPS. The figure presents estimates for large states that have admission and release records going back to the 1980s. The average correlation of the two estimates for all the states in my sample is 0.935.

Figure A4: Number of People Serving Time in Prison Per 1,000 Arrests

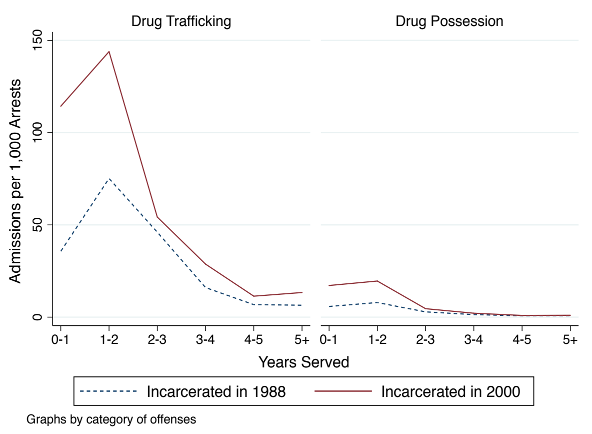
(A) Violent Offenses



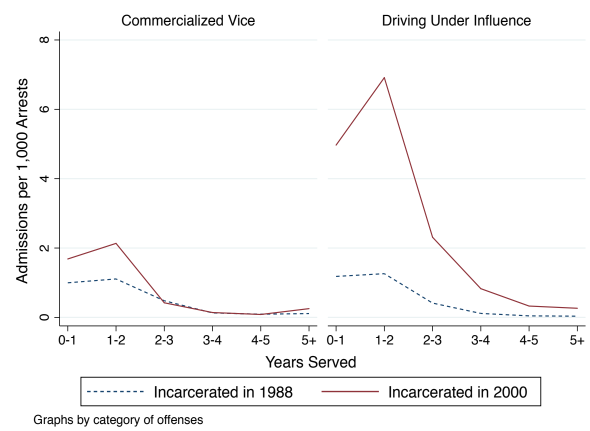
(B) Property Offenses



(C) Drug Offenses

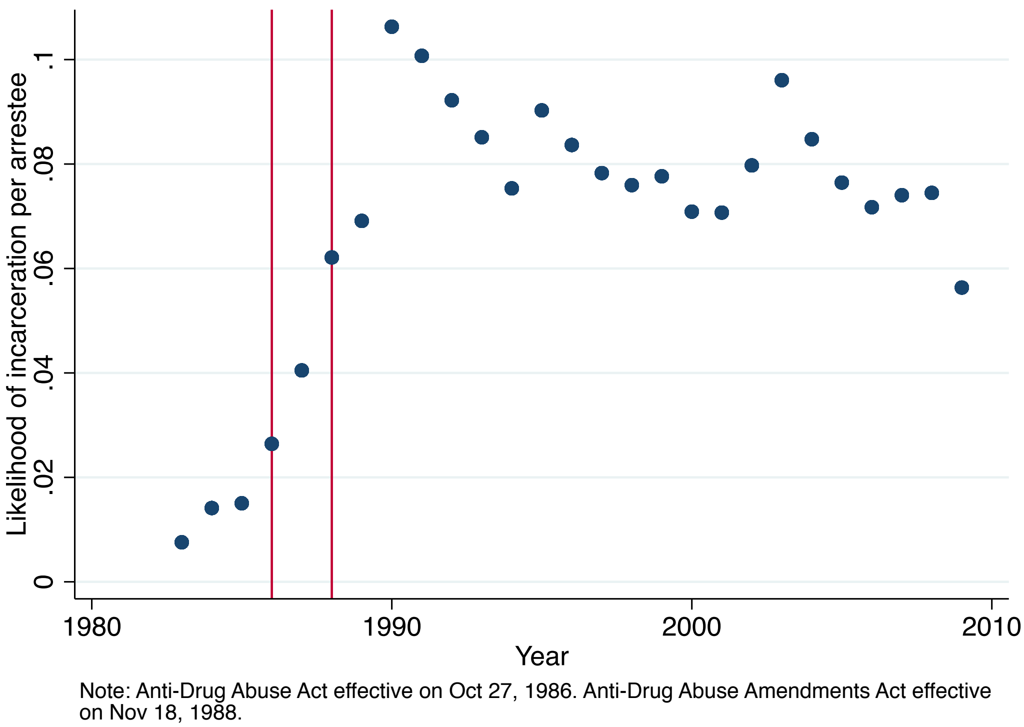


(D) Offenses Against Public Order



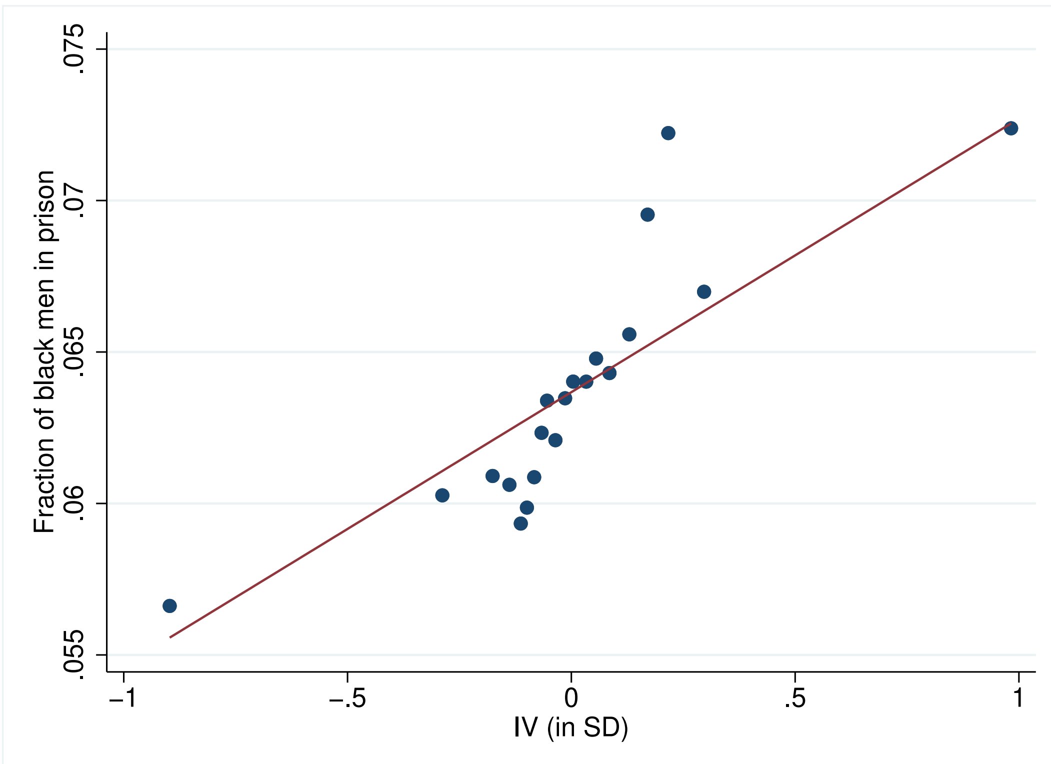
Note: The figures present the number of persons *per 1,000 arrests* who served t years in prison for those who were arrested in 1988 (in dotted blue lines) and in 2000 (in solid red lines) for each crime category. t is divided into 6 groups shown on the x-axis: 0-1 year, 1-2 years, 2-3 years, 3-4 years, 4-5 years, and 5 or more years. The number of arrests is from the UCR. The number of admissions and the average time served in prison are estimated from the NCRP. Estimating distributions requires more accurate data than constructing the IV, so same as Neal and Rick (2016), the figures are obtained using data from eight states with high data quality: California, Colorado, Michigan, New Jersey, New York, South Carolina, Washington, and Wisconsin. Neal and Rick (2016) show that the prison population patterns in these states are comparable to those in all state. My results are comparable to their results in Table 2.

Figure A5: Probability of Incarceration Conditional on Arrest
(Black Adults, Drug Possession)



Note: This figure presents the probability of incarceration conditional on arrest for drug possession among Black adults. The number of arrests is from the UCR and the number of prison admissions is from the NCRP.

Figure A6: First-stage Relationship



Note: This figure presents a binned scatter plot of the relationship between the Black male incarceration rate and the IV (in SD). The sample comprises U.S.-born, Black non-Hispanic respondents aged from 18–25 who turned 18 between 1983 and 2008. I control for age, cohort, and MSA fixed effects. I estimate the fitted line on the binned points using OLS.

Table A1: Sentencing Policy Changes

States	Determinate Sentencing[a]	Sentencing Guidelines Years[b]	Sentencing Guidelines Voluntary[c]	Truth in Sentencing[d] Years	Truth in Sentencing[d] Requirements	Three-Strikes Laws[e]
Alabama						
Alaska	1980**	1980	No		100%	
Arizona	1994	1978***		1994	85%	
Arkansas*	1994**	1994	Yes		70%	1995
California*	1976	1976***		1994	85%	1994
Colorado*	1979-1985	1979***			75%	1994
Connecticut	1981-1990			1994	50%	1994
				1996	85%	
Delaware	1990	1987	Yes	1990	85%	
District of Col.				2000	85%	
Florida*	1983	1983	1983-1994	1995	85%	1995
Georgia				1995	85%	1995
Hawaii						
Idaho					100%	
Illinois	1978			1995	85%	
Indiana	1977	1977***			50%	1994
Iowa				1996	85%	
Kansas	1993	1993	No	1993	80%	1994
				1995	85%	
Kentucky				1998	85%	
Louisiana*		1987	Yes	1997	85%	1994
Maine	1976			1995	85%	
Maryland		1983	Yes		50%	1994
Massachusetts					75%	
Michigan*		1984	1984-1999	1994	85%	
Minnesota*	1980	1980	No	1993	85%	
Mississippi	1995-2000			1995	85%	
Missouri*		1997	Yes	1994	85%	
Montana					25%	1995
Nebraska					50%	
Nevada					100%	1995
New Hampshire				1982	100%	
New Jersey*		1977***		1997	85%	1995
New Mexico	1977	1977***				1994
New York*				1995	85%	
North Carolina*	1981	1994	No	1994	85%	1994
North Dakota				1995	85%	1995
Ohio	1996	1996	No	1996	85%	
Oklahoma*				1998	85%	
Oregon*	1989	1989	No	1990	80%	

				1995	100%	
Pennsylvania*		1982	No	1911	85%	1995
Rhode Island		1981***				
South Carolina*				1996		1996
South Dakota					85%	
Tennessee*	1989**	1989	No	1995	85%	1994
Texas*					50%	
Utah		1979	Yes	1985	85%	1995
Vermont						1995
Virginia*	1995	1991	Yes	1995	85%	1994
Washington*	1984	1984	No	1990	85%	1993
West Virginia						
Wisconsin*	1999	1985-1995	Yes	1999	100%	1994
Wyoming						

Note: 20 states with * are in the sample of analysis because of data limitations of the NCRP. Details are discussed in Section 2.

[a]Years of determinate sentencing are from Stemen et al. (2006). A range means that indeterminate sentencing reinstated later. Some states (**) partially abolished parole release (Frase, 2005).

[b]Years of sentencing guidelines are from Frase (2005). Some states (***) adopted presumptive sentencing, a system of single recommended terms or narrow sentence ranges (Stemen et al., 2006).

[c]Information on whether sentencing guidelines are voluntary is from Frase (2005) and Stemen et al. (2006). A range means that guidelines were voluntary during the period.

[d]Years of truth-in-sentencing laws and requirements are from Ditton and Wilson (1999) and Sabol et al. (2002).

[e]Years of three-strikes laws are from Marvell and Moody (2001).

Table A2: Mean of Characteristics for Compliers and All Black Male Offenders

	Compliers	All Black Offenders
Violent offense	0.20	0.27
Property offense	0.21	0.28
Drug offense	0.43	0.36
Public offense	0.16	0.08
Prior felony	0.79	0.49

Note: The sample comprises all Black male offenders who were admitted to prison between 1986 and 2009. For simplicity, I assume a binary instrument Z_{mt}, where $Z_{mt} = 1$ if the change in the IV for MSA m and year t (relative to 1986) is greater than the median. Similarly, I assume a binary treatment W_{mt}, where $W_{mt} = 1$ if the change in the Black male incarceration rate (relative to 1986) is greater than the median. Define $W_{mt}(j)$ to be the potential outcome of W_{mt} given that $Z_{mt} = j, j \in \{0, 1\}$. Therefore, compliers are prisoners who were sentenced in MSA m and year t, where $W_{mt}(0) = 0$ or $W_{mt}(1) = 1$.

Table A3: Impact of Black Male Incarceration on Education Distribution

Panel A						
Dependent variable	Have high school diploma or GED					
	Black males			Black females		
	P10	P25	P75	P10	P25	P75
	IV	IV	IV	IV	IV	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Incarceration rate	0.0038	-8.803***	-0.462	-3.212**	-3.760	0.035
(faced at age 18)	(2.180)	(1.697)	(0.629)	(1.543)	(4.925)	(0.265)
Observations	2,470	2,470	2,470	2,466	2,466	2,466
Mean of dep. var.	0.148	0.490	0.994	0.187	0.698	0.997
SD of dep. var.	0.355	0.500	0.076	0.390	0.459	0.055

Panel B				
Dependent variable	Complete at least 1 year of college			
	Black males		Black females	
	P75	P90	P75	P90
	IV	IV	IV	IV
	(1)	(2)	(3)	(4)
Incarceration rate	-5.465**	-2.418*	-4.144**	-0.282
(faced at age 18)	(2.310)	(1.408)	(1.956)	(1.525)
Observations	2,470	2,470	2,466	2,466
Mean of dep. var.	0.627	0.918	0.849	0.968
SD of dep. var.	0.484	0.274	0.358	0.176

Note: The sample comprises U.S.-born, Black non-Hispanic respondents aged from 19–23 who turned 18 between 1983 and 2008. The sample excludes MSAs where the Black population (15–24 years) is below the 5th percentile. Each observation is a year-MSA-age cell. The dependent variable is an indicator that the individual at the pth ($p = 10, 25, 75, 90$) percentile of the education distribution has a high school diploma or GED (Panel A) or completes at least 1 year of college (Panel B). All regressions control for age, cohort, and MSA fixed effects. The standard errors are clustered at the state level. Robust standard errors in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A4: Impact of Sentencing Harshness on High School Completion

Dependent variable	Have high school diploma or GED			
	Black males		Black females	
	OLS (1)	OLS (2)	OLS (3)	OLS (4)
IV	-0.0147*** (0.00312)		-0.0134 (0.0112)	
IV (extensive)		-0.0277*** (0.00604)		-0.0169* (0.00853)
IV (intensive)		0.00999 (0.00837)		0.00158 (0.0116)
Observations	56,968	56,968	64,263	64,263
R-squared	0.046	0.047	0.027	0.027

Note: The sample comprises U.S.-born, Black non-Hispanic respondents aged from 19–23 who turned 18 between 1983 and 2008. The sample excludes MSAs where the Black population (15–24 years) is below the 5th percentile. The independent variable in columns 1 and 3 is the IV (in standard deviation) of the year and MSA in which an individual was at age 18. The independent variables in columns 2 and 4 are the corresponding IVs that exploit variation in sentencing at the extensive margin and the intensive margin separately. All regressions control for age, cohort, and MSA fixed effects. Standard errors are clustered at the state level. Robust standard errors in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A5: Impact of Black Male Incarceration on High School Completion:
Controlling for Black Juvenile Incarceration Rates

Dependent variable	Have high school diploma or GED					
	Black males			Black females		
	IV (1)	IV (2)	IV (3)	IV (4)	IV (5)	IV (6)
Incarceration rate	-1.697***	-1.044**	-1.024*	-1.426	-0.570	-0.574
(faced at age 18)	(0.406)	(0.521)	(0.575)	(0.912)	(1.413)	(1.432)
Observations	56,968	42,865	42,865	64,263	49,074	49,074
Mean of dep. var.	0.747	0.765	0.765	0.804	0.820	0.820
SD of dep. var.	0.435	0.424	0.424	0.397	0.384	0.384
Sample year	All	w/o 1990	w/o 1990	All	w/o 1990	w/o 1990
Juvenile incarceration rate	No	No	Yes	No	No	Yes

Note: The sample comprises U.S.-born, Black non-Hispanic respondents aged from 19–23 who turned 18 between 1983 and 2008. The sample excludes MSAs where the Black population (15–24 years) is below the 5th percentile. Columns 2 and 5 excludes data from the 5% census for 1990. Columns 3 and 6 further control for the state-level Black juvenile incarceration rate that an individual was exposed to at age 18. All regressions control for age, cohort, and MSA fixed effects. The standard errors are clustered at the state level. Robust standard errors in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.