

Mass Incarceration Reforms: The Alabama Experience¹

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Abstract

Alabama’s prison system has been among the most scrutinized in the United States, operating under active federal litigation over unconstitutional conditions while simultaneously enacting some of the most significant sentencing reforms in its history. Between 2013 and 2016, Alabama implemented a sequence of graduated criminal justice reforms: the transition from voluntary to presumptive sentencing guidelines, and the creation of a new Class D felony classification that established a presumption against incarceration for low-level drug and property offenses. Using facility-level data from the Alabama Department of Corrections and a state-year panel from the Vera Institute of Justice, we evaluate the effect of these reforms on both the level and racial composition of Alabama’s prison population. We employ a two-way fixed effects event-study design with a comparison group of nine states that remained untreated throughout the sample window. We find that Alabama’s total custodial prison population declined by approximately 13.5 percent between 2014 and 2019. More strikingly, the black share of new prison admissions, or the flow into prison, declined by approximately 19.6 percent relative to comparison states in the post-reform period, while the black share of the stock of the standing prison population declined by approximately 7 percent. The gap between the racial composition of new admissions and the existing prison stock widened by approximately 13 log points post-reform confirming a sharp and immediate flow-margin response.

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1. Introduction

Mass incarceration in the United States has long fallen under heavy scrutiny (Alexander 2010; Thompson 2010; Western 2006). That scrutiny has only increased over the past several years, as many have called for systemic reform, citing the country's exceptionally high incarceration rate alongside stark racial disparities and the documented social and economic harms that incarceration imposes on families and communities (Prison Policy Initiative 2026; Council on Criminal Justice 2021; Pettit and Western 2004), with one state in particular, Alabama, landing front and center in the conversation surrounding mass incarceration.

Following a multi-year investigation initiated in 2016 under the Civil Rights of Institutionalized Persons Act (CRIPA), the Department of Justice (DOJ) issued a landmark 2019 report concluding that Alabama's men's prisons violated the Eighth Amendment's prohibition against cruel and unusual punishment. The report documented rampant prisoner-on-prisoner violence and sexual abuse, pervasive drug use, extreme overcrowding (facilities operating at roughly 170% of capacity), and chronic understaffing so severe that a warden described conditions as "barbaric." A second DOJ report in July 2020 found a pattern and practice of excessive, and at times deadly, force by correctional officers across 12 of 13 facilities reviewed, and the federal government ultimately filed suit against the state in December 2020.

Further, through use of firsthand accounts often filmed with contraband cell phones, the 2025 documentary, *The Alabama Solution* provided a scathing depiction of the conditions of Alabama prisons. The film documented conditions similar to those described in the DOJ report including prisons operating well beyond reasonable capacity while simultaneously being grossly understaffed and evidence of rampant inmate violence (Jarecki and Kaufman 2025).

The state's response to the findings in the DOJ investigation was mixed. Governor Ivey acknowledged the DOJ's concerns and pledged an "Alabama solution" to the crisis, forming an executive study group on criminal justice policy and launching a plan to build new mega-prisons (Alabama Appleseed Center for Law and Justice 2023). Yet critics noted that the state's primary remedy—construction of new 4,000-bed facilities at a projected cost exceeding \$1.3 billion—directly contradicted the DOJ's own warning that "new facilities alone will not resolve the contributing factors to the overall unconstitutional condition of ADOC prisons, such as understaffing, culture, management deficiencies, corruption, policies, training, non-existent investigations, violence, illicit drugs, and sexual abuse" (U.S. Department of Justice, Civil Rights Division 2019). The state also increased the ADOC budget by 48% and provided substantial pay raises to corrections officers, steps that advocates described as insufficient absent structural reform (Alabama Appleseed Center for Law and Justice 2023).

Contrastingly, the state did not initially respond to the release of *The Alabama Solution* until it was nominated for the Academy Award for Best Documentary Feature at the 98th Academy Awards, at which point Governor Kay Ivey's office issued a statement: "...[T]here has never been an Alabama governor more dedicated to solving the longstanding challenges facing the system than Governor Ivey. From recruiting a record number of corrections officers to doing sentencing reforms to constructing needed, new facilities, Governor Ivey is getting the job done and making it safer for inmates, officers and the public alike."

Consistent with Governor Ivey's claims, the state of Alabama recently enacted several important criminal justice reforms including the introduction of structured sentencing guidelines

(Rushin et al. 2019) and the creation of a new Class D felony for low-level drug and property offenses with a presumption against prison sentence among other things. The effect of these changes on incarceration levels and on the Black shares of prison admissions and the standing prison population remains unstudied and is the central focus of our work here.

Using Alabama Department of Corrections facility level data, we first find evidence that key criminal justice reform policies lead to a decrease in the prison population in Alabama. Second, using a state panel of incarceration data, we find that criminal justice reform policies implemented in Alabama around 2015 led to a decrease in the Black share of new prison admissions and the standing prison population.

2. Background

To understand the Alabama prison experience, and to potentially measure the effect of the “Alabama Solution”, it’s necessary to understand incarceration generally in the United States. The US incarcerates a larger share of its population than any other nation on earth. Between 1970 and the early 2000s, the total U.S. prison and jail population grew from roughly 200,000 to over 2.2 million far outpacing population growth (Thompson 2010; Western 2006). This phenomenon, commonly described as “mass incarceration,” emerged from a series of policies, laws and attitudes that dramatically increased the reach and severity of criminal punishment (Travis et al. 2014). Scholars across disciplines have characterized the resulting system as racially skewed, economically counterproductive, and damaging to the social fabric of communities of color in ways that persist across generations (Alexander 2010; Pettit and Western 2004).

2.1 The History of Mass Incarceration

Perhaps the single largest contributor to the mass incarceration movement in the US was the federal government’s declaration of a “War on Drugs.” In June 1971, President Richard Nixon officially designated drug abuse “public enemy number one” in the United States and launched a broad enforcement-centered campaign involving expanded federal funding for drug-control agencies, the creation of the Drug Enforcement Administration (DEA) in 1973, and the introduction of mandatory minimum sentencing for drug offenses (Brennan Center for Justice 2019; Nixon 1971). Subsequent work revealed that the War on Drugs had ulterior motives to destabilize two large voting populations: antiwar activists and black communities (Hinton 2016).

The Reagan administration dramatically intensified these policies in the 1980s which in turn dramatically increased the prison population (Brennan Center for Justice 2019). Reagan’s administration redirected federal drug enforcement resources, imposed harsher state-level penalties, and pushed legislation that imposed severe mandatory minimum sentences for a wide range of drug offenses. The Anti-Drug Abuse Act of 1986 allocated \$1.7 billion to drug enforcement, established a series of mandatory minimum sentences, and created a provision that made possession and distribution of crack cocaine subject to penalties one hundred times more severe than those applicable to equivalent quantities of powder cocaine (*Anti-Drug Abuse Act of 1986* 1986). This 100-to-1 crack-to-powder sentencing disparity would become one of the most widely condemned racial injustices in modern criminal law (Alexander 2010; Forman 2012). Even recent research has found that mandatory minimums in relation to crack cocaine are commonly used to manipulate sentences and enhance punishment for drug related crimes (Tuttle 2026).

“Truth in sentencing” laws requiring violent offenders to serve at least 85% of their sentences created structural incentives for states to grow their incarcerated populations (Murakawa 2014). Between 1975 and 2019, the U.S. prison population rose from 240,593 to approximately 1.43 million, with drug offenses accounting for roughly one in five of those incarcerated at any given time (Travis et al. 2014) though there is little evidence truth-in-sentencing laws achieved their stated purposes (Macdonald 2024; Shepherd 2002).

In addition to several legislative and political moves that led to the mass incarceration of the US, many theorized that changes to policing strategies, like the “broken windows” theory, played a significant role in expanding the scope and severity of criminal justice (Wilson and Kelling 1982). The theory held that policing low-level disorder would prevent the escalation to serious crime and was made popular in the 1990’s when New York City Mayor Rudy Giuliani and Police Commissioner William Bratton implemented an aggressive “quality-of-life” or “zero tolerance” policing strategy built explicitly on the broken windows framework (Kelling and Coles 1996). Though some initial evidence suggested a possibility of the efficacy of broken window policing (Corman and Mocan 2005), follow up analyses cast doubt on the efficacy of the deterrent effect of broken window policies (Harcourt and Ludwig 2006). Recent evidence suggests that while increased police deter crime, the deterrent effects are weak and inconsistent (Chalfin and McCrary 2017).

2.2 The Cost of Mass Incarceration

At the individual level, incarceration imposes economic costs that extend well beyond the period of confinement. Kling (2006) used randomly assigned judges as instruments for sentence length and found limited effects of incarceration duration on post-release earnings, but Garin et al. (2025) estimate that a one-year sentence reduces cumulative earnings by 13 percent over five years. Bhuller et al. (2020) show that incarceration effects are heterogeneous: individuals who were employed prior to incarceration suffer lasting earnings losses, while those who were not working experience gains from prison programming that improve their post-release employment. Rose and Shem-Tov (2021) estimate the full dose-response function of sentence length on recidivism and find diminishing returns, with cost-benefit analysis indicating that the fiscal costs of longer sentences outweigh the public safety benefits at current margins.

2.3 Racial Disparities in Incarceration

Research has found that black defendants face mandatory minimum charges at 1.75 times the rate of comparable white defendants (Rehavi and Starr 2014), bail often gets set consistent with a bias against black defendants (Arnold et al. 2018) and that juvenile incarceration itself is criminogenic, reducing high school completion and increasing adult incarceration, with effects concentrated among minority youth (Aizer and Doyle 2015). These findings suggest that racial disparities in incarceration reflect compounding decisions across the criminal justice pipeline, from policing through charging, bail, sentencing, and post-release supervision.

The combination of diminishing crime-reduction returns, substantial individual economic costs, and persistent racial disparities has generated an economic case for sentencing reform that complements the legal and sociological arguments reviewed above. Lofstrom and Raphael (2016) evaluate the most prominent state-level reform to date, California’s 2011 Public Safety Realignment, which shifted lower-level offenders from state prisons to county supervision. They find minimal effects on violent crime and only modest increases in auto theft. However, virtually

no research has examined whether sentencing reforms reduce racial disparities in the prison population itself, and evidence from high-incarceration Southern states is absent from the literature. Alabama's graduated reforms provide an opportunity to address both gaps.

2.4 Efforts to Address Mass Incarceration

By the mid-2000s, a bipartisan consensus had begun to emerge that the punitive policies of the prior three decades had overshot their public safety aims—generating unsustainable fiscal burdens and producing deeply inequitable outcomes without commensurate crime reduction benefits (The Sentencing Project 2022; Travis et al. 2014). This consensus gave rise to a wave of federal and state criminal justice reforms aimed at reducing prison populations while maintaining public safety—a project framed under the rubric of “justice reinvestment” at the federal level and through a variety of sentencing, classification, and diversion reforms at the state level.

The Justice Reinvestment Initiative (JRI), launched by the Bureau of Justice Assistance beginning in 2010, sought to help states analyze the drivers of their prison populations and redirect the resulting cost savings into community-based interventions targeting recidivism. Under the JRI framework, states partnered with federal agencies and nonprofit organizations—including the Council of State Governments and the Pew Charitable Trusts—to redesign sentencing structures, diversion programs, and supervision practices (The Sentencing Project 2022).

At the federal level, the culminating legislative achievement of this reform period was the First Step Act, signed into law by President Donald Trump in December 2018 (*First Step Act of 2018* 2018). The Act, which passed with bipartisan support, combined meaningful sentencing reforms with prison programming improvements. While advocates noted that the First Step Act left significant mandatory minimums in place and did not address the state prison systems that house the vast majority of incarcerated Americans, it nonetheless marked the most consequential reduction in federal drug sentences in decades (The Sentencing Project 2022).

2.5 The History of Sentencing Reform in Alabama

Alabama's experience with sentencing reform mirrors the national arc but with a distinctive institutional history that makes it a particularly useful laboratory for studying the effects of structured sentencing on incarceration and disparity. Between 1970 and 2000, Alabama's incarceration rate grew by 326%, reaching levels substantially above the national average (Rushin et al. 2019). For much of this period, Alabama operated without formal sentencing guidelines: judges exercised largely unguided discretion within broad statutory ranges, a system that predictably generated wide variation in sentence length—both across defendants with similar offense profiles and across judicial circuits—and that research consistently linked to racial disparities in outcomes (Rushin et al. 2019).

A large economics literature documents that unstructured judicial discretion generates racial disparities in sentencing. Mustard (2001) finds that approximately 55 percent of the Black-white sentencing gap in federal courts is generated by departures from guidelines rather than within-guideline variation, and Yang (2014) shows that when the Supreme Court rendered federal guidelines advisory in *United States v. Booker*, racial disparities in sentencing increased. Alabama's move toward structured sentencing can be understood as an attempt to constrain the same sources of disparity.

The Alabama Sentencing Commission was established by the Alabama Sentencing Reform Act of 2003 (Act 2003-354) as an agency of the state Supreme Court, charged with developing evidence-based sentencing standards designed to reduce unwarranted disparity while controlling prison population growth (*Alabama Sentencing Reform Act of 2003* 2003). In response, the Commission adopted the voluntary sentencing standards effective near the end of 2006 applying to many felony sentences handed down on or after that date. The voluntary standards were structured around a scoring-based worksheet that considered the seriousness of the offense and the defendant's criminal history, producing a recommended sentence range. Because the standards were voluntary, however, judges could depart from them without explanation. Using a difference-in-differences framework applied to 221,934 sentences handed down by 355 judges between 2002 and 2015, Rushin et al. (2019) found that the introduction of voluntary sentencing guidelines coincided with a statistically significant decrease in average sentence length of approximately seven months, as well as modest reductions in inter-judge and race-based sentencing disparities.

In 2013, Alabama updated the voluntary sentencing guidelines and created a set of presumptive guidelines that applied mostly to non-violent drug related offenses. Under a presumptive system, judges must impose a sentence within the guidelines range given the worksheet score unless they identify specific aggravating or mitigating factors sufficient to justify a departure—and any departure is subject to appellate review. The shift to presumptive guidelines proved significantly more effective than the voluntary standards at constraining judicial discretion, particularly for judges who had previously been the most punitive. Rushin et al. (2019) found that the transition to presumptive guidelines coincided with a drop in average sentence length of nearly two years, as well as reductions in race-based sentencing disparities of eight to twelve months—effects concentrated among judges who had been statistical outliers for punitiveness under the voluntary regime. It is important to be precise about the reach of the 2013 change: the presumptive guidelines bound sentencing only for a limited set of non-violent drug and property offenses (Rushin et al. 2019). Violent offenses carrying the longest sentences remained under the voluntary worksheet regime.

The third major reform in Alabama's recent history was the creation of the Class D felony classification through the Alabama Prison Reform Bill (Act 2015-185), passed in 2015 (*Alabama Prison Reform Bill* 2015). Prior to this reform, Alabama's felony code contained only three classes: A, B, and C. The new Class D category was designed to capture low-level, non-violent drug and property offenses—crimes considered too serious for misdemeanor treatment but not sufficiently grave to justify the sentence ranges applicable to Class C felonies. Key to the reform's logic was a presumption against imprisonment for first-time Class D offenders: rather than defaulting to incarceration, the sentencing guidelines for Class D convictions presumed non-prison sanctions—probation, community corrections, split sentencing, or treatment programs—in the absence of aggravating circumstances. By diverting low-level offenders from state prison to community supervision, the reform aimed simultaneously to reduce Alabama's chronically overcrowded prison population and to break the recidivism-reinforcing cycle that accompanies incarceration for minor offenses.

Together, these three reforms—the 2007 voluntary guidelines, the 2013 transition to presumptive guidelines for non-violent offenses, and the 2015 Class D felony classification—constitute the most significant restructuring of Alabama's felony sentencing system in the state's history. Prior empirical work by Rushin et al. (2019) demonstrated that the sentencing guidelines changes

produced meaningful reductions in both sentence length and racial sentencing disparities. However, the downstream effects of these reforms on actual incarceration outcomes—and in particular on the racial composition of Alabama’s prison population—have not been systematically examined.

The theoretical case for why Alabama’s reforms should differentially affect Black defendants operates through two channels. The first is compositional: Class D reclassification targets low-level drug possession and minor property offenses, the precise offense categories that decades of racially disparate enforcement have concentrated disproportionately among Black defendants. A race-neutral reform targeting these offense classes will therefore have racially asymmetric effects simply because of the racial structure of the underlying offense distribution. The second channel runs through judicial discretion. Rushin et al. (2019) demonstrate that Alabama’s transition to presumptive guidelines reduced race-based sentencing disparities by eight to twelve months, consistent with Mustard (2001) federal evidence that unstructured departure decisions are the primary site at which racial gaps are generated. If constraining discretion at sentencing narrows the racial gap in sentence length, it should follow that it narrows the racial gap in the flow into prison as well. The two channels differ in kind: Class D changes the presumptive disposition for covered low-level offenses, whereas the guidelines constrain how judges sentence within categories that remain prison-eligible.

A null result, however, would be equally informative. A well-documented phenomenon in the sentencing literature is that constraining discretion at one margin displaces it to another—prosecutors adjust charging behavior, judges manipulate offense level findings, and plea bargaining absorbs residual discretion that formal guidelines cannot reach. If Alabama’s decision makers found workarounds, a null effect on the racial composition of admissions would imply not that the reforms were inconsequential but that the system successfully routed around them—a finding that speaks directly to whether formal sentencing reform can achieve racial equity goals without complementary changes in prosecutorial and judicial culture.

3. Data and Models

The first data set we use to establish changes in the overall incarcerated population comes from the Alabama Department of Corrections’ (ADOC) monthly statistical reports. These reports are drawn directly from individual facilities and contain monthly counts, facility type, and reported assaults on inmates and employees. We restrict the aggregate series to 2008–2019, allowing us to focus on the 2015 reforms while avoiding the COVID-19 period. A separate, unbalanced cleaned facility-tier extract spans January 2010 through December 2020 and contains 5,533 observations across 99 reporting units and 131 distinct observed months.⁵

⁵ The aggregate ADOC trend is reported through 2019 to avoid the pandemic. The separate cleaned facility-tier extract is unbalanced: it spans January 2010 through December 2020 and contains 5,533 observations across 99 reporting units corresponding to 70 distinct facility names, with 131 distinct calendar months represented overall; July 2019 is unavailable in the source reports. The monthly reports list facility tiers rather than whole institutions: a single physical institution can appear as separate general-population, work-release, and annex units. Thus, 99 units times 131 months is not the implied observation count, and the reporting convention explains why the unit count exceeds the number of distinct named institutions.

Figure 1 plots the raw trends in prison population as reported by the ADOC. From Figure 1, we observe a fairly steady prison population between 2008 and 2013 with signs of a decreasing population beginning in earnest to emerge around the passage of the presumptive sentencing guidelines in 2013 (marked by the dashed line) and continue to drop through the 2015 designation of Class D felonies (marked by the dotted line). From its height in 2014, to 2019 the total custodial prison population dropped 13.5% from 32,684 to 28,266.

While the ADOC database provides a granular look at prison populations generally, it does notably omit race level population and admittance data. For this information, and to allow for interstate comparison, we turn to the Vera Institute of Justice’s Incarceration Trends database (Vera Institute of Justice 2023), which collates the Bureau of Justice Statistics’ National Prisoner Statistics series into a single state-year panel. The disadvantage of the Vera dataset is that it reports data only at the state by year level. The advantage, however, is that it does include prison population counts by race and gender, as well as prison admission rates by race and gender for the panel of time in which we are interested. The primary measure of prison population in the Vera dataset which corresponds to the BJS jurisdiction count includes those held in local jails on contract, private facilities, and facilities in other jurisdictions. While this does differ from the ADOC data, in Alabama, the two measures mirror each other closely.⁶

The Vera dataset which contains a panel of state by year prison data allows us to perform a two-way fixed effects (TWFE) implementation of a difference in differences model with the treated state being Alabama and the treated year triggering on 2015. To provide a basis of comparison—specifically for the second difference—we gathered information on other states that adopted JRI-style prison reform through the Pew Charitable Trusts’ JRI state tracker (The Pew Charitable Trusts 2018) and the Council of State Governments Justice Center documentation (Council of State Governments Justice Center 2021). What resulted is a pool of nine states that serve as never treated during our time window. Those states are: Florida, New Mexico, Wyoming, Arizona, Maine, Iowa, Minnesota, Washington, and Wisconsin.

4. Empirical Model

The variation we exploit to explore the effect of Alabama’s criminal justice reform comes from the timing of the passage of the laws. As mentioned previously, there were a number of relevant reforms that the state legislature passed around the time frame of our dataset. We begin our dataset in 2008 to avoid the initial waves of reforms that included the passage of the voluntary sentencing worksheet. In terms of policy changes, what remains is the presumptive structured

⁶ While the BJS National Corrections Reporting Program would add admission-level offense codes and time-served distributions, allowing an offense-category mechanism analysis beyond the present state-year design, we do not use it in this analysis for several reasons. First, NCRP maps each state’s statutory offense codes into standardized BJS offense categories, whereas Class D eligibility is defined by Alabama statute through offense-and-degree combinations and offender history. Trying to crosswalk standardized BJS codes to Alabama specific Class D-eligible admissions would get messy and require a more detailed level of manual crime offense recording than we feel comfortable doing. Additionally, state participation and record quality in NCRP vary over time, and admission-type counts in NCRP are known to diverge from the National Prisoner Statistics counts underlying our panel in some states—a nontrivial concern for a design with a single treated state.

sentencing change in 2013 and the 2015 approved reform package that most noticeably included the creation of Class D felonies as mentioned previously. To attempt to measure the effect of these legal changes, we employ a two-way fixed effects framework to measure the effect by year akin to an event study. Formally, the equation looks like this:

$$Y_{st} = \alpha_s + \lambda_t + \sum_{k \neq -1} \beta_k \mathbf{1}[t - 2015 = k] \cdot AL_s + \varepsilon_{st},$$

where Y_{st} represents a variety of state-year measures of prison populations and admissions discussed next, α_s represents state fixed effects, λ_t year fixed effects, and the event-time dummies are indexed relative to the 2015 adoption year. We omit the coefficient at $k = -1$ so that 2014 is the reference period. Standard errors are clustered at the state level. With Alabama as one treated state and nine controls, we supplement the conventional clustered inference with wild-cluster-bootstrap and placebo-in-space diagnostics for corresponding specifications in the appendix (Cameron et al. 2008). The 2008–2013 coefficients describe pre-2015 differences relative to 2014, and the 2015–2019 coefficients describe the post-2015 period. Because the two policies took effect in close proximity, the latter capture the combined policy environment; the appendix reports a descriptive two-anchor timing comparison.

We model the above equation using several outcomes to attempt to tease out any effect on the racial composition of both the stock and flow of the prison population in Alabama. The first outcome we estimate is the log of the proportion of the Alabama prison population that is black. Using this fraction allows us to capture some potential hidden factors that might be driving incarceration. The underlying fraction is bounded between zero, which would imply a prison population with no black offenders, and one, which would imply a prison population containing only black offenders; the estimated outcome is its logarithm. This outcome is interesting as it gets at the stock of the populations in prisons, but because sentences are typically multi-year, we would anticipate the stock to adjust slowly relative to our second measure, the proportion of new prison admissions that are black. Whereas the fraction of the prison population that is black measures the stock of the prison population, the fraction of new admissions that are black measure the incoming flows into prisons. Both outcomes are compositional shares—the Black share of the standing population and of new admissions—rather than group-specific incarceration rates; the Black share can move even when the Black incarceration rate per capita does not, and we frame the results accordingly. We prefer this metric of compositional shares as it allows us to control for unobserved factors that might be driving incarceration generally through the denominator.

Finally, the difference between the flow measure and the stock measure exploits the fact that the stock reflects the accumulated racial composition of incarceration decisions made over many years under prior criminal justice regimes while the flow reflects current criminal justice laws. Because we would anticipate the stock to adjust slowly as long sentences are carried out, a meaningful reform that reduces racial disparity in new incarceration decisions will manifest first and most sharply in the flow measure, creating a widening gap between the two. Using the difference between the two as the outcome therefore gives the analysis greater sensitivity to reform effects in the short-to-medium run than either measure alone would provide, since it isolates the component of racial disparity that is actively being shaped by current policy from the component that is an artifact of past policies.

In terms of interpretation, a negative treatment effect where the difference becomes more negative post-reform indicates that black offenders are becoming a smaller share of new admissions relative to their share of the existing population consistent with the idea that these policy changes disproportionately divert black offenders away from prison at a higher rate than white offenders. In contrast, a null or positive result would suggest that whatever racial disparity existed in the stock of the prison population remained or was made worse by new admission flows into prison.

5. Results

As mentioned previously, Figure 1 displays the total custodial prison population as measured by the ADOC which shows a consistent increase in the prison population until late 2013 and decreases through 2018. Summary statistics for the treated and control groups appear in Table 1.

Table 2 and Figures 2 through 4 report coefficients for the main analysis on the effect of the 2015 criminal justice reform policies on the Black shares of Alabama's prison stock and admissions. Column (1) of Table 2 corresponds to

Figure 2 and reports the coefficients and standard errors relative to the outcome measured as the log fraction of the prison population that is black. Looking both visually at Figure 2 and at the reported numbers in Table 2, there is little evidence of concerning pre-trend behavior. Pre-2015 coefficients are small and statistically indistinguishable from zero consistent with the parallel trends assumption. Post-reform coefficients decline monotonically, from -0.016 in 2015 to -0.136 in 2019. Under conventional state-clustered inference, the 2017–2019 coefficients are significant at the 5 percent level or better.⁷ The five-year post-reform average is -0.071 , equivalent to approximately a 6.9% reduction in the share of the prison population that is black relative to the control states. At a pre-reform fraction of 58% black, this average corresponds to a reduction of about four percentage points in the Black share of the prison stock over 2015–2019.

Column 2 of Table 2 and Figure 3 report coefficients for the logged Black fraction of admissions. Pre-2015 coefficients are again insignificant though post-reform coefficients are substantially larger than for the previous measure of the population stock: -0.041 in 2015, -0.274 in 2016, -0.287 in 2017, -0.302 in 2018, and -0.186 in 2019. Under conventional state-clustered inference, the 2016–2019 coefficients are significant at conventional levels. The five-year post-reform average is -0.218 , or approximately a 19.6% reduction in the share of admissions that are black. This is roughly three times the magnitude of the stock-share effect and appears immediately at the January 2016 implementation date. The relative magnitudes between Figures 2 and 3 are consistent with the idea that any policy change that may affect incarceration will have a more immediate and strong impact on admissions while the overall prison population trends will respond slower and more gradually as longer prison sentences run their course. The flow coefficient attenuates in the final year, from -0.302 in 2018 to -0.186 in 2019; we return to this attenuation in Section 6, where we discuss contemporaneous changes in parole and probation enforcement as a possible contributor.

Column 3 of Table 2 and Figure 4 report coefficients for the admissions-minus-stock black-share gap. Pre-2015 coefficients are small and insignificant. Post-reform coefficients range from

⁷ The Appendix reports the complementary single-treated-state diagnostics for each regression result in Table 2.

−0.021 in 2015 to −0.232 in 2016, with a five-year average of −0.132. The admissions and stock shares therefore diverge by about 13 log points in the post-reform window: admissions become meaningfully less Black than the sitting prison population, confirming the sharp flow-margin response documented previously.

Because the 2013 presumptive guidelines and the 2015 Class D declassification arrive in close succession, we report a two-anchor timing exercise using the population and Black-share specifications. The incremental post-2015 coefficient is larger in magnitude than the 2013–2014 coefficient for each outcome. Full results appear in the Appendix.

6. Discussion

The four specifications suggest a cohesive pattern. Alabama’s total prison population falls by approximately 13% through our period of study. The black share of the stock falls by approximately 7%, the black share of admissions falls by approximately 20%, and the composition gap between the two shifts by approximately 13 log points in favor of a less-black flow.⁸ These patterns remain conditional on the parallel-trends assumption. The population series shows the clearest pre-period deviation, which motivates the appendix sensitivity analysis.

Both margins of the reform—incarceration levels and racial composition—move most sharply through admissions rather than through the standing stock. This timing is consistent with reform content that reclassifies drug and property offenses at sentencing: the Class D provisions at the heart of SB 67 apply at the point of admission, while the existing prison population turns over more slowly.

These results should be considered in the wider context of what happened downstream of sentencing. Alabama’s Bureau of Pardons and Paroles—institutionally separate from the judges and prosecutors that the sentencing reforms target—sharply tightened parole after 2017: the parole grant rate fell from roughly 53 percent in 2016 to 9.5 percent by 2019. Over the same window parole readmissions roughly doubled and probation revocations rose several-fold. The timing of these back-end changes is consistent with partial offsetting of the front-end admissions decline and offers one possible explanation for the 2019 attenuation in the flow coefficient. The Appendix reports the parole series and a robustness check that drops 2019 from the estimation window.

A second contemporaneous event that might be driving the results is the federal investigation of prison conditions. The DOJ findings arrived only in April 2019, near the end of the study window, and Alabama entered no consent decree or settlement during that window (U.S. Department of Justice, Civil Rights Division 2026). Excluding 2019 leaves the main results virtually unchanged, showing that the post-findings observation does not drive the estimate; the appendix reports the full check and case timeline.

All of this together seems to suggest that efforts made to reform criminal justice in Alabama have been at least effective in reducing the total prison population and the Black shares

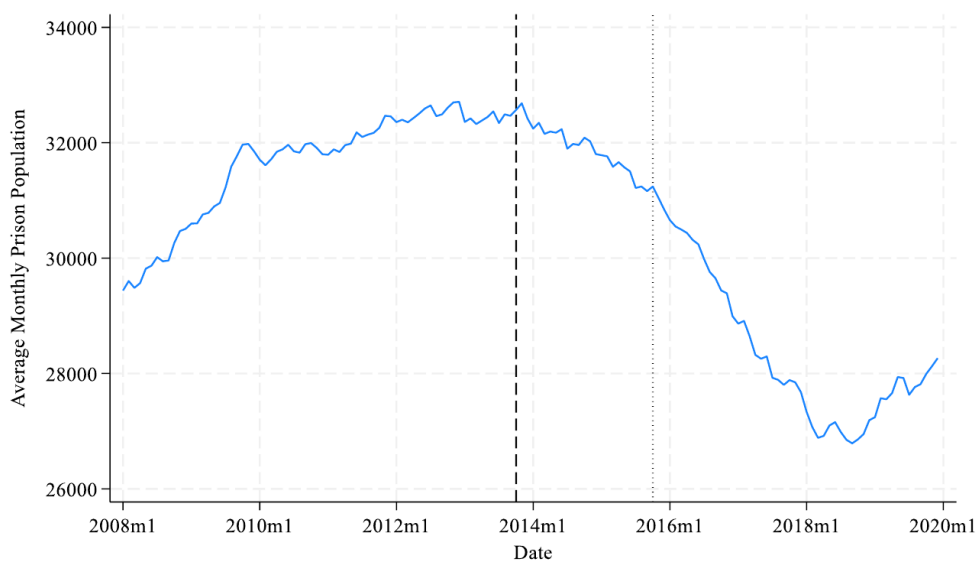
⁸ Crime is not included as a separate covariate in Equation (1). Common crime trends are absorbed by the year effects and comparison states. For an omitted crime trend to explain the Black-share result, it would have to be specific to Alabama, coincide with the reforms, be absent from the comparison states, and differentially change Black admissions relative to overall admissions.

of prison admissions and the standing prison population. We do, however, present these results with several caveats. First, the design has one treated state and a modest comparison pool. To address this, we include in the Appendix alternative specifications for each Figure where the baseline set of states is not just the never treated states but rather every available state. As is seen in the Appendix, the results do not change substantially with this alternative baseline group. Placing the magnitude in context, Alabama's five-year prison-population decline ranks fourth among ten justice-reinvestment peer states measured on comparable windows; some peer windows extend into the pandemic-release period (8).

It is important that we also emphasize that evidence presented here suggests an improvement in the system but does not imply that the system is not without flaws. The findings presented here suggest that Alabama's recent sentencing reforms have modestly reduced both prison overcrowding and the Black shares of prison admissions and the standing population, and we think it is worth acknowledging that these are meaningful policy achievements in a domain where progress is difficult and slow. That said, this conclusion should not be read as a suggestion that the work is done or that the system does not require continued attention and reform. What this research offers is some empirical evidence that the policies enacted are working and speaks well of Alabama's willingness to enact criminal justice reform. It's also important to note the potential counterfactuals here: had these reforms not been enacted, the evidence suggests that Alabama's prisons would be more crowded and their admissions and standing populations would have higher Black shares than they do today. What we find suggests that Alabama is willing and capable of enacting meaningful incarceration policy reforms. Several extensions follow naturally from this work. Because the presumptive guidelines apply to a limited set of nonviolent drug and property offenses, a longer panel could show how their effects accumulate as affected cohorts turn over; a design with multiple treated states—defining treatment broadly as prison reform aimed at mass incarceration—would sharpen inference beyond a single treated cluster, and adding crime as an outcome would let future work weigh reductions in incarceration against any public-safety cost.

7. Tables and Figures

Figure 1



Notes: ADOC Trend Data on Total Prison Population. Notes: Total custodial prison population as reported in the Alabama Department of Corrections monthly statistical reports, 2008–2019. The vertical dashed line marks the 2013 presumptive sentencing guidelines; the dotted line marks the 2015 designation of Class D felonies. Data: Alabama Department of Corrections.

Table 1: Summary Statistics, 2008–2019

Group	Period	Mean Prison Population	Mean Admissions
Alabama	2008–2014	31,857	12,052
Alabama	2015–2019	28,489	12,810
Control States	2008–2014	23,825	10,236
Control States	2015–2019	23,913	10,314

Notes: Control consists of Arizona, Florida, Iowa, Maine, Minnesota, New Mexico, Washington, Wisconsin, and Wyoming. Prison-population and admissions counts are Bureau of Justice Statistics jurisdiction values as compiled by the Vera Institute.

Table 2: Effect of Alabama's 2015 Justice Reinvestment Act on incarceration and its racial composition

	(1)	(2)	(3)
	Log Black Share (Stock)	Log Black Share (Flow)	Log Flow-Log Stock
2008	0.081 (0.057)	0.163 (0.09)	0.116 (0.082)
2009	0.085 (0.067)	0.047 (0.09)	0.002 (0.072)
2010	0.03 (0.028)	0.002 (0.059)	-0.011 (0.065)
2011	0.034 (0.039)	0.105 (0.153)	0.072 (0.124)
2012	0.014 (0.023)	0.063 (0.053)	0.049 (0.044)
2013	0.004 (0.015)	0.052 (0.065)	0.048 (0.051)
2014	—	—	—
2015	-0.016* (0.008)	-0.041 (0.044)	-0.21 (0.039)
2016	-0.028** (0.01)	-0.274** (0.085)	-0.232** (0.08)
2017	-0.063** (0.021)	-0.287*** (0.053)	-0.209*** (0.038)
2018	-0.112*** (0.027)	-0.302*** (0.063)	-0.171*** (0.044)
2019	-0.136** (0.039)	-0.186* (0.081)	-0.028 (0.09)
State fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Observations	120	120	120
States (clusters)	10	10	10

Notes: Each column is a separate regression estimating Equation (1); all three outcomes are in logs. The reference year is 2014. Standard errors clustered at the state level are in parentheses. Significance stars use the small-sample t distribution: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$. The appendix reports complementary wild-cluster-bootstrap and placebo-in-space diagnostics for corresponding specifications. Data: Vera Institute Incarceration Trends.

Figure 2: Event Study of the Proportion Black of the Prison Population (Stock)



Notes: The dependent variable is the log of the fraction of the prison population that is black. Each dot represents an estimated coefficient and red bands represent 90 percent confidence intervals. 2014 is left out as the basis year of comparison. The vertical dashed line represents the treatment enactment year where the treated state is Alabama and the control states are the nine states that were never treated in the time window. Data: Vera Institute Incarceration Trends.

Figure 3: Event Study of the Proportion Black of Prison Admissions (Flow)



Notes: The dependent variable is the log of the fraction of prison admissions that are black offenders. Each dot represents an estimated coefficient, and red bands represent 90 percent confidence intervals. 2014 is left out as the basis year of comparison. The vertical dashed line represents the treatment enactment year where the treated state is Alabama and the control states are the nine states that were never treated in the time window. Data: Vera Institute Incarceration Trends.

Figure 4: Event Study of the Difference Between the Logged Admissions and Population Fractions



Notes: The dependent variable is the difference between the log of the fraction of prison admissions that are black offenders and the log of the fraction of the prison population that is black. Each dot represents an estimated coefficient and red bands represent 90 percent confidence intervals. 2014 is left out as the basis year of comparison. The vertical dashed line represents the treatment enactment year where the treated state is Alabama and the control states are the nine states that were never treated in the time window. Data: Vera Institute Incarceration Trends.

8. Appendix

8.1 Alternative Baseline: All Available States

As a robustness check on the choice of comparison group, we re-estimate each of the three main specifications replacing the nine never-treated control states with all available states in the Vera panel. The results do not change substantially and are presented in Figures A1 through A3.

Figure A1: Event Study of the Proportion Black of the Prison Population with All Available States



Notes: This figure replicates Figure 2 in every way except that in place of the never-treated control states, it includes all available states. Data: Vera Institute Incarceration Trends.

Figure A2: Event Study of the Proportion Black of Prison Admissions with All Available States



Notes: This figure replicates 3 in every way except that in place of the never-treated control states, it includes all available states. Data: Vera Institute Incarceration Trends.

Figure A3: Event Study of the Difference Between the Logged Admissions and Population Fractions with All Available States



Notes: This figure replicates 4 in every way except that in place of the never-treated control states, it includes all available states. Data: Vera Institute Incarceration Trends.

8.2 Alternative estimators

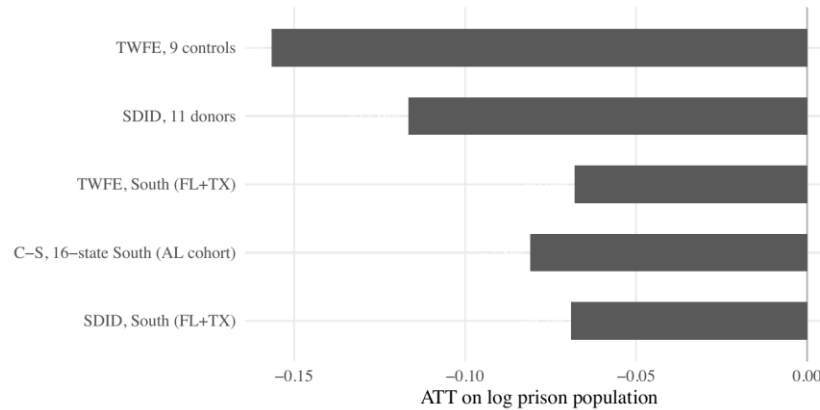
The main results are robust to several alternative estimate techniques summarized in Table A1 and Figure A4. Synthetic difference-in-differences (Arkhangelsky et al. 2021) on an extended donor pool returns -0.117 for the log population outcome—about an 11 percent decline—close to the two-way fixed-effects estimate and reweighted toward donors that match Alabama’s pre-period. Leave-one-out estimates in Figure A5 show that no single control drives the result: the population effect stays within $[-0.167, -0.148]$ as each control is dropped in turn, and Florida, the largest donor, does not dominate. Restricting the comparison to the never-treated Census South (Florida and Texas) is a more demanding counterfactual, since several Southern states were themselves decarcerating over this window. Against that two-state pool the population effect is roughly -6 to -7 percent (TWFE -0.068 ; SDID -0.069), and the admissions-ratio effect is -0.084 . Using the broader not-yet-treated Southern comparison, the Callaway–Sant’Anna estimates are -0.081 and -0.107 , respectively. Alabama’s decline is smaller against these stricter benchmarks but remains negative on every margin.

Table A1: Effect across donor pools and estimators

	Log prison population	Log B/W admit rate ratio	Log B/W stock rate ratio
TWFE, main pools	-0.156	-0.189	-0.081
SDID, extended donor pool	-0.117	—	—
TWFE, Southern pool (FL+TX)	-0.068	-0.084	-0.089
Callaway–Sant’Anna, South (AL cohort)	-0.081	-0.107	-0.078
SDID, Southern pool (FL+TX)	-0.069	—	—
Leave-one-out range	$[-0.167, -0.148]$	$[-0.222, -0.143]$	$[-0.103, -0.053]$

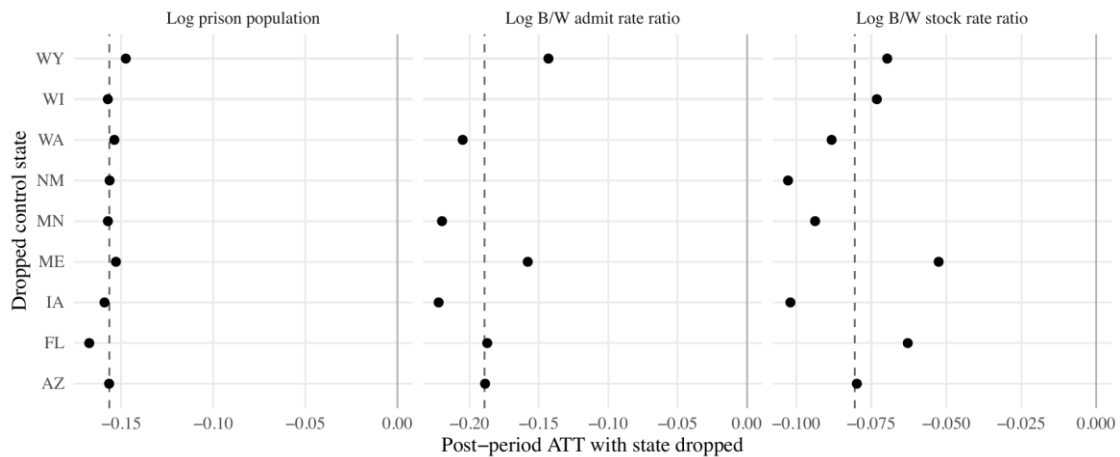
Notes: Post-period (2015–2019) ATT estimates in logs. The main public-data population and stock pools contain Alabama and nine controls (120 state-year observations); the admissions pool contains Alabama and seven controls (92 observations). TWFE rows are two-way fixed-effects estimates; SDID is synthetic difference-in-differences (Arkhangelsky et al. 2021) with placebo-variance inference; the Callaway–Sant’Anna row (Callaway and Sant’Anna 2021) reports Alabama’s 2015-cohort group effect against not-yet-treated Southern states. SDID is estimated for the population outcome only. The leave-one-out row gives the range of ATTs across omission of each control state in turn. Columns labeled “Log B/W ...rate ratio” report the Black-to-White rate ratio, a distinct object from the main text’s log Black share. Data: Vera Institute Incarceration Trends.

Figure A4: Population-margin Effect Across Designs



Notes: Post-period ATT on log total prison population under each estimator and donor-pool combination in 3. Data: Vera Institute Incarceration Trends.

Figure A5: Leave-one-out Estimates



Notes: Post-period effects re-estimated dropping each control state in turn; the vertical dashed line is the full-pool baseline. Data: Vera Institute Incarceration Trends.

8.3 Placebos and wild cluster bootstrap inference

Figure A6 plots the placebo-in-space distributions that reassign the 2015 treatment to each available state in turn. Alabama ranks 1 of 10 for log population ($p = .10$), 1 of 8 for the admissions rate ratio ($p = .125$), and 4 of 10 for the stock rate ratio ($p = .40$). The denominators count all available state assignments, including Alabama. Full-enumeration Rademacher wild-cluster-bootstrap p -values are .157, .375, and .439, respectively. The two procedures provide complementary inference checks for the single-treated-state design.

Because the headline racial-composition outcomes are shares, Table A2 repeats the same diagnostics for the data log-share counterparts. The reconstructed effects remain negative; Alabama ranks 2 of 10 for the stock share and 2 of 8 for both the admissions share and the

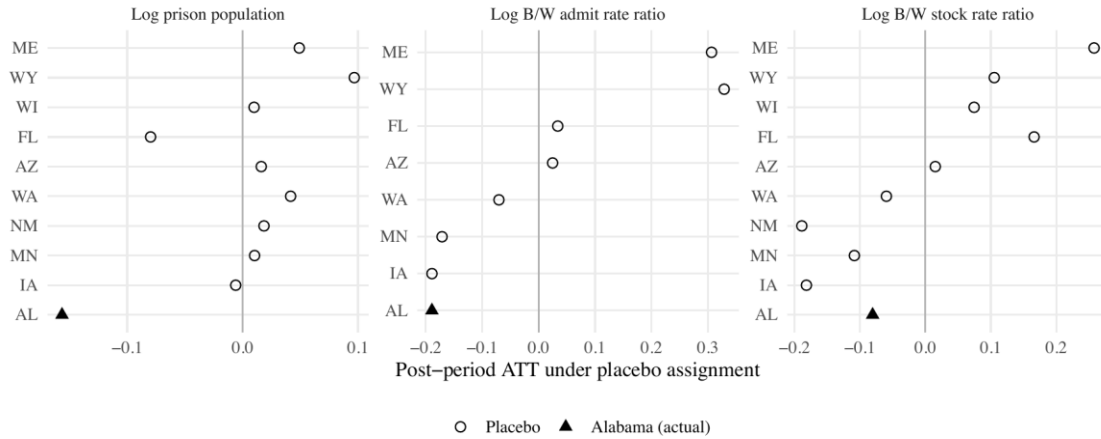
admissions-minus-stock gap. The finite placebo distributions are necessarily coarse, and the full numerical results are reported in the table.

Table A2: Inference checks for logged-share outcomes

	ATT	RI rank [p]	Rademacher WCB p
Log Black stock share	-0.106 (0.044)	2/10 [0.200]	0.437
Log Black admissions share	-0.283 (0.105)	2/8 [0.250]	0.422
Log admissions–stock share gap	-0.171 (0.072)	2/8 [0.250]	0.469

Notes: Post-period two-way fixed-effects ATTs, 2008–2019, with state-clustered standard errors in parentheses. The stock-share outcome uses the ten-state main pool; the admissions-share and gap outcomes use the eight-state flow pool. RI is the one-sided placebo-in-space rank test; its denominator includes Alabama. WCB uses full enumeration of Rademacher weights. These outcomes use the current public Vera vintage and provide complementary diagnostics; they do not revise the submitted estimates in 2. Data: Vera Institute Incarceration Trends.

Figure A6: Placebo-in-space distributions



Notes: Post-period ATT when treatment is assigned to each state in turn (triangle: Alabama's actual estimate; circles: placebos). Alabama is rank 1 of 10 for the population outcome, rank 1 of 8 for the admissions ratio, and rank 4 of 10 for the stock ratio. Data: Vera Institute Incarceration Trends.

8.4 Sensitivity to parallel-trends violations

The log-population outcome displays the most pronounced pre-period deviation, so we report Rambachan and Roth (2023) relative-magnitude bounds for its five-year average effect. The robust confidence set excludes zero for post-period violations as large as roughly the full maximal pre-period violation: it is $[-0.206, -0.049]$ when post-period violations are bounded by half the maximal pre-period violation ($\bar{M} = 0.5$) and first admits zero near $\bar{M} = 1$. The

population result therefore withstands parallel-trends violations up to about the magnitude actually observed before the reform.

8.5 Timing of the 2013 and 2015 reforms

Table A4 attempts to tease out the separate effects across both reform dates. Within each outcome panel, every column is a separate two-way fixed-effects regression: column (1) includes only an Alabama interaction that switches on in 2015, column (2) only one that switches on in 2013, and column (3) includes both, so that its 2013 coefficient captures the 2013–14 level shift and its 2015 coefficient the incremental change after the Class D reform. Estimated alone, either anchor absorbs the full post-reform decline, which is why columns (1) and (2) are individually large; the joint column separates them. There, the incremental post-2015 coefficients are -0.145 for log population, -0.250 for the log Black share of admissions, and -0.073 for the log Black share of the prison population, against 2013–14 coefficients of -0.016, -0.046, and -0.047. The larger movement on every margin therefore follows the Class D reform. The racial outcomes are the same log Black-share measures used elsewhere in this Appendix, matching the manuscript's headline estimands.

Table A4: 2013 versus 2015: two-anchor timing comparison

	(1) 2015 anchor	(2) 2013 anchor	(3) Both anchors
<i>Panel A: Log prison population</i>			
Alabama × Post-2013	—	-0.120*** (0.016)	-0.016 (0.014)
Alabama × Post-2015	-0.156*** (0.015)	—	-0.145*** (0.012)
Observations	120	120	120
<i>Panel B: Log Black share of admissions</i>			
Alabama × Post-2013	—	-0.224* (0.103)	-0.046 (0.061)
Alabama × Post-2015	-0.283** (0.105)	—	-0.250*** (0.069)
Observations	92	92	92
<i>Panel C: Log Black share of prison population</i>			
Alabama × Post-2013	—	-0.099* (0.047)	-0.047 (0.034)
Alabama × Post-2015	-0.106** (0.044)	—	-0.073 (0.023)
Observations	120	120	120

Notes: Two-way fixed-effects estimates on the public Vera panel, 2008–2019, with state-clustered standard errors in parentheses. Within each panel, each column is a separate regression: column (1) includes only an Alabama indicator for 2015 onward, column (2) only an indicator for 2013 onward, and column (3) both, so the 2013 coefficient is the 2013–14 level shift and the

2015 coefficient the incremental post-2015 effect. Panels A and C use Alabama and nine controls; Panel B uses Alabama and seven controls with admissions data. These specifications use a different data vintage from the originally submitted Table 2. Data: Vera Institute Incarceration Trends.

8.6 Compositional-share specifications

Table A5 expresses the Black share of the stock and of admissions in levels, log-share, and log-odds form. All three have the same qualitative direction. The Black share of admissions falls by 13.1 percentage points in levels, 24.6 percent in log-share, and 42 percent in odds; the Black share of the standing population falls by 4.3 percentage points. The qualitative conclusion is invariant to the functional form of the outcome.

Table A5: Share outcomes: levels, log-share, and log-odds

	Levels	Log-share	Log-odds
Black share of prison stock	-4.3 pp	-0.106 (-10.1%)	-0.202 (-18% odds)
Black share of admissions	-13.1 pp	-0.283 (-24.6%)	-0.543 (-42% odds)

Notes: Post-period ATTs for the Black share of the prison stock (ten-state pool) and of admissions (eight-state pool), 2008–2019. Levels coefficients are expressed in percentage points; the log-share and log-odds columns report the log coefficient with the implied percent change in parentheses. Estimates here are reconstructed from public state-year data (a different data vintage from the originally submitted 2); the admissions log-share (−0.283) demonstrates qualitative invariance across functional forms and is not the headline 2 coefficient (−0.218). Data: Vera Institute Incarceration Trends.

8.7 The DOJ CRIPA investigation and a drop-2019 check

Figure A7 overlays the DOJ CRIPA milestones on Alabama’s prison population series. The DOJ case chronology (U.S. Department of Justice, Civil Rights Division 2026) dates the findings report to April 2019, near the end of the analysis window, and lists no consent decree or settlement during that window. More directly, Table A6 shows that re-estimating through 2018 changes the ATT only from −0.156 to −0.159. This check shows that the post-findings observation does not drive the estimated decline. Federal scrutiny also bears most directly on population levels rather than the racial composition of new admissions.

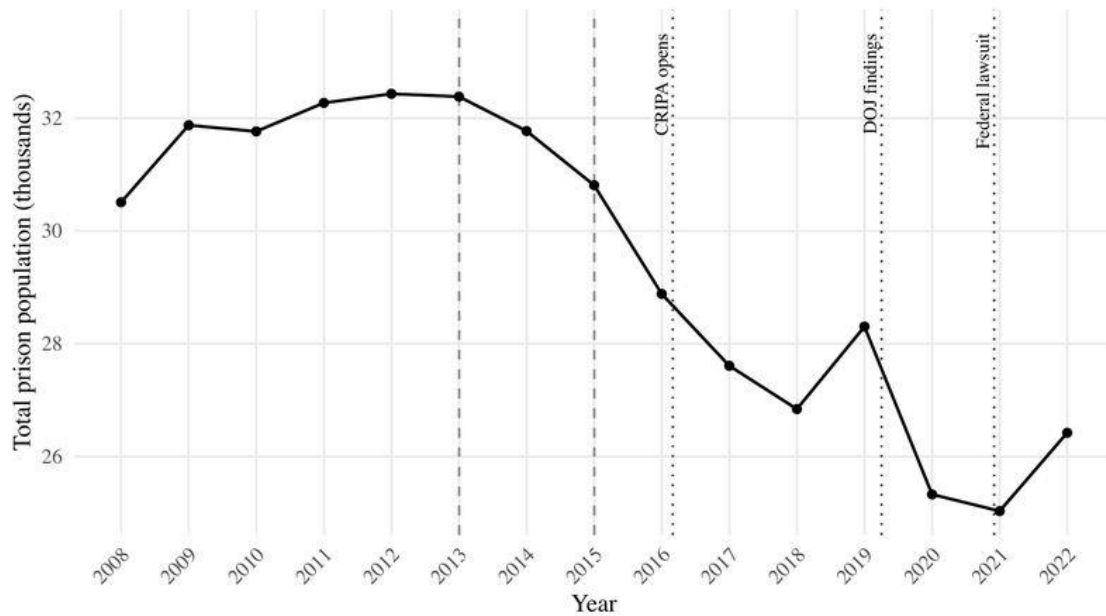
Table A6: Sensitivity to excluding 2019

Outcome	2008–2019 ATT	Excluding 2019	Change
Log prison population	-0.156 (0.015)	-0.159 (0.015)	-0.002
Log B/W admissions-rate ratio	-0.189 (0.072)	-0.180 (0.067)	0.009
Log B/W stock-rate ratio	-0.081 (0.049)	-0.065 (0.045)	0.015
Log Black admissions share	-0.283 (0.105)	-0.291 (0.107)	-0.009

Outcome	2008–2019 ATT	Excluding 2019	Change
Log Black stock share	-0.106 (0.044)	-0.090 (0.039)	0.016
Log admissions/stock share ratio	-0.171 (0.072)	-0.195 (0.079)	-0.024

Notes: Post-period two-way fixed-effects ATTs and state-clustered standard errors. The first column uses 2008–2019; the second excludes 2019; “Change” is the latter minus the former, computed before the displayed values are rounded. Population and stock outcomes use the ten-state main pool, while admissions and admissions-minus-stock outcomes use the eight-state flow pool. These public Vera specifications use a different data vintage from the originally submitted 2. Data: Vera Institute Incarceration Trends.

Figure A7: Alabama prison population and DOJ CRIPA milestones

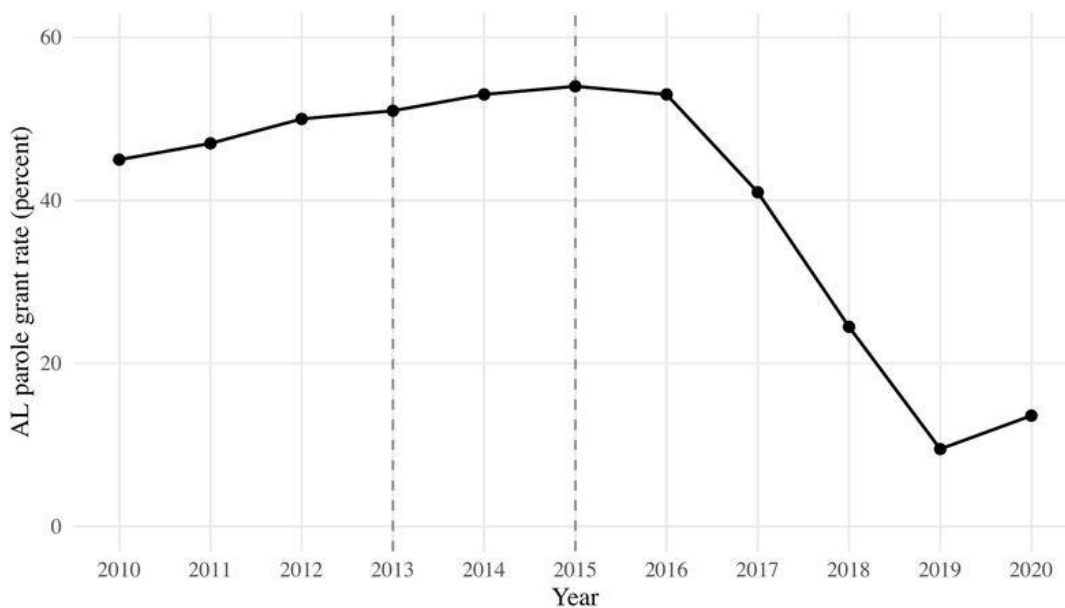


Notes: Annual total prison population from Vera’s BJS/NPS state panel, with the 2015 reform (dashed) and DOJ milestones (dotted): investigation opening (2016), findings report (April 2019), and federal lawsuit (December 2020). Data: Vera Institute Incarceration Trends; milestone dates from DOJ public filings.

8.8 Parole and the composition of admissions

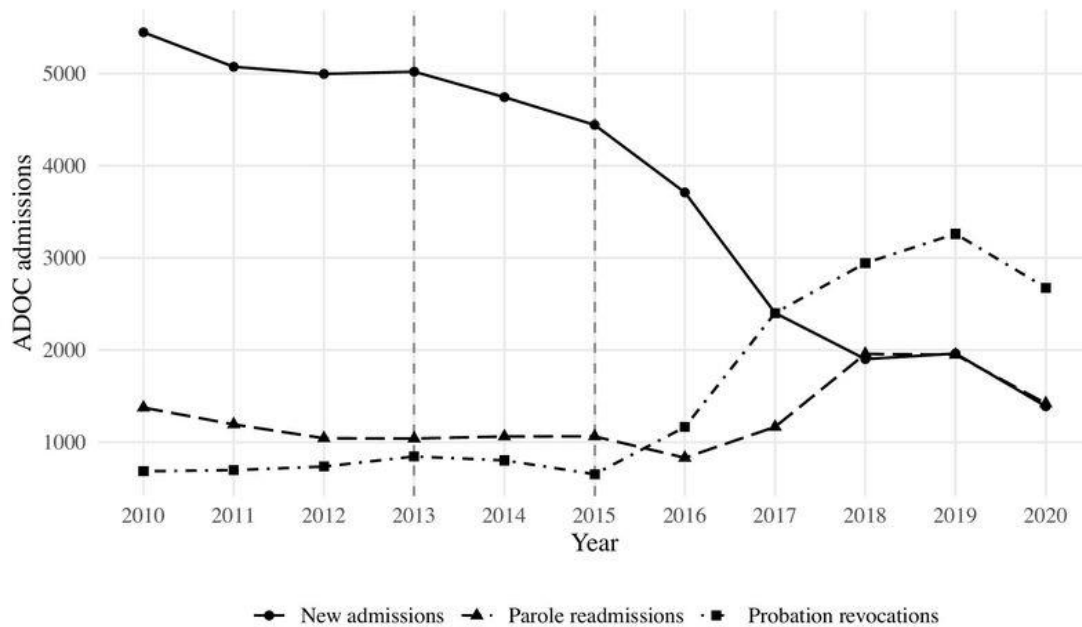
As mentioned in the Discussion, our main finding, that the share of admissions and the prison population that are Black decline following the criminal justice reforms of 2015, need be framed in the context of the larger criminal justice ecosystem. Figure A8 provides a little more context to some of those changes. As mentioned previously, Alabama's parole grant rate fell from roughly 53 percent in 2016 to 9.5 percent by 2019. Additionally, Figure A9 decomposes ADOC admissions by entry type: new court commitments—the margin the reform directly targets—fell sharply, while parole readmissions roughly doubled and probation revocations rose several-fold over 2018–2019. The Vera data we use here would classify parole readmissions as part of the total prison admissions but separate from new court commitments. The inclusion of parole readmissions in the data thus causes us to underestimate the effect of the Class D felony reform from a pure policy effect standpoint. Of course, as a general equilibrium, some might argue it matters less how offenders end up in prison and more that they end up in prison. Considered all together, the changing dynamics in other areas of criminal justice—particularly parole proceedings—serve to mute the overall effect of the criminal justice reform done in Alabama.

Figure A8: Alabama parole grant rate, 2010–2020



Notes: Share of parole hearings resulting in a grant, from Alabama Bureau of Pardons and Paroles annual reports. The dashed line marks the 2015 reform; the collapse begins in 2018. Data: AL Bureau of Pardons and Paroles.

Figure A9: ADOC admissions by entry type

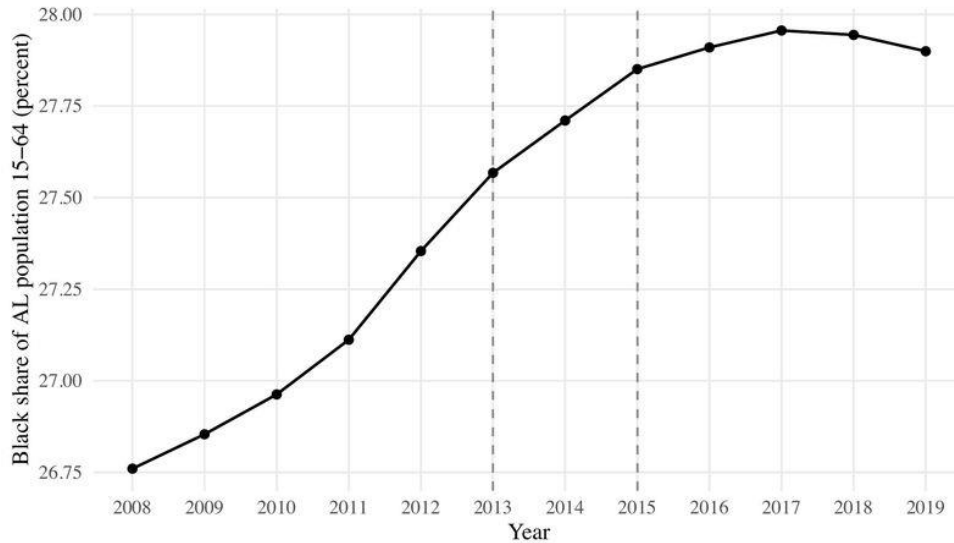


Notes: Annualized admissions from ADOC monthly statistical reports, by entry type. The 2017 totals are truncated in the source reports and should be read with caution. The dashed line marks the 2015 reform. Data: Alabama Department of Corrections.

8.9 Demographic shifts

The intent of the constructed ratios used for the outcomes in our main analysis is to control for unobserved factors that might be driving prison populations generally. One aspect that deserves further discussion is the possibility of changing demographics. For instance, if for reasons that are not well understood to us, the Black population in the state in correspondence to the reforms, what we measure as the effect of the reform might just be an unrelated drop in the pool of potential Black offenders. Figure A10 shows that Alabama's Black share of the population moved only trivially over the sample window and the movement that occurred was a slight *increase* in the Black share of the population while we estimate the reforms lead to a *decrease*. This makes us feel pretty comfortable that declining demographics are not driving the results.

Figure A10: Black share of Alabama's population aged 15–64



Notes: Derived from Vera group population counts (ACS-based). The dashed line marks the 2015 reform. Data: Vera Institute Incarceration Trends.

8.10 Benchmarking against peer JRI reforms

To put Alabama’s reforms and their effect in greater context, we present ten justice-reinvestment peer states in Table A7. Alabama’s five-year prison-population decline ranks fourth. The three larger declines belong to later reformers whose five-year windows extend further into the pandemic-release period, while two peers saw their populations rise after reform.

Table A7: Five-year prison population change after JRI reforms

State	Reform year	Pre-reform pop.	Post-5-year pop.	% change
MD	2016	20,764	15,134	-27.1%
LA	2017	35,682	27,296	-23.5%
TN	2016	28,172	21,995	-21.9%
AL	2015	31,771	25,328	-20.3%
SC	2010	24,288	20,929	-13.8%
MS	2014	21,969	19,417	-11.6%
NC	2011	40,382	35,697	-11.6%
GA	2012	55,944	53,667	-4.1%
OK	2012	25,977	28,143	+8.3%
KY	2011	20,544	23,022	+12.1%

Notes: Percent change in total prison population from the year before each state’s justice reinvestment reform to five years after it, from the Vera panel. Post-windows extending past 2019 include pandemic-era releases. Data: Vera Institute Incarceration Trends.

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