Drug-Related Arrest Rates and College Enrollment: Examining the Impact of Two Federal Drug Control Acts

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Abstract

I examine the impact of two federal drug acts on the drug-related arrest rate and the probability of college enrollment among Black males aged 18-24 by using a variety of counterfactual groups. The Anti-Drug Abuse Act of 1986 transformed the formerly rehabilitation-focused justice system into a punitive one, imposing sentencing minimums and increasing racial disparities. The Fair Sentencing Act of 2010 was intended to reverse some of these policies. I construct estimates of the impact of these two acts by comparing high-intensity to low-intensity drug-arrest states. I also use White males and Black females as two additional counterfactual groups. Although my estimates are quite noisy, I find that the Anti-Drug Abuse Act had a small, positive effect on arrest rates and resulted in an approximate 2% decrease in the probability of college enrollment. The second-stage estimates imply that an increase in drug-related arrest rates has a small, negative impact on the probability of college enrollment for Black males. I find some evidence of differential pre-treatment trends in outcomes which makes the Fair Sentencing Act particularly difficult to analyze.

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1 Introduction

There's 2.2 million people in the American prison system, and half a million of those are locked up for drug offenses. A lot of them were in the same boat as me: victims of the mandatory minimum. Passed by Congress in 1986, it's the reason hundreds of thousands of nonviolent people, mostly black and brown people, are rotting in prison... I'd been in prison for ten years by the time my petition reached the Supreme Court.

(HONY Interview, 2022)

College enrollment rates provide insights into several aspects of the educational landscape, such as access to higher education, socioeconomic factors, and workforce development. In the decades following 1970, the rate of college enrollment growth for black males began to fall behind all other groups, including white females and black females. Simultaneously, incarceration rates for black males began to climb during this period, as demonstrated in figure 1. Many blame the Anti-Drug Abuse Act of 1986 for the increase in minority incarcerations. In 2010, the Fair Sentencing Act was passed, which aimed to undo many of the racial disparities potentially caused by the Anti-Drug Abuse Act. In this paper, I investigate the link between drug enforcement policies and educational outcomes, as I examine the impact of the Anti-Drug Abuse Act of 1986 and Fair Sentencing Act of 2010 on both drug-related arrest rates and college enrollment among Black males.

This paper has two main objectives. First, I investigate the impact of the Anti-Drug Abuse Act of 1986 and the Fair Sentencing Act of 2010 on drug-related arrests. The second objective is to evaluate the impact of the two acts on college enrollment rates for Black males. Additionally, I examine the relationship between drug-related arrest rates and college enrollment.

I use three separate research designs. The first and primary research design combats the endogeneity problem by leveraging variations in the intensity of drug arrests across different states. High-intensity states are defined as states with Black adult drug-related arrest rates above the 75th percentile two years before the passage of the federal drug act (e.g. for the Anti-Drug Abuse Act, 1984 was used). When examining Figure 1a, it seems like the rise in arrest rates in high-intensity states started increasing at a faster pace. I compare high-intensity states to low-intensity states, and I calculate both the first-stage and reduced-form estimates where the first-stage is the impact of the act on Black adult drug-related arrest rates and the reduced-form is the impact of the act on college enrollment. For additional supplemental analysis, I adapt the second and third research designs in Britton (2022) to estimate the impact of the federal drug acts on college enrollment at the individual level.

I use repeated cross-sectional data from the Current Population Survey (CPS) October education supplement for data at the individual level, and I limit my analysis to persons aged 18-24 at the time of the passage of the act. For data on arrests, I use data from the Uniform Crime Reporting (UCR) Program, which contains information on the number of arrests by race, crime type, and juvenile crimes. To construct college enrollment rates from the CPS, I count any individual who is currently in their first year or has more than one year of college education [1].

This data has several limitations. First, the CPS excludes currently incarcerated populations. Second, the UCR has well-documented quality issues, such as many counties failing to report in certain years. In certain years, entire states failed to report any arrest data. Overall, if the effect size is small, the data might be insufficiently powered.

I find evidence that the Anti-Drug Abuse Act of 1986 had a modest impact on Black adult male arrest rates and a small negative impact on college enrollment. In addition, I find some evidence that an increase in Black adult drug-related arrest rates has a small negative impact on college enrollment. The Fair Sentencing Act of 2010 is harder to study due to substantial pre-trends [2], but I find some evidence of a potentially positive impact on college enrollment for Black males. For my estimates to be considered causal, important pre-trend and exclusion restrictions must be satisfied.

This paper heavily builds upon recent work by Britton (2022), who estimates the impact of the Anti-Drug Abuse Act on college enrollment and the impact of state-level legislation on college enrollment. She finds that "Black males had a 2.2 percentage point decrease in the relative probability of college enrollment after the passage of the Anti-Drug Abuse Act of 1986." I build upon her work by introducing a new counterfactual group that allows the impact to be split into a first-stage and reduced form. I also introduce more modern and robust methods to deal with the issue of pre-trends, and I spend some time replicating her results.

This paper generally contributes to the literature on the effects of incarceration on education and labor outcomes. My main contribution to the literature is a relatively imprecise and subpopulation-specific estimate of the causal effect of an increase in Black adult drug-related arrest rates on college enrollment.

Some related prior literature includes Aizer and Doyle Jr (2015), which evaluates the relationship between juvenile incarceration and later life outcomes using a judge-IV research design. They find that juvenile incarceration "results in large decreases in the likelihood of high school completion and large increases in the likelihood of adult incarceration." Western, Kling, and Weiman (2001) evaluates the impact of incarceration on labor market outcomes. Mitchell (2016) finds that changes in drug arrest rates did not reduce the rate of drug offenses but had substantial negative impacts on employment outcomes for Black males. Hatzenbuehler et al. (2015) finds that high incarceration rates have psychiatric impacts on non-incarcerated populations, implying that if the federal drug acts have a large impact on arrest rates, there may be spillover effects.

The rest of the paper is organized as such: in section 2, I summarize the history and known impacts of the Anti-Drug Abuse Act and Fair Sentencing Act. In section 3, I describe my empirical strategies, and in section 4, I discuss the data and its flaws. I present my results in section 5, and I close in section 6 with conclusions and implications.

[1] I count individuals with a master's or Ph.D. as having been enrolled in college. I also count persons enrolled in two-year colleges.

[2] When looking Figure 1b, there does not seem to be a change in arrest rates after the passage of the Fair Sentencing Act, suggesting that any results may be driven by pre-trends. I will formalize this finding in the results section.

2 Background

The Anti-Drug Abuse Act of 1986 was passed in October of 1986 as part of Reagan's War on Drugs in response to the growing concerns over drug abuse, particularly the crack cocaine epidemic. The Anti-Drug Abuse Act of 1986 marked a pivotal shift in the nation's approach to drug policy, emphasizing punitive measures and significantly impacting the criminal justice system. Several notable changes in drug policy were introduced, including the establishment of mandatory minimum sentences for drug offenses. One of the most controversial aspects of the legislation was the implementation of the 100-to-1 sentencing disparity between crack and powder cocaine offenses. This disparity mandated that possession of 5 grams of crack cocaine resulted in a minimum 5-year prison sentence, while it took 500 grams of powder cocaine to trigger the same penalty. Critics argued that this disparity disproportionately affected minority and low-income communities, as crack cocaine was more prevalent in these populations. The Equal Justice Initiative (2021) claims that after the act, the number of Black people in federal prison increased from 50 in 100,000 to 250 in 100,000, yet there was no change in the number of White people in federal prison [3]. The act also allocated substantial funds for drug enforcement, treatment, and prevention programs, reflecting the government's commitment to addressing the drug crisis comprehensively. The Office of National Drug Control Policy (1988) estimated that drug control funding increased from $2.9 billion in 1986 to $4.8 billion in 1987. The Act has faced additional criticism, as in recent years, researchers such as Bewley-Taylor, Trace, and Stevens (2005) have found that "there is little correlation between incarceration rates and drug use prevalence; and the impact of enforcement action on price is much less powerful than other market factors."

Due to the War on Drugs, arrest rates and sentencing times were trending up even before the Anti-Drug Abuse Act of 1986. U.S. Bureau of Labor Statistics (1988) reported that from 1980 to 1986, the number of federal drug law violations that resulted in convictions increased by 134% to a total of 12,285 convictions in 1986 [4]. The BLS report also notes that average prison sentences for drug offenders increased by 33%, from less than four years to just over five years.

The Fair Sentencing Act of 2010 was a piece of legislation enacted by the United States Congress to address the sentencing disparities between crack and powder cocaine offenses by amending the previously imposed 100-to-1 sentencing disparity. Under the new legislation, the ratio was reduced to 18-to-1, meaning that it now took 28 grams of crack cocaine to trigger a mandatory minimum sentence of 5 years, instead of the previous 5 grams, while the threshold for powder cocaine remained at 500 grams. Furthermore, the Fair Sentencing Act eliminated the mandatory minimum sentence for simple possession of crack cocaine, emphasizing a more equitable approach to drug offenses. Although the act did not completely equalize the sentencing for crack and powder cocaine, it represented a significant step toward a fairer criminal justice system and demonstrated the government's commitment to addressing the consequences of previous drug policies. A United

[3] The Equal Justice Initiative did not include their source, and I could not find any other source making the same claim.

[4] Federal convictions of all other types increased by 27%.

States Sentencing Commission (2015) report estimated that implementing the Fair Sentencing Act of 2010 resulted in the saving of 29,653 bed years [5].

3 Empirical Strategy

I take three discrete approaches to my empirical strategy, establishing three unique counterfactual groups for identifying the impact of the Anti-Drug Abuse Act of 1986 and the Fair Sentencing Act of 2010.

My first and primary empirical approach looks at changes in college enrollment rates in high-intensity drug arrest states relative to the low-intensity drug arrest states before and after the passage of both the 1986 and 2010 acts. High-intensity drug arrest states are defined as states above the 75th percentile two years before the passage of the federal law [6]. I primarily rely on data on adult arrests instead of juvenile arrests due to issues with pre-trends [7]. I examine both the first-stage impact where the outcome is the change in drug-related arrest rates and the reduced-form impact where the outcome is college-enrollment. The first-stage is evaluated using an event-study model that allows me to assess the evolution of relative outcomes while controlling for fixed differences across states and national trends over time. Using data at the state (s) by year (t) level, I estimate:

$$y_{st} = \alpha_s + \gamma_t + X'_{st}\phi + \sum_{m=-G}^{M} \beta_m z_{s,t-m} + \epsilon_{st} \tag{1}$$

where α_s and γ_t are individual and time fixed effects, X'_{st} is a vector of control variables, and ϵ_{st} represents a shock uncorrelated with the policy. The coefficients $\{\beta_m\}_{m=-G}^{M}$ summarize the magnitude of the dynamic effects and are displayed in an event-study plot [8]. In each event-study plot I also include the difference between the average of the post-period and pre-period coefficients; to account for potential pre-trends, I also report the average of the post-period coefficients, which is equivalent to the average treatment effect on the treated (ATT). When testing for pre-trends, if the first-stage arrest rates are trending similarly before the passage of the Anti-Drug Abuse Act of 1986 and the Fair Sentencing Act of 2010, I expect the coefficients associated with pre-periods to be small and statistically insignificant.

For the reduced-form estimates, I follow a similar approach where I use an event-study model identical to the model specified in 1, except I replace (s) with(i), as data is at the individual by year

[5] The same United States Sentencing Commission (2015) report also found that the Fair Sentencing Act of 2010 did not "disrupt the ongoing decline in the number of people who reported using crack cocaine in the last year."

[6] For the Anti-Drug Abuse Act of 1986, states with high Black adult normalized arrest rates include CT, DC, GA, IL, MD, MA, MO, NJ, NY, and TN. States with high Black juvenile normalized arrest rates include AR, CT, DE, DC, ME, MD, MA, MT, NJ, SD, and TN. For the Fair Sentencing Act of 2010, states with high Black adult normalized arrest rates include AL, AR, DE, DC, GA, KY, LA, MD, MS, MO, NJ, NC, TN, and VA. States with high Black juvenile normalized arrest rates include AL, DE, DC, GA, IL, KY, LA, MD, MS, MO, NJ, NC, PA, and SC. Note that state-years without drug arrest data are omitted from the analysis.

[7] As discussed in the introduction, Aizer and Doyle Jr (2015) finds that juvenile incarceration has a larger impact on education than adult incarceration. However, since states with high adult arrests tend to have high juvenile arrest rates, my estimates are robust to which arrest rate is used. I also present estimates using the juvenile arrest rate.

[8] Since the Anti-Drug Abuse Act was passed in October, I normalize the β in 1986 to be zero. Similarly, since the Fair Sentencing Act of 2010 was passed in August, I normalize the β on 2010 to be zero.

level. In addition to the event study regressions, I also report traditional difference-in-differences estimates as a summary of the effect across all post-expansion years using the following regression specification at the individual (i) by year (t) level:

$$y_{it} = \alpha + \zeta_s + \xi_t + \delta D_i + \gamma Post_t + \beta D_i Post_t + X'_{it}\phi + \epsilon_{it} \tag{2}$$

where ζ_s and ξ_t are fixed effects at the state and year level, D_i is an indicator for belonging to the treatment group (in this case, states with high drug arrests), $Post_t$ is a time indicator for belonging to the post-period, $D_i Post_t$ is the interaction term where β is potentially the causal effect of the federal policy if the identifying assumptions are satisfied, and X'_{it} is a vector of control variables. For the reduced-form estimates, I report three specifications of the difference-in-difference estimates: the first omits control variables and fixed effects, the second omits fixed effects, and the third is the full regression as specified in 2. To increase power, I also include difference-in-difference regressions with a continuous treatment variable instead of an indicator variable.

To address issues of low power associated with event study tests for pre-trends in the first stage, I implemented suggestions from Roth (2022), which include calculating the slope of a linear violation of parallel trends that a pre-trends test would detect a specified fraction of the time, the expected value of the coefficients conditional on passing the pre-test under the hypothesized trend, and useful statistics such as power, Bayes Factor, and likelihood ratio. If the parallel trends assumption is satisfied, then my first-stage and reduced-form estimates would identify the local causal effect.

Overall, examining both the first-stage and reduced-form impact of the federal has the advantage of enabling me to estimate the impact of arrest rates on college enrollment for Black males. I use an instrumented difference-in-differences (DDIV) estimator where the endogenous variable is arrest rates and the instrument is an indicator for living in a high-intensity state. I follow the advice given in Hudson, Hull, and Liebersohn (2017). For my DDIV estimates to be causal, the exclusion restriction must be satisfied, i.e. it must be the case that college enrollment is only impacted through changes in arrest rates. I report the DDIV estimator as the simple ratio of means, and I also calculate the DDIV estimator via two-stage least squares (2SLS). If the exclusion restriction is satisfied, then my estimate applies to populations whose arrest rate was increased by the federal drug act. To check whether my estimates may be affected by the weak-instrument problem, I report F-statistics.

My second and third empirical strategies mirror the approach taken in Britton (2022). In my second empirical strategy, I compare changes in college enrollment rates in Black males aged 18-24 relative to white males aged 18-24 at the time of the federal law passage. My third approach is very similar to the second approach, where I look at the change in college enrollment rates in Black males aged 18-24 at the time of the federal law passage relative to female Blacks aged 18-24 at the time of the federal law passage. I report traditional difference-in-difference estimates using the regression specified in 2, except in the second empirical approach D_i is an indicator for being Black (while the entire sample is limited to Black and White males), and in the third empirical approach D_i is an indicator for being a Black male (while the entire sample is limited to Blacks aged 18-24 at the time of the federal law passage.).

Finally, for supplemental analysis, I also include a triple difference-differences specification in the appendix, where the first difference is pre and post-time periods, the second difference is race, and the third difference is living in a high or low-Black drug arrest state. If White men have a similar first-stage pre-trend to Black males in high vs low-intensity states, the triple difference-difference specification will remove that bias from that specific pre-trend.

To help test whether the identifying assumptions for the three counterfactual groups were satisfied, aside from including event-study plots, I include plots of the raw outcomes over time.

In all the regression specifications, I estimate the equation using ordinary least squares (OLS). I also follow the recommendations outlined in Bertrand, Duflo, and Mullainathan (2004) and calculate heteroskedasticity-robust standard errors that are clustered at the state level. All analyses use CPS October supplement final person weights and limit the sample to Black males aged 18-24 at the time of the passage of the federal act.

4 Data and Descriptive Statistics

4.1 Data

To conduct my analysis, I use data from three sources. My primary data source is the Current Population Survey (CPS) October Education Supplement from 1980-2016, which I accessed via the IPUMS-CPS database (Flood et al., 2022) [9]. The Current Population Survey Education Supplement (CPS) is an annual cross-sectional survey conducted by the United States Census Bureau and the Bureau of Labor Statistics, collecting data from a nationally representative sample of approximately 60,000 households. Focused on educational attainment, enrollment status, and related socio-economic factors, the CPS provides a snapshot of the U.S. population's educational landscape, which is instrumental in shaping educational policies and understanding trends, and the CPS is commonly used in the social sciences. Following the approach in Britton (2022), I excluded observations missing relevant data such as family income and educational attainment, which reduced the sample by about five percent. I defined college enrollment to be persons who had 1 year or more of higher education, and I limited my analysis to persons aged 18-24 in the year the federal law was passed (1986 and 2010). One notable limitation of the CPS is that it excludes the currently incarcerated population, which would result in an underestimate of the impact of both the Anti-Drug Abuse Act of 1986 and the Fair Sentencing Act of 2010. The CPS also does not account for movement across state lines.

For data on arrests, I used the Uniform Crime Reporting (UCR) Program Data from the

[9] Following Britton (2022), for my analysis of the Anti-Drug Abuse Act of 1986, I only use data from years from 1984 and onwards due to potential pre-trends in earlier years. Further discussion is included in the appendix.

United States Department of Justice. Federal Bureau of Investigation (1980-2016). The UCR Program is a data collection initiative led by the FBI, which amasses crime statistics from local law enforcement agencies throughout the United States, and the UCR data is commonly used for crime-related social science research. The UCR Program has data at the county-year level and includes information on the number of arrests for each arrest type (e.g. drug possession, drug distribution, assault, robbery, etc) and also records data on the age [10] and race of the arrested. In my analysis, I use Black adult arrests and Black juvenile arrests related to all drug crimes [11]. Since I'm using arrest data at the state-by-year level, I constructed a normalized arrest rate per 100,000 by averaging the arrest rate for counties with available data in a state together, dividing by the state's population in the given year, and scaling up. [12]. In any analysis where I used both UCR and CPS data, I merged the two datasets at the state and year level [13].

It is important to note that the UCR data has several substantial limitations. First and foremost, many counties failed to report their arrest rates for certain years. Secondly, the UCR's hierarchical reporting system requires that only the most severe crime be recorded in cases of multiple offenses, which may result in undercounting. Finally, variations in reporting practices among different law enforcement agencies, as well as changes in reporting standards over time, can affect the consistency and comparability of the data. Notably, in my sample, in certain years, one state failed to report any arrest data, which resulted in all persons living in said state being dropped from the analysis [14]. Further, arrest data from Florida is missing from many years, particularly in years relevant to the Fair Sentencing Act of 2010. This is likely to bias our estimates downwards, as Florida has high rates of drug use and sales [15]. In the criminology literature, Gove, Hughes, and Geerken (1985) conclude that the personal characteristics of the offender have minor effects on whether the crime is reported and that the UCR is a valid indicator for serious crimes. Lynch and Jarvis (2008) conclude that "missing data are substantial in the UCR program and certainly worthy of attention. They are not randomly distributed and cannot, therefore, simply be ignored. Much of the work done with the unimputed UCR data has overrepresented the experience of larger urban places and underrepresented smaller and less urban places (LaFree, 1998). It is difficult to determine if this overrepresentation has substantial effects on conclusions based on these data. The imputation strategies employed by the UCR program are reasonable and appear to reduce the overrepresentation of larger places. However, these methodologies can clearly be improved upon." Notably, there were a few observations in certain years that were extreme outliers. For example, the 99th percentile was 142 arrests in a certain state year, but the outlier would have over 1000 arrests. To deal with these extreme outliers (which are very likely to be erroneous), I implement

[10] The UCR data separately tracks arrest rates for adults and juveniles.

[11] The universe of all drug crimes corresponds to code 18 in the UCR program data. The UCR program includes data on drug offenses at a more granular level, such as type of drug, weight, and sale to a minor.

[12] The population in each state-year was taken from the UCR.

[13] The merged dataset is henceforth referenced as CPS-UCR merged dataset

[14] Approximately 10,000 observations per year were dropped, which was less than 10% of the total sample.

[15] Florida is the epicenter of the recent prescription drug epidemic in the United States (Lee et al., 2014), and most Colombian cocaine was initially transported to the United States through the Caribbean and Florida (Williams, 1998).

a 95% right tail winsorization [16].

Finally, I used unemployment data at the state-by-year level from the U.S. Bureau of Labor Statistics (1980 - 2018).

4.2 Descripitive Statistics

Tables 1 and A1 (in the appendix) report sample means separately for the pre and post-periods of both the Anti-Drug Abuse Act of 1986 and the Fair Sentencing Act of 2010. Table 1 uses the CPS-UCR linked dataset (which drops unmerged observations), while Table A1 uses the CPS dataset only.

Comparing Table 1 to Table A1 provides a rough test for evaluating whether the states with missing UCR data certain years in are substantially different from the state without missing data. The number of observations is slightly smaller in the CPS-UCR merged dataset, except for the 2010-2016 period which is much smaller (likely due to the missing UCR data from Florida). The "enrolled in college" row is approximately identical between Table 1 and Table A1, and the proportion of Blacks and males is also largely the same.

In Table 1, in all periods, about 15% of the sample is Black. The total proportion of males enrolled in college fluctuates heavily, but the proportion of Black males enrolled in college is more stable, as most of the fluctuations in total male college enrollment are driven by non-Black males. Overall, my summary statistics in Table 1 and A1 are similar to the one presented in Britton (2022), but there remain minor differences.

In figure 5, I present a heatmap of Black adult drug-related arrests in 1984, two years before the passage of the Anti-Drug Abuse Act. I also present kernel density estimates of the distribution of arrest rates at the state level in figure 6 with a vertical line denoting high-intensity states.

5 Results

In the first subsection, I first evaluate the impact of the Anti-Drug Abuse Act of 1986, broken down into the first stage, reduced form, and second stage. In the next subsection, I discuss the Fair Sentencing Act of 2010.

For important event-study figures, I include tables in the appendix with all the coefficients of interest.

5.1 Anti-Drug Abuse Act of 1986

5.1.1 First Stage Estimates

I first estimate the impact of the Anti-Drug Abuse Act of 1986 on drug-related arrest rates for Black adults by looking at changes in arrest rates in high-intensity relative to low-intensity states before and after 1986.

[16] I use winsorization over trimming since it is likely the case that the outliers still belong to the distribution and occurred because of reporting error.

The first-stage results are presented in figures 7, 8, 9, table 2, and table A2 in the appendix.

For Black adults, using the event study model detailed in equation 1, I find an increase in drug-related arrest rate associated with the Anti-Drug Abuse Act with gains between 3.08 and 12.71 arrests per 100,000 during each post-expansion year. When using the event-study model specified in equation 1, I find that the average between the post and pre-period coefficients is 2.96, and the ATT [17] is a bit larger at 3.85 arrests with a similar standard error. The estimates for years 2 and 3 in the post-period are larger than the estimates for years 1 and 4, 5, and 6, indicating that the effect of the law took some time to "kick in", and the effect potentially decreased in the later years [18]. If I omit fixed effects from the event-study model (in Figure 8), the event-study model estimates a larger and statistically significant effect, where the difference between the post and pre-periods is 7.1 with a robust, clustered standard error of 2.5. Similarly when using the difference-in-difference model specified in equation 2, I find a statistically insignificant increase of approximately 3.3 arrests per 100,000.

To check for pre-trends in my event study model, I test the joint null hypothesis that all of the pre-treatment event study coefficients are zero via an F-test, which I report at the bottom of each event study figure. In this case, in figure 7, I get a p-value of 0.27, which increases my confidence in the parallel trends assumption. I additionally included plots of raw outcomes over time in Figure 3. As an additional robustness check, I implement suggestions from Roth (2022) in figure 10 [19]. The results from Roth's pre-trend package imply that if there exists a pre-trend, a quadratic pre-trend is more worrying than the linear pre-trend, as I have less power to detect a significant pre-trend and the likelihood ratio is much higher. However, a quadratic pre-trend is also less likely than a linear one. Notably, if there exists a pre-trend, figure 7 suggests that the pre-trend is possibly negative, which would imply that my first-stage estimates have attenuation bias.

This suggests that the Anti-Drug Abuse Act had an impact on the justice system by incarcerated people that would have otherwise not been incarcerated, ruling out the possibility that the Act only impacted persons who would have been incarcerated regardless by extending their sentence length (through mandatory minimum sentencing requirements). Therefore, although the concern that the only populations impacted by the Anti-Drug Abuse Act would not have attended college in the absence of the Act is still valid [20], this result does provide some counter-evidence.

[17] The ATT is calculated as the average of the post-period coefficients.

[18] I confirmed that the large standard errors in certain post-periods were not due to coding errors by regressing the outcome in the year of interest minus outcome in year zero on the treatment group.

[19] Using Roth's R package "pre-trends", I calculate the power defined as the probability that I would find a significant pre-trend under the hypothesized pre-trend, the Bayes Factor defined as the ratio of the probability of failing to reject the pretest under the hypothesized trend relative to under parallel trends, and the likelihood ratio which is defined to be the ratio of the likelihood of the observed coefficients under the hypothesized trend relative to under parallel trends.

[20] If the populations primarily impacted by the Anti-Drug Abuse Act were not going to attend college in the absence of the Act, I would expect the Act to have no impact on college enrollment.

5.1.2 Reduced Form Estimates from Comparing High vs Low-Intensity States

The results from comparing high to low-intensity states are presented in figure 12 and tables 4 and 5.

In figure 12, I find that there is likely a pre-trend, as I calculate an f-statistic of 0.035. However, the pre-trend appears to be in the opposite direction of the effect, implying that if there existed a linear pre-trend, my model would underestimate the effect of the Anti-Drug Abuse Act on college enrollment, i.e. my estimate would once again be attenuated. I calculate that the average between the post and pre-period coefficients is -0.0004, and the ATT is -0.02 which is equivalent to a 2% decrease in the probability of college enrollment, matching the estimate in Britton (2022). Similar to the first stage, I find that the effect size increases at first in years 1, 2, and 3, and then the effect stops increasing around year 4. Overall, I find my estimates reduced-form estimates are quite noisy.

I also present simple difference-in-difference estimates of the treatment effect in tables 4 and 5. To increase power, I also calculated the same regression model, except I used a continuous treatment (arrest rates) variable instead. My difference-in-difference estimates are consistent with the event-study results, as most are small, negative, and statistically insignificant.

5.1.3 Second Stage Estimates

In table 2, I present a simple table of means without any controls or fixed effects to consolidate the results from the first-stage and reduced-form models. Notably, in table 2, when examining a simple difference in means, the difference in college enrollment probability between pre and post-period for high and low-intensity states for Black adult drug-related arrests is -2.3%, which once again matches the result in Britton (2022) The instrumented difference in difference estimate from this table is equivalent the ratio between the reduced-form and first stage, which is -0.2326/3.332 = -0.0698 = -6.9%.

The more robust second-stage instrumented difference-in-difference coefficients are estimated via 2SLS and are presented in table 3. Overall, I find a small negative effect of arrest rates on college enrollment in all three specifications; however, the coefficients are small and statistically insignificant with large standard errors. Notably, my 2SLS estimates are at least 10 times the magnitude of the OLS estimate (presented in column 1). If I scale the variables up, my most robust estimate (with fixed effects and control variables) implies that if the arrest rate increased by 10 Black Adult drug-related arrests per 100,000, then the probability of college enrollment for a Black male would decline by approximately 2.5%. My 2SLS estimate with just controls gives us a much smaller estimate, implying that if the arrest rate increased by 10 Black Adult drug-related arrests per 100,000, then the probability of college enrollment for a Black male would decline by approximately 0.4%.

I also report F-statistics from the first-stage regression, following conventional advice from Staiger, Stock, and Watson (1997). In my case, I find an F-statistic ranging from 15.9 to 20.2, providing evidence that my estimates are likely not biased by the weak-instrument problem.

5.1.4 Additional Reduced-Form Estimates

The results from the second and third empirical strategies (following the approach in Britton (2022)) are presented in tables 10 and 11. My estimates from using White males as a counterfactual group are similar to Britton's, except my estimates are a bit larger, with a decrease in the probability of college enrollment for Black males between 3.3% and 5.7%. When I use Black females as a counterfactual group, I find smaller, statistically insignificant estimates of approximately -1.6%. To test for pre-trends, I plot the raw outcomes by treatment group over time. These outcome figures can be found in the appendix.

5.2 Fair Sentencing Act of 2010

Due to pre-treatment trends in outcomes, I find it difficult to study the Fair Sentencing Act using my research design. The next few sections are mostly dedicated to discussing the failures associated with my approach.

It is important to note that any effect on any outcome where the Fair Sentencing Act is the treatment is not due to the elimination of the mandatory sentencing minimum but rather, is due to the overall lightening of drug-related prosecutions. United States Sentencing Commission (2015) found that in the years before the Fair Sentencing Act was passed, the raw count of offenders in prison due to the minimum sentencing requirements was already in the single digits.

5.2.1 First Stage Estimates

I attempt first to estimate the impact of the Fair Sentencing Act on drug-related arrest rates for Black adults and Black juveniles separately by looking at changes in arrest rates in high-drug arrest intensity states relative to the low-drug arrest intensity states before and after 2010. I find that empirical strategy is not an appropriate research design for studying the impact of the Fair Sentencing Act of 2010 on drug-related arrest rates due to the failure of the parallel trends assumption. The results of the event study regressions specified in equation 1 can be found in figure 11. The coefficients provide strong evidence of significant pre-trends in arrest rates before the passage of the Fair Sentencing Act, and the calculated f-statistics is essentially 0.

Using adult arrests, I find that the difference between the post and pre-period coefficients is -20.8, while the ATT is -22.8 Black adult drug-related arrests per 100,000. However, in this case, the pre-trend is trending in the same direction as the measured effect. Therefore, my estimated effect is likely significantly larger than the true effect.

5.2.2 Reduced form estimates

The results from comparing high to low-intensity states are presented in figure 13 and tables A4 and A5 in the appendix. Similar to the first-stage result, I find significant pre-trends trending in the same direction as the effect. I conclude that comparing high and low-intensity states is not an approach research design for measuring the reduced-form impact of the Fair Sentencing Act on the probability of college enrollment.

I next use White males as a counterfactual group to Black males, and the results from the difference-in-differences regression are presented in table 14. Finally, I use Black females as a counterfactual group to Black males, and the results from the difference-in-differences regression are presented in table 15. For both estimates, the coefficient of interest is statistically significant and negative, which is the opposite of the expected result. This estimate is not very reliable and is likely driven by an invalid research design.

As a rough test of the parallel trends assumption for comparing Black males to White males and Black females during the Fair Sentencing Act of 2010, I include graphs of raw outcomes over time in figures 14 and 15. Although the parallel trends assumption looks reasonable in the college enrollment graph in figure 14, the parallel trends assumption seems to be violated when plotting the family income covariate, demonstrated in figure 15.

5.2.3 Second Stage Estimate

Due to significant pre-trends in both the first-stage and reduced-form estimates, I refrain from calculating a second-stage estimate for the Fair Sentencing Act.

5.3 Robustness Checks

For the first empirical strategy where I compare high and low-intensity states, I repeat my calculations using Black juvenile arrests as a robustness check. I calculated both the first-stage and reduced form, and the results are presented in figures 9, 12, 11, and 13. Overall, I find that the estimates are similar between the models using adult arrest and the models using juvenile arrests. Using juvenile arrests tends to lead to smaller estimates, but the pre-trends are likely more significant.

As a robustness check for the reduced-form estimates from comparing high vs low-intensity states, I run the same regressions using Black juvenile arrests instead of Black adult arrests, and I present the results in tables 6 and 7. I also run two separate control experiments in tables 8 and 9. In table 8, I limit my sample to females exclusively, and in table 9, I use Black males aged 30-50 in 1986. In both cases, I find very small, insignificant results.

To check the robustness of my additional reduced-form estimates from using White males and Black females as counterfactual groups for the Anti-Drug Abuse Act, I estimate the same model while restricting the sample to Males aged 30-50 in tables 12 and 13 and find mostly insignificant results. I extend the same robustness check to the Fair Sentencing Act in tables 16 and 17, and I find some evidence that the parallel trends assumption is not holding, as my estimates tend to be large and statistically significant, especially in the control experiment using males aged 30-50.

6 Conclusion

I find some evidence that the Anti-Drug Abuse Act of 1986 modestly increased drug-related arrest rates and decreased college enrollment rates among Black men. To the best of my knowledge, my paper presents the first direct estimate of the relationship between drug-related arrest rates and college enrollment. I estimated that if the drug-arrest rate in Black adults increased by 10 persons per 100,000, the probability of college enrollment would decrease by 0.4% to 2.5%.

Using the estimated average treatment effect on the treated (ATT=3.85) (to eliminate potential biases from pre-trends) for the Anti-Drug Abuse Act from the CPS-UCR merged sample, my back-of-the-envelope calculations approximately imply an approximate total increase of 2,600 Black adult drug-related arrests in each year in the treated states [21]. When I apply my first-stage estimate to my second-stage estimates, I find that the total college enrollment in treated states decreased by 900 to 5,600 students in each year [22]. The upper range of my back-of-the-envelope calculations implies that spillover effects might exist. It is important to note that my back-of-the-envelope estimates are extremely imprecise and require all the causal assumptions to hold.

I find limited results for the Fair Sentencing Act of 2010 due to parallel trend assumptions that are very unlikely to hold. The Fair Sentencing Act is hard to study from a causal inference perspective. The report by the United States Sentencing Commission (2015) presents a reasonable analysis of the impact by looking at changes in outcomes.

6.1 Limitations

It is important to summarize and acknowledge several important issues this study faces. First, many assumptions must be satisfied for the estimates presented in this paper to be interpreted as causal. For the first-stage and reduced-form estimates, the parallel trends assumption must be satisfied, and for the instrumented difference in differences estimate to be causal, the exclusion restriction must also be satisfied. While I include many checks for these assumptions, some more convincing than others, certain assumptions may still be invalid.

On the data side, the poor quality of the UCR program's arrest data may bias my estimates. Many counties failed to report arrest data, and it's highly unlikely that counties randomly failed to report data. Furthermore, since the subpopulation impacted by these acts is small, the CPS dataset may not be sufficiently powered to answer the research questions presented in this paper.

Finally, it may be the case that the subpopulation impacted by these acts would not have attended college with or without the treatment. In other words, the local average treatment effect might have been 0, and in many instances, my estimates are very close to 0. However, it is unclear whether my estimates are small due to insufficient power and noise or the true local average

[21] Using population from data from the 1980 Census, I find approximately 68 million people reside in high-intensity states. Juvenile arrests are not included in this calculation. For see a list of treated states, see the empirical strategy section.

[22] I calculated the change in college enrollment by using national college enrollment count for Black undergraduates in 1986 (776,000) from the Census multiplied by high-intensity state population to national population ratio in 1980 (68 million / 220 million), divided by 4 to account for the size of each entering class, multiplied by the ATT (3.85), multiplied by the second-stage estimated treatment effect.

treatment effect being zero.

6.2 Future Research

Other ideas for future research include exploring the impact of state-level policies on arrest rates and educational outcomes. Britton (2022) uses differences in marijuana laws, and future research can leverage variation in other drug laws while introducing modern methods such as synthetic control designs. Spillover effects may also be important to investigate. Further confirmation of the results in this paper using better-quality arrest data will result in much more precise and trustworthy estimates. In particular, there is still much to study about the Fair Sentencing Act of 2010, especially since I find my research design is not appropriate. With better datasets, future researchers can also focus on labor and health outcomes.

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Figure 1: Adult Black Arrest Rate Per 100,000

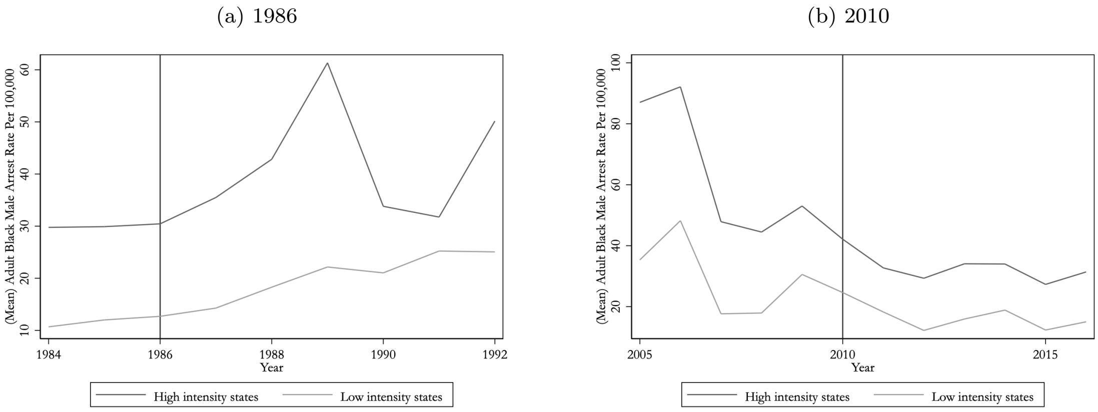
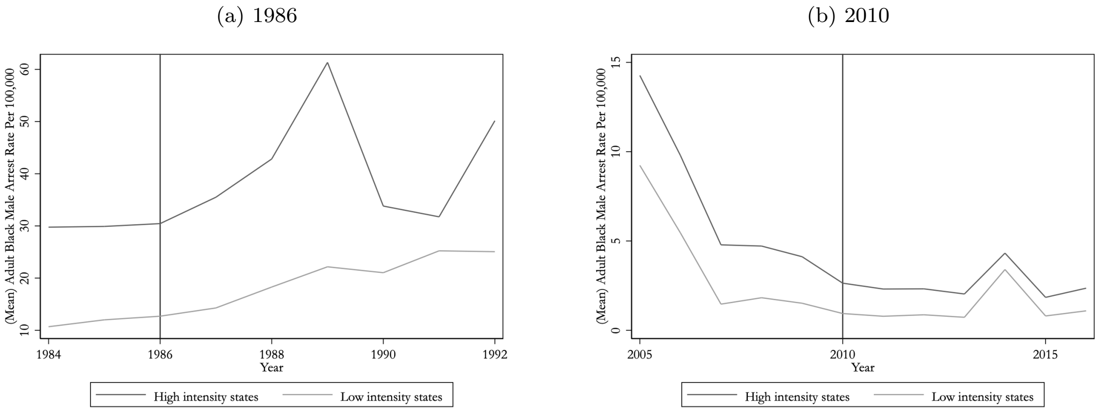


Figure 2: Juvenile Black Arrest Rate Per 100,000



Note: These figures report the drug crime arrest rate per 100,000 for black adults and black juveniles separately over time using CPS-UCR merged data from 1984-1992 and 2005-2016. A vertical line is drawn to denote the passage of the Anti-Drug Abuse Act of 1986 and the Fair Sentencing Act of 2010.

Figure 3: College Enrollment By States with High vs Low Black Adult Drug Arrest Rates

(a) 1986 (b) 2010

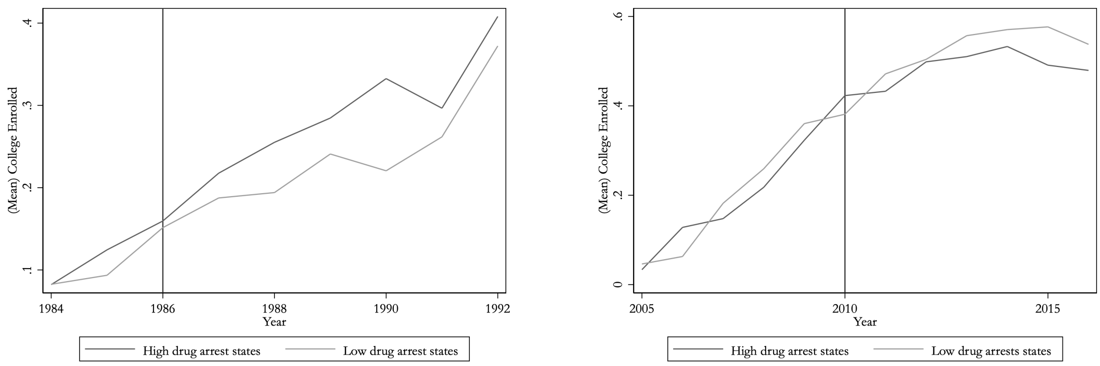
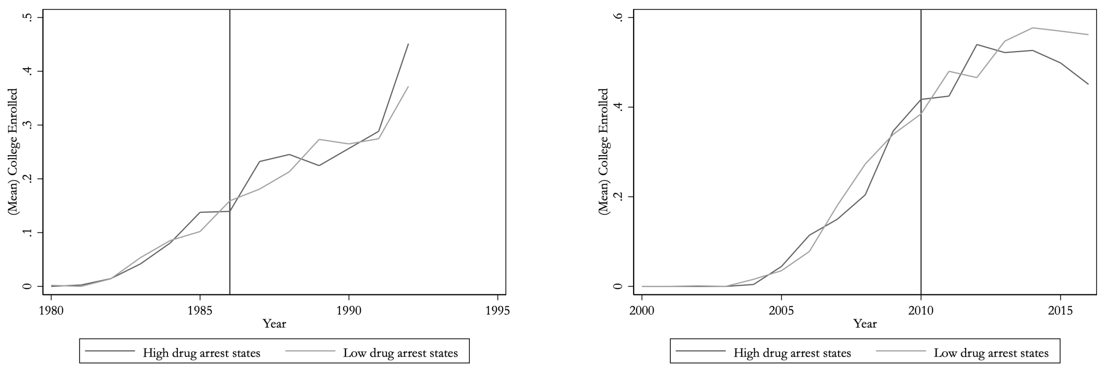


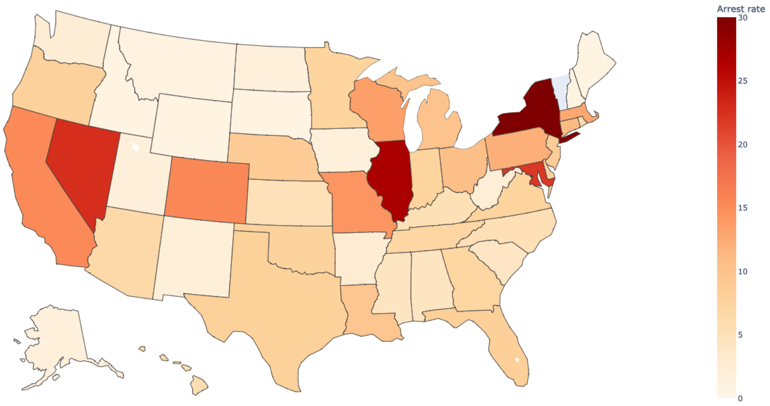
Figure 4: College Enrollment By States with High vs Low Black Juvenile Drug Arrest Rates

(a) 1986 (b) 2010



Note: These figures report the proportion enrolled in college plotted over time using CPS data from 1984-1992 and 2005-2016 for high black adult/juvenile drug arrest states and low black adult/juvenile drug arrest states, where high black adult/juvenile drug arrest states are defined to be those above the 75th percentile in 1984 and 2008. A vertical line is drawn to denote the passage of the Anti-Drug Abuse Act of 1986 and the Fair Sentencing Act of 2010. The sample is defined as black males aged 18-24 in 1986 and 2010 who were not incarcerated at the time of the survey.

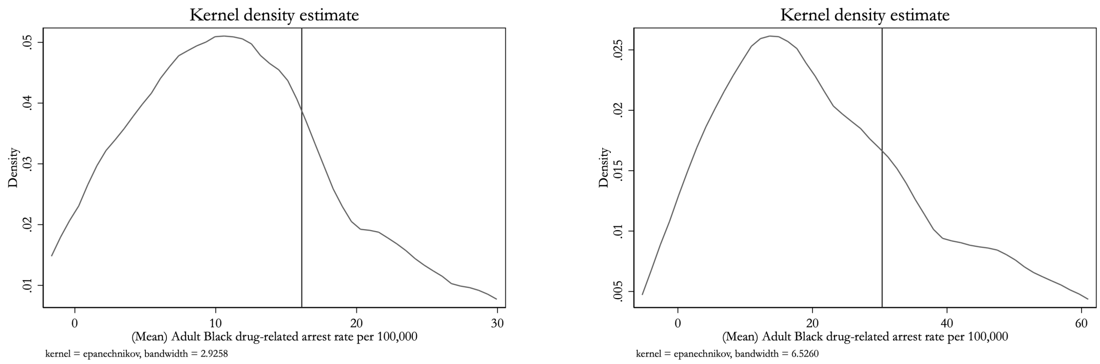
Figure 5: Black Adult Drug-related Arrest Rate Per 100,000 in 1984



Note: This figure presents a heatmap of the United States at the state level using UCR Program data. The data is from 1984, and I use all drug-related arrests for Black adult men. Although New York's normalized arrest rate is at 48, I capped the maximum at 30 for clarity of states with low normalized drug arrest rates, since the distribution is heavily right-skewed. High drug arrest states are defined as states above the 75th percentile, and the 75th percentile is at 17.4 Black adult arrests per 100,000.

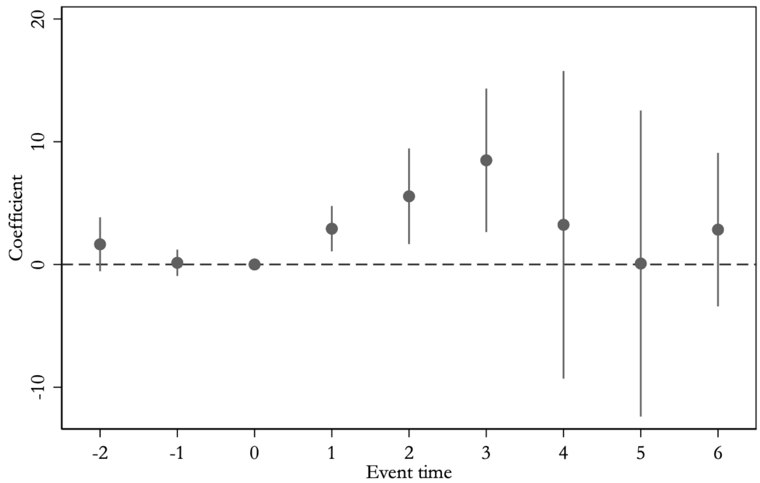
Figure 6: Distribution of Black Adult Drug-Related Arrest Rates

(a) 1984 (b) 2008



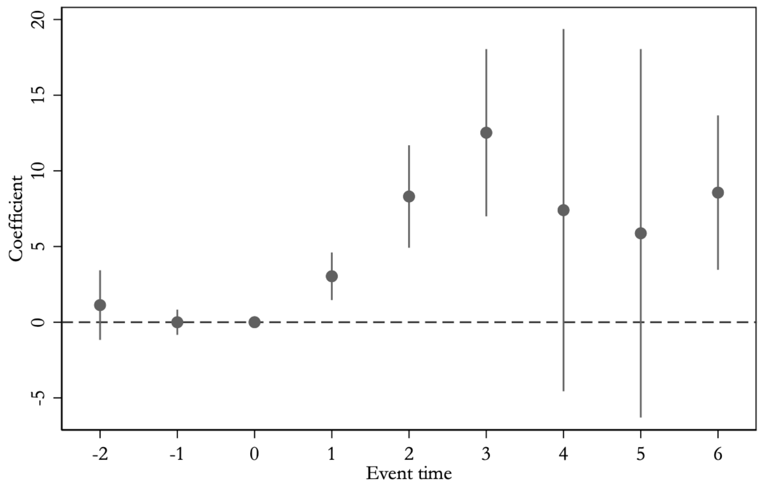
Note: These figures report the kernel density estimates for the normalized drug-related Black adult arrest rate in 1984 and 2008. The vertical line denotes the 75th percentile. Right tail outliers were winsorized at the 95% level for both figures.

Figure 7: Effect of Anti-Drug Abuse Act on Drug-related Arrest Rate of Adult Black Men, Comparing States with High and Low Black Adult Drug-Related Arrest Rates (with Fixed Effects)



Pretrends p-value (F-test): 0.274
Traditional DiD estimate (SE in parentheses): 2.96 (3.004)
ATT: 3.85 (3.122)

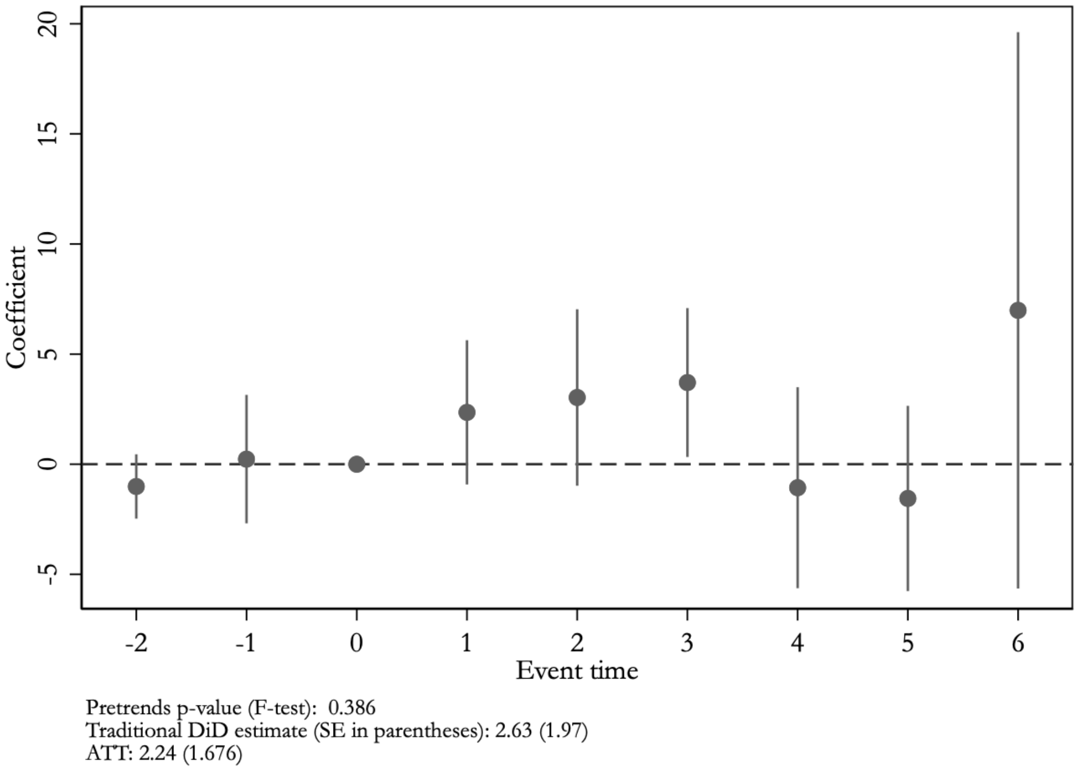
Figure 8: Effect of Anti-Drug Abuse Act on Drug-related Arrest Rate of Adult Black Men, Comparing States with High and Low Black Adult Drug-Related Arrest Rates (without Fixed Effects)



Pretrends p-value (F-test): 0.458
Traditional DiD estimate (SE in parentheses): 7.05 (2.552)
ATT: 7.62 (2.873)

Note: This figure reports coefficients from the estimation of equation 1 evaluating the impact of the Anti-Drug Abuse Act of 1986 on arrest rates per 100,000 related to drug violations using CPS and UCR data from 1982-1992. Event time 0 := 1986. The coefficients represent the change in outcomes for high-drug arrest states relative to non-high-drug arrest states, where high black adult drug arrest states are defined to be those above the 75th percentile in 1984. The sample is defined as black males aged 18-24 in 1986 who were not incarcerated at the time of the survey. Control variables include population and unemployment rates at the state-year level. Right tail arrest rate outliers were winsorized at the 95% level.

Figure 9: Effect of Anti-Drug Abuse Act on Drug-related Arrest Rate of Black Men, Comparing States with High and Low Black Juvenile Drug-Related Arrest Rate

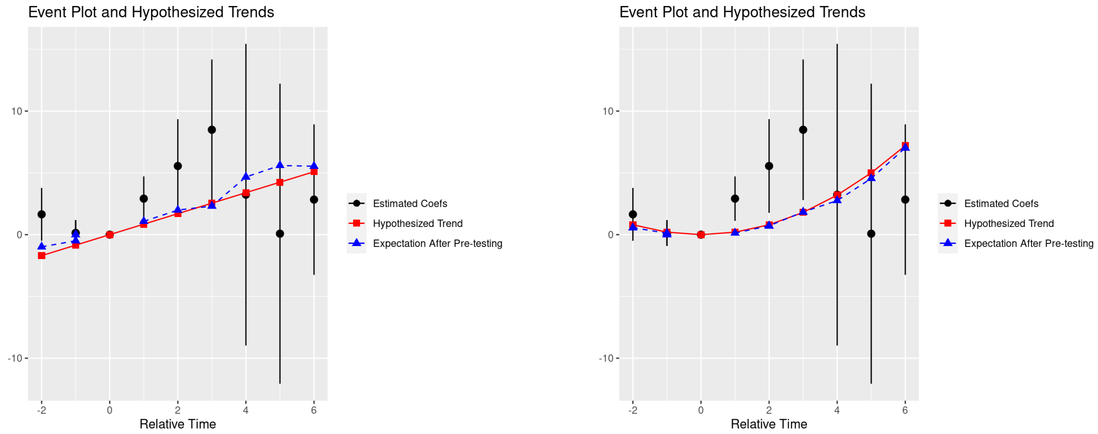


Note: This figure reports coefficients from the estimation of equation 1 evaluating the impact of the Anti-Drug Abuse Act of 1986 on arrest rates per 100,000 related to drug violations using CPS and UCR data from 1982-1992. Event time 0 := 1986. The coefficients represent the change in outcomes for high black juvenile drug arrest states relative to non-high-drug arrest states, where high-drug arrest states are defined to be those above the 75th percentile in 1984. The sample is defined as black males aged 18-24 in 1986 who were not incarcerated at the time of the survey. Control variables include population and unemployment rates at the state-year level.

Figure 10: Additional Pre-trend Testing for Coefficients from Figure 7

(a) Linear pre-trend

(b) Quadratic pre-trend



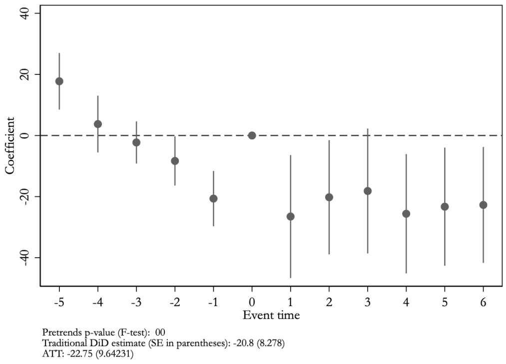
Note: These two figures were constructed using an R package written by Roth (2022). Relative time 0 represents 1986. The figures and pre-trend statistics were calculated using the estimated coefficients from Figure 7 and their corresponding covariance matrixes. The slope in Figure A was constructed such that the power would be about 0.5, while the quadratic trend in Figure B was chosen visually. The blue expectation after pretesting coefficients represents the expected value of the coefficients conditional on passing the pre-test under the hypothesized trend.

Figure A statistics: 1) power = 0.499, 2) Bayes Factor = 0.549, 3) likelihood ratio = 0.033.

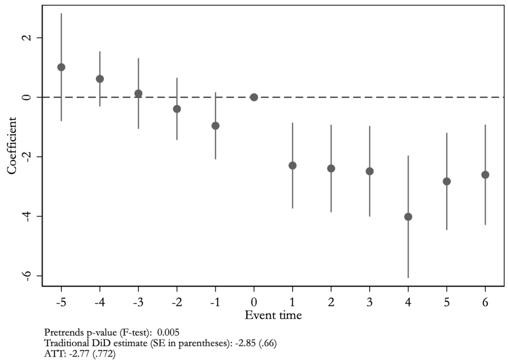
Figure B statistics: 1) power = 0.155, 2) Bayes Factor = 0.928, 3) likelihood ratio = 2.323.

Figure 11: Effect of Fair Sentencing Act on Drug-related Arrest Rate of Adult Black Men, Comparing High and Low-Intensity States

(a) Using adults arrests



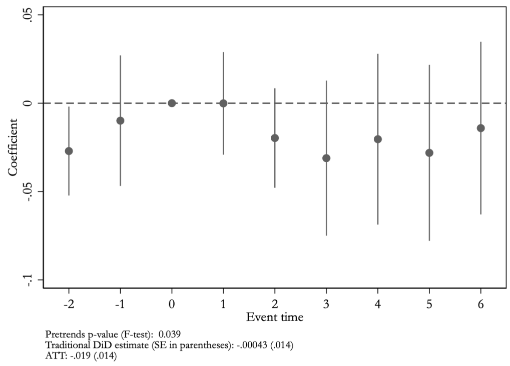
(b) Using juvenile arrests



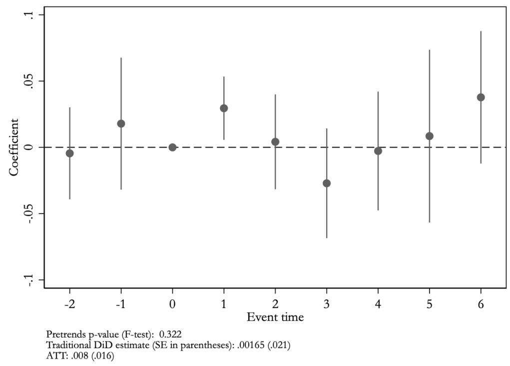
Note: These figures report coefficients from the estimation of equation 1 evaluating the impact of the Fair Sentencing Act of 2010 on arrest rates per 100,000 related to drug violations using CPS-UCR merged data from 2005-2015. Figure A defines high-intensity states using Black adult arrests, while Figure B defines high-intensity states using Black juvenile arrests. Event time $0 \coloneqq 2010$. The coefficients represent the change in outcomes for high black adult drug arrest states relative to non-high-drug arrest states, where high-drug arrest states are defined to be those above the 75th percentile in 2008. The sample is defined as black males aged 18-24 in 2010 who were not incarcerated at the time of the survey. Control variables include population and unemployment rates at the state-year level.

Figure 12: Effect of Anti-Drug Abuse Act on the College Enrollment Rate of Adult Black Men, Comparing High and Low-Intensity States

(a) Using adults arrests



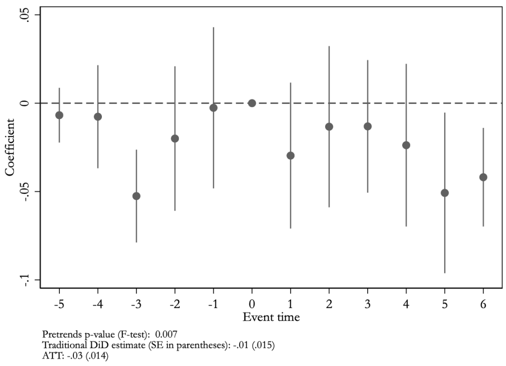
(b) Using juvenile arrests



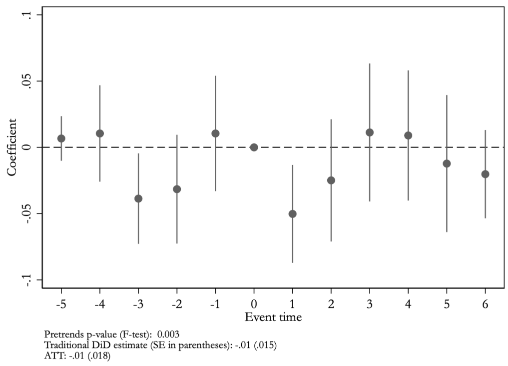
Note: These figures report coefficients from the estimation of equation 1 evaluating the impact of the Anti-Drug Abuse Act of 1986 on college enrollment rates using CPS-UCR merged data from 1984-1992. Figure A defines high-intensity states using Black adult arrests, while Figure B defines high-intensity states using Black juvenile arrests. Event time $0 \coloneqq 1986$. The coefficients represent the change in outcomes for high black adult drug arrest states relative to non-high-drug arrest states, where high-drug arrest states are defined to be those above the 75th percentile in 1984. The sample is defined as black males aged 18-24 in 1986 who were not incarcerated at the time of the survey. Control variables include population and unemployment rates at the state-year level.

Figure 13: Effect of Fair Sentencing Act on the College Enrollment Rate of Adult Black Men, Comparing High and Low-Intensity States

(a) Using adults arrests



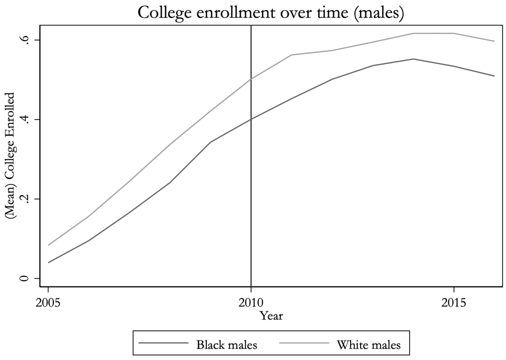
(b) Using juvenile arrests



Note: These figures report coefficients from the estimation of equation 1 evaluating the impact of the Fair Sentencing Act of 2010 on college enrollment rates using CPS-UCR merged data from 2005-2015. Figure A defines high-intensity states using Black adult arrests, while Figure B defines high-intensity states using Black juvenile arrests. Event time $0 \coloneqq 2010$. The coefficients represent the change in outcomes for high black adult drug arrest states relative to non-high-drug arrest states, where high-drug arrest states are defined to be those above the 75th percentile in 2008. The sample is defined as black males aged 18-24 in 2010 who were not incarcerated at the time of the survey. Control variables include population and unemployment rates at the state-year level.

Figure 14: College Enrollment Around 2010

(a) By race



(b) By sex

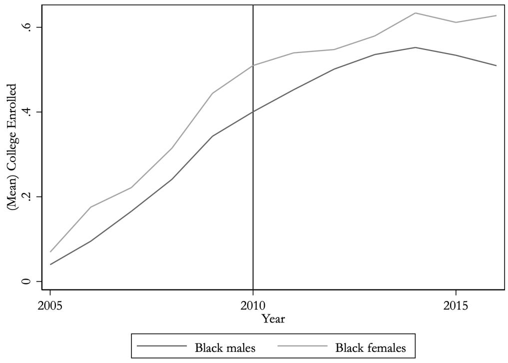
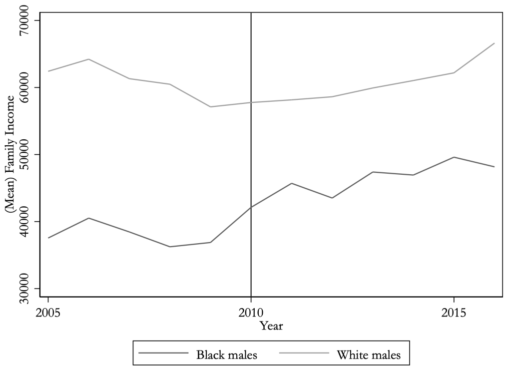
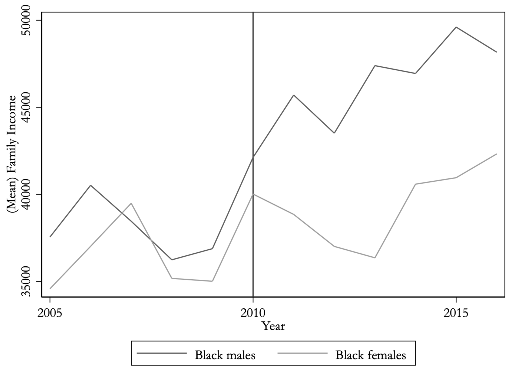


Figure 15: Family Income Around 2010

(a) By race



(b) By sex



Note: These figures report the outcomes for various subgroups plotted over time using CPS data from 2005-2016. Figure 14 reports the proportion enrolled in college, while figure 15 reports the average family income. A vertical line is drawn to denote the passage of the Fair Sentencing Act of 2010. The universe of samples is defined as participants aged 18-24 in 2010 who were not incarcerated at the time of the survey.

Table 1: CPS-UCR Merged Summary Statistics

	(1) 1984-86	(2) 1987-92	(3) 2005-09	(4) 2010-16
Male	0.49	0.49	0.51	0.50
	(0.500)	(0.500)	(0.500)	(0.500)
Black	0.15	0.13	0.15	0.15
	(0.353)	(0.339)	(0.355)	(0.357)
HS Graduate	0.71	0.85	0.51	0.90
	(0.453)	(0.355)	(0.500)	(0.295)
Enrolled in college	0.19	0.38	0.29	0.62
	(0.393)	(0.485)	(0.455)	(0.485)
Enrolled in college (Black males)	0.02	0.04	0.03	0.08
	(0.133)	(0.188)	(0.181)	(0.272)
Enrolled in college (Non-Black males)	0.17	0.34	0.26	0.54
	(0.379)	(0.475)	(0.438)	(0.498)
Observations	44778	79510	45264	72713

mean coefficients; sd in parentheses

Note: Sample means with education supplement weights are calculated from the CPS-UCR merged dataset from 1984 to 1992 and 2000 to 2016. The sample in columns 1 and 2 is defined as persons aged 18-24 in 1986, and the sample in columns 3 and 4 is defined as persons aged 18-24 in 2010, both of whom were not incarcerated at the time of the survey.

Table 2: Means of Arrest Rate and College Enrollment Rate By Pre and Post-Period and High-Intensity States

	Drug-related arrest rate			College enrollment		
	Level of drug arrests			Level of drug arrests		
	High	Low	Diff.	High	Low	Diff.
	(1)	(2)	(3)	(4)	(5)	(6)
Pre-1986	36.55	14.21	22.33	.3822	.3602	.02192
	(3.007)	(1.667)	(3.439)	(.01735)	(.009471)	(.01977)
Post-1986	27.87	8.867	19	.2182	.173	.04518
	(5.005)	(.8349)	(5.074)	(.01067)	(.005647)	(.01207)
Difference	8.68	5.348	3.332	.164	.1873	-.02326
	(2.999)	(1.094)	(3.193)	(.01594)	(.008612)	(.01812)

Note: The CPS-UCR merged dataset is used for this table. Mean estimates are weighted using CPS October supplement weights. No controls or fixed effects were used, and significance stars are omitted. Robust standard errors are clustered at the state level. The sample is defined as males aged 18-24 in 1986 who were not incarcerated at the time of the survey.

Table 3: Impact of the Anti-Drug Abuse Act on College Enrollment: OLS and 2SLS Estimates Comparing Individuals from High vs Low Intensity States

	(1) OLS	(2) 2SLS	(3) 2SLS	(4) 2SLS
Normalized arrest rate	-.0000909	-.0004802	-.0006427	-.002474
	(.0000778)	(.004375)	(.002097)	(.005646)
Post-1986	0	.1903***	.0163	.1202
	(.)	(.04319)	(.02322)	(.08039)
High drug arrest state	0	.05004	.01962	-.06341
	(.)	(.08592)	(.03632)	(.09983)
Constant	-3.422***	.1764***	-3.045***	-3.344***
	(.1885)	(.05134)	(.15)	(.2685)
Observations	59749	59749	59749	59749
Adjusted R^2	0.135	0.036	0.123	0.114
FE	Y	N	N	N
Controls	Y	N	Y	Y
Fstat	n/a	15.92	15.92	20.22

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The CPS-UCR merged dataset is used for this table. Estimates are weighted using CPS October supplement weights. The controls used at the individual level include age, age-squared, Latino ethnicity, and binned family income. The controls used at the state level include unemployment and population. Robust standard errors are clustered at the state level. The sample is defined as males aged 18-24 in 1986 who were not incarcerated at the time of the survey.

Table 4: Impact of the Anti-Drug Abuse Act on College Enrollment: DiD Estimates Comparing Individuals from High and Low Black Adult Drug Arrest States

	(1)	(2)	(3)
Post-1986	.1858***	.009332	0
	(.006211)	(.007692)	(.)
High-drug arrest state (AB)	.04129***	.01171	0
	(.009746)	(.01004)	(.)
Post-1986 X High-drug arrest state	-.001543	-.004895	-.006098
	(.01388)	(.01239)	(.01176)
Constant	.1707***	-3.028***	-3.425***
	(.004236)	(.1377)	(.1887)
Observations	59749	59749	59749
Adjusted R^2	0.038	0.126	0.135
FE	N	N	Y
Controls	N	Y	Y

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Estimates are weighted using CPS October supplement weights. Robust standard errors are clustered at the state level. The controls used include age, age-squared, Latino ethnicity, yearly state average unemployment rates, and (binned) family income. The sample is defined as males aged 18-24 in 1986 who were not incarcerated at the time of the survey.

Table 5: Impact of the Anti-Drug Abuse Act on College Enrollment: DiD Estimates Using Normalized Black Adult Drug Arrest Rate as Continuous Treatment

	(1)	(2)	(3)
Post-1986	.2067***	.01463*	0
	(.007324)	(.007744)	(.)
Drug arrest rate per 100000	.001471***	.0003557	.0001528
	(.0001937)	(.0002394)	(.0003038)
Post-1986 x Drug arrest rate per 100000	-.001329***	-.0003494	-.0002374
	(.0001735)	(.0002096)	(.0002952)
Constant	.1577***	-3.036***	-3.426***
	(.006122)	(.1341)	(.1894)
Observations	59749	59749	59749
Adjusted R^2	0.037	0.126	0.135
FE	N	N	Y
Controls	N	Y	Y

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Estimates are weighted using CPS October supplement weights. Robust standard errors are clustered at the state level. Controls: age, age-squared, Latino ethnicity, yearly state average unemployment rates, and (binned) family income. The sample is defined as males aged 18-24 in 1986 who were not incarcerated at the time of the survey.

Table 6: Impact of the Anti-Drug Abuse Act on College Enrollment: DiD Estimates Comparing Individuals from High and Low Juvenile Drug Arrest States

	(1)	(2)	(3)
Post-1986	.1835***	.009727	0
	(.005482)	(.008197)	(.)
High-drug arrest state (JB)	.03193**	-.001271	0
	(.01506)	(.01214)	(.)
Post-1986 X High-drug arrest state	-.004745	-.000734	-.003227
	(.01824)	(.01724)	(.01694)
Constant	.1798***	-3.081***	-3.454***
	(.00442)	(.158)	(.2124)
Observations	53145	53145	53145
Adjusted R^2	0.035	0.107	0.116
FE	N	N	Y
Controls	N	Y	Y

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Estimates are weighted using CPS October supplement weights. Robust standard errors are clustered at the state level. The controls used include age, age-squared, Latino ethnicity, yearly state average unemployment rates, and (binned) family income. The sample is defined as males aged 18-24 in 1986 who were not incarcerated at the time of the survey.

Table 7: Impact of the Anti-Drug Abuse Act on College Enrollment: DiD Estimates Using Normalized Black Juvenile Drug Arrest Rate as Continuous Treatment

	(1)	(2)	(3)
Post-1986	.1871***	.002257	0
	(.006973)	(.008466)	(.)
JB Drug arrest rate per 100000	.00328***	6.97e-06	-.000524
	(.0004335)	(.001328)	(.001574)
Post-1986 x Drug arrest rate per 100000	-.0006388	.001012	.0003658
	(.000682)	(.0008617)	(.0007823)
Constant	.1341***	-2.2***	-2.437***
	(.003833)	(.09171)	(.122)
Observations	68669	68669	68669
Adjusted R^2	0.043	0.137	0.148
FE	N	N	Y
Controls	N	Y	Y

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Estimates are weighted using CPS October supplement weights. Robust standard errors are clustered at the state level. The controls used include age, age-squared, Latino ethnicity, yearly state average unemployment rates, and (binned) family income. The sample is defined as males aged 18-24 in 1986 who were not incarcerated at the time of the survey.

Table 8: Control Experiment Using Females: Impact of the Anti-Drug Abuse Act on College Enrollment: DiD Estimates Using Normalized Black Adult Drug Arrest Rate as Continuous Treatment

	(1)	(2)	(3)
Post-1986	.2038***	.00981	0
	(.01002)	(.009624)	(.)
AB Drug arrest rate per 100000	.001031*	-.0001892	-.0006538**
	(.0006106)	(.0004555)	(.000266)
Post-1986 x Drug arrest rate per 100000	-.000803	.0002644	.0005441**
	(.0006191)	(.0004353)	(.0002626)
Constant	.1819***	-3.316***	-3.661***
	(.009618)	(.1211)	(.139)
Observations	64539	64539	64539
Adjusted R^2	0.038	0.147	0.157
FE	N	N	Y
Controls	N	Y	Y

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Estimates are weighted using CPS October supplement weights. Robust standard errors are clustered at the state level. The controls used include age, age-squared, Latino ethnicity, yearly state average unemployment rates, and (binned) family income. The sample is defined as females aged 18-24 in 1986 who were not incarcerated at the time of the survey.

Table 9: Control Experiment Using Black Males Aged 30-50: Impact of the Anti-Drug Abuse Act on College Enrollment: DiD Estimates Using Normalized Black Adult Drug Arrest Rate as Continuous Treatment

	(1)	(2)	(3)
Post-1986	.02742**	.02573	0
	(.01251)	(.01593)	(.)
AB Drug arrest rate per 100000	.0002067	-.0000454	.0001846
	(.0003841)	(.0002649)	(.0004338)
Post-1986 x Drug arrest rate per 100000	-.000165	.0000445	-.0001857
	(.0003652)	(.0002538)	(.0004155)
Constant	.2658***	.1729	.1207
	(.02247)	(.2068)	(.2451)
Observations	13848	13848	13847
Adjusted R^2	0.001	0.131	0.144
FE	N	N	Y
Controls	N	Y	Y

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Estimates are weighted using CPS October supplement weights. Robust standard errors are clustered at the state level. The controls used include age, age-squared, Latino ethnicity, yearly state average unemployment rates, and (binned) family income. The sample is defined as males aged 30-50 in 1986 who were not incarcerated at the time of the survey.

Table 10: Impact of the Anti-Drug Abuse Act on College Enrollment: DiD Estimates Comparing Black and White Males

	(1)	(2)	(3)
Post-1986	.1863***	.009509	0
	(.0058255)	(.007776)	(.)
Black	-.08174***	-.02264**	-.02303**
	(.010066)	(.0096726)	(.011247)
Post-1986 X Black	-.03381***	-.05614***	-.05704***
	(.012598)	(.012576)	(.012421)
Constant	.1944***	-2.931***	-3.372***
	(.0066376)	(.13312)	(.17649)
Observations	63724	63724	63724
Adjusted R^2	0.041	0.125	0.134
FE	N	N	Y
Controls	N	Y	Y

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Estimates are weighted using CPS October supplement weights. Robust standard errors are clustered at the state level. The controls used include age, age-squared, Latino ethnicity, and binned family income. The sample is defined as Black and White males aged 18-24 in 1986 who were not incarcerated at the time of the survey. This table is partially replicated from Britton (2022).

Table 11: Impact of the Anti-Drug Abuse Act on College Enrollment: DiD Estimates Comparing Black Males and Females

	(1)	(2)	(3)
Post-1986	.1525***	.003058	0
	(.015307)	(.014885)	(.)
Male	-.01951*	-.03152***	-.0316***
	(.01049)	(.010247)	(.010332)
Post-1986 X Male	-.0000211	-.01565	-.01527
	(.017215)	(.016577)	(.016949)
Constant	.1321***	-1.864***	-2.207***
	(.0085438)	(.20205)	(.24802)
Observations	14942	14942	14940
Adjusted R^2	0.031	0.125	0.137
FE	N	N	Y
Controls	N	Y	Y

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Estimates are weighted using CPS October supplement weights. Robust standard errors are clustered at the state level. The controls used include age, age-squared, Latino ethnicity, and binned family income. The sample is defined as Black males and Black females aged 18-24 in 1986 who were not incarcerated at the time of the survey. This table is partially replicated from Britton (2022).

Table 12: Control Experiment Using Males 30-50. Impact of the Anti-Drug Abuse Act on College Enrollment: DiD Estimates Comparing Black and White Males

	(1)	(2)	(3)
Post-1986	.03069***	-.00412	0
	(.0040187)	(.0054982)	(.)
Black	-.1663***	-.08032***	-.07345***
	(.012227)	(.011076)	(.010752)
Post-1986 X Black	-.004636	.0163*	.01906**
	(.010655)	(.0087455)	(.0087068)
Constant	.4357***	-.1163**	-.1683***
	(.014587)	(.056728)	(.053788)
Observations	168149	168149	168149
Adjusted R^2	0.011	0.164	0.177
FE	N	N	Y
Controls	N	Y	Y

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Estimates are weighted using CPS October supplement weights. Robust standard errors are clustered at the state level. The controls used include age, age-squared, Latino ethnicity, and binned family income. The sample is defined as Black and White males aged 30-50 in 1986 who were not incarcerated at the time of the survey. This table is a control experiment for table 2 in Britton (2022).

Table 13: Control Experiment Using Blacks Aged 30-50. Impact of the Anti-Drug Abuse Act on College Enrollment: DiD Estimates Comparing Black and Males and Females

	(1)	(2)	(3)
Post-1986	.03105***	.02523***	0
	(.0068842)	(.0078127)	(.)
Male	.01562*	-.01907**	-.02101**
	(.0083324)	(.0083229)	(.0081009)
Post-1986 X Male	-.004999	-.005029	-.004284
	(.0098612)	(.0089705)	(.0089533)
Constant	.2537***	.3235**	.2992*
	(.010825)	(.15492)	(.16046)
Observations	33583	33583	33583
Adjusted R^2	0.001	0.141	0.152
FE	N	N	Y
Controls	N	Y	Y

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Estimates are weighted using CPS October supplement weights. Robust standard errors are clustered at the state level. The controls used include age, age-squared, Latino ethnicity, and binned family income. The sample is defined as Black males and Black females aged 30-50 in 1986 who were not incarcerated at the time of the survey. This table is a control experiment for table 3 in Britton (2022).

Table 14: Impact of the Fair Sentencing Act on College Enrollment: DiD Estimates Comparing Black and White Males

	(1)	(2)	(3)
Post-2010	.4605***	.1655***	0
	(.007575)	(.00999)	(.)
Black	-.04992***	-.007978	-.006736
	(.006469)	(.006119)	(.0061)
Post-2010 X Black	-.04527***	-.06463***	-.06378***
	(.01473)	(.01645)	(.01639)
Constant	.1649***	-.9772***	-.6099***
	(.004676)	(.01645)	(.02007)
Observations	126471	126471	126471
Adjusted R^2	0.216	0.304	0.323
FE	N	N	Y
Controls	N	Y	Y

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Estimates are weighted using CPS October supplement weights. Robust standard errors are clustered at the state level. The controls used include age, age-squared, Latino ethnicity, and binned family income. The sample is defined as Black and White males aged 18-24 in 2010 who were not incarcerated at the time of the survey.

Table 15: Impact of the Fair Sentencing Act on College Enrollment: DiD Estimates Comparing Black Males and Females

	(1)	(2)	(3)
Post-2010	.4619***	.1839***	0
	(.01217)	(.01617)	(.)
Male	-.04083***	-.0413***	-.04052***
	(.006098)	(.005454)	(.005418)
Post-2010 X Male	-.04668***	-.06413***	-.066***
	(.01305)	(.01302)	(.01337)
Constant	.1558***	-.8294***	-.412***
	(.005561)	(.02378)	(.03725)
Observations	29982	29982	29982
Adjusted R^2	0.211	0.294	0.310
FE	N	N	Y
Controls	N	Y	Y

Standard errors in parentheses

Weights used. SEs clustered at state level. Still missing some demographic controls.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Estimates are weighted using CPS October supplement weights. Robust standard errors are clustered at the state level. The controls used include age, age-squared, Latino ethnicity, and binned family income. The sample is defined as Black males and females aged 18-24 in 2010 who were not incarcerated at the time of the survey.

Table 16: Control Experiment Using Males 30-50. Impact of the Fair Sentencing Act on College Enrollment: DiD Estimates Comparing Black and White Males

	(1)	(2)	(3)
Post-2010	.03196***	-.01465***	0
	(.003626)	(.003733)	(.)
Black	-.08432***	-.03364***	-.03036***
	(.01511)	(.009867)	(.009577)
Post-2010 X Black	.01276*	.03048***	.03098***
	(.006513)	(.006333)	(.006254)
Constant	.5623***	.4879***	.4174***
	(.00764)	(.02959)	(.02721)
Observations	364613	364613	364613
Adjusted R^2	0.004	0.149	0.154
FE	N	N	Y
Controls	N	Y	Y

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Estimates are weighted using CPS October supplement weights. Robust standard errors are clustered at the state level. The controls used include age, age-squared, Latino ethnicity, and binned family income. The sample is defined as Black and White males aged 30-50 in 2010 who were not incarcerated at the time of the survey.

Table 17: Control Experiment Using Black Males and Females 30-50. Impact of the Fair Sentencing Act on College Enrollment: DiD Estimates Comparing Black Males and Females

	(1)	(2)	(3)
Post-2010	.05163***	.01697*	0
	(.00919)	(.009392)	(.)
Male	-.08145***	-.1037***	-.1047***
	(.006787)	(.006601)	(.006532)
Post-2010 X Male	-.006913	-.004929	-.005098
	(.007812)	(.008108)	(.008121)
Constant	.5594***	.401***	.4563***
	(.0101)	(.04762)	(.05628)
Observations	77836	77836	77836
Adjusted R^2	0.010	0.127	0.132
FE	N	N	Y
Controls	N	Y	Y

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Estimates are weighted using CPS October supplement weights. Robust standard errors are clustered at the state level. The controls used include age, age-squared, Latino ethnicity, and binned family income. The sample is defined as Black males and females aged 30-50 in 2010 who were not incarcerated at the time of the survey.

Appendix

A Data

I include a summary statistics table for the CPS data that is not merged with the UCR program data. The CPS unmerged dataset is used in any analysis that does not require arrest data.

B First stage arrest rate estimates

I include table A2 for the equation 1 estimates presented in figure 7.

In Britton (2022), when examining the impact of the Anti-Drug Abuse Act of 1986, she commences her analysis in 1984 due to "fluctuations in Black college enrollment during the early 1980s that were unrelated to the emergence of drug markets U.S. Department of Commerce, CPS (2017)". I create an event study plot in figure A3, and my coefficients provide evidence that there were significant pre-trends before 1984.

C Reduced form college enrollment estimates

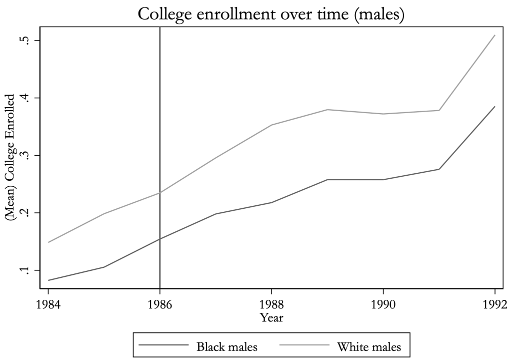
I include table A3 for the equation 1 estimates presented in figure 12.

Although it is likely the case that my estimates of the impact of the Fair Sentencing Act of 2010 using high vs low-intensity states are biased by significant pre-trends, in tables A4 and A5 I include simple difference-in-difference estimates under three similar specifications; table A4 uses discrete treatment while table A5 uses continuous treatment. Overall my estimates imply a small negative effect on college enrollment probability, but my estimates are not sufficiently powered to be statistically significant.

In tables A6-A8, I include the triple difference-in-differences results. I find the estimates are similar across various model specifications. I use both adult and juvenile arrests for the Anti-Drug Abuse Act, but I omit the juvenile table from the Fair Sentencing Act analysis since the pre-trends assumption is already quite weak. Counter to my previous results, when using adult arrest rates in the 1986 Act, I find a significant, positive effect on college enrollment. However, the significance drops away when I switch to juvenile arrests as a robustness check. Since the bias needs to be the same in each difference-in-difference estimate, if the parallel trends assumption is not satisfied, the triple difference-in-difference estimators may be more biased than my normal estimates.

Figure A1: College Enrollment Around 1984

(a) By race



(b) By sex

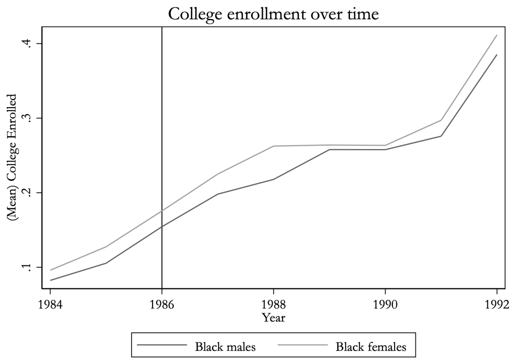
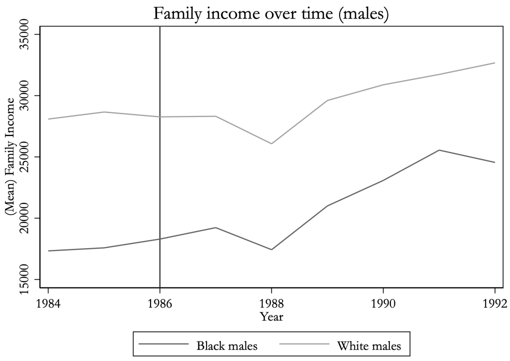
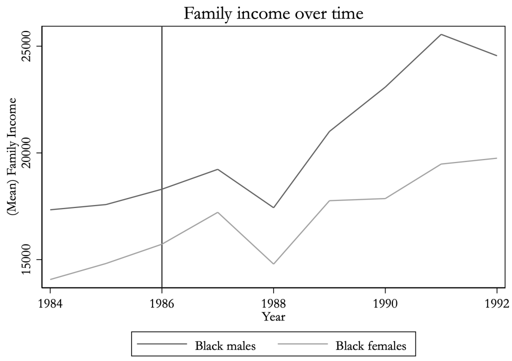


Figure A2: Family Income Around 1984

(a) By race

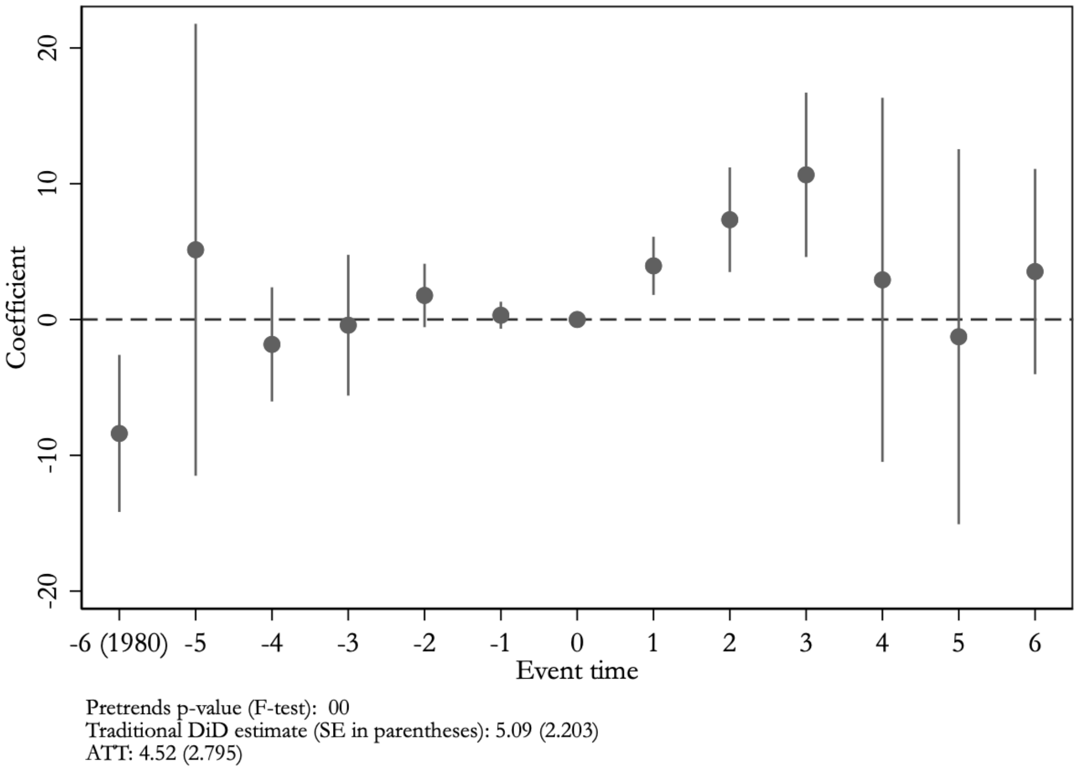


(b) By sex



Note: These figures report the outcomes for various subgroups plotted over time using CPS data from 1984-1992. Figure A1 reports the proportion enrolled in college, while figure A2 reports the average family income. A vertical line is drawn to denote the passage of the Anti-Drug Abuse Act of 1986. The universe of samples is defined as participants aged 18-24 in 1986 who were not incarcerated at the time of the survey.

Figure A3: Effect of Anti-Drug Abuse Act on Drug-related Arrest Rate of Adult Black Men, Comparing States with High and Low Black Adult Drug-Related Arrest Rates



Note: This figure reports coefficients from the estimation of equation 1 evaluating the impact of the Anti-Drug Abuse Act of 1986 on arrest rates per 100,000 related to drug violations using CPS and UCR data from 1982-1992. Event time 0 := 1986. The coefficients represent the change in outcomes for high-drug arrest states relative to non-high-drug arrest states, where high black adult drug arrest states are defined to be those above the 75th percentile in 1984. The sample is defined as black males aged 18-24 in 1986 who were not incarcerated at the time of the survey. Control variables include population and unemployment rates at the state-year level. Right tail arrest rate outliers were winsorized at the 95% level.

Table A1: CPS Summary Statistics

	(1) 1984-86	(2) 1987-92	(3) 2005-09	(4) 2010-16
Male	0.49	0.49	0.51	0.50
	(0.500)	(0.500)	(0.500)	(0.500)
Black	0.14	0.13	0.15	0.15
	(0.349)	(0.336)	(0.354)	(0.355)
HS Graduate	0.71	0.85	0.51	0.92
	(0.452)	(0.353)	(0.500)	(0.277)
Enrolled in college	0.19	0.38	0.29	0.64
	(0.393)	(0.485)	(0.454)	(0.479)
Enrolled in college (Black males)	0.02	0.04	0.03	0.08
	(0.131)	(0.185)	(0.179)	(0.277)
Enrolled in college (Non-Black males)	0.17	0.34	0.26	0.56
	(0.379)	(0.475)	(0.437)	(0.496)
Observations	46995	85648	49962	132783

mean coefficients; sd in parentheses

Note: Sample means with education supplement weights are calculated from the CPS October supplement dataset from 1984 to 1992 and 2000 to 2016. The sample in columns 1 and 2 is defined as persons aged 18-24 in 1986, and the sample in columns 3 and 4 is defined as persons aged 18-24 in 2010, both of whom were not incarcerated at the time of the survey. This table is partially replicated from Britton (2022).

Table A2: Impact of ADAA and FSA on Drug Related Arrest Rates

	(1) 1986 Adult	(2) 1986 Juvenile	(3) 2010 Adult	(4) 2010 Juvenile
Event Study Model:				
Year 6	2.837	6.987	-22.72**	-2.605***
	(3.107)	(6.2636)	(9.4129)	(.83678)
Year 5	.07729	-1.555	-23.29**	-2.827***
	(6.1943)	(2.0855)	(9.5921)	(.80989)
Year 4	3.233	-1.067	-25.61**	-4.016***
	(6.2271)	(2.2625)	(9.6893)	(1.0205)
Year 3	8.488***	3.709**	-18.16*	-2.486***
	(2.9029)	(1.6754)	(10.144)	(.75558)
Year 2	5.555***	3.03	-20.2**	-2.392***
	(1.9346)	(1.9862)	(9.2795)	(.72932)
Year 1	2.915***	2.355	-26.52**	-2.293***
	(.91789)	(1.6241)	(9.9832)	(.71605)
Year 0 (Omitted)	0	0	0	0
	(.)	(.)	(.)	(.)
Year -1	.1364	.231	-20.65***	-.9569*
	(.53346)	(1.4462)	(4.4902)	(.55974)
Year -2	1.644	-1.011	-8.319**	-.3917
	(1.0908)	(.72415)	(3.9951)	(.51897)
Year -3			-2.27	.1283
			(3.4234)	(.58941)
Year -4			3.751	.618
			(4.6076)	(.46046)
Year -5			17.75***	1.01
			(4.5919)	(.89859)
Constant	13.92**	6.116***	39.25***	7.476***
	(6.1012)	(1.942)	(3.9058)	(.63997)
Observations	414	516	765	765
Adjusted R^2	0.249	0.140	0.626	0.831
FE	Y	Y	Y	Y
Controls	Y	Y	Y	Y

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A3: Impact of ADAA and FSA on College Enrollment

	(1) 1986 Adult	(2) 1986 Juvenile	(3) 2010 Adult	(4) 2010 Juvenile
Event Study Model:				
Year 6	-.01412	.03775	-.0419***	-.02023
	(.024267)	(.024778)	(.013854)	(.016532)
Year 5	-.02811	.008487	-.05078**	-.01224
	(.024742)	(.032325)	(.022563)	(.025662)
Year 4	-.02039	-.002747	-.02378	.00897
	(.024007)	(.022231)	(.022852)	(.024388)
Year 3	-.03111	-.0271	-.01313	.01124
	(.021802)	(.020534)	(.018634)	(.025847)
Year 2	-.01971	.004192	-.01331	-.0249
	(.013986)	(.017749)	(.022645)	(.022891)
Year 1	-.0001091	.02951**	-.02966	-.05021***
	(.014423)	(.011856)	(.020499)	(.018346)
Year 0 (Omitted)	0	0	0	0
	(.)	(.)	(.)	(.)
Year -1	-.009894	.01789	-.00261	.01049
	(.018357)	(.024691)	(.022638)	(.021618)
Year -2	-.0271**	-.004498	-.02001	-.03155
	(.012496)	(.01721)	(.020303)	(.020373)
Year -3			-.05257***	-.03867**
			(.01304)	(.016954)
Year -4			-.007658	.01048
			(.014485)	(.018061)
Year -5			-.006796	.006691
			(.007688)	(.0083532)
Constant	-3.419***	-2.109***	-.4879***	-.4875***
	(.18921)	(.12571)	(.032214)	(.030151)
Observations	59749	67487	91313	91313
Adjusted R^2	0.135	0.149	0.332	0.332
FE	Y	Y	Y	Y
Controls	Y	Y	Y	Y

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A4: Impact of the Fair Sentencing Act on College Enrollment: DiD Estimates Comparing Individuals from High and Low Black Adult Drug Arrest States

	(1)	(2)	(3)
Post-2010	.0791***	.01007	0
	(.0055)	(.008337)	(.)
High-drug arrest state (AB)	-.009114	-.009613	0
	(.008626)	(.01063)	(.)
Post-2010 X High-drug arrest state	-.01609	-.01003	-.01418
	(.01032)	(.009917)	(.009063)
Constant	.154***	-1.182***	-.741***
	(.006074)	(.03851)	(.03469)
Observations	91313	91313	91313
Adjusted R^2	0.009	0.116	0.144
FE	N	N	Y
Controls	N	Y	Y

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Treated observations are defined as those living in states with a high-drug arrest rate for black adults, where high black adult drug arrest states are defined to be those above the 75th percentile in 2008. Estimates are weighted using CPS October supplement weights. Robust standard errors are clustered at the state level. Controls: age, age-squared, Latino ethnicity, yearly state average unemployment rates, and (binned) family income. The sample is defined as males aged 18-24 in 1986 who were not incarcerated at the time of the survey.

Table A5: Impact of the Fair Sentencing Act on College Enrollment: DiD Estimates Using Normalized Black Adult Drug Arrest Rate as Continuous Treatment

	(1)	(2)	(3)
Post-2010	.04163**	-.007904	0
	(.01725)	(.01266)	(.)
Drug arrest rate per 100000	-.001179***	-.0008151***	-2.05e-06
	(.0003564)	(.0002955)	(.00007)
Post-2010 x Drug arrest rate per 100000	.0004254	.0001995	-.001028**
	(.0005762)	(.0004235)	(.0004675)
Constant	.1994***	-1.144***	-.7268***
	(.0142)	(.03875)	(.03565)
Observations	90058	90058	90058
Adjusted R^2	0.014	0.118	0.144
FE	N	N	Y
Controls	N	Y	Y

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Estimates are weighted using CPS October supplement weights. Robust standard errors are clustered at the state level. Controls: age, age-squared, Latino ethnicity, yearly state average unemployment rates, and (binned) family income. The sample is defined as males aged 18-24 in 2010 who were not incarcerated at the time of the survey.

Table A6: Impact of the Anti-Drug Abuse Act on College Enrollment: Triple DiD Using Black Adult Drug-Related Arrest Rate

	(1)	(2)	(3)	(4)
Post-1986	.01797**	.01797**	0	
	(.008325)	(.008325)	(.)	
Black	-.01235	-.01235	-.006872	
	(.01223)	(.01223)	(.01337)	
Lived in high black adult drug arrest state	.0197*	.0197*	0	
	(.01066)	(.01066)	(.)	
Post-1986 X Black	-.07599***	-.07599***	-.07928***	
	(.01626)	(.01626)	(.01589)	
Black X Lived in high AB	-.03512*	-.03512*	-.04534**	
	(.0195)	(.0195)	(.02159)	
Post-1986 X Lived in high AB	-.01056	-.01056	-.01302	
	(.01363)	(.01363)	(.0128)	
Triple DiD Coefficient	.05405**	.05405**	.05935***	.06568***
	(.02121)	(.02121)	(.02127)	(.02138)
Constant	-3.009***	-3.009***	-3.402***	-3.391***
	(.1358)	(.1358)	(.1884)	(.1876)
Observations	59749	59749	59749	59748
Adjusted R^2	0.128	0.128	0.137	0.139
FE	N	N	Y	Y
Controls	N	Y	Y	Y

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Column 4 is from a regression where all the covariates excluding the triple-interaction term and the controls are included as fixed effects. Estimates are weighted using CPS October supplement weights. Robust standard errors are clustered at the state level. The controls used include age, age-squared, Latino ethnicity, and binned family income. The sample is defined as males aged 18-24 in 1986 who were not incarcerated at the time of the survey.

Table A7: Impact of the Anti-Drug Abuse Act on College Enrollment: Triple DiD Using Black Juvenile Drug-Related Arrest Rate

	(1)	(2)	(3)	(4)
Post-1986	.01637*	.01637*	0	
	(.008297)	(.008297)	(.)	
Black	-.0289**	-.0289**	-.02849**	
	(.01161)	(.01161)	(.01279)	
Lived in high black adult drug arrest state	.006094	.006094	0	
	(.01355)	(.01355)	(.)	
Post-1986 X Black	-.05564***	-.05564***	-.05759***	
	(.01542)	(.01542)	(.01502)	
Black X Lived in high AB	-.03329	-.03329	-.0443	
	(.02816)	(.02816)	(.0322)	
Post-1986 X Lived in high AB	-.002857	-.002857	-.00666	
	(.01825)	(.01825)	(.01782)	
Triple DiD Coefficient	.02763	.02763	.03134	.0398*
	(.02635)	(.02635)	(.02591)	(.02232)
Constant	-3.055***	-3.055***	-3.419***	-3.413***
	(.154)	(.154)	(.211)	(.2125)
Observations	53145	53145	53145	53144
Adjusted R^2	0.110	0.110	0.119	0.121
FE	N	N	Y	Y
Controls	N	Y	Y	Y

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Column 4 is from a regression where all the covariates excluding the triple-interaction term and the controls are included as fixed effects. Estimates are weighted using CPS October supplement weights. Robust standard errors are clustered at the state level. The controls used include age, age-squared, Latino ethnicity, and binned family income. The sample is defined as males aged 18-24 in 1986 who were not incarcerated at the time of the survey.

Table A8: Impact of the Fair Sentencing Act on College Enrollment: Triple DiD Using Black Adult Drug-Related Arrest Rate

	(1)	(2)	(3)	(4)
Post-2010	.006829	.006829	0	
	(.007323)	(.007323)	(.)	
Black	.02714*	.02714*	.03152**	
	(.01495)	(.01495)	(.01545)	
Lived in high black adult drug arrest state	-.01529	-.01529	0	
	(.01151)	(.01151)	(.)	
Post-2010 X Black	-.02294	-.02294	-.02174	
	(.01938)	(.01938)	(.01884)	
Black X Lived in high AB	-.07203***	-.07203***	-.08345***	
	(.02226)	(.02226)	(.02223)	
Post-2010 X Lived in high AB	-.003133	-.003133	-.004321	
	(.008994)	(.008994)	(.009003)	
Triple DiD Coefficient	.05003*	.05003*	.04933*	.04898*
	(.02662)	(.02662)	(.02613)	(.02756)
Constant	1.201***	1.201***	.6597***	.6617***
	(.2154)	(.2154)	(.2093)	(.1992)
Observations	90672	90672	90672	90672
Adjusted R^2	0.166	0.166	0.171	0.170
FE	N	N	Y	Y
Controls	N	Y	Y	Y

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Column 4 is from a regression where all the covariates excluding the triple-interaction term and the controls are included as fixed effects. Estimates are weighted using CPS October supplement weights. Robust standard errors are clustered at the state level. The controls used include age, age-squared, Latino ethnicity, and binned family income. The sample is defined as males aged 18-24 in 2010 who were not incarcerated at the time of the survey.