White, But not Black, Incarceration Reduces Crime

Liam F. Clegg

July 2, 2016

1 Introduction

There were over two million people incarcerated in the United States in 1999. Half of them were black, compared to 13% of the total population (Beck, 2000). In a panel of 37 U.S. states from 1979 to 1999, I find that white incarceration caused significant decreases in robbery and burglary, consistent with the predictions of an economic model of crime (Becker, 1968) and previous empirical findings (Levitt and Miles, 2007). However, black incarceration did not reduce these crimes, although evidence suggests that black criminals are responsible for a substantial portion of them (Appendix, C.1). This contradicts previous findings for overall incarceration, but I show how it could follow from features of the criminal justice system. My paper is the first to estimate the impact of incarceration on crime separately for different races, and offers a rational account of how more punishment can lead to more crime.

The 1980s and 90s in the United States saw significant changes in crime, incarceration, criminal sentencing law, and the rhetoric and public opinion surrounding all three. Nationwide, violent crime increased through the 1980s, before reversing course and dropping steadily after the early nineties (Levitt, 2004). Property crime declined throughout this period (Rennison, 2001). Incarceration increased sharply over this period (Raphael and Stoll, 2013), and African Americans bore the brunt of this increase (Western, 2006). Beginning in the mid-1970s, and continuing throught the 80s and 90s, U.S. states enacted many significant changes to their sentencing laws, including both broad overhauls of sentencing procedures and laws specifically targeted at drugs, guns, or other issues (Tonry, 1996). Crime figured heavily in the political agendas of both parties from the mid-1980s on (Alexander, 2012), and polls consistently found majorities of Americans believing that crime was increasing nationwide until after 2000 (McCarthy, 2014). By then, a growing number of researchers and political leaders were voicing concerns about "mass incarceration", and particularly it's large and disproportionate impact on the nation's young black men (Mauer, 1999).

Previous studies have estimated the impact of incarceration on crime (see Levitt and Miles, 2007, for a review); while substantial evidence suggests that incarceration affects black and white people differently (e.g. Peffley and Hurwitz, 2010). To disentangle the effects of black and white incarceration on crime, I construct incarceration rates for black men and non-Hispanic white men, and estimate the effects of these on the FBI's seven index crimes: murder, rape, robbery, assault, burglary, larceny, and car theft. Incarceration rates reflect both the probability and severity of punishment, and their sharp increase over the course of my data followed increases in both factors (Cohen, 1991). I use changes in state sentencing laws as instruments for both black and white incarceration rates. These include major overhauls to state sentencing law, as well as specific changes such as weapons enhancements, three strikes laws, mandatory minimum sentences for drugs, and the abolition of parole. The history of these laws supports the idea that their passage is exogenous to crime rates. Overidentification tests corroborate their validity, while underidentification tests confirm that they have sufficient rank to identify the effects of both black and white incarceration rates.

2 Related Literature

There are huge literatures variously related to crime, punishment, and race. For the current paper, there are two directly relevant literatures. First, there are several papers in criminology and economics of crime that feature panel regressions of crime rates on incarceration rates, as I use here. I describe the most relevant of these below. Second, there is a broad literature across various disciplines detailing differences in the way black and white Americans experience the criminal justice system in this country. I do not attempt an exhaustive review of this literature, but present a few classic and recent works which jointly suggest that it is worth asking whether the incarceration of black and white Americans might have different effects on crime rates. Finally, I review the few extant papers which address questions similar to mine.

2.1 Previous Studies of Incarceration and Crime

Beccaria (1764/1986) presents punishment as an incentive problem, and suggests that the likelihood of punishment and its severity ought to be chosen so as to be sufficient to outweigh the profits of crime. Bentham (1789/1843) presents a similar argument in the context of his utilitarian framework. Becker (1968) formalizes this intuition in his economic model of crime, in which an agent choses to commit crimes if the profit minus the expected sanction cost exceeds his potential earnings in the licit labor market. He models the expected sanction cost as the product of the probability and severity of punishment, and thence concludes that any inconsistency in the application of punishments may be compensated by making them harsher.

Beginning in the 1970s, many researchers attempted to measure what impact prison populations had on crime, estimating some variant of the linear model

$$\Delta \ln \mathcal{O}_{it} = \beta \Delta \ln X_{it-1} + C'_{it-1}\gamma + \epsilon_{it}$$

where $\mathcal{O}$ is some crime rate, X is the incarceration rate, C is a vector of controls and state and year fixed effects, and ϵ is a random error (see Nagin, 2013, for a review). The question is naturally fraught with simultaneity, as crime rates have an obvious impact on the number of people in prison (Blumstein et al., 1978). Marvell and Moody (1994) use multiple lags and Granger causality tests in an attempt to isolate the impact of incarceration on crime, and estimate an overall prison elasticity of crime of -0.159 for 1971–1989, driven mainly by elasticities of -0.2 to -0.26 for robbery, burglary, and car theft. Levitt (1996) uses the progress of prison overcrowding litigation as instruments for the incarceration rate and finds significantly larger elasticities, particularly for property crime, for 1971–1993. Levitt's paper was the first to address the simultaneity of prison and crime using instrumental variables, but the results are of questionable external validity, since they arguably measure the effects of isolated shocks to the prison population which may not affect would-be criminals' subjective perceptions of the sanction regime (Doob and Webster, 2003). Persistent changes in incarceration due to policy changes may not have the same effects. Johnson and Raphael (2012) model the temporal dynamics of crime and incarceration rates to construct synthetic instruments for changes in the incarceration rate. They compute the steady state incarceration rate in each state-year as a function of current year prison entries and exits, and then use the difference between this and the actual current incarceration rate as an instrument for next year's change in incarceration. They find significant effects for most crimes, but smaller than those Levitt found. Their data cover 1978–2004, and they find their effects to be significantly stronger in the first half of the time period.

Results of these three papers for the seven index crimes are reproduced in Table 1. These researchers all find significant, negative effects, but there is little consistent pattern in the relative effect sizes on different crimes. When estimating the effect of the total incarceration rate, I find similar results to these previous studies. For example, I find elasticities of -0.43, -0.15, and -0.18 for robbery, burglary, and larceny (Table 8), statistically indistinguishable from the -0.33, -0.29, and -0.18 found by Johnson and Raphael (2012) using their completely different identification strategy.

	Marvell & Moody (1994)		Levitt (1996)		Johnson & Raphael (2012)	
Murder	-0.065	(0.085)	-0.147	(0.373)	-0.193	(0.231)
Rape	-0.113*	(0.052)	-0.246	0.250)	-0.277*	(0.126)
Robbery	0.056	(0.053)	-0.703*	(0.309)	-0.334†	(0.200)
Assault	-0.260***	(0.059)	-0.410†	(0.249)	.031	(0.167)
Burglary	-0.253***	(0.031)	-0.401*	(0.172)	-.288**	(0.106)
Larceny	-0.138***	(0.026)	-0.277†	(0.147)	-.181***	(0.031)
Car Theft	-0.200***	(0.048)	-0.259	(0.235)	-0.291†	(0.163)

Standard errors in parentheses

† $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 1: Incarceration rate elasticities of crime rates from three studies. Regressions in all three papers include state and year fixed effects and economic and demographic controls, and report standard errors clustered at the state level. Since Johnson & Raphael report estimates in levels, the elasticities associated with them in the table are constructed by multiplying each of their estimates by the ratio of the relevant sample means.

2.2 Punishment in the Context of History

There are several circumstances in which increased incarceration may not reduce crime rates, three of which plausibly apply to African Americans. The first is an exception to the central assumption of Becker (1968):

> Practically all the diverse theories agree, however, that when other variables are held constant, an increase in a persons probability of conviction or punishment if convicted would generally decrease [...] the number of offenses he commits. (p. 176)

While this ought to hold for decisions between obeying the law and not, it need not hold for decisions between less and more serious crimes: an increase in the penalty for a lesser crime reduces the marginal deterrent effect of the more serious consequences for a worse crime (Stigler, 1970). As a corollary, if a rational person expects a significant likelihood of punishment in all feasible scenarios, then the threat of punishment imposes a small marginal cost on crime.

Second, simply locking people up need not have a meaningful incapacitation effect. To the extent that incarceration is arbitrary, the people thereby prevented from committing crimes are not those most likely to commit crimes.

Both these reasons fit into the traditional Becker (1968) or Stigler (1970) models of crime, which assume perfect information. Yet potential criminals do not in fact have perfect information, and must continuously learn about the probability and severity of punishment (Kleck et al., 2005). This yields a third reason why more prisoners might not lead to less crime, since a rational Bayesian who perceives the justice system to arbitrarily incarcerate people does not learn much about the sanction regime from seeing someone arrested and sent to prison. All three fit under the same basic point: if punishment does not predictably follow behavior, a rational person may not respond to punishment.

Any of these three reasons could be sufficient to generate my empirical results, and all three held for some black Americans at the end of the twentieth century. For instance, under the Anti-Drug Abuse Act of 1986, 5 grams of crack cocaine carried a mandatory minimum sentence of 5 years without parole in federal prison. Once the act was passed, black people caught with crack were routinely tried in federal court (Berk, 1993), where they faced such minimum sentences. Meanwhile, a person could expect less time for any other crime short of murder: among state prisoners released in 1990, those serving time for rape had served an average of 55 months; for robbery, 41 months; and for property crimes, less than two years (Ditton and Wilson, 1999). For the most marginalized blacks, laws like this added up a situation where punishment seemed inevitable: Western (2006) finds that fully 60% of black male high school drop-outs who came of age at the peak of the 1980s-90s crime wave had spent time in prison by age 35.

In light of numbers like these, scholars of race have argued that the United States treats black people as presumed criminals (e.g. Armour, 1994), a view shared by many black Americans. The poll results reported in Table 2 are typical: when asked how much confidence they have in police to treat blacks and whites equally, 35% of blacks responded "Very little" and only 22% responded a "Great deal."

White Americans' relative trust in the law is well founded. For example, Berk (1993) examines the race and ethnicity of those arrested or charged for selling crack in Los Angeles in 1990–92, finding that while whites constituted 3% each of persons arrested and cases handled by the District Attorney, none was charged in federal court. Meanwhile, blacks accounted for 58% of arrests, 53% of the DA's cases, and

How much confidence do you have in police officers in your community to treat blacks and whites equally?

	White	Black
Great deal/Fair amount	77%	50%
Just some	10%	15%
Very little	11%	35%

Table 2: McClatchy-Marist Poll, Dec 2014.

83% of federal indictments. Four of these black men brought a selective prosecution claim against the U.S. Attorney, based on concordant evidence: in 24 federal crack cases recently tried by the federal public defender, all the defendants had been black.[1] The plaintiffs alleged that the federal prosecutor's office was sending white defendants to county prosecutors, where they would face shorter terms in state prison, and prosecuting only black defendants in federal court. Thus 5 years for 5 grams was not a fate white Americans particularly needed to fear.

Long before the War on Drugs, slavery criminalized blackness in America. A 1691 act of the Virginia legislature "empowered and commanded" lawmen to apprehend "negroes, mulattoes, or other slaves" reported to be "lying out" (Hening, 1823, p. 86). The act also forbade interracial procreation and the freeing of slaves, thus ensuring that any dark-skinned person encountered at large could be considered a criminal. Blackmon (2009) describes how after emancipation, southern states found ways to convict black men by the thousands and then lease them out for manual labor, in conditions sometimes worse than slavery. Vagrancy laws, which made it illegal to be unemployed, were a common tactic (p. 53). Muhammad (2010) documents a concurrent process by which early twentieth century social scientists took blacks' overrepresentation in the criminal justice system as evidence of their inherent

[1] United States v. Armstrong et al., 517 U.S. 456 687 (1996)

inferiority. Throughout the twentieth century, the prevailing counterargument was that black crime merely followed black socioeconomic deprivation; e.g. Moynihan (1965) wrote in his influential report that "the combined impact of poverty, failure, and isolation among Negro youth has had the predictable outcome in a disastrous delinquency and crime rate" (Ch. IV). A rare exception, cited by Muhammad (2010, p. 43), was the prison doctor M.V. Ball, who wrote in 1894 that "in the South, where lynch-law is most commonly dealt out to the negro, we might attempt to ascribe this greater criminality to lack of fair treatment, and prejudice on the part of the white man."

2.3 Studies of deterrence which allowed for different effects by race

Among the many earlier attempts at estimating the impact of incarceration on crime, one study allowed for racial differences, though not in the way I do here. Sampson (1986) constructs homicide and robbery offending rates for black and white juveniles and adults, and estimates the effects of jail and prison incarceration rates on each. He uses a sample of 156 large American cities in 1980. He constructs race-specific offending rates from race-specific arrest rates and overall offense rates by assuming arrests per offense are equal for blacks and whites, and defends this assumption for robbery and homicide. He does not, however, disaggregate incarceration rates by race. Indeed, his identification strategy does not allow it: he uses rated jail capacity per 1000 violent offenses as an instrument for jail population per 1000 violent offenses, and calls this local incarceration risk. He claims to avoid the common problem of spurious correlations between ratios[2] since there are "no common terms" (p. 288) in his offense and sanction variables, but this is disingenuous: in 1980, 42% of reported violent crimes were robberies; across states in that year the robbery rate and the violent crime rate are correlated at $r = 0.94$.[3] Sampson's large, negative estimates for the effect of local incarceration risk on black and white adult robbery offending (Sampson, 1986, Table 6) are therefore likely suprious. Furthermore, the sort of racial difference he considers is that black and white would-be robbers respond differently to the overall likelihood of going to jail; his analysis does not allow for an impact of the racial make-up of the incarcerated population. In a follow-up paper, Sampson and Cohen (1988) construct race-specific measures of police aggressiveness, defined as disorderly conduct and DUI arrests per officer, and estimate their effects on race-specific offending rates. They find significant negative effects, and effects twice as large for blacks as for whites. However, the cities in Sampson and Cohen's sample have on average nearly three times as many white as black residents, and they use log offending rates but levels of the police aggressiveness measure, so the magnitudes of the resulting coefficients are not directly comparable. In a comprehensive review, Nagin (1998) cites this latter study as "the only ecological deterrence study I know of that attempts to estimate deterrent effects across subpopulation groups" (p. 30). A more recent review by the same author mentions no further such studies (Nagin, 2013).

[2]Given variables a and b, if b is measured with error, then any estimate of the effect of $\frac{a}{b}$ on b will be negatively biased. In this context, a is prisoners and b is crimes, both in rates per capita, and b is measured with error because not all crimes are reported.

[3]In the notation of previous footnote, Sampson estimates the effect of $\frac{a}{d}$ on b, where $d = b + c$.

3 Background on Sentencing, Prison, and Crime

The political determinants of sentencing laws make them ideal instruments for studying crime. Since voters' views on crime are have generally been found to vary independently of whether crime has recently gotten better or worse (Roberts and Stalans, 1997), year-to-year variations in local crime rates are unlikely to affect the actions of a politician primarily interested in reelection. Furthermore, given national trends in sentencing reform, the modal law in the period I study was passed at a time when there was little history with which to predict its effects. Therefore, even a politician seeking some particular outcome could only make an educated guess about which policy she ought to support. Since different states passed different laws, and since they passed laws in different years, there is substantial temporal and cross-sectional variation in the state sentencing laws in my data.

All states changed their sentencing laws during the period I study, in three rough phases. Beginning in 1975, the sentencing reform movement imposed new constraints on judges and prison administrators, changing sentencing for all crimes. Amidst the 1980s war on drugs, many states passed harsh penalties for drug crimes. After violent crime rose from the 1980s into the 1990s, states got "tough on crime" with laws designed to keep more offenders locked up. These national political trends, rather than conditions in individual states, were the main drivers of many sentencing reforms. As this history thus provides important context for my identification strategy, I review it here.

For most of the twentieth century, incarceration in the United States followed a rehabilitation model (Allen, 1981). Accordingly, sentencing was highly flexible: judges had broad authority to sentence convicted defendants, that they might hand down sentences individually tailored to each convict's needs. Parole boards then decided when prisoners actually got out, which was supposed to be whenever they were sufficiently reformed.

In the early 1970s the system faced substantial criticism from both sides of the political spectrum. Conservatives railed against the leniency of judges who let offenders off easy, while liberals decried the disparate treatment of minorities and the poor (Tonry, 1996). The discontent coalesced into a reform movement with the appearance of Judge Marvin Frankel's *Criminal Sentences: Law without Order* (1973). Though hard data was scarce, Judge Frankel compiled enough individual cases to make a compelling argument for change. He also made two specific proposals that would be widely adopted in the following decades. First, that governments adopt *structured sentencing*, wherein the judge in each case is allowed a narrow range of sentences as a function of the crime and the defendant's prior record. By comparison, the federal code at the time let judges sentence bank robbers to any sentence of "not more than twenty-five years." Second, he suggested that states create permanent *sentencing commissions*, which would continuously study sentencing and have some authority to revise sentencing laws.

Minnesota created the first sentencing commission in 1975, and within a decade all states and the federal government had adopted some sort of sentencing reform (Tonry, 1996). Many states moved to *determinate sentencing*, with sentences of fixed duration replacing ranges like "twenty to life." Many also adopted some form of structured sentencing, typically including *sentencing guidelines*: these consist of a grid with prior convictions along the top and crime severity down the side, and narrow ranges in each cell. Some states adopted *voluntary guidelines*, which served as a coordination device but imposed no legal constraints on sentencing judges. Others adopted *presumptive guidelines*, mandating that a judge sentence a convicted defendant within a "presumptive range" or else state some reason for giving a longer or shorter sentence. *Presumptive sentencing* takes this a step farther, legislating a fixed sentence for each crime and prior record pair, absent mitigating or aggravating circumstances.

Once discretion over criminal sentencing had been removed from the courtroom, lawmakers could use sentencing legislation to address practical concerns about crime and prisons, or to respond to political demands. As public concern about crime and drugs grew, sentencing reform gave way to mandatory minimums for drug crimes, sentence enhancements for guns, and three strikes laws. Researchers consistently find that public perceptions of crime in the United States have almost no connection to actual crime rates (Roberts and Stalans, 1997), and accordingly the politics of sentencing legislation are generally disconnected from the reality on the street.

Stemen (2007) finds that the determinants of major sentencing reforms, such as determinate and structured sentencing, were states' fiscal positions and legislators' capacity for bipartisan cooperation. Alongside these substantive changes were policies he labels "expressive," which he argues served mainly to assert a moral stance. Such laws included the mandatory minimums and sentencing enhancements which condemned drugs and violence, and these laws were not consistently associated with any particular political or statistical trends.

3.1 Data

In 1980, the United States population was 80% non-Hispanic white and 12% African American. By 2000, these numbers were 70% and 13%, with much of the change attributable to a rapidly increasing Hispanic population. For this paper, I consider only two groups: black men and non-Hispanic white men. Men commit the vast majority of most crimes and account for the vast majority of prisoners. Female incarceration also responds differently to legislative changes, and as female prisoners are typically housed in separate women's prisons they may be subject to different crowding pressures. Although increasing numbers of Americans, including prisoners, are Hispanic, this was not recorded consistently for much of my study period. This makes it impossible to construct meaningful Hispanic incarceration rates.[4] No other racial or ethnic group constitutes a sizable fraction of prisoners in more than one state.[5] Of the male prisoners in my dataset, on average 48% are black and 40% are non-Hispanic white. In what follows, "prisoners" means "male prisoners", "white" means "non-Hispanic white", and "black" and "African American" both mean "black."

For this paper I begin with a panel of 50 states and 21 years, from 1979–1999. The Bureau of Justice Statistics began collecting data on the race of prisoners in state prisons in 1978, and as I use once-lagged incarceration rates my dataset begins in the following year. The end year is somewhat arbitrary, selected based on previous findings that the effectiveness of prison has decreased over time (Johnson and Raphael, 2012). Using one to three more or fewer years does not substantially change the results. I drop the four states which have significant missing incarceration rate data, plus nine states where the population was less than 1% black at some point in my period of study. The 37 remaining states are listed in Appendix Table 19. A few missing data points leave 771 observations, with similar variance in log black and white incarceration rates. Summary statistics for crime rates, incarceration rates,

[4]Though this uncertainty has a symmetric effect on non-Hispanic counts, the relative noise it introduces is much smaller, since fewer than 10% of whites and fewer than 1% of blacks were Hispanic in most of my observations.

[5]A substantial fraction of prisoners in Hawaii are classified as "Native Hawaiian or other Pacific Islander"; many in North Dakota as "American Indian or Alaska Native."

	Logs		Levels			
Variable	Mean	Std. Dev.	Mean	Std. Dev.	Min.	Max.
Murder	1.96	0.50	8	4	2	20
Rape	3.58	0.39	38	15	10	102
Robbery	4.94	0.55	162	89	31	461
Assault	5.69	0.48	329	148	65	786
Burglary	7.05	0.32	1,213	390	531	2,907
Larceny	7.98	0.25	3,011	712	1236	5,106
Car Theft	5.98	0.45	436	192	142	1,158
White male incarceration	5.53	0.49	283	137	68	791
Black male incarceration	7.58	0.46	2,175	984	453	5,646
Unemployment Rate			6.6	2.2	2.2	18.0
% Population Age 15–17			4.6	0.6	3.5	6.6
% Population Age 18–24			11.3	1.6	7.9	15.7
% Population Age 25–34			16.4	1.7	12.0	23.6
N	771					

Table 3: Summary statistics. Crimes are all annual rates per 100,000 population. Incarceration rates are per 100,000 of the relevant population. Demographics are percentages.

and controls are given in Table 3.

Crime rates come from the FBI's Uniform Crime Reports (UCR). While these only capture reported crime and do not include every jurisdiction in the country, the data have been cleaned and checked by statisticians at the Bureau of Justice Statistics, and they provide the best information available on crime trends. I construct black and white incarceration rates from the National Prisoner Statistics, following the procedure described in the appendix. Sentencing laws are mostly taken from Stemen (2007), though some I compiled myself from state session laws and other secondary sources, also described in the appendix.

4 Estimation and Identification

As I describe in the introduction, there are several reasons why black and white incarceration rates may affect crime differently. For this paper, I do not attempt to differentiate between them, and so I take a reduced form approach. I model each state-level annual log crime rate as a linear function of the log white incarceration rate, the log black incarceration rate, economic and demographic controls, state and year fixed effects, and a random error:

$$y_{it}^{crime} = \beta^W X_{it-1}^W + \beta^B X_{it-1}^B + \boldsymbol{C'_{it-1}\gamma} + \delta_i + \eta_t + \epsilon_{it}.$$

As such, I look at variation over time of incarceration and crime within each state, in order to estimate the impact of the former on the latter.

All regressions include standard errors clustered at the state level, and so allow for heteroskedastic errors and correlation between errors within each state. With 37 clusters, there is potential concern with the standard interpretation of clustered standard errors. Cameron and Miller (2015) consider theoretical and simulation evidence and conclude that the threshold for "too few" clusters in balanced panels is between 20 and 50. In marginal cases, too few clusters produces downward bias in the standard errors of estimated coefficients. Therefore, the number of clusters here suggests caution in interpreting marginally significant results. However, my main results for robbery are statistically significant at the 1% level, so even if the standard errors do include some downward bias this is not what is generating the results.

4.1 Sentencing Laws are Exogenous to Crime Rates and Predict Incarceration Rates

Changes in sentencing legislation often follow national trends caused by national politics or federal incentives, and precisely which laws each state passes in each year varies significantly with the local political climate. Stemen (2007) finds that sentencing laws vary with the governor's political party, the average experience of state legislators, and the state's fiscal health. Crucially, though, he finds little relation between sentencing legislation and changes in crime rates or in prison utilization. Furthermore, as I show below different laws have had different effects on the black and white incarceration rates. Based on exploratory analysis, I use thrice lagged values of all laws in the first stage regressions. Since the median prison term in the United States is roughly three years (Ditton and Wilson, 1999), this means I estimate incarceration rates each year from laws under which the majority of those in prison that year were sentenced. Using lags of one, two, or four years does not yield substantially different results.

In theory, the fixed size of prisons might generate spurious correlations between incarceration rates, crime, and sentencing legislation: once the state prison is full, if crime continues to increase while the population grows, there will appear a negative correlation between incarceration rates and crime. At the same time, lawmakers might respond to crowded prisons with legislation designed to divert some offenders elsewhere. However, the ubiquitous civil rights lawsuits and prolific prison construction that began in the 1970s suggest a persistent willingness by states and the public to overload prisons and build more of them, so that the flow of prisoners is generally independent of the stock. (Guetzkow and Schoon, 2015). A rare exception is Florida's

Safe Streets Act of 1994 (Fl. Laws 93-406): at the height of the tough-on-crime era, Florida was releasing violent offenders early, to make room in the state's overflowing prisons for the newly sentenced. This act repealed mandatory minimum sentences for drug crimes and other non-violent first offenders so that violent criminals could be sent away for longer (Lee, 1993). In keeping with the general pattern, though, Florida soon built new prisons, passed several harsh sentencing laws, and reinstated mandatory minimums for drug trafficking in 1999 (Fl. Laws 99-188).

I use 17 types of laws. Stemen et al. (2005) finds that interactions between determinate sentencing and various forms of structured sentencing are strong predictors of incarceration, so I include five such interactions as well. Table 4 shows regressions of black male, white male, and total male rates on all 22 sentencing instruments. Determinate sentencing, which curtails prison officials' discretion in when prisoners get out, significantly decreased the white incarceration rate, but had a smaller impact on black incarceration. Presumptive sentencing, which constrains judges discretion in imposing sentences, had the inverse effect, increasing the white incarceration rate with no significant effect on the black incarceration rate. The combination of determinate and structured sentencing make the time a person spends in prison depend almost entirely on the charges pled to or convicted of, thus removing most opportunities for racial bias to enter the system after the charging stage. Accordingly, the interaction of determinate sentencing and presumptive guidelines had the strongest effect on the black incarceration rate. However, only six states had both of these policies in place at some point in the period I examine: Florida, Kansas, Minnesota, North Carolina, Oregon, and Washington.

The most obvious difference between the results in the first two columns is the greater number of significant coefficients in the left one; 7 vs. 5 at the 5% level and

11 vs. 5 at the 10% level. This particular specification, log per-capita incarceration rates weighted by the relevant state population, because these are not too different; in other specifications the difference is even more severe. For example, with level (vs. log) prisoners per capita as the dependent variable, weighting by black or white male population, the white male rate has six significant regressors at the 5% level—Determinate Sentencing, Presumptive Guidelines, Determinate × Presumptive Sentencing, Determinate Sentencing × Two Strikes, Sentencing Commission, Weapon Enhancement—while the black rate only has one, Two Strikes. This is consistent with a model in which the white incarceration rate is more tightly coupled to the law, while the black rate is subject to greater control by individual actors in the justice system.

Looking at the individual laws, white but not black incarceration increased in response to structured sentencing reforms including presumptive sentencing and voluntary guidelines. This picture at the state level accords with the finding of Ulmer et al. (2011) at the federal level, that judicial discretion increases racial disparity in federal sentencing because judges use their discretion to sentence more white defendants to shorter or alternative sentences. As the shortest sentences are thus imposed on mostly white defendants, a mandatory minimum can have a strong effect on the white incarceration rate, but only a weak one on the black rate.

Overall, these results accord with part of the conventional wisdom about sentencing reform: determinate and structured sentencing reduce racial disparity (Tonry, 1996). However, they do not support the story that harsh, mandatory sentences for drug crimes were the major causes of the sharp increase in black incarceration during the 1980s and 90s (Tonry, 2011), since the estimated elasticities of the black incarceration rate with respect to the two drug laws are tiny and insignificant.[6]

[6]It could be that many states passed strict drug laws, but the extent of their impact depended mainly on local choices about enforcement priorities. It is also possible that various sentencing

Log Prisoners Per Captia:	White Male	Black Male	Total Male
Determinate Sentencing	-0.34***	-0.23***	-0.26***
Presumptive Guidelines	0.018	0.041	0.052
Presumptive Sentencing	0.17*	0.081	0.13*
Voluntary Guidelines	0.17*	0.016	0.076
2 Strikes Law	0.086*	0.13**	0.11*
3 Strikes Law	-0.032	-0.0097	0.0070
D.S. × Presump Guid.	-0.18	-0.32**	-0.29*
D.S. × Presump. Sent.	0.23†	0.17	0.14
D.S. × Vol. Guid.	-0.24†	-0.11	-0.17
D.S. × 2 Strikes	0.18**	0.086	0.13*
D.S. × 3 Strikes	-0.12	-0.035	-0.11
Sentencing Commission	-0.079*	-0.040	-0.039
Weapon Enhancement	0.093*	0.049	0.051
Cocaine Mandatory Minimum	-0.063	-0.033	-0.084*
Cocaine School Enh.	-0.014	0.011	-0.020
Uniformed Enh.	0.052	0.077*	0.042
Community Corr.	-0.0081	-0.040	-0.037
Probation Fee	0.020	0.014	0.021
Parole Fee	-0.032	-0.012	-0.0062
Abolished Parole	0.063	0.22*	0.15†
Megan's Law	-0.057†	-0.017	-0.060*
Truth in Sentencing	0.079†	-0.015	0.013
%Unemployed	-0.0047	-0.00027	0.00031
%15–17	-0.022	0.085	-0.058
%18–24	0.014	0.094*	0.029
%25–34	-0.059	-0.046	-0.032
N	771	771	771
R^2	0.95	0.95	0.96

Standard errors clustered at the state level and robust to heteroskedasticity

† $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < .001$

Table 4: Regressions of white male, black male, and total male incarceration rates on all instruments, controls, and state and year fixed effects.

4.2 First Stage Results

I use the Lasso method of Belloni et al. (2012) to select the best predictors of each incarceration rate from among the full set of available instruments. The main text describes results of two-stage least-squares with the union of the selected instruments, while the appendix includes results of their full procedure of estimating optimal instruments.

The first two columns of Table 5 show results of the regressions of white and black incarceration rates on the sentencing variables selected for each by Lasso. All sentencing variables are three year lags, so these regressions capture both certainty and severity effects. Determinate sentencing reduced incarceration for both groups, but especially for whites. This is consistent with the possibility that parole boards and prison wardens are less racially biased than judges and prosecutors, given that determinate sentencing shifts discretion from the former group to the latter. This, in turn, is consistent with statistical discrimination (Phelps, 1972), since prison officials presumably have more information about each prisoner than do judges and therefore have less reason to rely on racial stereotypes. Presumptive sentencing significantly increased incarceration rates for whites, but not for blacks. Again, this is consistent with previous research finding that judges tend to give short or alternative sentences to whites more more often than to blacks when they have the discretion to do so (Ulmer et al., 2011). The same interpretation applies to the estimated effects of two strikes laws, which impose minimum sentences on offenders with prior convictions. Results for the interaction of determinate sentencing and presumptive guidelines show that

reforms were adopted by people who wanted, consciously or subconsciously, to lock up more young black men. Then, the legislative changes and their associated public rhetoric defined a tough-on-crime climate in which hyperpolicing of black neighborhoods and a retreat from leniency for young offenders were politically feasible, and these individual and agency-level changes were the true drivers of increased black incarceration.

in states which adopted presumptive guidelines, determinate sentencing had similar effects for blacks and whites.

Underidentification tests suggest that the instruments can identify the separate effects of both incarceration rates. The Kleibergen-Paap Wald test gives $\chi^2(4) = 12.48, p = .01$, rejecting the null that the matrix $X'P_ZX$ lacks full column rank and the model is thus underidentified. This test is similar to the traditional Cragg-Donald test, but overcomes that test's sensitivity to the order of variables (Kleibergen and Paap, 2006). As shown in the following section, Hansen J statistics testing overidentifying restrictions are all close to zero, allowing that these sentencing laws are fully orthogonal to the second stage errors in all seven index crime equations.

Log Prisoners Per Captia:	White Male	Black Male	White Male	Black Male
Determinate Sentencing	-0.25***	-0.14*	-0.25***	-0.15*
	(0.041)	(0.059)	(0.049)	(0.057)
Presumptive Sentencing	0.20**		0.20**	0.11†
	(0.062)		(0.062)	(0.061)
Two Strikes Law	0.11*		0.11*	0.066
	(0.041)		(0.039)	(0.048)
Determinate Sentencing		-0.16*	0.0020	-0.13†
× Presumptive Guidelines		(0.079)	(0.058)	(0.077)
Sentencing Commission	-0.055†		-0.056†	-0.0035
	(0.028)		(0.028)	(0.032)
%Unemployment	-0.0069	0.0038	-0.0069	0.0039
	(0.0099)	(0.0078)	(0.0097)	(0.0078)
%15-17	0.041	-0.037	0.041	-0.013
	(0.093)	(0.11)	(0.090)	(0.10)
%18-24	0.00095	0.084**	0.0010	0.085**
	(0.019)	(0.026)	(0.019)	(0.027)
%25-34	0.013	0.019	0.013	0.025
	(0.020)	(0.024)	(0.020)	(0.021)
R^2	0.94	0.92	0.87	0.88
F	13.4	15.8	11.9	7.78

N=771. Standard errors clustered at the state level.

† $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < .001$

Table 5: Regressions of white and black incarceration rates on the sentencing laws selected as the best predictors of each.

5 Main Results on Race, Incarceration and Crime

5.1 Second Stage Estimation Procedure

Using fitted values of both incarceration rates from the first stage, I estimate the impacts of both on the seven index crime rates. Murder is murder and non-negligent manslaughter. Rape is forcible rape, as defined by Blackstone: carnal knowledge of a woman by a man, forcibly and against her will. Robbery is theft from a person, by violence or threat of violence. Assault is any other act or threat of violence against a person. Burglary is theft with trespass or breaking and entering. Larceny is theft of anything other than a motor vehicle which is neither robbery nor burglary. Car theft is theft of an unoccupied motor vehicle.

The crimes are sometimes aggregated into violent and property crimes, but this has several problems. First, it conflates crimes with much different presumable motivations: do murder, rape, and robbery follow similar enough decision processes to respond similarly to changes in punishment? Second, given the relative frequencies with which the crimes are reported, the variation in violent and property crime are mostly driven by variation in assault and larceny, respectively (cf. Table 3). Third, it washes out substitution effects; an overall switch from burglary to larceny could increase the property crime rate but would constitute a significant improvement in public safety.

	Murder	Rape	Robbery	Assault	Burglary	Larceny	Car Theft
White Male	-0.045	-0.19	-0.48**	-0.019	-0.23*	-0.21**	-0.079
Incarceration	(0.10)	(0.12)	(0.14)	(0.095)	(0.091)	(0.073)	(0.12)
Black Male	-0.17†	0.10	0.11	-0.071	-0.019	0.045	-0.38**
Incarceration	(0.087)	(0.081)	(0.11)	(0.11)	(0.069)	(0.050)	(0.14)

N=771; Standard errors in clustered at the state level.

† $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 6: OLS regressions of crime rates on black and white incarceration rates. All regressions include state and year fixed effects and demographic controls.

5.2 Main Results

Table 6 shows results of the ordinary least squares regressions of crime rates on black and white incarceration rates, estimating the equation

$$y_{it}^{crime} = \beta^W X_{it-1}^W + \beta^B X_{it-1}^B + \boldsymbol{C'_{it-1}\gamma} + \delta_i + \eta_t + \epsilon_{it}.$$

The OLS results foreshadow the IV results, with significant negative coefficients on the white incarceration rate for robbery, burglary, and larceny. Coefficients on the black rate are statistically zero for most crimes.

Table 7 shows the main results of the paper. These are estimates of the second-stage regression

$$y_{it}^{crime} = \beta^W \widehat{X}_{it-1}^W + \beta^B \widehat{X}_{it-1}^B + \boldsymbol{C'_{it-1}\gamma} + \delta_i + \eta_t + \epsilon_{it}$$

for each of the seven index crimes. For robbery and burglary, the estimated elasticities with respect to the white incarceration rate are large, negative, and significant. They suggest that a 10% increase in the white incarceration rate associated with changes in sentencing laws on average reduces robbery by 15% and burglary by 9%

in the following year. This is consistent with a standard economic model of crime, assuming that white prisoners and white robbers and burglars are drawn from the same population. Meanwhile, a similar increase in the black incarceration rate causes comparable increases in these crimes.

While all crimes reflect decisions at some level, we may expect crimes of theft to follow greater deliberation and less accident of circumstance than murder, rape, or assault. The reported larceny rate is about three times the reported burglary rate, but more people go to prison for burglary than for larceny (Ditton and Wilson, 1999), suggesting that marginal changes in incarceration policy might have a greater impact on decisions about burglary.

To rule out the possibility that my results are driven by some peculiarities of the states and years used, I do the analysis using a single X, the total male incarceration rate. Table 8 shows estimates of the effect of total male incarceration on crime using two-stage least squares with the five sentencing instruments used in the main text. The first-stage F-stat here is 13.9. With the exception of car theft, all estimated crime reduction effects of incarceration are close to those found in previous studies, particularly Johnson and Raphael (2012): for murder, rape, robbery, assault, and larceny, my estimated coefficients are within a fraction of a standard deviation of theirs. This despite the fact that they use a completely different identification strategy than I use.

5.3 Robustness

The data-driven penalty in Belloni et al. (2012) replaces the investigator-chosen penalty parameter in the traditional Lasso estimator (Tibshirani, 1996). One possible concern is that the penalty term is nonetheless driving the results. To check this, I

	Murder	Rape	Robbery	Assault	Burglary	Larceny	Car Theft
White	0.28	0.14	-1.54**	-0.19	-0.88*	-0.26	-0.43
Incarc.	(0.46)	(0.56)	(0.51)	(0.47)	(0.37)	(0.24)	(0.46)
Black	-0.55	-0.43	1.68***	0.23	1.07**	0.17	-0.12
Incarc.	(0.73)	(0.76)	(0.51)	(0.63)	(0.40)	(0.28)	(0.51)
%Unemp.	-0.016	-0.031*	-0.023	-0.0020	0.00023	0.0045	0.0010
	(0.011)	(0.014)	(0.016)	(0.0086)	(0.0099)	(0.0062)	(0.011)
%15-17	-0.065	0.053	0.14	0.13	0.22*	0.17***	0.087
	(0.085)	(0.10)	(0.13)	(0.098)	(0.10)	(0.050)	(0.10)
%18-24	0.076	0.040	-0.086	0.051	-0.020	0.045	0.10*
	(0.080)	(0.067)	(0.066)	(0.061)	(0.042)	(0.030)	(0.047)
%25-34	0.034†	0.052*	0.023	0.036	0.036	0.047***	0.057
	(0.018)	(0.025)	(0.036)	(0.027)	(0.030)	(0.014)	(0.037)
$p : \widehat{\beta}^W = \widehat{\beta}^B$	0.48	0.66	0.0027	0.70	0.015	0.41	0.75
p: Hansen's J	0.64	0.71	0.77	0.38	0.79	0.54	0.87

N=771. Standard errors clustered at the state level.

† $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 7: Second stage results. The second row from the bottom gives the p-value for the F-statistic under the null hypothesis that $\beta^W = \beta^B$; i.e., it tests the null that the effects of black and white incarceration rates on crime rates are the same. The bottom row gives the p-value for the Hansen J statistic testing overidentifying restrictions; it gives no evidence that the instruments are endogenous.

	Murder	Rape	Robbery	Assault	Burglary	Larceny	Car Theft
Incarc.	-0.15	-0.21	-0.43*	-0.041	-0.15	-0.18†	-0.59**
	(0.23)	(0.24)	(0.21)	(0.20)	(0.15)	(0.095)	(0.19)

N=771; Standard errors clustered at the state level.

† $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 8: Second-stage estimates using total male incarceration identified by the five sentencing instruments used in the main results.

vary the penalty multiplier c above and below its recommended value so as to include fewer or more instruments. The main results use the five instruments selected when using the tuning parameter $c = 1.1$ as recommended by Belloni et al. (2012). Table 9 shows second-stage results for robbery using instruments selected at various higher and lower values of c; the pattern of results is consistent across these. Theoretically, c should be just greater than 1; values of 1 and 1.25 select the same five instruments as the recommended 1.1, so the results do not depend on this specific choice. Reducing c causes more, weaker instruments to be included, and thus attenuates the estimated coefficients. Raising c sufficiently above 1 causes fewer instruments to be selected; at $c = 1.5$ Lasso selects only two instruments and the fitted values of the black and white incarceration rates appear collinear. At this penalty level, the same two instruments are selected for the black incarceration rate, determinate sentencing and the interaction of determinate sentencing and presumptive guidelines; meanwhile only determinate sentencing is selected for the white rate.

The general pattern of results is also robust to varying the first-stage lag, including more or fewer controls, varying the end year, and adding or removing states. Ranges of estimates for robbery are collected in Table 10; the underlying results are collected in the appendix.

The main results use a 3-year lag in the first stage, as this approximates looking for the impact of laws under which the majority of prisoners in any given year were sentenced. However, using a 1, 2, or 4 year lag gives similar results, including when the instrument selection is done at the different lags. Results in Table 10 use instruments selected at each of the different lags; using the same instruments as in the main results at a 1 or 2 year lag gives substantially weaker first-stage estimates and collinear fitted values.

c	0.01	0.10	0.25	0.50	0.75	1	1.25	1.50
White	-0.59*	-0.61*	-0.64*	-0.90**	-1.20**	-1.54**	-1.54**	-1.76
Incarc.	(0.25)	(0.26)	(0.26)	(0.28)	(0.45)	(0.51)	(0.51)	(1.18)
Black	0.43	0.45	0.49†	0.90*	1.26**	1.68***	1.68***	2.00
Incarc.	(0.27)	(0.28)	(0.29)	(0.35)	(0.48)	(0.51)	(0.51)	(1.24)
$\widehat{\lambda}$	2.32	23.2	57.9	115.9	173.8	231.8	289.7	347.7
Instruments	22	20	17	9	6	5	5	2
F White	13.0	14.4	13.8	10.5	11.8	11.9	11.9	20.2
F Black	5.78	5.17	5.19	7.37	6.52	7.78	7.78	16.6
p: K-P Wald	***	***	***	0.029	0.018	0.014	0.014	0.084
p: $\beta^W = \beta^B$	0.047	0.045	0.038	0.0048	0.010	0.0027	0.0027	0.13

Standard errors in parentheses

† $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 9: Estimated Black and White Incarceration rate elasticities of the Robbery rate with instruments selected at different penalty multipliers c. F White and F Black are the first-stage F-statistics for the two incarceration rates. p: K-P Wald is the p-value of the Kleibergen & Papp rk Wald statistic, testing the null hypothesis that the matrix of fitted values is rank-deficient (i.e. that the instruments cannot separately identify the black and white incarceration rates). p: $\beta^W = \beta^B$ is the p-value of the F-test that the estimated elasticities are the same.

The choice of controls among those common in the literature (e.g. Abrams, 2012) has little effect on the results. The range of results in Table 10 includes those from using only the age profile, and then adding successively the unemployment rate, the poverty rate, the (log) police per capita, and the black percentage of the population. All use the same instruments as in the main results.

The end year is somewhat arbitrary. Going too far past 2000, the results shrink and then disappear, in accordance with the decreasing effectiveness of prison over this period found by Johnson and Raphael (2012). Going too much earlier, the sample size becomes too small to give statistically significant results. The main results end in 1999, but ending in 1997, 1998, or 2000 gives generally similar results. Lasso selects identical instruments regardless of the end year; this is unsurprising as few states changed their sentencing laws in the last few years of the nineties.

The main results use 37 states. Besides the four states with significant missing data, I throw out nine states whose population was less than 1% black during any year in my data. Adding all of these increases the variance on the black incarceration rate so much that the effect sizes I find are not statistically significant. Removing many more states makes the sample too small to get statistically signficant results. However, no individual state has an outsized impact on the results. The second-to-bottom row of Table 10 shows the ranges of results from dropping each of the 37 included states, while the bottom row shows the range from including each of the 9 dropped states. All results use the same instruments and controls as the main results.

5.4 Comparison with Levitt (1996)

In order to compare my results directly with those of Levitt (1996), I extend his overcrowding litigation variables through the present and use them as instruments

	$\widehat{\beta}^W$	$\widehat{\beta}^B$
Main Results	-1.54	1.68
1st stage lag 1–4 years	(-1.67, -1.16)	(1.08, 1.96)
Different Controls	(-1.64, -1.54)	(1.68, 1.80)
End Year 1997–2000	(-1.81, -1.08)	(1.14, 2.01)
Dropping States	(-1.87, -1.21)	(1.20, 1.93)
Adding States	(-1.65, -1.27)	(1.34, 1.82)

Table 10: Ranges of Black and White Incarceration rate elasticities of the Robbery rate estimated under different specifications.

for both incarceration rates. I use case summaries from the Civil Rights Litigation Clearinghouse to extend the variables through 2012, though for consistency I use the same range of years here as in my main results. Following the Helms amendment to the 1994 Violent Crime Control Act and the 1996 Prison Litigation Reform Act, both of which raised the standards of evidence and narrowed the allowable scope of civil rights lawsuits involving prison conditions, no new statewide suits of the type Levitt considers were filed after 1993, when his dataset ends. Coincidentally (or not), the 12 states whose statewide lawsuits he uses as instruments are all among the 37 states with significant black populations that I use in my analysis.

The results of two-stage least squares using prison overcrowding litigation as instruments are shown in Tables 11 and 12. The first stage results reveal that insofar as overcrowding lawsuits impacted incarceration rates, the various stages of their progression through the courts affected black and white rates rather differently. Thus, this instrumentation strategy can identify the separate effects of black and white incarceration, to the extent these are affected by overcrowding lawsuits. They still leave open the possibility that there are significant deterrent effects to which the prison overcrowding instruments are blind.

Using this identification strategy gives the only results that differ substantially

	White Inc.		Black Inc.	
Filed 1	0.20**	(0.059)	0.042	(0.074)
Filed 2/3	0.16**	(0.052)	0.12†	(0.069)
Prelim 1	-0.0018	(0.040)	0.038	(0.038)
Prelim 2/3	-0.020	(0.063)	0.025	(0.058)
Final 1	-0.0060	(0.038)	-0.034	(0.056)
Final 2/3	-0.048	(0.052)	-0.080	(0.050)
Further 1	-0.069	(0.054)	-0.063	(0.040)
Further 2/3	-0.052	(0.058)	-0.020	(0.037)
Released 1	-0.067†	(0.037)	-0.043	(0.030)
Released 2/3	-0.068*	(0.030)	-0.0095	(0.026)
F	7.15		4.27	
K-P Wald p	0.0098			

Standard errors in parentheses

† $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < .001$

Table 11: Regressions of changes in black and white incarceration rates on prison overcrowding litigation status dummies, following Levitt (1996). Dummies indicate whether a lawsuit in each state was in each of five categories in the previous year, or 2 or 3 years prior.

	Murder	Rape	Robbery	Assault	Burglary	Larceny	Car Theft
White Inc.	0.57	-0.028	-0.59	-1.11*	-0.28	-0.24	-0.89†
	(0.38)	(0.31)	(0.49)	(0.50)	(0.33)	(0.18)	(0.46)
Black Inc.	-0.49	-0.55	-0.43	0.45	-0.54	-0.23	0.34
	(0.44)	(0.67)	(0.67)	(0.72)	(0.46)	(0.37)	(0.54)

N=771; Standard errors clustered at the state level.

† $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 12: Estimated impacts of white and black incarceration rates on crime rates using prison overcrowding lawsuit status as instruments for both rates

from the main results. Regressions with overcrowding litigation variables similar to Levitt's suggest that white incarceration mainly reduces assault. These results are consistent with the idea that the overcrowding instruments measure the impact of letting people out of prison, thereby reducing incapacitation. Locking more people up also increases deterrence.

6 Conclusion

Prison's effectiveness in controlling crime is a question of obvious importance. But prison, like police and the law, has been given to mean different things to black and white Americans. This paper has addressed the former question in light of the latter fact, and thus demonstrated the importance for economic analysis of crime to consider racial differences concealed within population averages.

Specifically, this paper shows two things. First, the white incarceration rate is more sensitive to changes in incarceration policy than the black rate. Second, crime rates have decreased in response to increases in the white incarceration rate, but have not been reduced by increases in black incarceration.

More broadly, these findings suggest a new perspective on institutional racism and black crime. Contemporary discussions of race, crime, and law enforcement in the United States have tended to assume one of two competing narratives. The first narrative holds that racial differences in experience with law enforcement generally mirror racial differences in law obedience, and any example of obvious racism is a solitary relic of a past which is past. Such was the narrative underlying FBI director James Comey's February 2015 speech on law enforcement and race: though superficially contrite, he subtly attributed the major part of problems between police and people of color to the latter's lack of role models, education, and jobs. In this story, the real issue is these underlying causes of black criminality, and the needed police reforms are limited to weeding out bad apples, a dash of diversity training, and better PR.

The counter narrative alleges that racial disparities in punishment reflect the fact that criminal justice is currently the main front in the centuries-old structural oppression of black people (e.g. Loury, 2008). Under this narrative, the staggering numbers of black men in prison are fruit of the same vine as underinvestment in majority-minority schools and cops going unpunished for killing unarmed black children. While this narrative holds that racial differences in punishment are disproportionate to racial differences in crime, most would agree that people of color are still more likely than their white counterparts to commit most street crimes. Proponents of the structural narrative tend to attribute this latter fact to other manifestations of racism, such as past and present housing and employment discrimination.

The results of this paper suggest a different story: that the system which restrains the criminal impulses of most white Americans fails to do the same for many blacks. That is, black Americans are more likely to be in prison in part because black people commit more crimes, and in part because blacks are more often victims of unjustified punishment, and the former results from the latter. This explanation of racial differences in crime and punishment balances competing assumptions of both narratives, since it allows that the black-white crime gap results from a racist system while simultaneously allowing the possibility that most cops are free of significant racial bias. This story makes the testable prediction that increases in white incarceration will reduce crime more than will increases in black incarceration, just as I find.

References

ABRAMS, D. S. (2012): "Estimating the deterrent effect of incarceration using sentencing enhancements," *American Economic Journal: Applied Economics*, 4, 32–56.

ALEXANDER, M. (2012): *The New Jim Crow: Mass Incarceration in the Age of Colorblindness*, The New Press.

ALLEN, F. A. (1981): *The decline of the rehabilitative ideal: Penal policy and social purpose*, New Haven: Yale University Press.

ARMOUR, J. D. (1994): "Race ipsa loquitur: Of reasonable racists, intelligent Bayesians, and involuntary Negrophobes," *Stanford Law Review*, 781–816.

BECCARIA, C. (1764/1986): *On Crimes and Punishments*, Hackett Publishing Company, trans. Young, David.

BECK, A. J. (2000): *Prisoners in 1999*, US Department of Justice, Bureau of Justice Statistics.

BECKER, G. S. (1968): "Crime and punishment: an economic approach," *Journal of Political Economy*, 76, 169–217.

BELLONI, A., D. CHEN, V. CHERNOZHUKOV, AND C. HANSEN (2012): "Sparse models and methods for optimal instruments with an application to eminent domain," *Econometrica*, 80, 2369–2429.

BENTHAM, J. (1789/1843): *Introduction to the Principles of Morals and Legislation*, Edinburgh: William Tait.

BERK, R. (1993): "Preliminary data on race and crack charging practices in Los Angeles," *Federal Sentencing Reporter*, 36–38.

BLACKMON, D. A. (2009): *Slavery by another name: The re-enslavement of black Americans from the Civil War to World War II*, Anchor.

BLUMSTEIN, A., J. COHEN, AND D. NAGIN (1978): *Deterrence and incapacitation: Estimating the effects of criminal sanctions on crime rates*, Washington, DC: National Academy of Sciences.

CAMERON, A. C. AND D. L. MILLER (2015): "A practitioners guide to cluster-robust inference," *Journal of Human Resources*, 50, 317–372.

COHEN, R. L. (1991): *Prisoners in 1990*, US Department of Justice, Bureau of Justice Statistics.

COMEY, J. B. (2015): "Hard Truths: Law Enfocement and Race," Speech delivered at Georgetown University 12 Feb. 2015, https://www.fbi.gov/news/speeches/hard-truths-law-enforcement-and-race.

DITTON, P. M. AND D. J. WILSON (1999): *Truth in sentencing in state prisons*, US Department of Justice, Bureau of Justice Statistics.

DOOB, A. N. AND C. M. WEBSTER (2003): "Sentence severity and crime: Accepting the null hypothesis," *Crime and justice*, 30, 143–195.

FRANKEL, M. E. (1973): *Criminal Sentences: Law Without Order*, Hill and Wang New York.

Guetzkow, J. and E. Schoon (2015): "If You Build It, They Will Fill It: The Consequences of Prison Overcrowding Litigation," *Law and Society Review*, 29, 401–432.

Hahn, J. and J. Hausman (2002): "Notes on bias in estimators for simultaneous equation models," *Economics Letters*, 75, 237–241.

Hening, W. W. (1823): *The statutes at large: being a collection of all the laws of Virginia, from the first session of the legislature, in the year 1619*, vol. 3.

Johnson, R. and S. Raphael (2012): "How much crime reduction does the marginal prisoner buy?" *Journal of Law and Economics*, 55, 275–310.

Kleck, G., B. Sever, S. Li, and M. Gertz (2005): "The Missing Link in General Deterrence Research," *Criminology*, 43, 623.

Kleibergen, F. and R. Paap (2006): "Generalized reduced rank tests using the singular value decomposition," *Journal of Econometrics*, 133, 97–126.

Lee, H. E. (1993): "Sentencing sleight of hand," *Sarasota Herald-Tribune*, 3 Dec 1993.

Levitt, S. D. (1996): "The Effect of Prison Population Size on Crime Rates: Evidence from Prison Overcrowding Litigation," *The Quarterly Journal of Economics*, 319–351.

——— (2004): "Understanding why crime fell in the 1990s: Four factors that explain the decline and six that do not," *The Journal of Economic Perspectives*, 18, 163–190.

Levitt, S. D. and T. J. Miles (2007): "Empirical study of criminal punishment," *Handbook of law and economics*, 1, 455–495.

Loury, G. C. (2008): *Race, Incarceration, and American Values*, Cambridge, MA: MIT Press.

Marvell, T. B. and C. E. Moody (1994): "Prison population growth and crime reduction," *Journal of Quantitative Criminology*, 10, 109–140.

Mauer, M. (1999): *Race to Incarcerate*, The New Press.

McCarthy, J. (2014): "Most Americans still see crime up over last year," http://www.gallup.com/poll/179546/americans-crime-last-year.aspx.

Moynihan, D. P. (1965): *The Negro family: The case for national action*, Washington, DC: Office of Policy Planning and Research, US Department of Labor.

Muhammad, K. G. (2010): *The Condemnation of Blackness*, Harvard University Press.

Nagin, D. S. (1998): "Criminal deterrence research at the outset of the twenty-first century," *Crime and justice*, 23, 1–42.

——— (2013): "Deterrence: A Review of the Evidence by a Criminologist for Economists," *Annual Review of Economics*, 5, 83–105.

Peffley, M. and J. Hurwitz (2010): *Justice in America: The separate realities of Blacks and Whites*, Cambridge University Press.

Phelps, E. S. (1972): "The statistical theory of racism and sexism," *The American Economic Review*, 62, 659–661.

Raphael, S. and M. A. Stoll (2013): *Why Are So Many Americans in Prison?*, New York: Russell Sage Foundation.

Rennison, C. M. (2001): "Criminal Victimization 2000," Bureau of Justice Statistics.

Roberts, J. V. and L. J. Stalans (1997): *Public opinion, crime, and criminal justice*, Boulder, CO: Westview Press.

Sampson, R. J. (1986): "Crime in cities: The effects of formal and informal social control," *Crime and Justice*, 8, 271–311.

Sampson, R. J. and J. Cohen (1988): "Deterrent effects of the police on crime: A replication and theoretical extension," *Law and Society Review*, 22, 163–189.

Schaffer, M. E. (2010): "xtivreg2: Stata module to perform extended IV/2SLS, GMM and AC/HAC, LIML and k-class regression for panel data models," http://ideas.repec.org/c/boc/bocode/s456501.html.

Stemen, D. (2007): "Policies of Imprisonment: Sentencing and Corrections Policy Innovation in the United States, 1970-2002," Ph.D. thesis, New York University.

Stemen, D., A. Rengifo, and J. Wilson (2005): "Of fragmentation and ferment: The impact of state sentencing policies on incarceration rates, 1975–2002," *Vera Institute of Justice*.

Stigler, G. J. (1970): "The Optimum Enforcement of Laws," *Journal of Political Economy*, 78, 526–536.

Tibshirani, R. (1996): "Regression shrinkage and selection via the lasso," *Journal of the Royal Statistical Society. Series B (Methodological)*, 267–288.

Tonry, M. (1996): *Sentencing Matters*, Oxford University Press.

Tonry, M. H. (2011): *Punishing Race*, New York: Oxford University Press.

Ulmer, J. T., M. T. Light, and J. H. Kramer (2011): "Racial disparity in the wake of the *Booker/Fanfan* decision," *Criminology & Public Policy*, 10, 1077–1118.

Western, B. (2006): *Punishment and Inequality in America*, New York: Russell Sage Foundation.

A Method of Optimal Instruments

Many instruments can create problems, as 2SLS is biased in finite samples, and the bias is proportional to the number of instruments (Hahn and Hausman, 2002). One way to deal with this to estimate *optimal instruments*. In the traditional IV setting with outcome y and endogenous regressors X, there is a hypothetical instrument D which captures all of the exogenous variation in X and none of the endogenous variation. With many exogenous instruments Z, we can approximate D as a function of Z, and use the resulting $\widehat{D}$ as instruments for X in a standard two-stage least squares estimation procedure. This subsection describes the procedure I use to estimate $\widehat{D}$ in detail.

First stage equations take the form

$$\begin{aligned} X_{it}^W &= \boldsymbol{Z'_{it-3}\alpha_1^W} \quad + \boldsymbol{C'_{it}\alpha_2^W} + \xi_{it}^W \\ X_{it}^B &= \boldsymbol{Z'_{it-3}\alpha_1^B} \quad + \boldsymbol{C'_{it}\alpha_2^B} + \xi_{it}^B \end{aligned} \tag{1}$$

where i and t are state and year indices, X^B and X^W are log black and white incarceration rates, Z is a vector of dummies describing a state's sentencing laws, C is a vector of controls and state and year fixed effects, and ξ^W and ξ^B are random errors.

The second stage is:

$$y_{it}^{crime} = \beta^B \widehat{X}_{it-1}^B + \beta^W \widehat{X}_{it-1}^W + \boldsymbol{C'_{it-1}\gamma} + \epsilon_{it} \tag{2}$$

where y is the log per-capita annual crime rate for a given crime and ϵ is a random error.

I use the Lasso procedure for optimal instruments with data-driven penalties of Belloni et al. (2012, hence BCCH) as a principled means of selecting a subset of the primitive laws from which to estimate optimal instruments. This procedure has four steps: First, estimate a Lasso regression of each endogenous regressor on the primitive instruments. The Lasso regression returns a vector of coefficient estimates containing mostly zeroes. Second, estimate a post-lasso regression for each endogenous regressor, which is just ordinary least squares on the primitive instruments that got non-zero coefficients in the Lasso. Third, generate fitted values of the endogenous regressors using the post-lasso coefficients, as estimates of the optimal instruments. Finally, do standard two-stage least squares using the estimated optimal instruments.

The first step begins with partialing out the fixed effects and controls. For any matrix A, let M_A denote the annihilator matrix $I - A(A'A)^{-1}A'$. Then I use $\tilde{X}^W = M_X M_C X^W$, $\tilde{X}^B = M_X M_C X^B$, $\tilde{Z} = M_X M_C Z$, where C includes the unemployment rate and the percentage of the population in each age range of 15–17, 18–24, and 25–34. The BCCH lasso estimator for each $l \in \{B, W\}$ is then given by[7]

$$\widehat{\boldsymbol{\phi}}^{\boldsymbol{l}} = \underset{\boldsymbol{\phi} \in \Re^p}{\operatorname{argmin}} \sum_{i,t} (\tilde{X}_{it}^l - \tilde{Z}_{it}' \phi^l)^2 + \widehat{\lambda}^l \sum_{j=1}^{p} \left| \widehat{\Upsilon}_j^l \phi_j^l \right| \tag{3}$$

where $p = \dim Z$,

$$\widehat{\lambda} = 2.2\sqrt{n}\Phi^{-1}\left(1 - \frac{0.1}{4p\ln(n)}\right),$$

and the penalty loadings Υ are recursively approximated to satisfy

$$\widehat{\Upsilon}_j^l = \sqrt{\frac{1}{n}\sum_{i,t} Z_{itj}^2 \widehat{\xi}_{itl}^2},$$

[7]The estimator for $\widehat{\lambda}$ in the text combines the general version given by Belloni et al. with the parameter values they recommend based on theory and simulations. The general estimator is $\widehat{\lambda} = 2c\sqrt{n}\Phi^{-1}\left(1 - \frac{\gamma}{2k_e p}\right)$, where c is a constant greater than one, γ satisfies $\gamma \gtrsim 1/\ln(p \vee n)$ and $\gamma \to 0$, and k_e is the number of endogenous regressors.

where $\widehat{\xi}_{itl} = X^l_{it} - Z_{it}\widehat{\phi}^l$. The penalty loadings serve to normalize each dimension of Z by both its mean, and its explanatory power given the other dimensions. Thence, $\widehat{\lambda}$ follows from the bound on estimation error implied by self-normalized moderate deviation theory.

Belloni et al. (2012) show that under mild regularity assumptions,

$$\widehat{D}^l_{it} = \tilde{Z}'_{it}\widehat{\phi}^l$$

is a near-efficient estimator of D^l_{it}, the optimal instrument for X^l_{it}, and using $\widehat{D}^l$ as instruments for X^l yields $\sqrt{n}-$consistent asymptotically normal estimators of the second-stage coefficients β^l.

Accordingly, I first estimate the lasso (3) for each of $\tilde{X}^W, \tilde{X}^B$, to select relevant instruments Z^W, Z^B with which to approximate the optimal instruments:

$$Z^W = (Z_j, \dots)'_{j \in I^W}, I^W = \{j : \phi^W_j \neq 0\}$$
$$Z^B = (Z_j, \dots)'_{j \in I^B}, I^B = \{j : \phi^B_j \neq 0\}.$$

Thence, I estimate (1) using Z^W, Z^B as instruments, weighting by the white male and black male populations. The fitted values from this regression, $\widehat{D}^W, \widehat{D}^B$, then become instruments in (1). Finally, I estimate (2) using the resulting $\widehat{X}^W, \widehat{X}^B$.

A.1 First Stage Estimates

The first stage regressions, of black and white incarceration rates on sentencing laws, are shown in Table 13. They differ minimally from the results in Table 4 in the main text (which established instrument relevance) and show again that the white incarceration rate is more specifically determined by changes in the law than the black incarceration rate. Nonetheless, the selected instruments are jointly significant in both post-lasso regressions, at $F(4,36) = 13.2$ for whites and $F(2,36) = 17.3$ for blacks. The log white and black incarceration rates have a correlation of 0.64 and standard deviations of 0.49 and 0.46, respectively. $\widehat{D}^W$ and $\widehat{D}^B$ have a correlation of 0.65 and standard deviations of 0.50 and 0.51; $\widehat{X}^W$ and $\widehat{X}^B$ have a correlation of 0.89 and standard deviations of 0.44 and 0.43. With the estimated optimal instruments, the Kleibergen-Paap Wald test gives $\chi^2(1) = 4.33, p = .04$.

A.2 Second Stage Estimates

The results of the second stage regressions of crime on incarceration are shown below in Table 14. They mirror the main result of this paper: the coefficients on white incarceration are almost all negative, the one on robbery large and significant, and the one on burglary large and near-significant. Meanwhile, none of the few negative coefficients on black incarceration is significant, while those on robbery and burglary are significant and positive. The tests reported in the bottom row of Table 14 confirm that the impacts of white incarceration on robbery and burglary are significantly negative relative to the impacts of the black incarceration rate.

The J statistic suggested by BCCH as a specification test gives $\chi^2(2) = 24.2$ with the robbery rate as the dependent variable and $\chi^2(2) = 9.2$ with the burglary rate, versus $P(\chi^2(2) > 9.21) = .01$. This test compares the coefficients $\widehat{\beta}_1$ estimated with the estimated optimal instruments $\widehat{D}$ to those estimated with the primitive instruments Z, on the theory that they should both be close to the true value if the $\widehat{D}$ are exogenous. These results are somewhat worrisome, although the results of the Kleibergen-Paap Wald test rejecting underidentification and the consistent overall pattern across specifications suggest that the pattern of results, at least, is accurate.

Log Prisoners Per Captia:	White Male	Black Male	White Male	Black Male
Determinate	-0.29***	-0.11		
Sentencing	(0.049)	(0.086)		
Presumptive	0.21**			
Sentencing	(0.061)			
Two Strikes	0.16*			
Law	(0.063)			
Determ. Sent.		-0.20*		
$\times$ Presump. Guid.		(0.076)		
Sentencing	-0.077*			
Commission	(0.033)			
$\widehat{D}^W$			0.79***	0.33*
			(0.15)	(0.14)
$\widehat{D}^B$			0.094	0.57*
			(0.20)	(0.27)
%Unemployment	-0.0085	0.0028	-0.0010	0.0040
	(0.014)	(0.0066)	(0.0099)	(0.0080)
%15–17	-0.041	0.051	0.069	-0.035
	(0.16)	(0.15)	(0.093)	(0.11)
%18–24	0.011	0.10*	-0.024	0.020
	(0.042)	(0.047)	(0.023)	(0.038)
%25–34	-0.043	-0.037	0.047*	0.059**
	(0.039)	(0.051)	(0.019)	(0.021)
N	771	771	771	771
F	13.1	16.9	26.6	16.4

Standard errors are clustered at the state level and robust to heteroskedasticity.
All regressions include state and year fixed effects.
† $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < .001$

Table 13: First stage results. The first two columns show each incarceration rate regressed on the instruments selected by lasso; the latter two show each regressed on the optimal instruments estimated from these regressions. The F statistic at the bottom of each column tests the joint significance of the instruments.

	Murder	Rape	Robbery	Assault	Burglary	Larceny	Car Theft
White Male	0.013	-0.16	-1.75*	-0.13	-0.97†	-0.44	-0.27
Incarceration	(0.53)	(0.64)	(0.69)	(0.52)	(0.51)	(0.37)	(0.55)
Black Male	-0.17	-0.0032	1.99*	0.17	1.19†	0.40	-0.34
Incarceration	(0.78)	(0.83)	(0.80)	(0.75)	(0.61)	(0.44)	(0.64)
%Unemployment	-0.018†	-0.034**	-0.025	-0.0015	-0.00065	0.0027	0.0026
	(0.010)	(0.013)	(0.017)	(0.0091)	(0.010)	(0.0071)	(0.012)
%15–17	-0.050	0.071	0.16	0.13	0.22*	0.18**	0.078
	(0.095)	(0.088)	(0.14)	(0.099)	(0.11)	(0.056)	(0.099)
%18–24	0.042	0.0025	-0.11	0.056	-0.030	0.025	0.12*
	(0.078)	(0.067)	(0.079)	(0.066)	(0.053)	(0.041)	(0.053)
%25–34	0.026	0.042†	0.016	0.037	0.033	0.042*	0.062†
	(0.021)	(0.025)	(0.047)	(0.028)	(0.036)	(0.018)	(0.037)
N	771	771	771	771	771	771	771
$p{:}\beta^W{=}\beta^B$	0.89	0.91	0.011	0.81	0.051	0.29	0.96

Standard errors are clustered at the state level and robust to heteroskedasticity.

All regressions include state and year fixed effects.

† $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 14: Stage 2 results.

	1 year	2 year	3 year	4 year
White	-1.27*	-1.67*	-1.54**	-1.16**
Incarc.	(0.54)	(0.84)	(0.51)	(0.42)
Black	1.41*	1.96*	1.68***	1.08**
Incarc.	(0.71)	(1.00)	(0.51)	(0.39)
N	771	771	771	771
p: $\beta^W = \beta^B$	0.034	0.054	0.0027	0.0070
W. 1st F	9.93	11.0	11.9	15.4
B. 1st F	6.44	8.01	7.78	8.88
Num Insts	4	4	5	5
K-P Wald p	0.012	0.29	0.014	***

Standard errors in parentheses

† $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 15: Estimated robbery elasticities of black and white incarceration with various lags in the first stage. The 3-year lag corresponds to the main results.

B Robustness Checks

Table 15 shows the second stage results for robbery using different lags in the first stage, and selecting the instruments via Lasso for each different lag. The instruments selected for the white incarceration rate are mostly the same as with the three year lag used in the main results, with a few differences. At a one year lag, Sentencing Commission and Two Strikes Law are not selected, but Abolished Parole is. At a two year lag, Two Strikes Law is not selected. The same instruments as in the main results are selected for the black rate at all lags, and for the white rate at a four year lag.

Table 16 shows the second-stage results adding each of the controls used by Abrams (2012) and Levitt (1996) one at a time. As the results in the table indicate, the inclusion of more or fewer controls does not have much impact on the estimates or their significance. This is unsurprising, as previous research has found minimal relationship between the demographic and economic profiles of states and their passage of sentencing reforms (Stemen, 2007).

Table 17 shows second-stage results for robbery when varying the end year from 1999 as used in the main results. In general, cutting the data off earlier appears to yield a stronger first stage and smaller second-stage coefficients. It is possible that the main results are inflated by correlation between the black and white incarceration rates in the later years in the sample, during which fewer sentencing laws changed. Nonetheless, the general pattern of results is quite robust within this range. With many fewer years, the panel becomes too short to yield statistically significant effects.

	Robbery	Robbery	Robbery	Robbery	Robbery
White Inc.	-1.64**	-1.54**	-1.58**	-1.61*	-1.62*
	(0.58)	(0.51)	(0.57)	(0.67)	(0.69)
Black Inc.	1.80**	1.68***	1.73**	1.77*	1.78*
	(0.58)	(0.51)	(0.57)	(0.71)	(0.73)
%15-17	0.12	0.14	0.14	0.15	0.19
	(0.14)	(0.13)	(0.14)	(0.14)	(0.14)
%18-24	-0.080	-0.086	-0.088	-0.088	-0.081
	(0.067)	(0.066)	(0.066)	(0.067)	(0.066)
%25-34	0.019	0.023	0.023	0.025	0.026
	(0.038)	(0.036)	(0.036)	(0.036)	(0.037)
Unemp. Rate		-0.023	-0.024	-0.024	-0.023
		(0.016)	(0.016)	(0.016)	(0.016)
Poverty Rate			0.0026	0.0015	0.00015
			(0.012)	(0.012)	(0.011)
Police Per Ca.				-0.19	-0.18
				(0.38)	(0.38)
%Black Pop.					0.047
					(0.053)
N	771	771	771	771	771
p: $\beta^W = \beta^B$	0.0046	0.0027	0.0058	0.018	0.021
W. 1st F	12.2	11.9	11.9	11.8	11.8
B. 1st F	7.61	7.78	7.61	7.86	7.70
Num Insts	5	5	5	5	5
K-P Wald p	0.025	0.014	0.021	0.076	0.079

N=771; Standard errors clustered at the state level.

† $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 16: Second stage results when including a variety of control variables.

	2000	1999	1998	1997	1996	1995	1994
White	-1.81**	-1.54**	-1.27**	-1.08**	-0.97*	-0.88*	-0.75
Incarc.	(0.69)	(0.51)	(0.42)	(0.41)	(0.39)	(0.43)	(0.46)
Black	2.01**	1.68***	1.35***	1.14*	1.04*	0.91†	0.73
Incarc.	(0.75)	(0.51)	(0.40)	(0.44)	(0.45)	(0.51)	(0.58)
N	806	771	735	698	661	624	587
p: $\beta^W = \beta^B$	0.010	0.0027	0.0024	0.012	0.021	0.060	0.16
W. 1st F	9.37	11.9	14.9	19.0	38.2	41.0	40.7
B. 1st F	7.05	7.78	8.68	12.4	25.8	30.2	36.5
K-P Wald p	0.15	0.014	0.0013	***	***	0.0029	0.028

Standard errors in parentheses

† $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 17: Second stage estimates for robbery ending in various years.

C Data

C.1 Crime Rates

I construct rime rates from Uniform Crime Reporting (UCR) data. The UCR data come from law enforcement agencies throughout the United States, who send monthly reports to the FBI. Participation is voluntary, yet most agencies participate; in the period I study rates range from a high of 95% in 1986 to a low of 84% in 1996. The FBI aggregates the reports to produce its annual "Crime in the United States." Data are later checked for accuracy and amended with late updates by the Bureau of Justice Statistics (BJS) and archived for public use at the Interuniversity Consortium for Political and Social Research (ICPSR), hosted online by the University of Michigan. I use the Offenses Known and Clearances by Arrest datasets for each year in my period of study, and sum the agency-month offense and population numbers therein at the state-year level. I thus compute annual offenses per 100,000 population for each state for each of the FBI's seven index crimes: murder, rape, robbery, assault, burglary, larceny, and car theft.

Nationwide, violent crime rates increased through the 1980s before peaking in 1993-4, and have declined steadily since. Property crime rates are noisier, but have declined on average since 1980. While the UCR data are richly detailed and finely disaggregated, they only capture reported crime. This means UCR incident numbers may not give an accurate measure of any particular crime rate, but they still yield a good measure of year-to-year changes in crime rates within each state.

The UCR data do include any information on the race of offenders. The best data available on the race of criminals is from the National Crime Victimization Survey (NCVS), which surveys roughly 50,000 households each year asking detailed questions about any crimes they have experienced in the past year. For robbery and assault, the surveys asks victims what race they perceived their attacker to be. All NCVS data are reported as national rates, constructed from the surveys. In the years I study, 40–58% of robbery victims perceived their attackers to be black, and 30–49% perceived them to be white. So substantial numbers of robberies are committed by both blacks and whites. Among assault victims, 19–25% perceived their attackers to be black while 64–75% perceived them to be white.

C.2 Incarceration Rates

I construct incarceration rates using prisoner counts from the National Prisoner Statistics (NPS) dataset and population numbers from the Survey of Epidemiological and End Results (SEER). The NPS is assembled by the Census Bureau from surveys completed once or twice a year by each state's department of corrections, and gives the number of prisoners held by each state state on December 31st of each year, divided up in various ways. Since 1978, the NPS has included prisoner counts by race and

Hispanic origin.

Unfortunately, the rules for recording race and Hispanic origin were not standardized until 2000, and the NPS data have not been cleaned by the BJS as the UCR data have been. In particular, Hispanic prisoners are sometimes counted as white, sometimes as "other race", and often a state's method changes during the dataset. For most states, it is nonetheless straightforward to determine how data were recorded and thence compute the numbers of black and non-Hispanic white prisoners held in each state each year. For example, raw NPS data for California are reproduced in Table 18. From looking at the numbers, it is clear that Hispanic prisoners were included with whites in years through 1994, then listed as unknown race for 1995–1998. Beginning in 1999, Hispanic was counted as a separate group, mutually exclusive to white, black, and the rest.

In four states—Massachusetts, New Jersey, New York, and Hawaii—there is no defensible way to infer these numbers from the data in NPS. The first three all have substantial black Hispanic populations, while Hawaii has an ethnic make-up unlike any other state in the US.

SEER population data is based on the U.S. Census, which first collected reliable counts of Hispanic origin in the 1980 Census; the SEER datasets including Hispanic origin begin in 1981. Therefore I impute the non-Hispanic white populations for 1978, 1979, and 1980. For each state, I compute the Hispanic percentage of the white population for each year after 1980, and estimate the regression

$$PCT\_HISP_t^i = \gamma_0^i + \gamma_1^i YEAR_t + \gamma_2^i YEAR_t^2 + \eta_t^i \tag{4}$$

separately for each state i. I then extrapolate backwards to estimate the percent

YEAR	WHITEM	BLACKM	HISPM	UNKRACEM	TOTRACEM
1978	12,968	6,743	.	109	20,178
1979	13,630	7,323	5,105	99	21,400
1980	14,748	8,017	5,687	86	23,253
1981	17,570	9,765	6,993	91	27,775
1982	20,495	11,679	8,648	549	32,987
1983	23,010	13,193	9,920	949	37,353
1984	25,513	14,007	11,151	1,297	41,018
1985	29,173	16,138	13,014	1,652	47,205
1986	34,038	19,549	15,877	2,053	55,920
1987	37,923	22,184	17,765	2,372	62,823
1988	41,741	26,438	19,808	3,099	71,278
1989	47,496	30,066	23,137	3,735	81,297
1990	54,409	32,190	27,716	4,208	90,807
1991	57,880	32,981	30,400	4,070	95,506
1992	63,015	34,279	33,749	4,825	102,749
1993	71,139	36,461	39,362	2,976	112,370
1994	74,992	37,472	41,377	2,786	117,390
1995	36,840	39,399	44,963	48,099	126,564
1996	39,702	42,300	48,053	51,500	135,942
1997	42,866	44,682	50,995	54,913	144,876
1998	43,229	46,235	52,358	56,595	148,600
1999	42,993	46,578	52,836	0	149,513
2000	43,078	46,243	53,186	0	149,677
2001	42,165	44,862	53,005	0	147,391
2002	42,714	44,807	54,787	0	149,899
2003	42,506	44,439	56,505	0	151,331
2004	42,561	44,487	57,799	0	152,966
2005	42,863	44,975	60,385	0	156,573
2006	43,059	46,294	62,672	0	160,770
2007	42,352	46,975	64,361	0	162,654
2008	41,141	47,123	64,709	0	162,050
2009	40,023	46,591	64,481	0	160,286
2010	38,231	44,956	62,956	0	155,104
2011	33,430	41,209	58,365	0	141,382
2012	29,193	37,990	53,584	0	128,436

Table 18: Raw NPS data for California. The NPS dataset also includes columns for ASIANM, TWORACEM, ADDRACEM, NOTHISPM, AIANM (American Indian or Alaska Native), NHPIM (Native Hawaiian or other Pacific Islander) APIM (Asian or Pacific Islander), and female counts for all categories.

Hispanic of the population in each state in the three earlier years, and subtract this from the white population to get the non-Hispanic white population. For all states, the R^2 of the regression (4) was greater than .975. For all but 6 states[8], the percentage Hispanic of the white population in 1981 was less than 10%, so the error introduced by computing the non-Hispanic white population from the imputed percentages is minimal. Following the NPS, I consider a "prisoner" anyone who is in a state prison serving a sentence of a year or more.

The overall incarceration rate more than tripled over the course of my dataset, from 125 prisoners per 100,000 population in 1978 to 450 in 1999. As shown in Figure 1, black and white incarceration rates both grew roughly linearly throughout, with the former rate always approximately eight times the latter. Geographic variation in incarceration rates is depicted in Figure 2.

C.3 Controls

I include control variables which have been deemed important in the previous literature on incarceration and crime rates, taken from Abrams (2012): unemployment rate and percentage of the population aged 15–17, 18–24, and 25–34, all as percentages. Though most studies include the black percentage of the population as a control, I leave this out, because as its numerator is the denominator of the black incarceration rate there is a spurious negative correlation between the two. Anyway, the percent black variable is rarely significant in other studies, and its inclusion is never defended.

[8] Arizona, California, Florida, New Mexico, New York, and Texas

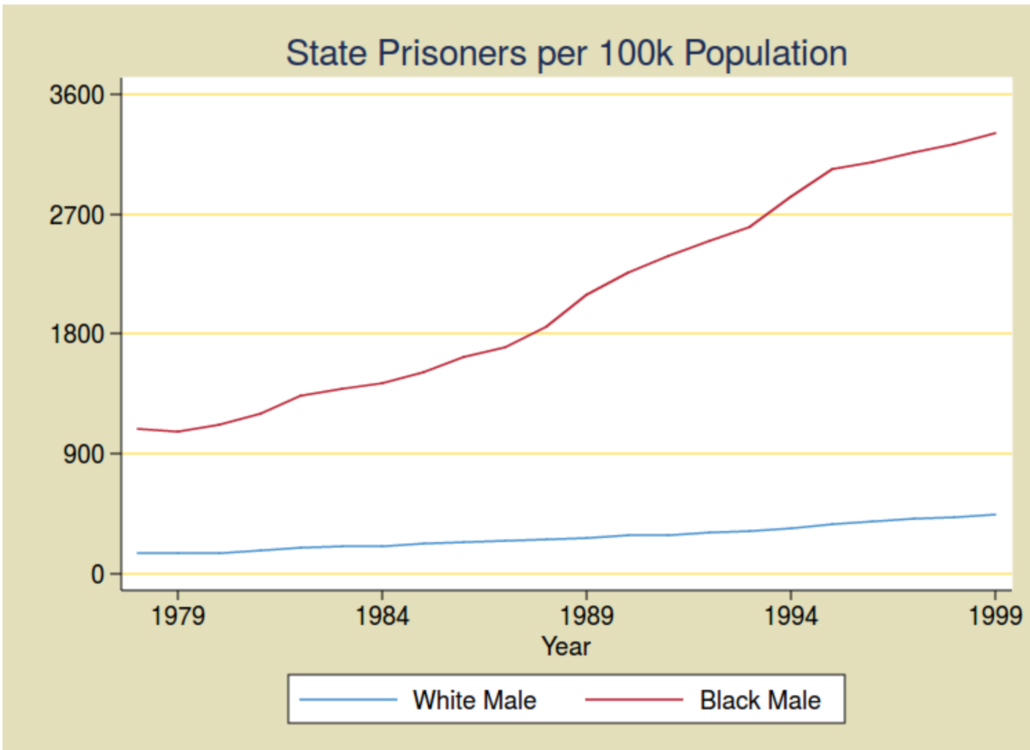

Figure 1: Nationwide incarceration rates in state prisons for blacks and non-Hispanic whites. The black rate was approximately eight times the white rate throughout this period.

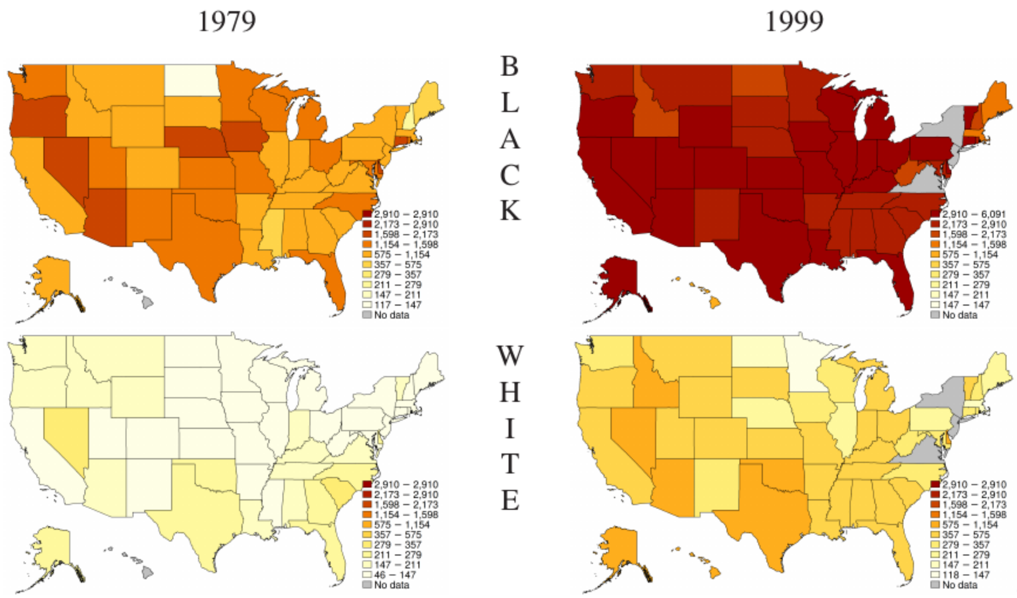

Figure 2: Black and non-Hispanic white state prisoners per 100,000 population in 1979 and 1999

C.4 Sentencing Legislation

All US states changed their sentencing laws in various ways between 1975 and 2000. Common changes included determinate sentencing, presumptive or voluntary guidelines, habitual offender laws, mandatory minimum sentences, and changes in the sentences for various crimes, especially drug crimes. Stemen et al. (2005) built a rich database of sentencing laws in all the US states from 1975-2002; I use the subset of these available in Stemen (2007) along with several other laws coded myself. The full set of laws and interactions I considered is in Table 4 in the text; Table 19 shows the laws I ultimately used as instruments and the years during which they were in effect in each state. As several states repealed sentencing laws during the period I study, these laws generate experimental variation in both directions.

	Determinate Sentencing	Presumptive Sentencing	Presumptive Guidelines	Sentencing Commission	2 Strikes Law
Alabama					
Alaska		80–		90–93	
Arizona	94–	78–			
Arkansas				93–	78–
California	76–	76–			81–
Colorado	79–85	79–		89–94	
Connecticut	81–90			79–80	81–
Delaware	90–			84–	
Florida	83–		94–	82–98	
Georgia					
Illinois	78–				
Indiana	79–	77–			
Iowa					90–
Kansas	93–		93–	89–	–91
Kentucky					78–
Louisiana				87–	
Maryland				96–	96–
Michigan				94–	
Minnesota	80–		80–	78–	
Mississippi	95–				
Missouri				93–	
Nebraska					
Nevada				95–	
New Mexico	77–	77–			
North Carolina	81–		95–	90–	
Ohio	96–	96–		90–	
Oklahoma				94–	
Oregon	89–		89–	87–	90–
Pennsylvania			82–	78–	84–
Rhode Island		81–			
South Carolina				82–85, 89–	
Tennessee			89–	85–95	
Texas				91–92	
Virginia	95–			94–	96–
Washington	84–		84–	81	–82, 93–
West Virginia					
Wisconsin	99–			83–95	

Table 19: Years in which states had different laws in effect. State law changes are typically effective midyear, so where laws were repealed the end year is the year the repeal went into effect. For the regressions, laws are coded as they stood at the end of each year. This table only includes the 37 states used in the regressions in this paper.