The Racial Politics of Mass Incarceration

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Abstract

Dominant accounts of America's punitive turn assume that black elected officials and their constituents resisted higher levels of imprisonment and policing. We gather new data and find little support for this view. Panel regressions and an analysis of federally-mandated redistricting suggest that black elected officials had a punitive impact on imprisonment and policing. We corroborate this with public opinion and legislative data. Pooling 300,000 respondents to polls between 1955 and 2014, we find that blacks became substantially more punitive over this period, and were consistently more fearful of crime than whites. The punitive impact of black elected officials at the state and federal level was concentrated at the height of public punitiveness. In short, the racial politics of punishment are more complex than the conventional view allows. We find evidence that black elected officials and the black public were more likely than whites to support non-punitive policies, but conclude that they were constrained by the context in which they sought remedies from crime.

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1 Introduction

The modern American way of doing criminal justice is both punitive and disproportionate. Between 1970 and the present, the proportion of adults in prison or jail exploded, and now exceeds that found in any comparable society. During this period racial disparities in incarceration remained very high, with African Americans five or six times more likely to be jailed than whites (Muller 2012). The combined intensity and disparity of punishment has had a devastating impact on African American communities, especially those marked by concentrated poverty (Weaver, Hacker, and Wildeman 2014; Lee, Porter, and Comfort 2014).

While there is no consensus on the origins of this punitive turn, its disproportionate impact has led most scholars to emphasize white protagonists and racial motives. For the scholarly mainstream, mass incarceration was the work of a revanchist, white, and mostly Southern elite determined to roll back the tide of black advancement after the Civil Rights movement (Beckett 2000; Weaver 2007; Tonry 2012). In the well-known words of Alexander (2012), mass incarceration amounts to 'The New Jim Crow'.

This emphasis on white protagonists has encouraged assumptions about the views and actions of African Americans and their representatives. Specifically, blacks are generally reduced to the status of unwilling or unwitting victims. By extension, many scholars believe that black enfranchisement, where and when it existed, should have attenuated or perhaps even reversed punitive trends over this period (Beckett 2000, 26; Behrens, Uggen, and Manza 2003, 596; Yates and Fording 2005, 1119).

Recent research, however, has questioned this conventional view. Several scholars have demonstrated that black political leaders in the 1970s and 1980s often supported the same "get tough" approach advocated by their white counterparts. Michael Javen Fortner (2013, 2015a) has shown that many civic leaders in Harlem were in favor of the 1973 Rockefeller drug laws, considered by many the model for the War on Drugs. Similarly James Forman Jr (2012, forthcoming) has documented that a majority-black legislature in D.C. passed tough-on-crime policies in the 1980s, leading to exceptionally high levels of incarceration in that city. Both authors present their findings as a challenge to the conventional view, arguing that it "oversimplifies the origins of mass incarceration" (Forman Jr 2012, 103) and fails to "take black agency seriously" (Fortner 2015a, 14).[1] In view of this evidence, Vanessa Barker (2009, 179) has concluded that "[b]lack incorporation and political participation have made them both accomplices and victims of penal reform."

While these authors marshal compelling evidence to make their case, there are reasons to be sceptical of this revisionist account. First, this account is based exclusively on case studies. We do not know how well their findings generalize to other times and places. Second, the conventional view can draw on supporting evidence that black public opinion is less punitive than white (Bobo and Johnson 2004), and that black political and civil rights organizations have played a leading role in recent decarceration efforts (Nadelman 2010). Finally, these studies do not settle the question of *why* black leaders supported punitive policies, if and when they did. For instance, while Fortner emphasizes that black constituents are particularly vulnerable to crime, and are thus amenable to punitive arguments, Forman tends to lay responsibility on black elites.

We take this controversy as an invitation to consider the relevant evidence in

1. In a similar vein Donna Murch (2015, 173) has argued that "[m]any black politicians and other prominent leaders supported drastic carceral policies in hopes of staunching the crack crisis facing black communities across the country."

greater detail. Our purpose is not to explain the origins of mass incarceration, but to scrutinize the image of black politics that the conventional account projects. What did black politicians accomplish during the era of the punitive turn? What did the black public demand? And how were these related?

1.1 Existing Literature

Other scholarship on crime and incarceration can be brought to bear on this debate, but our view is that direct evidence is wanting. For instance, a well-established sociological literature has argued that the racialized fears of a white majority drive temporal and cross-sectional variation in American regimes of social control (Jacobs and Jackson 2010). These studies tend to find that larger black populations are correlated with higher state-level incarceration rates (Beckett and Western 2001; Greenberg and West 2001; Jacobs and Carmichael 2001; Smith 2008; Campbell, Vogel, and Williams 2015) and larger city-level police forces (Sever 2003; Sharp 2006; Stults and Baumer 2007).[2] This evidence has obvious affinities to the conventional account of the punitive turn, but the evidence these authors present does not answer the questions posed above. First, the punitive turn unfolded over time in a period when black population shares at the state level changed very little (see Figure 5). They are thus unlikely to explain the sharp rise in prisons and police per capita. Second, demographic measures invite conceptual confusion, since the black population share is a plausible proxy for both white anxiety *and* black empowerment.

Some scholars have estimated the impact of black politicians on outcomes broadly related to the punitive turn, but most of this work focuses on mayors and judges, and finds ambivalent or clashing results (Hopkins and McCabe 2012; Uhlman 1978;

2. However, earlier studies find no or negative effects of percent black on incarceration (Michalowski and Pearson 1990; Myers 1990; Arvanites and Asher 1998), and Greenberg, Kessler, and Loftin (1985) find no effect on police force size in the 1970s.

Spohn 1990; Steffensmeier and Britt 2001). To our knowledge, no study has examined black federal legislators, despite the Congressional Black Caucus's support for the major crime bills of the mass incarceration era (Fortner 2015b). There has also been little work on the impact of black state legislators.[3] These lacunae are surprising, considering the well-known impact of state-level punitive legislation—on everything from mandatory minimums and structured sentencing to prison construction and parole eligibility

Similarly, there is great room for improvement upon existing work on the views of black constituents. We actually know very little about the contours of black public opinion over the period of the punitive turn. Existing work examines select questions and focuses on cross-sectional variation in particular periods (Bobo et al. 2004; Meares 1997; Beckett 2000). No one has yet used polling data to build a representative and long-run measure of the kind that Enns (2014, 2016) has proposed for the aggregate public.

1.2 Our Approach

To these ends we marshal new evidence. First, we estimate the impact of black representation on levels of imprisonment and policing using an original panel dataset spanning 1972 to 2008. We find no evidence that black political representation attenuated punitive outcomes, and some evidence for the contrary revisionist hypothesis. Second, we exploit an instance of federally-mandated majority-minority redistricting

3. Yates et al. (2005) estimate the effect of black officials on state prison populations, but they focus on racial disparities rather than total incarceration rates. They find that black politicians reduce the black incarceration rate but leave the white incarceration rate unaffected. We failed to replicate these results in our models. The discrepancy could be a function of model specification (they model most variables in first differences and do not include lags of the dependent variable), sample truncation (their data runs only from 1977 to 1995) or their measure of black representation (they include all local and city-level elected officials whereas we analyze only state and federal representatives).

in the early 1990s, in mainly Southern states. Here, we find even stronger evidence that black enfranchisement increased rather than attenuated punitive trends.

These results are surprising, and not robust to examining trends in corrections spending, so we dig a little deeper. We gather original data on black and white public opinion from dozens of nationally-representative opinion polls administered to over 300,000 respondents. These estimates confirm that blacks are less punitive than whites, but they also show that absolute levels of punitiveness in the black community were high, and that they were often more fearful of crime. We show that the positive effect of black politicians on carceral outcomes is concentrated in periods of high punitiveness, crime anxiety, and mistrust; in the opposite context, we find some evidence of a negative effect. Trends in partisanship and welfare disbursements around redistricting suggest that post-redistricting punitiveness is unlikely to be driven by revanchist whites. Last, to thicken our interpretation of the legislative dynamics that panel analysis leaves obscure, we examine original data on voting patterns in Congress, focusing on the passage of Clinton's 1994 Crime Bill.

All considered, our evidence contravenes the conventional view. "White backlash" does not adequately capture the racial politics of incarceration or policing. Neither black elected officials nor the public they represented were implacably opposed to punitive policies. Rather, during the heyday of incarceration and policing, they exercised their limited power and voice to support and even amplify punitive trends.

Our interpretation thus highlights black agency, but also the considerable constraints under which this agency was exercised (Miller 2010). As voting patterns at the Federal level make clear, black leaders supported these punitive policies in a political environment which foreclosed non-punitive strategies for handling crime and criminals. As the public opinion data makes clear, these alternatives enjoyed overwhelming support among the black public. Yet in a context of elevated concern about crime and high punitiveness—when, in effect, black communities were demanding that their representatives do something, *anything*, to reduce crime rates—these other demands fell by the wayside, and black political influence was channeled towards more prisons and more police.

2 Panel Regressions

Most research defines the punitive turn as the enormous increase in the rate of incarceration over the last several decades, but incarceration is only one dimension of the decades-long inflation of the American criminal justice apparatus.[4] As Figure 2 shows, the number of police officers for every 100,000 people has increased dramatically, as well. Given the high salience of intensive policing and police misconduct in recent discussions of the American way of doing criminal justice, we consider this dimension of the punitive turn throughout. These two measures—the incarceration rate and the officer rate—are our main outcomes of interest. They have been studied in past work, and they best capture the lay understanding of America's punitive turn.

Our central explanatory variable is a straightforward measure of the political clout of black politicians in a given state, defined as the proportion of state and federal elected officials who are African-American. We add a set of mostly standard controls to all models. We include a measure of the rate of violent crime, which plausibly affects the incarceration rate directly and both punitive outcomes indirectly (by inducing a response from politicians, prosecutors and the public). We also include a measure of partisanship: this is a variable denoting which party has true control of the state

4. We define the incarceration rate as the number of prisoners under a state's jurisdiction for every 100,000 people in the state's resident population. Our measure thus excludes those held in local jails or federal prisons. Others sometimes refer to this as the 'imprisonment' rather than incarceration rate.

legislature.[5] We account for the possibility that the punitive turn reflects the general modernization of the state by controlling for GDP per capita.[6] To take account of the revenue space available to state governments, we control for the amount of tax collected per capita. To control for the possibility that income inequality has driven some of the punitive turn, we include the state-level Gini coefficient. Finally, since Enns (2016) has argued that the punitive turn in policy was driven in part by a corollary turn in public opinion, we include a measure of punitiveness of the total population in a given state in a given year.[7]

We model the relationship between our independent and dependent variables at the state level. Data availability limits the sample, which is consistent across the specifications we trial, to a slightly unbalanced panel of 49 states, most observed in all years between 1972 and 2010.[8] Our focus here is to establish whether recent data reveal any broad associations between black representation and the incarceration or officer rates. Specifically, we estimate regressions of the form:

$$DV_{st} = \sum_{j=1}^{m} \alpha_j DV_{st-j} + \sum_{j=1}^{n} \gamma_n BP_{st-n} + \sum_{j=1}^{p} x'_{st-p}\beta_p + \delta_s + \mu_t + \epsilon_{st} \tag{1}$$

5. The variable is coded 1 if Democrats exercise control, 0 if neither, and −1 if Republicans. Past research has used the percentage of seats in a state legislature controlled by a given party and/or the affiliation of the governor, but we consider this composite measure a more informative gauge of partisanship. See Klarner (2003).

6. We also include the logarithmic growth rate of this variable to account for the ebb and flow of a state's economic fortunes. It is convention to include measures of the unemployment and poverty rate, but we omit these in order to to avoid truncating our sample. Intercensal estimates of the state-level unemployment rate are unavailable before 1976, and of the poverty rate before 1989. Because our state-level panel regressions consider only over-time variation in these variables (as discussed below) this is unlikely to be a very costly decision. We expect within-state movement in poverty and unemployment to be highly correlated with the growth rate of GDP per capita. All results of interest are robust to truncating the sample by including these variables. We also include them in our difference-in-difference analyses in the next section.

7. Sections 4.1 and D of the Online Appendix give details. Note that in using Enns' measure of punitiveness, we collapse the three different dimensions that we later opt to distinguish. This has two advantages: it avoids truncating the sample (questions about 'crime anxiety' are not present in our dataset before 1975), and it most closely matches what Enns himself did.

8. Nebraska has a non-partisan legislature, so it is missing from the entire sample.

where DV_{st} is the value of either of these dependent variables in state s at time t, and each of the α_j DV_{st-j} terms stands for a lag of this value. The independent variable of interest is BP_{st-n}, which represents the share of state and federal legislators that are African-American in state s at time $t-n$, and x'_{st-p} is a row vector containing all the controls. In the discussion that follows, we focus on the joint impact of the γ_n's on the dependent variable in question. We allowed dynamic structure to vary across models and variables, such that m, n and p represent the number of lags that maximized model fit.[9]

Otherwise, δ_s denotes a fixed-effect for state s at time t, the μ_t's represent a set of year fixed effects, and ϵ_{it} denotes the error in state s and time t, adjusted for clustering at the state level.[10] We include state-fixed effects to account for the likely existence of time-invariant confounders which we cannot measure. Of course, as we discuss in the next section, the inclusion of fixed-effects does not resolve the difficulties of causal inference. Two variables can be associated over time by virtue of their common association with a third, time-varying but omitted variable. Moreover, they can be associated when the causal effect runs in the reverse direction. We lag all independent variables by one year, but this is only a weak defense against this second possibility.

Finally, we examined all series for unit roots using tests appropriate for balanced panel data. As Section 7 of the Online Appendix illustrates, results are ambiguous.

9. De Boef and Keele (2008) notes that researchers often unthinkingly restrict their models by either excluding lagged dependent variables or including only a single lag of independent variables. We follow their recommendations and let the data decide. Our preferred measure of model fit is the Bayesian Information Criterion (BIC). Thus, when we report choosing a model which maximizes model fit, we mean that we chose the specification that minimized the BIC.

10. The inclusion of fixed-effects in models with lagged dependent variables raises the spectre of Nickell's bias since, by construction, the lagged dependent variable and the effective error term are correlated. However, as Nickell (1981) shows, the resulting bias fades as the length of the panel increases, and subsequent work has shown that it is not a grave concern when T is larger than 30 (Judson and Owen 1999).

All series are cleared by at least one test; all series are also implicated by at least one test. The standard remedy for nonstationarity is to first difference any culprit series. This is costly, since it discards all information contained in that variable's original level. In many cases, theoretical arguments that apply to a variable in levels may not apply to that same variable in differences. For this reason, and for brevity's sake, in the body of this paper we limit our discussion to the specification in which all variables are left in their levels. Where this decision affects our conclusions, we discuss it in the main body of the paper.

As specified, Equation 1 allows us to estimate both the immediate and the cumulative 'impact' of changes in any of the independent variables. In effect, including a lag of the dependent variable means that a change in any independent variable is transmitted for an infinite number of subsequent periods (De Boef et al. 2008). In the discussion below we focus on the long-run impact of a given change in our independent variables—specifically, we focus on the long-run consequences of a reasonably large increase in black political representation.[11]

2.1 Results

Table 2 presents estimates from the specifications that maximized model fit. In both cases, the estimated long-run impact of black politicians is positive, and in the case of the officer rate, statistically significant at conventional levels. This is the most noteworthy result. According to this model, an influx of black politicians into office is associated with the addition of about 6 police officers for every 100,000

11. This estimated long-run impact is the ratio of two or more estimates, which means that it is itself an estimated quantity. Calculating this uncertainty analytically is complicated (De Boef et al. 2008), so we proceeded by simulation. We simulated 5,000 draws from the estimated variance-covariance matrix of the model in question (adjusted for clustering), and computed a distribution for the long-run multiplier. The standard errors and p-values that we report summarize this distribution. Appendix B explains this procedure in more detail.

people (plus or minus 5.5).[12] Our estimate of the impact of black representation on incarceration is also positive, suggesting that a similarly-sized influx of black elected officials is associated with the subsequent addition of about 14 prisoners for every 100,000 people. The estimate of this latter effect is very imprecise, however; the 95% confidence interval ranges from -20 to +48. In Section 4.1 we offer a fuller interpretation of this ambiguous result.

Otherwise, we find that the only significant determinants of movements in the incarceration and officer rate are the antecedent rate of violent crime (positive), tax revenues (positive), and income inequality (negative). Neither partisanship nor the share of the population that is black has any clear consequences. Nor does the measure of punitiveness, which we discuss again in Section 4.1.

3 The Great Shock Forward

While these results are striking, they invite further scrutiny. The coincidence of black elected officials and the subsequent rise in the officer rate may be explained by the reverse causal sequence, or by an unobserved third variable. In many cases, researchers are forced to settle for correlations drawn from observational evidence. But in this case, the history of federal intervention into the state-level electoral process has produced spurts of black enfranchisement that can be considered exogenous to the covariates in our model. Our case selection follows Ueda (2005), who exploits these interventions to estimate the impact of minority representation on school funding.

12. To give some impression of effect size, we multiplied all estimates reported in Table 2 by their respective average within-state standard deviations. The average within-state standard deviation of our measure is 2.15.

3.1 1990s Redistricting

The Voting Rights Act of 1965 empowered the Federal Government to intervene in state elections to ensure minority representation. In 1982 Congress amended the act to explicitly prohibit voting schemes that result in minority vote dilution. Subsequent Supreme Court decisions simplified the legal criteria for overturning discriminatory electoral schemes. As a result, when it came time to redraw electoral districts in the aftermath of the 1990 census, states were under pressure to maximize the number of districts in which minorities would form a majority of voters. By the elections of 1992, a total of 83 new majority-black electoral districts had been created, a 25 percent increase from 1990 (Grofman 2003, 18-19).[13] The resulting influx of black politicians into state and federal legislatures has been characterized as "the single largest increase in black representatives in U.S. history"(Kim 2002, 65).

For inference, two facts about these changes bear emphasizing. First, while black mobilization and advancement certainly induced federal intervention into state elections, the influx of black politicians cannot be attributed to earlier state-level legal and political decisions. The sharp increase in black elected officials immediately after 1990 was the result of redistricting following the 1990 census, the timing of which was exogenous to black protest or progress (Grofman 2003, 16). Second, redistricting affected states covered by the Section 5 provisions of the Voting Rights Act much more dramatically than others. Figure 6 plots the average level of black electoral representation in the states in which these new black-majority electoral districts were concentrated. In the unaffected states, the percentage of black legislators increases slightly, but at a rate that is basically continuous with the trends prior to 1990. By contrast, there is a sharp discontinuity in the affected states between 1990 and 1995,

13. These figures combine state senate, state house, and congressional districts.

reflecting the post-90s influx.

3.2 Estimation

To estimate the impact of this influx, we exploit the fact that redistricting affected only a subset of all states and for a confined period of time.[14] Specifically, we compare trends before and after redistricting in states that were subject to it, to trends before and after in states that were not—an approach commonly known as difference-in-differences. This controls for unobserved differences between the two groups, and for trends over time that are common to both.[15]

More formally, we estimate models where

$$DV_{st} = (RD_s \times PD_t)\theta + x'_{st-1}\beta + \delta_s + \mu_t + \epsilon_{st} \tag{2}$$

Again, DV_{st} represents either the incarceration or officer rate in year t in state s. RD is a dummy variable denoting membership in the set of redistricted states, and PD similarly indicates years during which we expect redistricting to have an effect on outcomes.[16] θ thus estimates the impact of redistricting on the dependent variable

14. "Treated" states in this analysis are: Alabama, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, Texas, Virginia and New York. These are the states which match the following criteria: (1) they were bound by Section 5 of the Voting Rights Act to submit redistricting plans for all or some of their counties to Dept. of Justice pre-approval, (2) they had a black population of at least 10% in 1990, (3) the number of black majority districts increased between 1990 and 1992. Data on black majority districts come from Grofman (2003, 18-19).

15. Admittedly, redistricting was more pronounced in some states than in others. Our baseline approach assumes that the 'treatment effect' of redistricting was equivalent in all states that we count as affected, which is not ideal. In one specification reported in Table 3, we use the number of new majority-minority districts created as a proxy for the intensity of treatment. Because these data are only available for (almost all) redistricted states, the strategy rests on the generous assumptions that there were zero new districts created in non-redistricted states, and that the number of new districts created is a gauge of the magnitude of enfranchisement.

16. We assume that redistricting had its first impact on the percentage of black officials in the legislature in the elections that were held at the end of 1990. Supporting this, Figure 6 shows a discernible increase in black political representation in redistricted states beginning in 1991. Most redistricting was probably completed by the 1992 elections, but the resulting influx of black politi-

of interest.

While the timing of redistricting was exogenous, America in the early 1990s was far from an experimental setting. Other factors affecting punitive outcomes may have covaried with redistricting. For instance, at the same time as majority-minority redistricting produced an influx of black elected officials into redistricted states, Republicans increased their seat share in redistricted states. This did not result in veto-proof control of state legislatures in any of these states in the period in question, but it diminished Democrat control by the end of this period. Similarly, the crack epidemic hit redistricted states more severely than not-redistricted states, and violent crime also increased more. Section 4 discusses the plausibility of other explanations of these results in detail, but as a first-order defense our preferred models include all controls we employed in the panel specification, as well as measures of the state-specific intensity of the crack epidemic, the unemployment rate, and the poverty rate. The sample is truncated relative to the panel specification: from the left due to the sparsity of these additional data, and from the right due to the expectation that the effects of redistricting are likely to fade after the early 1990's.

3.3 Results

Table 3 reports estimates of θ from different specifications, which show that redistricting had a significant, positive effect on imprisonment and policing. Both the incarceration rate and the level of policing per capita increased more in redistricted than in not-redistricted states.[17] Specifically, redistricting seems to account for an in-

cians seems to have lasted till 1995. Because we expect these officials to have had an effect on the political process at a year's remove, we define our post-redistricted outcomes as those observed between 1992 (one year after the first increase of black politicians into office) and 1996 (one year after this influx ends).

17. Note that the estimated impact of redistricting on the incarceration rate is only significant at $\alpha = 0.10$. In the simple comparison of means, this estimate is larger and significant at $\alpha = 0.01$.

crease of roughly 47 prisoners (plus or minus 53) and 17 police officers (plus or minus 11) for every 100,000 people. These are sizeable effects, equivalent to a approximately one-fifth of the increase in the incarceration rate and the officer rate over the entire period.[18]

We report several specifications as a test of the robustness of these results.[19] Model (3) interacts treatment with a measure of treatment intensity.[20] Model (4) reports results from a specification in which suspect variables are first-differenced, given concerns about unit roots. As Table 3 shows, estimates of the impact of redistricting on policing are not substantively different from those obtained in our preferred specification. The estimated impact of redistricting on the incarceration rate is slightly less robust to these alternatives. All estimates remain positive, but θ is not statistically significant when a measure of intensity of redistricting is introduced, or when suspect variables are first differenced.

4 Racial Threat or Black Agency?

Given conventional wisdom, these results are surprising. They suggest that, where black elected officials had a discernible impact on punitive outcomes, they exacerbated rather than attenuated trends towards increased policing and imprisonment. Moreover, the evidence considered thus far is not just correlational. The exogenous

This gives an impression of the relative trends.

18. Between 1981 and 1996, the average imprisonment rate in redistricted states increased by about 267 prisoners per 100,000 people, and the average level of officers by about 87 for every 100,000 people.

19. The last two columns of Table 3 also show which of these results are robust to aggregation and block-bootstrapping, which are two strategies recommended as a defense against the fact that D-in-D data are often autocorrelated (Bertrand, Duflo, and Mullainathan 2004). Note that the recommended corrections reduce the probability of Type I error, but at the cost of lower power.

20. We measure intensity by the number of new districts created after redistricting as a percentage of electoral districts that existed pre-redistricting. The estimate shown in Table 3 reports the effect of redistricting evaluated at the median intensity of redistricting.

influx of black elected officials into mainly Southern states in the early 1990's seems to have sparked a punitive turn. However, any analysis of how covariates and outcomes move in tandem has the inescapable shortcoming of black-boxing the operative mechanisms. In this case, this problem manifests itself in an obvious way. For at least two reasons, one might doubt that black elected officials themselves did the legislative work of turning policy in a punitive direction.

First, the influx of black politicians caused by redistricting was not overwhelming. Between 1990 and 1995, black legislators as a percentage of all legislators increase by an average of 6 percentage points in the redistricted states (or, an average of 11 new minority legislators in every state), compared to about 1% in the not-redistricted states. Since they never amounted to a critical majority in state or federal legislatures, could their influence really explain large increases in punitive outcomes? On the other hand, in line with the racial threat perspective, it could be that the uptick in punitive trends was an indirect response to the influx. In other words, perhaps dominant forces in the state legislatures were antagonized and thus spurred to action by black political empowerment.

The next section considers the evidence for and against the "racial threat" interpretation of these results, but first we try to capture legislative dynamics by analyzing a dependent variable that is closer to the legislative floor. Spelman (2009) argues that spending outcomes have been neglected by the existing scholarship of the punitive turn. After all, several determinants of the state-level incarceration rate are in the hands of other actors (judges, prosecutors, and police), and only a small minority of police hires are made by state agencies. In effect, in modeling the officer rate at the state level, we assume that the intensity of policing reflects political and fiscal priorities set at the state level.

Thus, we also analyzed the level of state spending on corrections.[21] As gauges of state-level punitiveness, of course, this measure has its own shortcomings, which is why this was not our preferred strategy. Spending on corrections is only one way in which we expect politicians to affect incarceration. Tables 2 and 3 also present the results of substituting the corrections rate into the models described earlier. These results provide reason for pause. In the panel regressions, the estimated impact of black political representation on corrections spending is negative, and statistically significant at $\alpha = 0.10$. A within-state standard deviation change in the level of black representation is associated with just under a 4% decline in state spending on corrections (give or take 4 percentage points). The difference-in-differences analysis yields muddier results. As shown in Table 3, the level of state spending on corrections increased by more in redistricted states than in not-redistricted counterparts. The estimated impact of redistricting is negative once controls are included, but the estimates are imprecise. The only specification in which the estimated impact of redistricting is statistically significant from zero is Model (3), which incorporates a measure of treatment intensity. Here, redistricting of median intensity is estimated to cause a bit more than a 12% decline in corrections spending (give or take 10%).

Given earlier results, these results are puzzling. How can black political representation be positively associated with punitive outcomes, but negatively associated with spending on corrections? To answer this question we turn to other kinds of evidence to build a fuller interpretation of the racial politics of the punitive turn. In Section 4.1, we examine levels of and trends in black and white public opinion on a host of relevant questions. We do this in order to delimit plausible interpretations of our earlier findings. If significant majorities of blacks expressed hostility to police and

21. Spelman (2009) analyzed capital outlays on corrections facilities. We model total spending, instead, since data on capital spending are very sparse over the period we examine.

prisons this would cast doubt on the finding that their elected representatives exacerbated punitive outcomes, and provide support for the conventional wisdom reflected in the fiscal results. In Section 4.2 we test whether white revanchism better explains the coincidence of punitive outcomes and the BEO influx. Last, in Section 4.3, we examine the voting patterns of black Representatives at the federal level in order to gage the direct impact of black representation in one key arena for the production of criminal justice policy.

4.1 A Punitive Public

In order to analyze black and white public opinion, we gathered information from a number of nationally-representative surveys. In all, we examined 39 questions asked between 1955 and 2014 to roughly 300,000 respondents (251,000 of whom were white, and 34,000 black).[22] Our approach largely follows Enns (2014, 2016), but with one important amendment. Enns assumes that all questions illuminate a single latent dimension of public opinion, which he labels *punitiveness*. To this point, this is the measure we have employed in our panel analysis. Enns reasons that longitudinal and cross-sectional variation in responses to these questions are comparable. Increased distrust of the police, he reasons, should express the same shift in sentiment indicated by an increase in support for the death penalty. Correspondingly, respondents more likely to distrust the police should also be more likely to support the death penalty. We relax this assumption in our own analysis in order to shed light on what Zimring and Johnson (2006) identified as three distinct dimensions of public opinion bearing on the punitive turn: punitiveness (questions about the death penalty, suspects' rights, and the harshness of courts), crime anxiety (questions about how much should

22. For information on how frequently each question was asked, and during which years, see Table 6 in the Online Appendix.

be spent on fighting crime), and mistrust (questions about confidence in existing criminal justice institutions). Disaggregating the data in this way helps clarify the nature of the racial gap as well as trends over time, which vary depending on the dimension under analysis.

Figure 7 presents estimates of the proportion of black and white respondents giving punitive (or anxious or mistrustful) responses to each of the 39 questions in our dataset, out of all of those who gave either punitive or non-punitive responses. The y-axis is grouped by dimension, and ordered by the proportion of blacks answering punitively. Points to the right of the dashed line indicate that more respondents answered punitively than did not. Figure 8 displays the racial gap that corresponds. The y-axis is again grouped by dimension, and then ordered by the magnitude of the average difference between whites and blacks. Points to the right therefore indicate that whites were more punitive, anxious or mistrustful than blacks, and points to the left indicate the reverse.

In each dimension, the story is different. Whites tended to give much more punitive answers than blacks. This is in line with the conventional view (Bobo et al. 2004), but the magnitude and consistency of this gap is worth noting. In seventeen out of the nineteen questions we examined, whites were more punitive. The raw proportions do convey important information, alongside this gap, but it is not easy to discern patterns. This is because these raw proportions mix two kinds of variation: some is due to differences in when questions were asked, and some is due to the idiosyncrasies of the questions themselves. Sorting one from the other is not straightforward within the limits of our approach, but note one telling pattern. As shown in Figure 10, questions which prime respondents to a choice between alternative policies are significantly less likely to elicit punitive answers than questions which ask, in effect, for an up-or-down vote on a particular policy (Cullen, Fisher, and Applegate 2000). On average, almost

65% of blacks favor non-punitive to punitive options when given a choice. Contrast this to the roughly 75% of blacks giving clear-cut answers who expressed support for Clinton's crime bill, or the 90% who responded that courts should treat suspects more harshly. Questions without obvious alternatives seem to gauge desperation as much as punitiveness. They capture the fact that respondents prefer something be done to nothing at all. The high proportion who answer not-punitively when presented with alternatives bears emphasizing. We return to this point in our conclusion.

In the second dimension, racial differences were less stark. Both whites *and* blacks were consistently very anxious about crime. Blacks were about 10% more likely to say that they were worried or felt inadequately protected. The other five questions all ask whether money should be spent on dealing with crime. In the three which only refer to crime spending in the abstract, there is no significant racial gap. As Figure 7 shows, over 90% of those giving clear-cut answers say that it should. But in the two that refer to specific institutions (the police and law enforcement), a gap does appear, suggesting perhaps that these questions gauge mistrust alongside anxiety about crime.

Blacks were also significantly more likely to express mistrust of criminal justice institutions. These differences were particularly acute when respondents were asked about the police. In each case, blacks expressed higher levels of mistrust. When asked about the courts and/or the criminal justice system in the abstract, the pattern was more mixed and differences generally slighter: in one case whites were more mistrustful, and in two cases blacks were. Again, the racial gap should be interpreted alongside the raw proportions: even though blacks were much more mistrustful of the police, in some cases large majorities answered that they respected police.[23]

23. In other cases, a majority answered the opposite. Within the limits of our approach, we cannot clarify whether this specific variation is due to sampling or wording differences, or due to the different span covered by questions that are otherwise similar.

This approach illuminates the racial gap, but it ignores variation in punitiveness, anxiety and mistrust by time and place. To estimate this variation, we proceed in steps. First, for each question, we modeled the probability of a punitive, mistrustful or anxious response as a function of gender, race, age group, education level, year, and state (or region, where state-level info was unavailable).[24] We used the resulting model to predict the probability of a punitive (or anxious or mistrustful) response for each permutation of demographic and geographic characteristics. Last, to estimate state or state-race opinion in each of the years in which a question was asked, we weighted each of these demographic-geographic types by their share of the relevant population.[25] These steps give state-race-year estimates of responses to each question. To make analysis tractable, we follow Enns and estimate trends in each of the three dimensions, by pooling information across questions in a given dimension using Stimson's Dyad Ratios algorithm (Bartle, Dellepiane-Avellaneda, and Stimson 2011; Stimson 1999).

Figure 9 plots the results of running Stimson's algorithm separately in each of the three dimensions, and for each state-race permutation. The faded lines plot these state-race trends, and the bold lines plot the state-level averages. These indices reveal information that the earlier analysis obscured. There is a discernible increase in the proportion of white *and* black people giving punitive and mistrustful answers between the early 1970s and the mid-to-late 1990s—a period which corresponds quite well with the punitive turn in policy. In fact, black punitiveness increases more than white.

24. We fit models of varying complexity, the most complex of which made allowances for interactions between race, place, and time. To adjudicate between the three models we trialed, we estimated three different models on a training set (a random sample of 80% of the respondents), and picked the model which best fit respondent patterns in the test set (the other 20%). Not all questions are fit with the same model, either for reasons of fit or because more complex models failed to converge. Section D of the Appendix gives details.

25. Together, this set of steps has come to be known as MRP (multilevel regression and poststratification). For more details about the approach and its advantages vis-a-vis simple disaggregation, see Park, Gelman, and Bafumi (2004) and Lax and Phillips (2009b).

Anxiety about crime, on the other hand, remains at elevated levels throughout.[26]

4.1.1 Black Politicians and Public Opinion

These graphs reveal trends that broadly correlate with the punitive turn. While we found no evidence, unlike Enns (2016), that punitiveness drove policy directly (see Table 2), shifts in public opinion could still have shaped policy indirectly. To test this, we add interactions between our covariates and state-specific measures of public opinion to the models estimated earlier. Our first model interacts Enns's composite measure of punitiveness with each of the two political covariates in our model (democratic control, and black representation). To examine whether these results were robust to disaggregating the different dimensions of public opinion, we replaced Enns's measure of punitiveness with measures of state-level opinion in each of the three dimensions, and interacted each of the same two covariates. Table 4 presents estimates of the long-run effect of black elected officials in each of these models, evaluated under two different scenarios. Scenario (A) refers to the expected long-run effect of an influx of black elected officials in a situation of low concern—with punitiveness, anxiety, and mistrust set to their 20th percentile values. Scenario (B) calculates the same effect, but in a very different context—with punitiveness, mistrust and anxiety set to their 80th percentile values.

Note that Model (1) only includes punitiveness, as defined by Enns, so these scenarios are distinguished by movements in that variable alone. Regardless, in lumping movements in these three dimensions into two scenarios, in Model (2), one might wonder why we expect high levels of mistrust to have the same mediating effect on political outcomes as high punitiveness and anxiety. Indeed, if people turn mistrustful of crim-

26. There is no meaningful interpretation of the underlying scale in Stimson's algorithm, but some meaning can be imputed given an interpretation of the levels of all the questions on which it is based.

inal justice institutions, should they not be *less* likely to demand police and prisons? We considered this possibility, but all evidence points in the opposite direction. The interaction coefficients estimated below suggest that higher levels of mistrust make black politicians more punitive. We suspect this is because mistrust registers dissatisfaction with existing criminal justice institutions, much of which stems from anxiety about crime. In a political environment which forecloses other alternatives, it is not surprising that this is channeled into support for police and prisons.

Recall that in our baseline specification, the effect of black political representation on the incarceration rate was positive, but insignificant. With the interaction included, however, a more informative story takes shape. In both models, the effect of black politicians on the incarceration rate is significantly more positive at high than at low levels of concern.[27] In Model (1), this difference is significantly different from zero, even though the individual estimates are not themselves different. Results from Model (2) are even more suggestive. Here, where the population turns more punitive, anxious, and mistrustful of courts and criminal justice institutions, an influx of black politicians is associated with the subsequent addition of 26 prisoners for every 100,000 people (give or take 26). Where they are not, the same influx is associated with a decline of about 7 prisoners (give or take about 18).

This same specification invites an interpretation of the otherwise puzzling finding that black political representation had a negative effect on corrections spending. With interactions included, this negative effect is pronounced (substantially larger, and significant at $\alpha = 0.05$ and $\alpha = 0.10$) in a context of low concern—the same context in which the estimated effect on the incarceration rate is substantially lower and/or negative. It is attenuated in Scenario (B).[28]

27. This is shown in the row denoted by Δ, which is the estimate of the difference between the long-run multipliers in the two scenarios.

28. As Table 4 shows, this difference is statistically significant at $\alpha = 0.10$ in Model (1), and close

In summary, the evidence suggests black politicians had a positive effect on the incarceration rate, but that this effect was concentrated in a context of high black concern. In the opposite context, they may well have had the opposite effect. Future work might consider whether anything meaningful can be learned from the domain in which they had these dueling effects. A positive effect on the incarceration rate (absent the same on spending) suggests a legislative impact at times of high concern (in the late 1980s and early 1990s), whereas a negative effect on corrections spending (absent clear-cut evidence of the same, on the incarceration rate) suggests a predominantly budgetary influence in (later and more recent) low-concern periods.

With regards to policing, the implications are different. Here, we find that an influx of black legislators has a positive long-run effect on the officer rate in *both* scenarios. High black concern amplifies this association, but not dramatically. The balance of evidence suggests, as before, that black politicians had a positive effect on policing, and that this effect was largely impervious to the context set by aggregate public opinion.

4.2 Revanchist Whites?

Redistricting occurred in mostly Southern states at the same time as a flight of Southern whites to the Republican party. Could the rise in punitive outcomes around redistricting have been the work of Southern Republicans empowered by an influx of white voters? We consider this unlikely. Over the redistricted period, Republicans did not have veto-proof control of *any* of the state legislatures affected by redistricting. Their first gains, in these terms, came in the late 1990s, well after the end of the period we considered. Some scholars date the departure of Southern whites to the late 1960s (Kuziemko and Washington 2015), but, as Figure 11 shows, this was most

to statistically significant in Model (2).

pronounced in presidential elections. In congressional elections, realignment was more gradual. As Gavin Wright (2016, 18) argues, "...the median southern white voter cast a ballot for a moderate-to-liberal Democrat until 1994."

But if revanchism was not partisan, it could still have been racial. The influx of black politicians might have triggered a punitive alliance of the white majority, Democrats and Republicans alike. However, this interpretation understates the extent to which the Democratic party had been transformed by the Civil Rights movement. As Wright (2016) argues, by the 1980s and 1990s, the Democratic party had evolved into a multiracial coalition in the South. This strategy yielded success in the 1980s and immediately after the post-redistricting elections, where they increased their share of veto-proof control in redistricted states to 90% (in 1991 and 1992). Success of this kind would seem a strange pretext for a revanchist party revolt.

If outcomes around redistricting were the work of a majority white reaction to black advance, their hand should have been visible in other policy outcomes. We examined the level of AFDC benefits paid out by individual states. Given the highly racialized character of welfare provision, particularly in the South, we would expect a revanchist white majority to have cut these benefits. But we find no evidence of this. The estimated impact of redistricting is actually positive, and statistically significant in a simple comparison of means. Put another way, while all states were cutting welfare at this time (see Table 3), those affected by redistricting cut it *less* than those which were not. While Table 3 shows that this result is not robust to adding controls or to the other specifications we trial, the estimate never turns negative. In short, the balance of evidence suggests that redistricting empowered black representatives. Of course, our inference is not that they made policy unilaterally. But redistricting gave them the clout to bargain more effectively with whites inside and outside of their party. As the next section argues, they made policy, even if not under conditions of their own choosing.

4.3 Federal Voting Patterns

While the evidence accumulated thus far suggests a punitive impact on policing and incarceration, nothing in our analysis explains *how* an increase of black politicians at the state level translated to more prisoners or to more police. It is not straightforward, unfortunately, to analyze legislative histories at the state level. No long-run, centralized repository of voting records exists, nor is there a record of the identifiably punitive legislation to analyze. Constructing a database like this one for select states should be a priority for future research, but it is likely to be considerably resource-intensive. In lieu of this, we propose a substitute case study: we examine the role of African-American congressmen and congresswomen in the federal House of Representatives.[29] Their votes were a matter of public record, and the availability of roll call data for amendments as well as bills allows for fine-grained analysis of the legislative process.

4.4 The Punitive Turn in Congress

In 1968, when it passed Johnson's Omnibus Crime Control Act, the House of Representatives had 6 black members. By 2014, it had 45—the largest increases occurring in the 1970s and 1990s, a result of the voting rights reforms discussed earlier. These incoming members have almost all been affiliated with the Democratic party, and since 1971 they have been organized in the Congressional Black Caucus (CBC)—a remarkably cohesive voting bloc (Pinney and Serra 1999). We tracked the votes of these black representatives on federal crime policy from 1968 to 2015, and compared

29. We look only at the House because black members in the Senate have always been too few to constitute a significant voting bloc.

them to the voting record of non-black Democrats.

Figure 12 plots the percentage of representatives who voted in a punitive direction on federal crime bills. In the case of those bills and amendments that increased mandatory minimum sentences or gave more power and resources to prosecutors and police, this is the percentage who voted "yea." In the case of those bills and amendments that promised to reduce sentence length or severity, restrict police or prosecutorial power, or provide alternatives to incarceration this is the percentage who voted "nay." Non-voting members are included in the denominator.[30] In line with the evidence from the opinion polls, these data yield contrasting findings. On the one hand, they indicate that African-American members of the House have been consistently less punitive than their fellow Democrats in their voting patterns. In 22 of 28 votes a smaller percentage of African-American politicians took punitive positions than did other Democrats, and in 13 of these cases the difference was significant. Note that in this figure the error bars reflect the relative size of each group. They can be thought of as the impact of a marginal vote change, rather than an underlying population estimate. There is no clear trend in the gap between the percentage of Democrats and CBC members voting punitively, but the increasing numbers of African-American congressmen narrow the error bars and demonstrate that the gap is consistently significant in this sense.

On the other hand, Figure 12 shows that an absolute majority of African-American representatives voted in favor of each of the major federal crime bills of the punitive turn: the Omnibus Crime Control of 1968; the Comprehensive Crime Control Act of 1984; and the Violent Crime Control and Law Enforcement Act of 1994. It also shows that the majority of CBC members consistently supported bills that increased

30. We tried to include all bills relevant to crime and punishment, whether they increased or decreased punitiveness. Unfortunately many bills were subject to a voice vote, in which case individual votes were not recorded.

mandatory minimums for those at the centre of public outrage, such as drug dealers in the 1980s, and sex offenders in the 1990s and 2000s. For instance, a majority of Caucus members (65%) voted in favor of the Anti-Drug Abuse Act of 1986, which imposed the notorious 100-1 disparity in sentencing for crack vs. powder cocaine. Yet from the 1990s CBC members pushed to repeal that disparity, finally succeeding with the Fair Sentencing Act of 2010 (not shown in Figure 12 because it was a voice vote).

4.5 Clinton's Crime Bill

Overall, CBC members appear as reluctant supporters: at times opposing tougher amendments supported by most Democrats, but generally following the party line on crucial roll call votes. A telling example is the Violent Crime Control and Law Enforcement Act of 1994. Clinton's signature crime bill added 60 new federal death penalties, increased mandatory minimums for federal drug offenders, and granted $8 billion of federal aid for prison construction. Much of that aid was made contingent on states passing truth-in-sentencing policies requiring that all violent offenders serve at least 85% of their sentence. By 1998, 28 states and the District of Columbia had qualified. Since truth-in-sentencing unilaterally increased sentence length scholars have seen the 1994 bill as a major contributor to the continued expansion of American prison populations (Travis, Western, and Redburn 2014, 79-83).

The 1994 bill came at a time when the CBC was at the height of its influence (Frymer 1999, 150,176), and the CBC played an important role in shaping early drafts of the bill. The previous fall CBC members in the House had blocked passage of a draconian crime bill passed by the Senate, which Clinton had supported. In January Jesse Jackson's National Rainbow Coalition held a summit on crime, in which CBC leader Kweisi Mfume (D-MD) complained the Senate's bill would only "find better ways to incarcerate people." CBC members on the House judiciary committee subsequently ensured the first draft of the House bill contained crime-prevention programs that would direct funds to poor neighborhoods, and successfully fought to add a "racial justice" provision, allowing death row inmates to challenge a sentence as racially discriminatory.[31] Overall, the CBC's interventions led to a less punitive crime bill in the House.

The vote counts in Figure 13 show this influence. As the bill passed through the legislative process, CBC members often opposed amendments designed to make it more punitive. In particular, they were critical to defeating a Republican attempt (A019) to remove the racial justice provisions. A majority of members also opposed (albeit less successfully) amendments to extend and strengthen the federal death penalty (A003–A010), to add drug offenses to the bill's "three strikes and you're out" provisions (A011), and to abolish Pell Grants for federal prisoners (A023).[32] And while a third of the membership supported adding "truth-in-sentencing" to the bill (A014), CBC votes were critical to passing an amendment (A017) that replaced the truth-in-sentencing language ("85% time served") with the looser requirement that the state had to have "sufficiently severe punishment for violent repeat offenders" in order to receive federal funds.

Despite the influence CBC members exerted in both drafting and preserving the crime prevention and racial justice provisions of the House bill, a third of the mem-

31. Republicans in the House had previously blocked a Racial Justice Act containing the same provisions—on the grounds that it would lead to racial quotas in sentencing. 15% of the judiciary committee were CBC members in 1993-1994 (Canon 1995).

32. The Kopetski, Derrick and Wynn amendments each represented failed Democratic attempts to offer a compromise on a part of the bill that had already been amended in a punitive direction. For instance, Wynn (D-MD), a CBC member, proposed to restrict rather than abolish Pell Grants to prisoners, leaving it to individual states to decide. Although they would actually have made the bill more punitive than its initial draft, we have coded them as non-punitive in Figure 13 because they were intended to make the bill less punitive than it had become.

bership still opposed the bill when it first came to a general vote on April 21. These members were holding out against the death penalty provisions that had been added. These hopes were soon dashed, however. Democratic leaders would eventually win even more CBC members over to the final and far more draconian version of the bill. In this process the CBC was a victim of both maneuvering by House leaders and its own internal divisions over crime.

First, in the initial congressional conference, convened to combine House and Senate versions of the bill, Senate leader Joe Biden convinced Democratic senators to drop the racial justice constraints on the death penalty.[33] Conferees also reinserted the truth-in-sentencing language that the CBC had helped to remove. The conference report was widely seen as a defeat for the CBC. When it was brought to the House on 11 August, the nay votes of CBC members were pivotal to defeating the bill in a procedural vote. The loss came as a shock to Democratic leaders. Almost all Republicans voted against it, and they were joined by 48 anti gun-control Democrats and 11 CBC members. In justifying their defection from the party line they cited the dropping of the racial justice provisions (Seelye 1994; Frymer 1999, 175).

Next, in the following week the White House and Democratic leaders managed to convince some key CBC holdouts—Charles Rangel (NY), Cleo Fields (LA) and John Lewis (GA)—to back the bill. To do this they exploited divisions within the black community, by collecting endorsements for the bill from dozens of prominent African-American religious leaders and ten black big-city mayors. Moreover, House leaders discovered they they could pass the bill without CBC support, by attracting moderate Republicans to their side (Kim 2002, 66). The resulting compromise involved a drastic reduction of funding for crime-prevention measures that CBC members had fought

33. After House conferees had voted to include these provisions Biden announced: "The question is whether to accept the House provision, racial justice, which will kill the bill." Since Republican conferees abstained in this vote the provision was removed by a majority vote of Democratic senators.

to defend.[34]

Crucially, even as the CBC saw some of their most valued provisions stripped from the bill, the majority of members continued to support it—illustrating the bind confronting black elected officials. When the House voted on the final bill, on August 21, the CBC voted 24 to 12 in favor. How do we explain this? Some have argued that Democratic party loyalty, or pressure from higher ups, trumped concerns about the bill's punitiveness (Fortner 2015b). Others have claimed that they were motivated by fear of an even more punitive Republican bill if Clinton's was to fail (Hinton, Kohler-Hausmann, and Weaver 2016). But while both theories help to explain the swing votes of some CBC members, they provide less insight into the support shown by many CBC members for some of the amendments which made the bill even more punitive.

For example, eight CBC members (20% of the membership) supported the death penalty for "drug kingpins" (A006), one third voted in favor of the Chapman amendment that added truth-in-sentencing to the bill (A014), and more than half supported the Brooks amendment that restricted the ability of prisoners to sue prison administrators and banned weight-lifting in prison (A018). When questioned about theses decisions CBC members, like most other members of congress, tended to cite the urgent need do something about a widely perceived crime epidemic. For example an interview with Alan Wheat (D-KS) explained that "the crime bill's promise of more police, more prisons and more money for crime prevention was too important to jeopardize by holding out for the racial-justice provision"(Sawyer 1994).

In keeping with this sentiment, polls indicated strong public support for Clinton's

34. As critics of these measures focused on a small number of earmarks for "midnight basketball" programs in inner city neighborhoods, crime prevention was increasingly identified by both sides of the debate as a black issue (Wheelock and Hartmann 2007). In the end, funding for crime prevention was cut by 20% ($2 billion), while funding for prisons was cut by 7% ($800 million).

crime bill, especially among African Americans.[35] As Figure 9 shows, black punitiveness, anxiety and mistrust were all at or close to their peak levels at this time. Moreover, many prominent black leaders had come out strongly in favor of the bill, urging CBC members to put aside their reservations. In the end, the combination of political constraints and deep crime anxiety shoehorned CBC members into backing a bill that many of them had vociferously criticized.

5 Conclusion

In this article we have gathered a large amount of evidence that is difficult to reconcile with the conventional view of the racial politics of the punitive turn. In state-level panel regressions spanning almost forty years, we found that black representation was associated with higher levels of policing per capita in subsequent years. Results around carceral outcomes were also unexpected: while black elected officials had a negative and weakly significant effect on corrections spending, their effect on incarceration was positive. This ambiguous finding is probably explained by context, since the positive impact on incarceration was concentrated during the peak of public punitiveness, and the negative impact on carceral spending during its trough. Our analysis of the impact of federally-mandated redistricting in mostly Southern states in the early 1990s found that the positive association between black political representation and subsequent levels of policing and imprisonment was not obviously spurious. Redistricted states saw larger increases in policing and imprisonment than their non-redistricted counterparts.

In general, public opinion and legislative evidence affirmed that these trends are unlikely to be the product of "white backlash." Polling data shows that blacks became

35. The third row in Figure 7 shows that 76% of blacks with clear views supported the bill, compared to only 59% of whites.

more punitive during the period of the punitive turn, and that—alongside evidence of a "racial gap" in punitiveness—absolute levels of black punitiveness and crime anxiety were remarkably high. We found parallels in the congressional record: black representatives in Congress expressed concern with punitive legislation, but when faced with up and down votes a majority invariably opted for more police and prisons.

One could interpret these findings as a testament to the exceptionally democratic character of the American criminal justice system. We have shown that black Americans were racked by fears of crime, and that their representatives made policy accordingly. This interpretation fits with those who have emphasized the "populist" character of the punitive turn (Enns 2016). Yet we also found that black congressmen and congresswomen provided majority support for the Clinton crime bill only when the less punitive measures many of them had supported were defeated. Furthermore, we found that the public answered much less punitively when offered a choice between punitive and non-punitive alternatives. Both pieces of evidence suggest another reading of our results: that support for tough-on-crime policies among black politicians and their constituents may best be explained by structural and political constraints that narrowed the field of policy options (Alexander 2016; Hinton et al. 2016; Forman Jr, forthcoming). African-Americans and their elected representatives may have chosen punitiveness, but they did not do so under conditions of their own choosing.

While we believe that the evidence we have accumulated represents a significant advance, there is ample room for future improvements. First, we examined the politics of incarceration and policing together. The obvious benefit of this approach is that it allowed us to interrogate the conventional account in two salient domains of criminal justice policy–both of which have entertained variants of the "white backlash"-based explanation of punitiveness. The cost, of course, is that the politics of policing might be substantially different from the politics of incarceration. We found some evidence of this in our panel specifications: the effect of black elected officials on policing was consistently more statistically noticeable than the analogous effect on incarceration, and this effect seemed more impervious to context. It is plausible that black politicians saw police rather than prisons as their first line of defense against crime. Future work should consider disentangling these two outcomes more deliberately than we have done here.

Second, while our analysis of public opinion makes use of better data and better techniques than have previously been employed in this domain, our approach is inefficient. Recall that we estimate 39 different models, many of which were limited by data availability. These estimates were then pooled via Stimson's Dyad Ratios algorithm. One should merge these two steps, and fit a single model which would estimate each question's idiosyncrasies alongside the impact of key covariates on respondent answers. We are pursuing this approach in current work. Empirically, these estimates raise a host of relevant but as-yet unanswered questions. Do black and white publics respond to crime rates, to levels of policing, levels of incarceration, and/or to the media's presentation of crime? Do they do so differently, or in similar ways?

Third, though we have referred to the considerable constraints under which black politicians and the black public supported punitive legislation, this paper has done little to clarify their character or their precise importance. Certainly, the fact that the political process narrowed the field of possible policy responses is unsurprising, but alternatives were not abandoned because they were technically infeasible or unimaginable. Recall that in 1968 the Kerner Report urged the Federal government, as a matter of urgency, to combat riots and crime with a jobs program, an integrated education system, expanded welfare and decent housing. If a Marshall Plan could be crafted for Europe, why not for the ghetto?

The reasons are mostly beyond the scope of this paper, but they deserve to be better understood. Black elected officials were obviously bound by the fact that they were, at best, only ever a significant minority in state and federal legislatures. They had to make their way in an environment in which "law and order" policies were already dominant. Perhaps it is difficult to imagine that alliances with the incumbent political elite could have produced anything but a punitive agenda.

The structure of American political power hamstrung them further. As Miller (2010) argues, alternatives to a punitive agenda have flourished at the level of local government, where black victims are most likely to be involved, but have floundered as they have made their way up the political food chain. She attributes this to balkanizing effects of American federalism, which both impede collective action at the municipal level and limit the "scope and tenor of the central government's power to address social problems" (ibid., 807). These constraints combine to make tougher sentencing the policy of least resistance: state-level politicians can credibly claim representation while also avoiding well-supported but significantly more difficult alternatives.

This said, in recent years the political mainstream has shifted in favor of penal reform, and black politicians have often led this charge (Barker 2010). Our own data shows some signs of this shift. We found that the negative effect of black political representation on corrections spending was concentrated in times of low punitiveness. It is far from clear that these reform attempts will add up to a wholesale change in the criminal justice system (Gottschalk 2014), but falling rates of crime and a growing awareness of the social costs of mass incarceration may, we hope, augur a new politics of punishment.

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Table 1: In-Sample Descriptive Statistics

	μ	σ_{Within}	$\sigma_{Between}$	% Var. Within	Δ
Incarceration Rate	269.80	132.90	105.70	64.80	Yes
Officer Rate	230.90	35.10	47.30	39.80	No
Corrections Spending	4.41	0.55	0.34	72.50	No
Black Political Representation (%)	5.37	2.06	4.77	28.10	No
Democratic Control	0.24	0.46	0.52	48.20	No
Violent Crimes per 100,000	440.60	93.80	207.00	20.50	No
GDP per capita (Log)	10.30	0.17	0.10	76.10	Yes
Growth Rate	10.30	0.17	0.10	75.70	Yes
Income Inequality	0.54	0.05	0.02	87.10	Yes
Tax Collection	192.30	41.10	49.10	52.70	No
Black Population (%)	9.86	0.75	9.40	1.09	No
Punitiveness (Enns)	57.50	5.69	1.65	92.30	No
Crack Index	0.89	0.85	0.63	70.60	
Unemployment Rate	6.77	1.58	1.45	55.40	
Poverty Rate	13.00	0.85	3.56	6.80	
Punitiveness (Ours)	69.50	8.61	1.81	95.80	
Anxiety	67.90	2.27	1.89	58.90	
Mistrust	42.20	4.57	2.28	80.00	

[1] All statistics refer to a consistent, single sample. For dependent variables, this is the sample specific to the preferred models for the given dependent variable. For independent variables, this is the preferred sample for the models of the incarceration rate. For the variables present only in the D-in-D analysis, this is the difference-in-difference sample.
[2] In the tables presenting our regression results, all estimates are multiplied by the average within-state standarard deviation of the relevant variable, to give an impression of effect size. These are given in the column 'σ_{Within}'.
[3] '% Var. Within' is the ratio of the sum of squares within states, to the total sum of squares. As discussed in the main text, the use of state fixed-effects across our specifications means that we make use of only this dimension of variance in our analyses.

Table 2: Results from Panel Regressions

	Incarceration Rate	Officer Rate	Corrections Spending
Lagged Dep. Vars			
Incarceration Rate$_{t-1}$	1.089**		
	(0.063)		
Incarceration Rate$_{t-2}$	-0.131*		
	(0.062)		
Officer Rate$_{t-1}$		0.557**	
		(0.072)	
Officer Rate$_{t-2}$		0.228**	
		(0.020)	
Officer Rate$_{t-3}$		0.116^{+}	
		(0.066)	
Officer Rate$_{t-4}$		-0.060*	
		(0.029)	
Corrections Spending$_{t-1}$			0.806**
			(0.021)
Short-Run Impact			
Black Political Representation (%)$_{t-1}$	0.568	0.957*	-0.656^{+}
	(0.657)	(0.421)	(0.355)
Democratic Control$_{t-1}$	0.168	-0.119	-0.542^{+}
	(0.412)	(0.262)	(0.312)
Violent Crimes per 100,000$_{t-1}$	2.160**	0.840**	0.608
	(0.657)	(0.280)	(0.405)
GDP per capita (Log)$_{t-1}$	-2.126	-0.817	7.994**
	(2.387)	(2.132)	(2.415)
Growth Rate$_{t-1}$	0.934	3.180	-5.446^{+}
	(2.442)	(2.525)	(2.919)
Income Inequality$_{t-1}$	-0.913	-2.045*	-0.241
	(0.943)	(0.810)	(0.642)
Tax Collection$_{t-1}$	1.308**	0.832**	0.961**
	(0.200)	(0.243)	(0.246)
Black Population (%)$_{t-1}$	-0.617	-0.615	-0.038
	(0.563)	(0.447)	(0.279)
Punitiveness (Enns)$_{t-1}$	1.730	-0.708	-1.786
	(5.159)	(5.169)	(2.884)
Long-Run Multiplier			
Black Political Representation (%)	13.240	6.041*	-3.368^{+}
	(16.187)	(2.572)	(1.932)
Democratic Control	3.694	-0.733	-2.817^{+}
	(10.151)	(1.757)	(1.512)
Violent Crimes per 100,000	51.454**	5.288**	3.141
	(15.085)	(1.750)	(2.126)
GDP per capita (Log)	-52.912	-5.292	41.516**
	(58.487)	(14.054)	(13.695)
Growth Rate	21.887	20.161	-28.448^{+}
	(61.492)	(15.807)	(16.296)
Income Inequality	-21.482	-12.991*	-1.325
	(25.026)	(6.100)	(3.339)
Tax Collection	31.224**	5.249**	4.958**
	(6.306)	(1.774)	(1.307)
Model Info			
Observations	1802	1764	1813
States	49	49	49
Range	1972-2008	1973-2008	1972-2008
Avg. N_i	36.8	36	37
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Lags of DV	2	4	1
Adj. R^2	0.937	0.876	0.911

[1] $^{**}p < 0.01$, $^{*}p < 0.05$, $^{+}p < 0.10$. Two-tailed tests. Standard errors in parentheses.
[2] To give an impression of the conditional effect size, we multiply all estimates by the average within-state standard deviation of the relevant variable. These are given in Table 1.

Table 3: Difference-in-Difference Estimates Around 1990 Redistricting

	Model	θ_{RD}	Aggregation?	Block-Bootstrap?
Incarceration Rate	(1) Simple	65.090^{**} (21.8)	Yes^{**}	Yes^{*}
	(2) Full	47.469^{+} (27.3)	No	Yes^{+}
	(3) New Districts	20.545 (16.1)	No	No
	(4) Δ	8.198 (7.42)	No	No
Officer Rate	(1) Simple	24.569^{**} (5.54)	Yes^{**}	Yes^{**}
	(2) Full	16.928^{**} (4.68)	No	Yes^{**}
	(3) New Districts	21.260^{**} (5.77)	No	Yes^{**}
	(4) Δ	19.447^{**} (5.09)	Yes^{+}	Yes^{**}
Corrections Spending	(1) Simple	1.272 (7.26)	No	No
	(2) Full	-3.556 (10.1)	No	No
	(3) New Districts	-12.346^{*} (4.97)	No	No
	(4) Δ	-2.340 (8.43)	No	No
AFDC Benefits	(1) Simple	15.524^{**} (5.29)	Yes^{**}	Yes^{*}
	(2) Full	7.619 (5.52)	No	No
	(3) New Districts	1.591 (6.64)	No	No
	(4) Δ	9.622 (5.86)	No	No

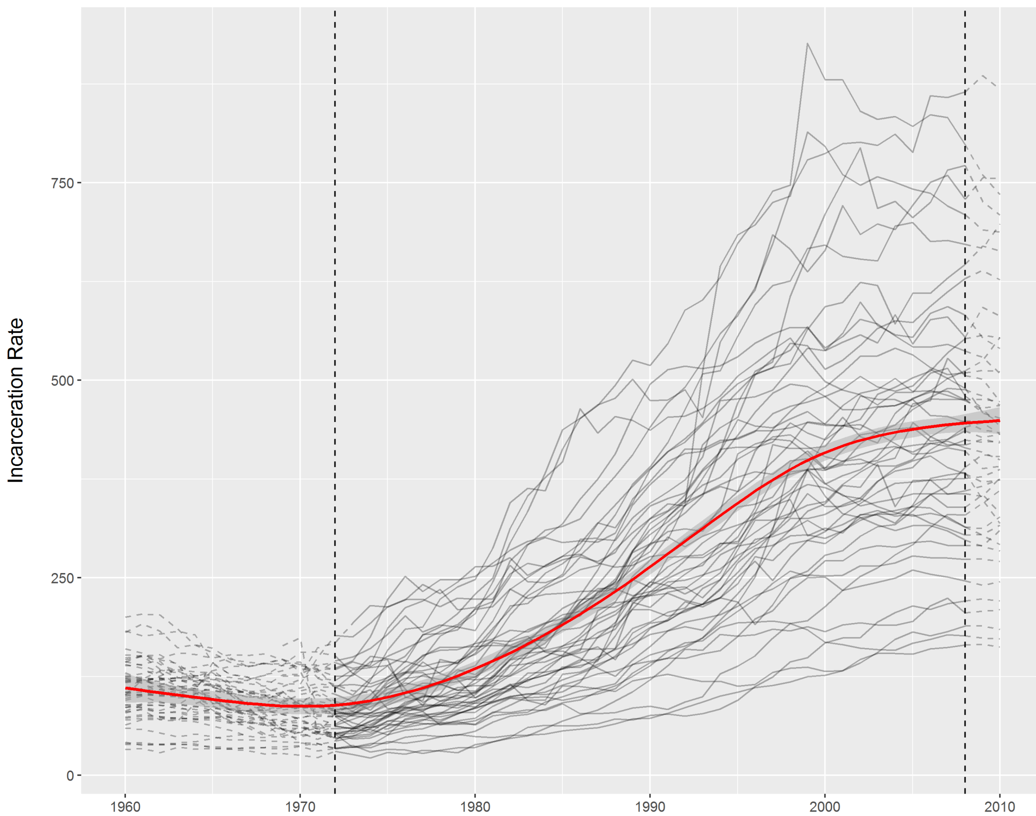
[1] $^{**}p < 0.01$, $^{*}p < 0.05$, $^{+}p < 0.10$. Two-tailed tests.
[2] The numbers in parentheses delimit a 95% confidence interval for the parameter estimate.
[3] Model (1) gives estimates of $\beta_{Treatment}$ from a straightforward comparison of means. Model (2) adds state and year fixed-effects, and a full set of controls. Model (3) interacts the treatment variable with a measure of the intensity of redistricting. Model (4) employs the first difference of all variables which might be unit root. See 7 for details.
[4] The last two columns report whether the relevant results were robust to aggregating the data to just two time periods and to block-bootstrapping, respectively. See Bertrand et al. (2004).
[5] As before, the estimates are multiplied by a scalar to ease intepretation. Where the dependent variable is logged, the estimate is multiplied by 100, and thus roughly interpretable as the expected percentage change in the dependent variable conditional on treatment. Where the treatment variable is interacted with a measure of the intensity of treatment, the estimates are multiplied by the median increase in black-majority districts in states which experienced redistricting.

Table 4: Long-Run Effect of Increase in Black Political Representation, Conditional on Black Public Opinion

	Scenario	(1)	(2)
Incarceration Rate	(A) Low	2.548	-7.096
		(13.3)	(17.5)
	(B) High	19.657	26.300*
		(14.9)	(13.8)
	Δ	17.283*	33.430**
		(8.53)	(12.4)
Officer Rate	(A) Low	8.330**	6.046*
		(2.09)	(2.95)
	(B) High	8.992**	10.279**
		(2.25)	(2.49)
	Δ	0.642	4.192
		(1.93)	(3.15)
Corrections Spending	(A) Low	-5.588*	-5.278^{+}
		(2.42)	(3.18)
	(B) High	-2.734	-1.699
		(1.99)	(2.06)
	Δ	2.887^{+}	3.611
		(1.68)	(2.67)

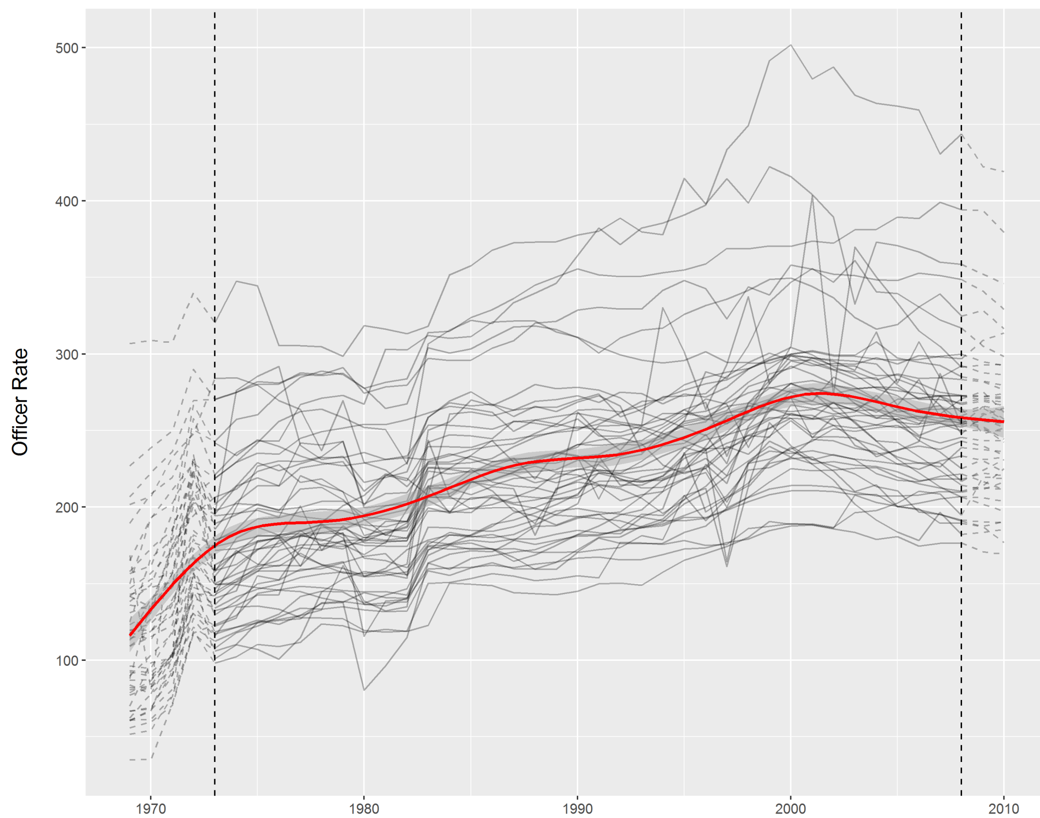
[1] $^{**}p < 0.01$, $^{*}p < 0.05$,$^{+}p < 0.10$. Two-tailed tests. Standard errors in parentheses.
[2] All estimates refer to the expected long-run effect of an influx of black legislators equivalent to the average within-state standard deviation.
[3] Under Scenario (A) we compute the estimated long-run effect when punitiveness, black mistrust, and black anxiety are set to their 20th percentile values. This denotes a 'low concern' scenario. By contrast, Scenario (B) denotes high punitiveness, high mistrust, and high anxiety. Here, both of these values are set to their 80th percentile values. Δ displays the estimated difference between the long-run multipliers in the two scenarios.

Figure 1: Incarcerated per 100,000



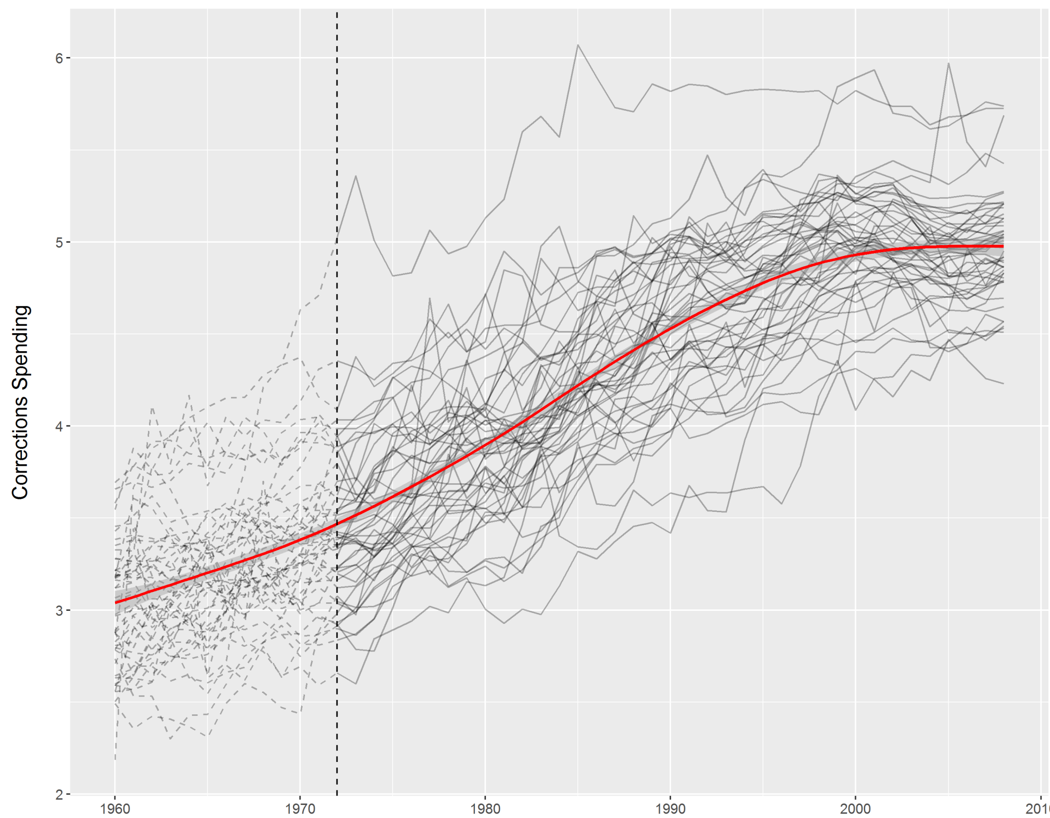
Dashed lines denote years for which we have data on the incarcerated population, but which are excluded from the regression analysis due to other missingness. The red line fits a locally-weighted smooth through the data.

Figure 2: Police Officers per 100,000



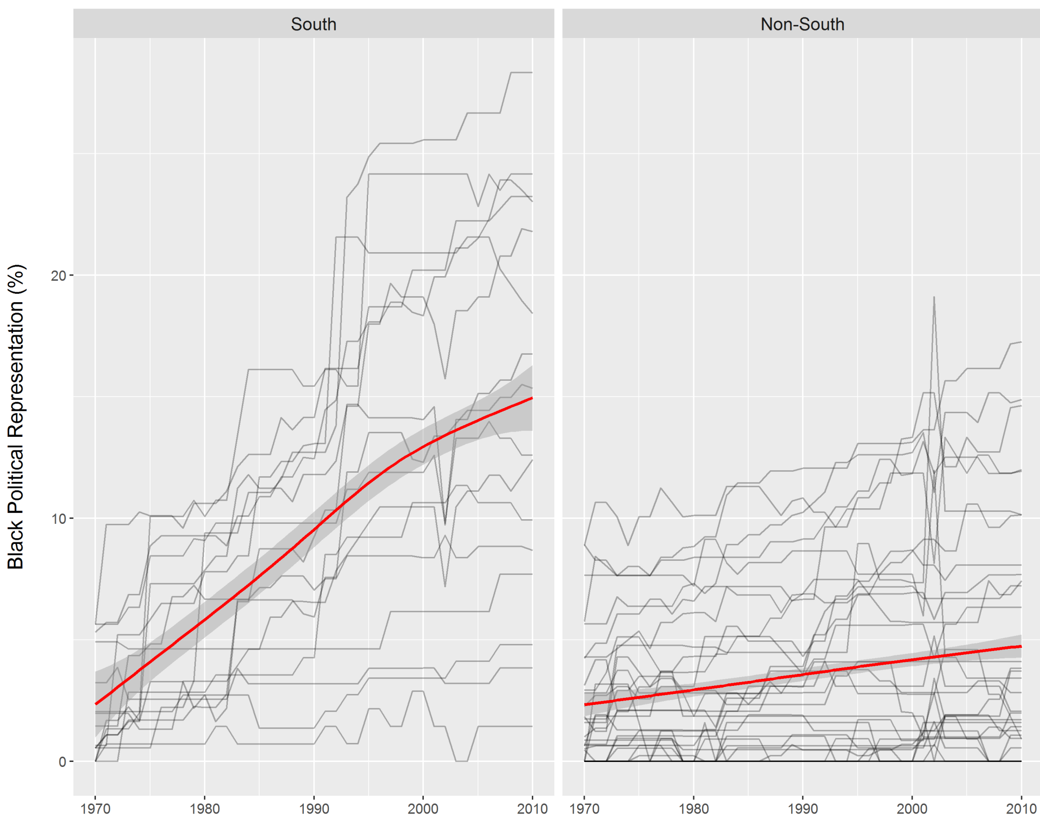
See notes after Figure 1

Figure 3: Corrections Spending per capita (Log)



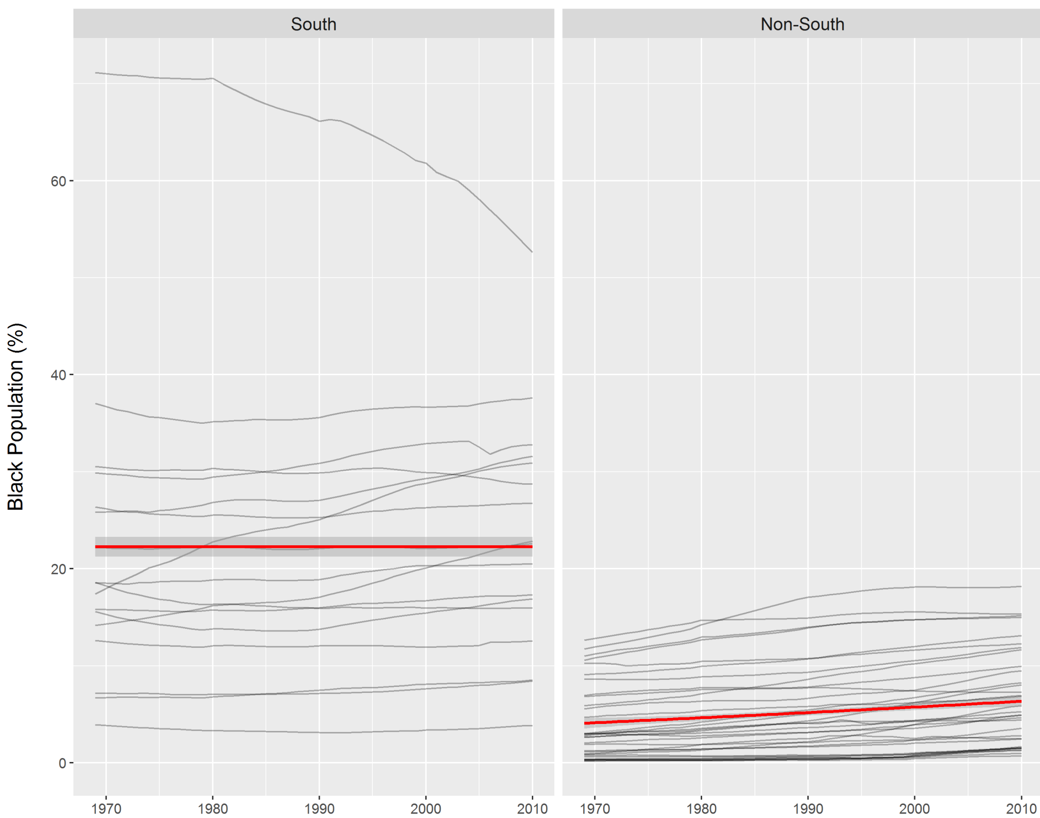
See notes after Figure 1

Figure 4: Black Political Representation



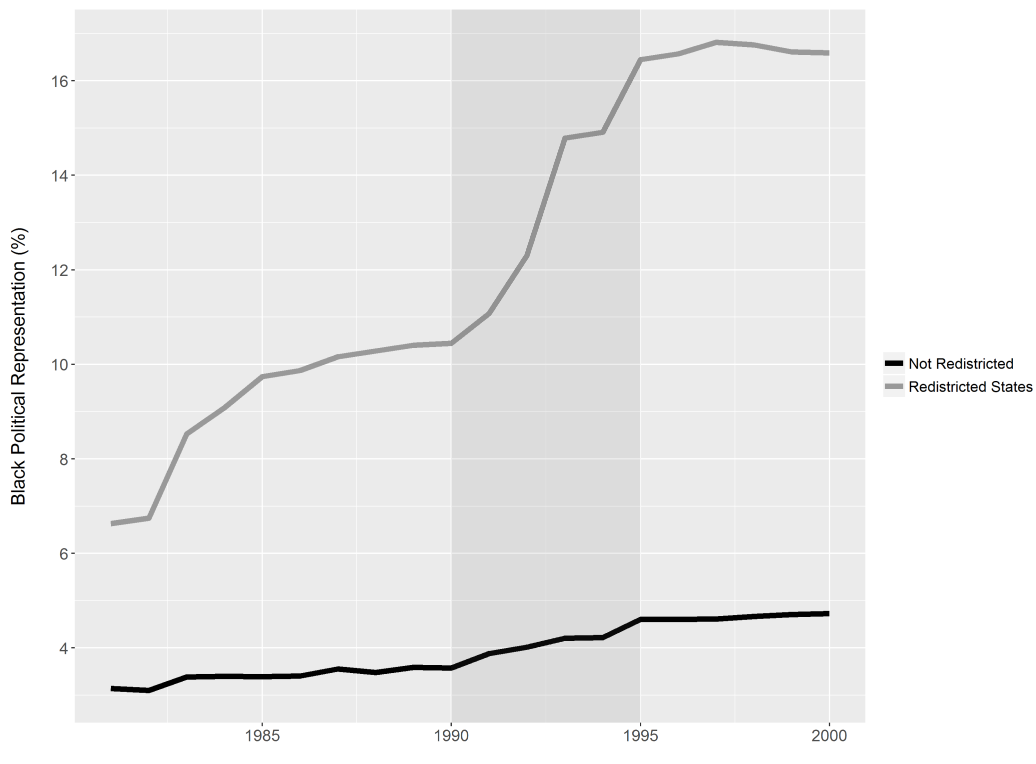
See notes after Figure 1

Figure 5: Black Population Share



See notes after Figure 1

Figure 6: Impact of 1990s Redistricting



Redistricting first affected electoral outcomes in the elections that took place at the end of 1990. This is reflected in the increase in black political representation first observed in 1991. The entire period of the influx is shaded in grey.

Figure 7: Proportion Punitive, Anxious or Mistrustful

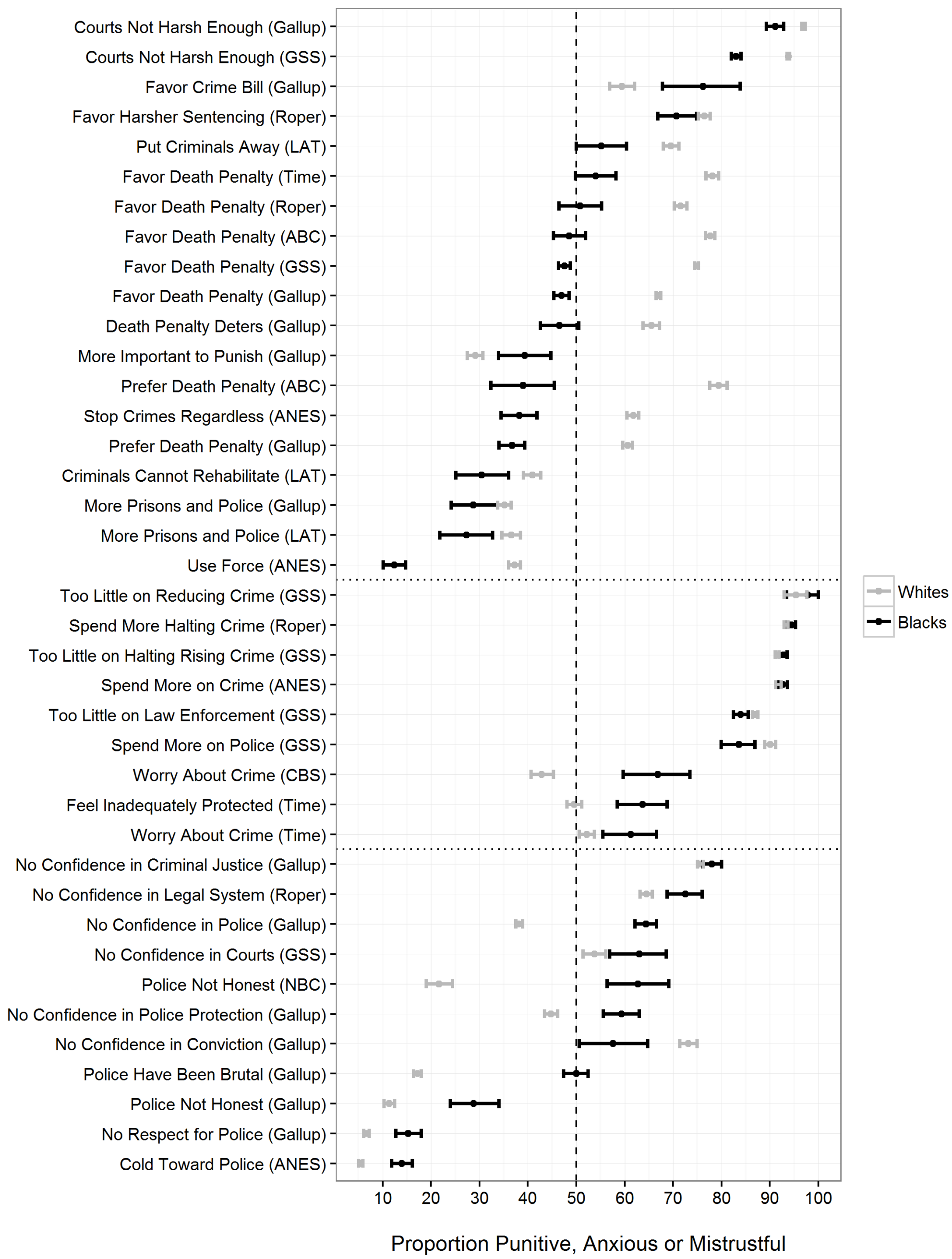
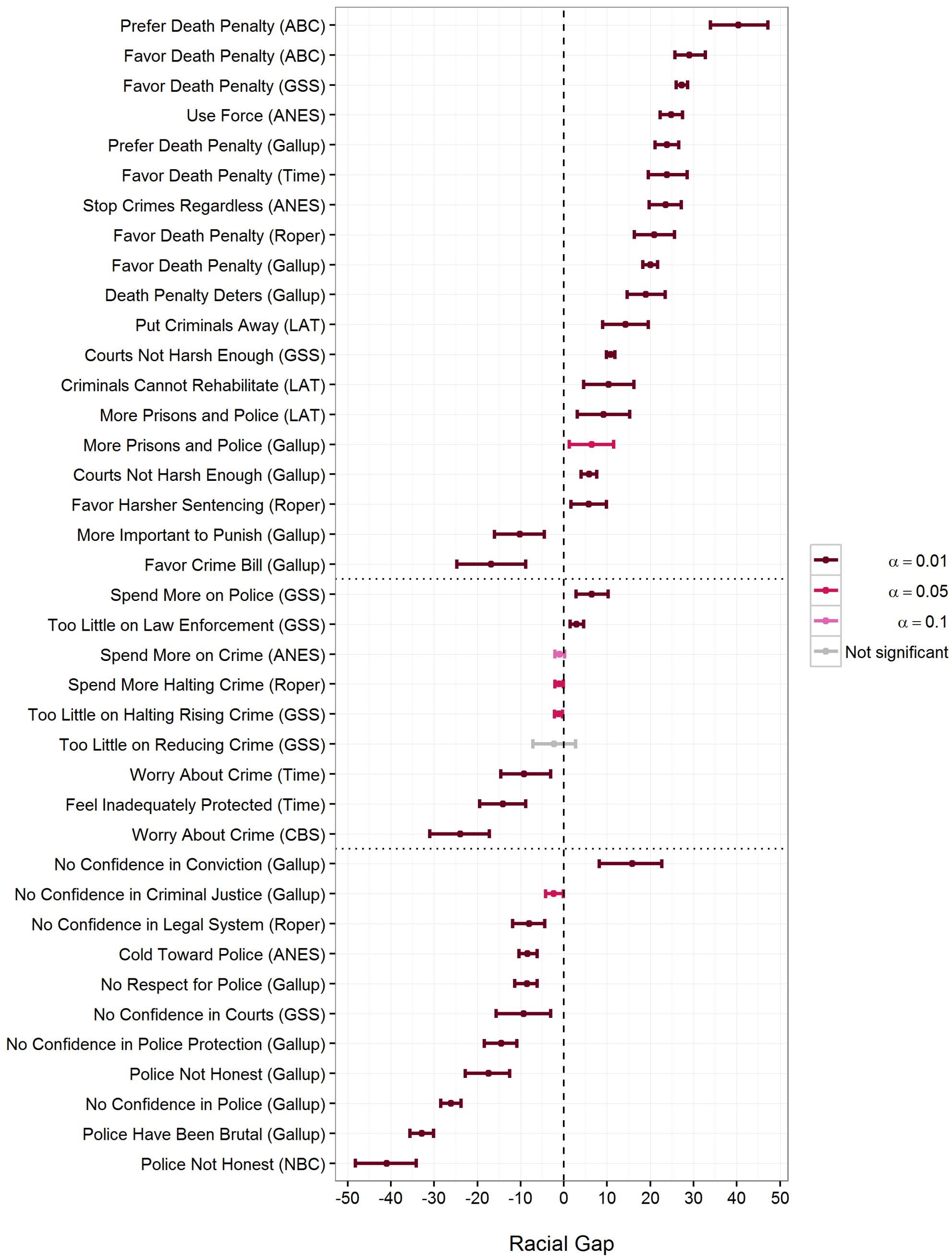
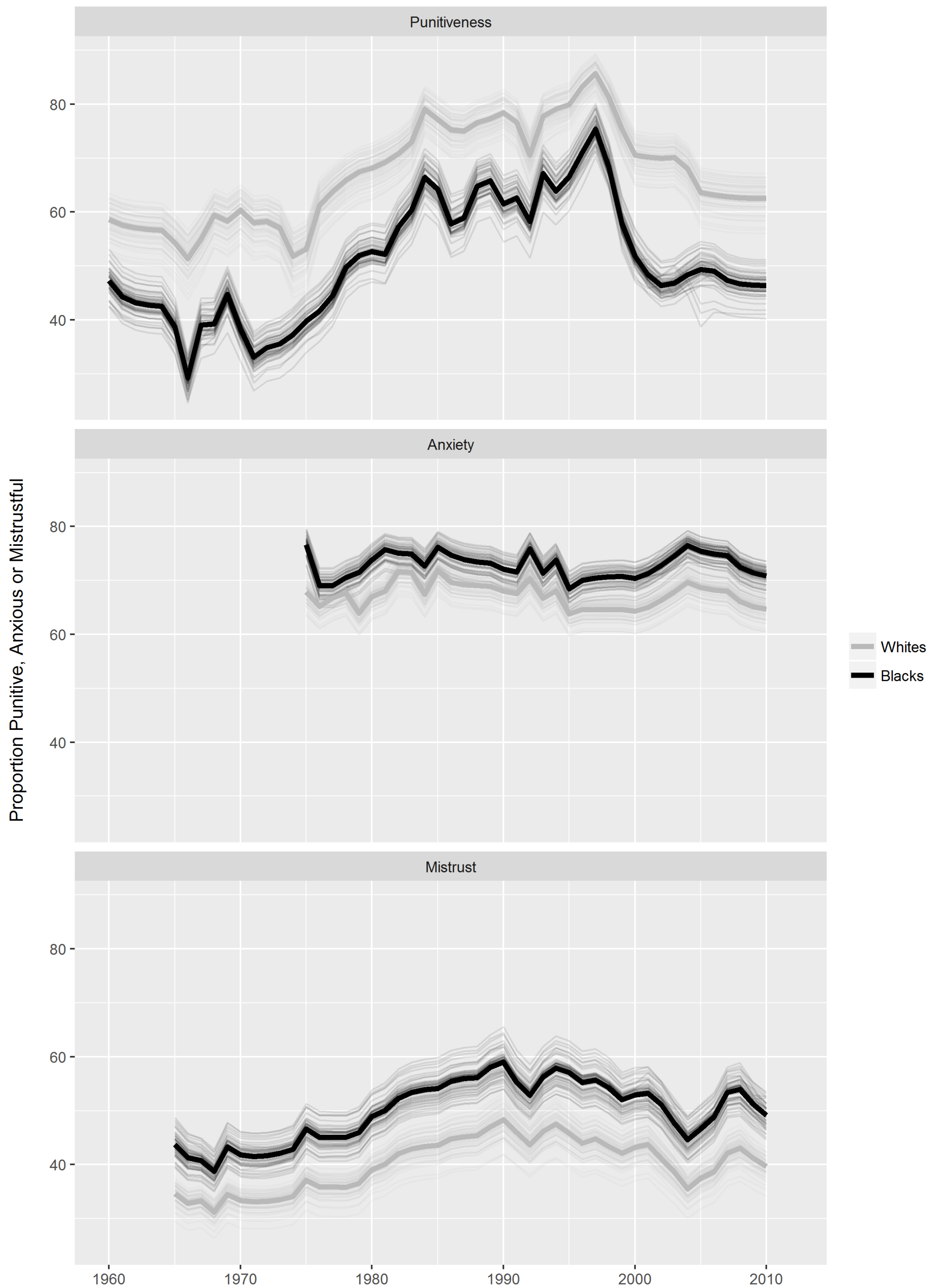


Figure 8: Average White-Black Difference in Proportion Punitive, Anxious or Mistrustful



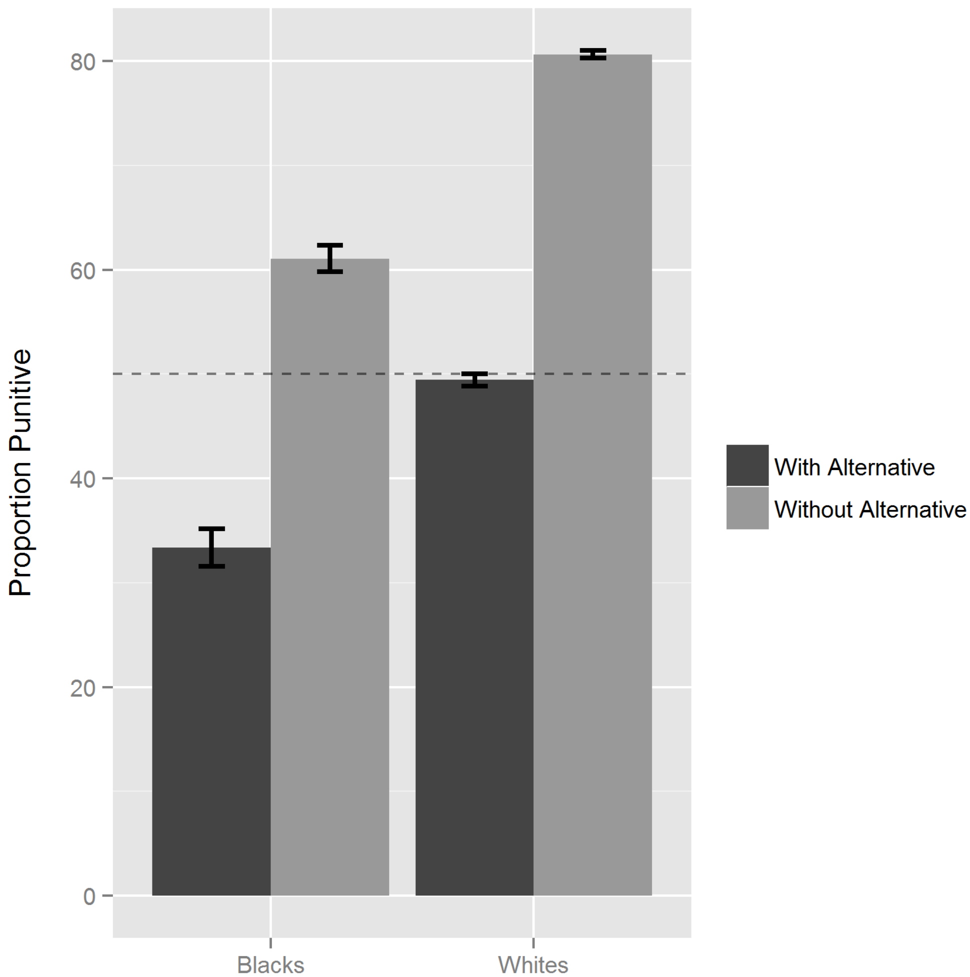
The point estimate and 95% confidence intervals are colored according to the level of α at which the null of no difference can be rejected.

Figure 9: Trends in Punitiveness, Anxiety and Mistrust



The thin lines plot trends over time, for each of the 50 states. The thick lines plot the average across states, for a given race in a given year.

Figure 10: Questions With Alternatives Elicit Significantly Less Punitive Responses



Questions which prime respondents to alternatives are: Prefer Death Penalty (Gallup), Use Force (ANES), More Prisons and Police (Gallup), Stop Crimes Regardless (ANES), More Important to Punish (Gallup), Prefer Death Penalty (ABC), Put Criminals Away (LAT).
*Questions which do not: Favor Death Penalty (Gallup), Favor Death Penalty (GSS), Favor Death Penalty (ABC), Courts Not Harsh Enough (Gallup), Favor Death Penalty (Time), Favor Death Penalty (Roper), Favor Harsher Sentencing (Roper), Courts Not Harsh Enough (GSS).

Figure 11: Difference in Southern vs. Not-Southern Whites Voting for Democratic Party

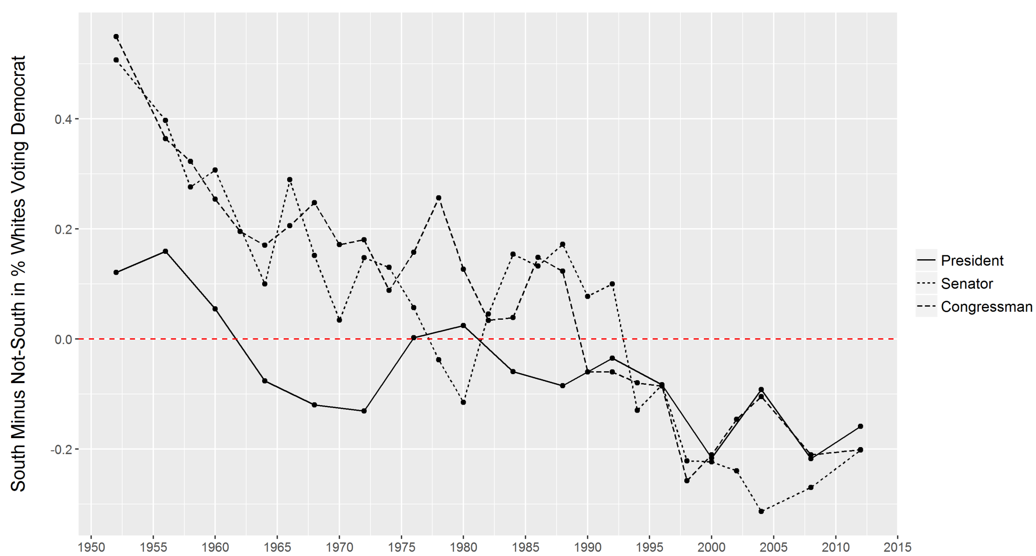
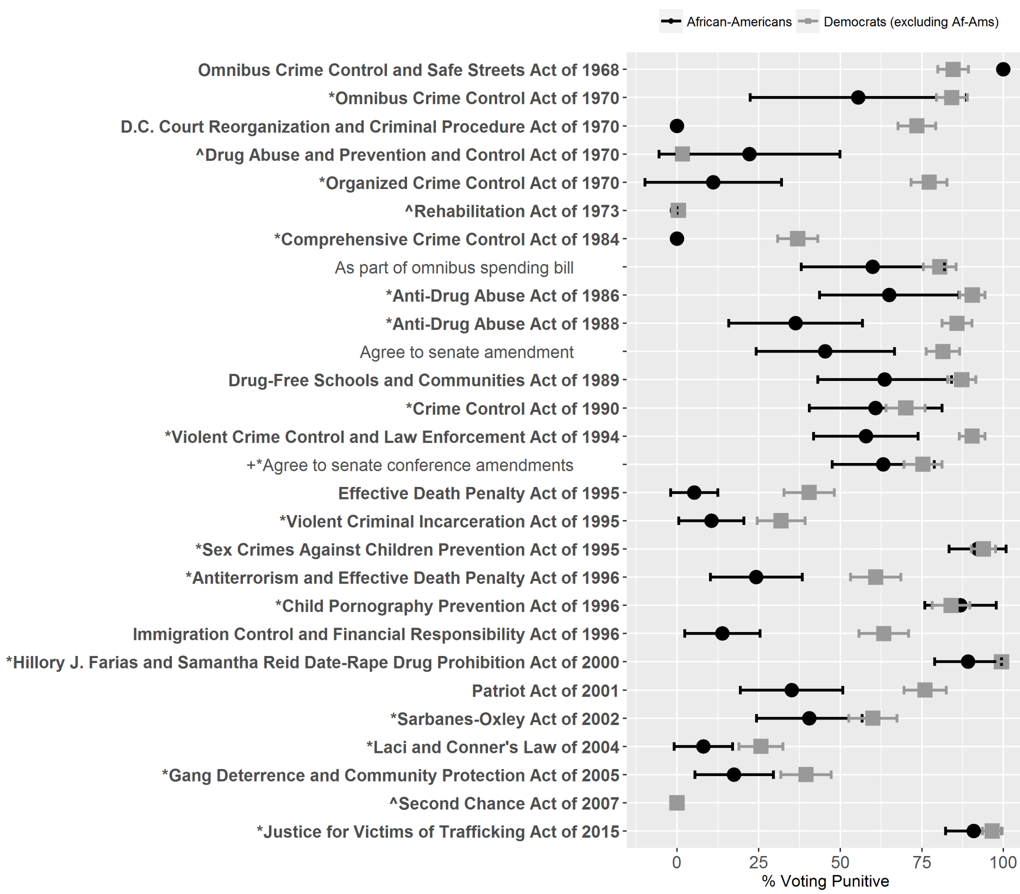
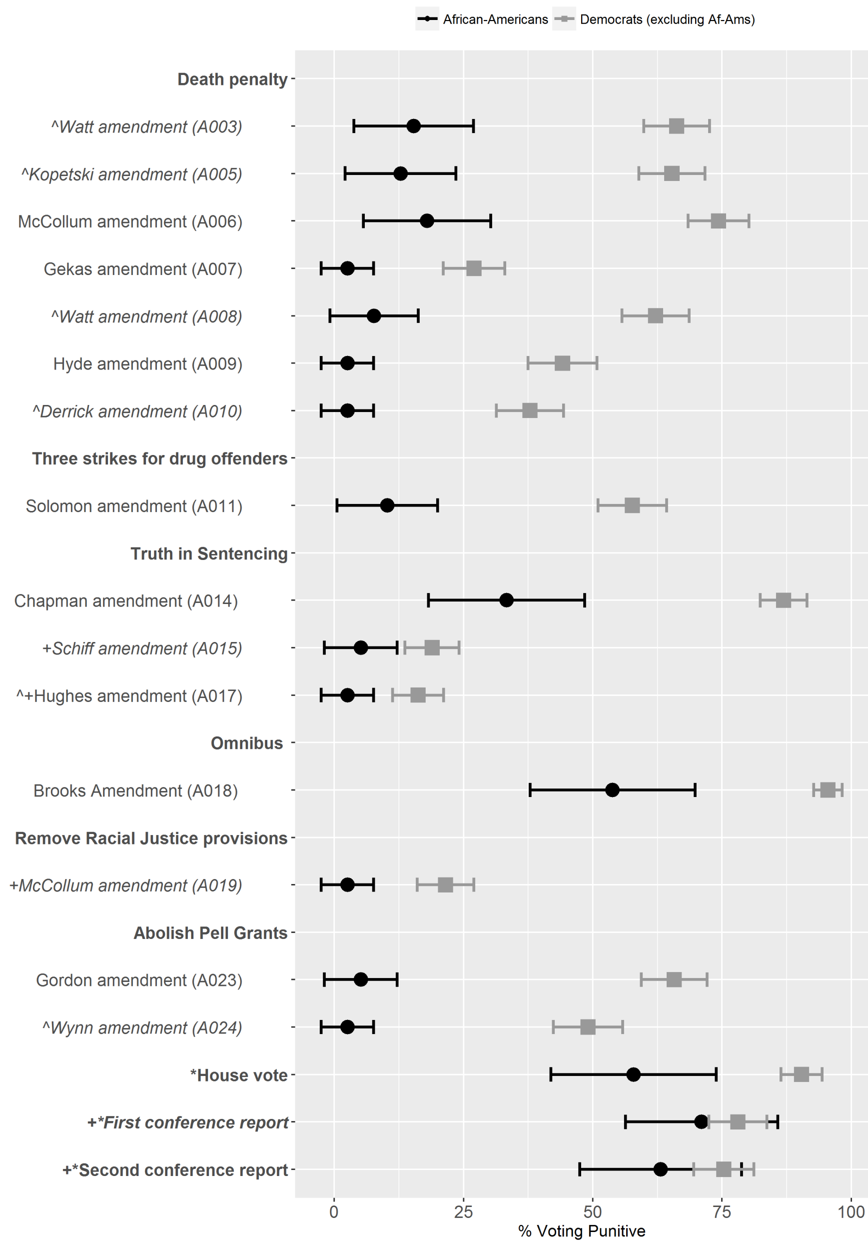


Figure 12: Percentage of Congressmen Voting Punitively, 1968 to 2015



[1] Bolded labels denote bills. Plain-text labels denote votes on conference amendments.
[2] Italics indicate bills and amendments which failed to pass.
[3] Asterisks indicate that the bill in question proposed to raise mandatory minimums.
[4] Crosses indicate that the vote of African-American congressmen was pivotal.
[5] Carets indicate that voting against a bill or amendment is considered the punitive response.

Figure 13: Voting in the Violent Crime Control and Law Enforcement Act of 1994



See notes following Figure 12

Online Appendix

A Sources and Definitions

Variable	Source	Coverage	Definition
Incarceration Rate	BJS National Prisoner Statistics,[36] and BJS Historical Statistics on Prisoners in State and Federal Institutions[37]	1925-2011	The number of prisoners under the jurisdiction of a given state for every 100,000 residents. See below for more details.
Police Officers Per Capita	LEOKA Master File from FBI Criminal Justice Information Services Division (by request), and BJS Directory of Law Enforcement Agencies[38]	1960-2012	The number of police officers employed in a given state for every 100,000 residents. See below for more details.
Spending on Corrections Per Capita	State Government Finances, U.S. Census Bureau[39]	1960-2007	Total state-level spending corrections. Adjusted to 2007 dollars using the deflator available in Klarner's State Economic Data.

36. Available as ICPSR 34540, at:http://www.icpsr.umich.edu/icpsrweb/NACJD/studies/34540/version/1
37. Available as ICPSR 8912, at:http://www.icpsr.umich.edu/icpsrweb/NACJD/studies/8912/version/1
38. Available at: http://www.icpsr.umich.edu/icpsrweb/NACJD/series/00169
39. Available at: http://www2.census.gov/pub/outgoing/govs/special60/State_Govt_Fin.zip

State and US Population	U.S. Census Bureau (various files)[40]	1900-2014	Estimate of resident population at mid-year. Used to calculate rates where they were not given in the original data (i.e., for the calculation of the incarceration rate and the number of police officers per capita).
% of Black Elected Officials	The Joint Center for Economic and Political Studies (by request), and Richard Fording (by request)	1970-2014	The proportion of state and federal legislators that are African-American.
Violent (or Property) Crime Rate	FBI's Uniform Crime Reports[41]	1960-2012	Number of violent (or property) crimes committed for every 100,000 residents
State Population, by Race	National Cancer Institute[42]	1969-2013	Estimate of resident population at mid-year, including estimates of the number of black, white, and other residents. Used to calculate the percentage of a state's population that is African-American.
Partisan Control	State Partisan Balance, Carl Klarner [43]	1937-2011	Coded 1 if Democrats have veto-proof control of the State, 0 if neither party does, and −1 if the Republicans do.
Income Per Capita (Level, and Growth Rate)	State Economic Data, Carl Klarner [44]	1929-2012	Real personal income in 2007 US dollars.

40. Available through: http://www.census.gov/popest/data/historical/index.html
41. Available at: http://www.ucrdatatool.gov/Search/Crime/State/StateCrime.cfm
42. Available at: http://seer.cancer.gov/popdata/download.html#state
43. Available at: http://hdl.handle.net/1902.1/20403
44. Available at: https://dataverse.harvard.edu/dataset.xhtml?persistentId=hdl:1902.1/20404

Tax Collections Per Capita	State Government Tax Collections, U.S. Census Bureau [45]	1951-2013	Taxes collected by state government (adjusted to 2007 dollars using deflators available in Klarner's State Economic Data)
Gini Coefficient	US State-Level Income Inequality Data, Mark W. Frank [46]	1917-2012	State-level Gini coefficient. See Frank (2009) for more details.
Unemployment Rate	Local Area Unemployment Statistics, Bureau of Labor Studies (by request)	1976-2014	Proportion of the labor force unemployed.
Poverty Rate	Small Area Income and Poverty Estimates, Census[47]	1960-2010[48]	Proportion of the population living below the poverty line.
Crack Prevalence Index	Measuring Crack Cocaine and Its Impact, Roland Fryer[49]	1980-2000	Proxy for the impact and prevalence of crack cocaine (combining arrests, emergency room visits, deaths, newspapers, and drug busts). See Fryer, Heaton, Levitt, and Murphy (2013) for more details.
AFDC Benefit Levels	Wexler and Engel (1999)	1940-1995	Average payment per recipient through Aid to Families with Dependent Children (AFDC). See Wexler et al. (1999) for more details.

45. Available at: http://www.census.gov/govs/statetax/historical_data.html
46. Available at: http://www.shsu.edu/eco_mwf/inequality.html
47. Available at: https://www.census.gov/did/www/saipe/data/statecounty/data/index.html
48. Note that estimates before 1989 are only available in Census years, and are not available online. We obtained these by request.
49. Available at: http://scholar.harvard.edu/fryer/publications/measuring-crack-cocaine-and-its-impact

Congressional Voting Data	Votes from GovTrack,[50] Race of Reps. from House of Representatives[51]	1968-2015	Vote percentages on major criminal justice bills and amendments calculated for black and white Democrats in the House.

A.1 Calculating the Incarceration Rate

As mentioned, the data used to calculate the incarceration rate were obtained from two separate sources: the Bureau of Justice Statistics' National Prisoner Statistics series (NPS), which contains detailed information on the status of prisoners in the custody or jurisdiction of State and Federal institutions between 1978 and 2011; and the Bureau of Justice Statistics' Historical Statistics on Prisoners in State and Federal Institutions (HS), which contains a single series counting the number of prisoners under the custody of these same institutions.

Given our question, we are searching for a measure of the incarcerated population that best captures the punitiveness of a given state. In our view, this requires counting the prisoners under a state's jurisdiction rather than those in its custody, since the latter figure excludes prisoners housed in local jails or sent to other jurisdictions, and may include prisoners imported from other jurisdictions. This is straightforward enough for the NPS dataset, which provide this category directly (the sum of *jurtotm* and *jurtotf*). However, the HS dataset only contains information about prisoners under the custody of a given jurisdiction.

Fortunately, these series overlap for nine years (1978-1986), which allows us to make some inferences about how the populations under custody and jurisdiction differed, in any given state. We proceeded by inflating (or, in some states, deflating) the number of prisoners under custody by the average ratio of the jurisdiction to the custody count over the first five years of overlap (1978-1982).[52] Again, in most cases this resulted in estimates of the incarceration rate for earlier states that are higher than estimates based on the population under custody alone, though the differences are relatively slight. The median factor of inflation is about 1.04, and the average is

50. Available at: http://www.govtrack.us/
51. Available at: http://history.house.gov/
52. We chose the first five years because the codebook suggests (and the data show) that the difference between the population under custody and jurisdiction grows wider as the 1980's progress, as more states began to rely on local and privately-operated facilities. We did not want to inflate by a factor that reflected new trends which did not apply to the previous era.

about 1.12.[53]

This approach does raise one additional issue for researchers interested in a continuous series extending to 1925, which is that the definition of the population under custody was changed, in 1940, to exclude all prisoners with maximum sentences of less than six months.[54] Before that year, all prisoners were included, regardless of sentence.[55] Inflating the entire series by a consistent amount, based on the 1978-1982 overlap, is likely to understate the punitiveness of the first era (1925-1940). This difference is probably slight, but it is difficult to know for sure. For our analysis this inconsistency is irrelevant, so we do not dwell on it.

A.2 Calculating Police Officers Per Capita

Our measure of police officers per capita required a count of all the police officers in a given state in a given year. To obtain this, we relied on two data sources: (1) the Law Enforcement Officers Killed and Assaulted dataset maintained by the Criminal Justice Information Services Division of the FBI, and (2) the Directory of Law Enforcement Agencies Series available through NAJCD. By request we obtained the former for all years between 1960 and 2013. The latter are a census of all existing police agencies; as such, they are available only in 1986, 1992, 1996, 2000, 2004, and 2008.[56]

While the purpose of the LEOKA data is to provide a record of law enforcement officers killed or assaulted in the course of duty, the dataset also provides information on the number of officers and employees employed by reporting agencies in any given year. To obtain the number of police officers per capita, we added the number of police employees employed by all police agencies falling under a given state, and divided by the total population (multiplying by 100,000 for informative units).

Unfortunately, these data have shortcomings. Not all police agencies report in all years. And of those agencies that do, not all appear in all years. This is the case even though LEOKA (which is under the purview of the Uniform Crime Reports program) is not a survey but a quasi-census of all existing agencies. Correspondence with both the FBI Criminal Justice Information Services Division and the National Archive of Criminal Justice Data at the ICPSR (which also hosts LEOKA) suggested no

53. Alaska is an outlier. Between 1978 and 1982, the number of prisoners under the jurisdiction were about twice the number that it held in its physical custody.

54. See: http://www.bjs.gov/content/pub/pdf/sfp2585.pdf for a discussion of this and other issues involved in using the HS dataset.

55. The HS dataset does not report overlapping counts of both these definitions, which makes it difficult to infer what this change implied. The difference between the 1939 and 1940 count was ca. 6,000 in all states combined, which suggests that it may not have been that large. But the population had grown by ca. 16,000 the year before this change, which may actually mean that the change was more significant than the simple difference suggests.

56. We could not find a version of the 2004 data with complete geographic identifiers, so it was unusable for our purposes.

easy fixes, but suggested looking at the census of law enforcement agencies available through the Directory of Law Enforcement Agencies Series in select years.

We used these data to adjust the LEOKA data, proceeding as follows. In each of the census years for which we had geographic identifiers (1986, 1992, 1996, 2000, and 2008), we compared state-level estimates obtained through LEOKA to the figures from the censuses. On average, we found that the LEOKA figures were off by about 14%. Understandably, 90% of the time, this was because the LEOKA figures underestimated the true count. At its largest the discrepancy between the two sources was 60%, but in general the errors were significantly lower (the IQR of the discrepancy ranged from 7% to 18%).

To adjust our estimates, we calculated a factor of inflation for each state in each census year ($inf_{st} = off.CENSUS_{st}/off.LEOKA_{st}$). We inflated our census-year estimates using this factor, which means that we are using census estimates in census years. We drew on this factor of inflation to estimate counts in intercensal years ($off.ESTIMATE_{st} = inf_{st} * off.LEOKA_{st}$) (i.e., 1987-1991, 1993-1995, etc.). To estimate the factor of inflation in intercensal years, we interpolated ($inf_{s,t=j} = \frac{(t_j - t_i)(inf_{s,t=k} - inf_{s,t=i})}{t_k - t_i} + inf_{s,t=i}$ where i and k are census years, and j is an intercensal year). In doing this we assume that the level of coverage in a state changed (mostly, improved) linearly between censuses. This is unrealistic, but we considered it the most plausible way to retain the information contained in the LEOKA dataset while adjusting those data to match the census. (For the years which predate the first census (i.e., 1960-1985), we used the factor of inflation observed in 1986.)

B Long-Run Multipliers

In any specification which includes a lag of the dependent variable on the right-hand side of the equation, independent variables will have both immediate (or short-run) effects on the dependent variable, as well as persistent (or long-run) effects. To see this, note that a single unit change in any of the independent variables at time t induces an initial adjustment in the dependent variable at time $t+1$ of magnitude equal to its estimated coefficient (call it β). This is the short-run effect. In the absence of a lagged term, this short-run effect captures the entirety of an independent variable's effect on the level of the dependent variable. Where a lagged term is included among the estimators, however, the initial adjustment at time $t+1$ will affect the level of the dependent variable at time $t+2$. The magnitude of this effect is $\beta \times \alpha$, where α is the estimated coefficient associated with the lagged term. In turn, the new level of the dependent variable will have a knock-on effect at time $t+3$, with magnitude $\beta \times \alpha^2$. This pattern persists, so the total effect of a single unit change at time t on the dependent variable at time $t+n$ is given by the geometric series $\beta + \beta\alpha + \beta\alpha^2 + \cdots + \beta\alpha^{n-1}$. The long-run effect is derived by setting n to ∞, in which case this series can be written as $\sum_{n=0}^{\infty} \beta\alpha^n$, which is equivalent to $\frac{\beta}{1-\alpha}$.

As (De Boef et al. 2008, pp.191-192) explain, calculating the standard error of this long-run estimate is not straightforward, since the long-run multiplier is the ratio of two (or more) coefficients.[57] It is possible to directly estimate this uncertainty using the Bewley transformation. It is also possible to estimate it by simulation, which is how we proceed in this paper. We simulated 5,000 draws from the appropriate variance-covariance matrix and calculated a distribution for the long-run multipliers of interest. We calculate a 95% confidence interval for the estimate by computing the 2.5th and 97.5th percentiles of this distribution. This is our preferred gauge of uncertainty. The standard errors we report are the standard deviations of these same distributions. We also used an empirical cumulative distribution function to summarize the distribution. This is how approximated the probability of rejecting the null hypothesis that the true value of the long-run multiplier was 0.

57. When more than one lag of the dependent variable is included, as in most of our preferred models, the long-run effect is equivalent to $\frac{\beta}{1-\alpha_1-\alpha_2-\cdots-\alpha_n}$, where α_n is the coefficient associated with the n_{th} lag of the dependent variable. If more than one lag of the independent variable is included, the numerator changes to $\beta_1 + \beta_2 + \cdots + \beta_m$, where β_m refers to the coefficient associated with the m_{th} lag of the independent variable.

C Unit Roots

As described in the main text, panel unit root tests were not decisive. All series were cleared by at least one test; all series were also indicted by at least one test. We consider this reasonable grounds for leaving all variables in their levels, but Table 9 gives the results of running a specification in which all suspect variables are first-differenced.

All series failed the Hadri test, which has the most exacting null (see Table 8. For this reason, we regarded series as suspect when they failed this and at least one of the other three tests. By this criterion, the problematic series were: the incarceration rate, police spending, GDP/capita, the growth rate, the Gini coefficient, tax collections, and Enns's measure of punitiveness. These suspect variables are identified in Table 1 in the main paper.

D Public Opinion

D.1 Methods

In order to obtain state-race-year responses to each of the questions in our dataset, we proceeded via a set of steps known together as multilevel regression and poststratification (MRP). First, for each question, we modeled the probability of a punitive, mistrustful or anxious response as a function of a respondent characteristics. Second, we used these estimates to predict the average response of a given race in a given state in a given year, using information drawn from the census about this population's characteristics. While most of what we do is fairly typical of past work in this domain (Park et al. 2004; Lax et al. 2009b, 2009a; Gelman and Hill 2006; Enns and Koch 2013; Enns 2016; Kastellec, Lax, and Phillips 2014), we made a few amendments to the procedure to better fit our aims and our data. (See Table 6 for details about the questions and sources).

D.2 Model Specification

Our individual-level models of public opinion focus on a small set of standard covariates.[58] We use the following respondent characteristics: race (nonblack vs. black),[59] sex, education (less than HS, HS graduate, some college, college grad), age (less than 29, 30 to 44, 45 to 64, and greater than 65), state, region (following others, we code DC as its own region), and the year in which the poll was administered. Our baseline model is thus

$$\Pr(y_i = 1) = logit^{-1}(\beta^0 + \alpha^{race}_{j[i]} + \alpha^{sex}_{k[i]} + \alpha^{ed}_{l[i]} + \alpha^{age}_{m[i]} + \alpha^{state}_{s[i]} + \alpha^{year}_{t[i]}) \tag{3}$$

β^0 is the intercept. Each other term represents a 'random effect': that is, a term drawn from a normal distribution with mean zero and a variance to be estimated.

58. Because final estimates rely on the combination of multilevel regression on the original poll data and weighting by information from the Census, only those covariates can be used which are available both in the constituent polls and in the Census.

59. We choose this binary coding for race in order to make it easy to fit the more elaborate models described below. We trialed an ordinal, three-level coding (white, black, other), but this made these more elaborate models much less likely to converge. This is due to the small number of respondents who were neither white nor black in our samples. Future work might consider improvements, but we believe the benefits of this specification outweigh its costs.

$$
\begin{aligned}
\alpha_j^{race} &\sim N(0, \sigma^2_{race}), \text{ for } j = 1, 2 \\
\alpha_k^{sex} &\sim N(0, \sigma^2_{sex}), \text{ for } k = 1, 2 \\
\alpha_l^{ed} &\sim N(0, \sigma^2_{ed}), \text{ for } l = 1, 2, 3, 4 \\
\alpha_m^{age} &\sim N(0, \sigma^2_{age}), \text{ for } m = 1, 2, 3, 4 \\
\alpha_t^{year} &\sim N(0, \sigma^2_{year}), \text{ for } t = 1, \ldots, t \\
\alpha_n^{region} &\sim N(0, \sigma^2_{region}, \text{ for } n = 1, 2, 3, 4, 5 \\
\alpha_s^{state} &\sim N(\alpha^{region}_{m[s]}, \sigma^2_{state}), \text{ for } s = 1, \ldots, 50
\end{aligned}
$$

As shown, states are nested within regions, so the various state effects are themselves drawn from a normal distribution centered around an estimated regional mean, with variance to be estimated. Note also that the number of year-based random effects varies, depending on the question under consideration. Some questions are asked in several years, and some in as few as two.

Given the data at our disposal, however, it is reasonable to wonder whether this baseline model extracts all useful information. Consider, for instance, our particular interest in capturing racial differences in the contours of public opinion. By construction, Equation 3 pools information across these groups, which may underestimate the impact of race on opinion formation. We therefore also estimated two additional models, which introduced a series of fixed effects and further, interaction-based random effects.

$$
\begin{aligned}
\Pr(y_i = 1) = logit^{-1}(\beta^0 + \beta^{race} RACE_i + \beta^{sex} SEX_i + \\
\beta^{ed} ED_i + \alpha^{age}_{m[i]} + \alpha^{state}_{s[i]} + \alpha^{year}_{t[i]} + \alpha^{race.state}_{j[i],s[i]}
\end{aligned}
\tag{4}
$$

$$
\begin{aligned}
\Pr(y_i = 1) = logit^{-1}(\beta^0 + \beta^{race} RACE_i + \beta^{sex} SEX_i + \beta^{ed} ED_i + \\
\alpha^{age}_{m[i]} + \alpha^{state}_{s[i]} + \alpha^{year}_{t[i]} + \alpha^{race.state}_{j[i],s[i]} + \alpha^{race.year}_{j[i],t[i]} + \alpha^{race.state.year}_{j[i],s[i],t[i]} +
\end{aligned}
\tag{5}
$$

The model described in Equation 4 estimates fixed effects for race, sex, and education, and introduces state-year random effects. This relaxes the restriction that the effect of race on opinion is the same in every state.[60]

$$
\alpha^{race.state}_{j,s} \sim N(0, \sigma^2_{race.state}), \text{ for } j = 1, 2, s = 1, \ldots, 50
$$

60. Of course, given that no sample ever contains respondents of both races in every state, some pooling across categories is mandatory.

The yet-more complex model described in Equation 5 introduces additional race-year and race-state-year random effects, further relaxing assumed restrictions on the effect of race.

$$\begin{aligned}
\alpha_{j,t}^{race.year} &\sim N(0, \sigma^2_{race.year}), \text{ for } j = 1, 2, t = 1, \ldots, t \\
\alpha_{j,s,t}^{race.state.year} &\sim N(0, \sigma^2_{race.state.year}), \text{ for } j = 1, 2, s = 1, \ldots, 50, t = 1, \ldots, t
\end{aligned}$$

In some cases, the middling and complex models had to be adjusted, due to data availability. We were working with the public-use version of the General Social Survey (GSS), in which access to state-level identifiers is not possible. Moreover, other questions lacked usable state-level identifiers. Rather than discarding these data, we used census division identifiers in lieu of state-level identifiers. These divisions were nested within a four-tiered classification of regions.[61] This is not ideal, and it means that our final series likely understate geographic variation in responses, but it is far preferable to discarding these data. It would be easy to improve upon this in future research.

D.3 Estimation and Selection

Of course, it is not always preferable to fit more complex models, even where possible. Over-elaborate models have a tendency to make inferential mountains out of stochastic molehills–to fit the 'noise', in other words. Thus, to choose between these three models, we relied on a predictive exercise. We partitioned all responses to a given question into a training set containing 80% of the responses, and a test set containing the other 20%. We fit each of the three models to this training set, and chose the model which best fit the test data. In some cases, more elaborate models failed to converge. Obviously, we could only pick from amongst those that did. Table 6 in the main paper lists which models were used for which questions: simple (Equation 3), middling (Equation 4), or complex (Equation 5).

Note one further wrinkle. Following Enns (2016), we employ Stimson's Dyad Ratios algorithm to estimate public opinion in each of the three dimensions to which these many different questions pertain. Stimson's algorithm requires a binary measure of punitiveness (or anxiety, or mistrust). Befitting this approach, we fit logistic regressions to each of the questions in our dataset.

This, however, required a strategy for handling so-called 'neutral' responses. What is to be done about those people who answer neither punitively or not-punitively? We chose against discarding these responses. If there is some pattern in the population answering neutrally, discarding responses would be a source of bias. Extrapolating

61. DC could not be its own region, here, since could not identify DC-based respondents in these data.

from this sub-population to the general population (via poststratification) would not be justifiable.

Instead, we chose to estimate parallel models predicting a neutral response to the question under consideration. In other words, we estimated the probability that a respondent chose to answer the question non-neutrally. In most cases, given the small number of people answering neutrally, the best estimate was simply the sample mean. Where neutral responses were more common, however, we were able to fit the basic model described in Equation 3. Again, we chose our model of neutral response through a predictive exercise on the training and test datasets. See Table 6 for details.

As a result, our final predictions are based on these two parallel models. The best model of neutral response gives the probability that a respondent in our sample answers a question non-neutrally. And the main models described above give the probability that a respondent, having answered non-neutrally, gives a punitive, anxious or mistrustful response.[62] In other words,

$$\Pr(P \mid S) = \Pr(P \mid NN, S) \times \Pr(NN \mid S) \tag{6}$$

where $\Pr(P \mid NN, S)$ is the probability of giving a punitive, anxious, or mistrustful response if the respondent is sampled and answered not neutrally, and $\Pr(NN \mid S)$ is the probability of giving a not neutral response if the respondent was in the sample.

D.4 Questions

Here we list all the questions for which we collected data The responses to these questions were obtained either from the American National Election Studies, the General Social Survey, or through the Roper Center for Public Opinion Research. As discussed in the paper, the proportion punitive (or anxious or mistrustful) was calculated as the number of respondents answering punitively divided by the number who gave clear-cut answers. For this reason, next to each question we list the responses we counted as punitive (or anxious or mistrustful), neutral, and not punitive.

Punitiveness

1. **Courts Not Harsh Enough (GSS)**: In general, do you think the courts in this area deal too harshly or not harshly enough with criminals?
   (P: *Not Harsh Enough*; NP: *Too Harsh*; N: *About Right, Don't Know*)
2. **Favor Death Penalty (GSS)**: Do you favor or oppose the death penalty for persons convicted of murder?
   (P: *Favor*; NP: *Opposed*; N: *Don't Know*)

62. As this implies, these main models are thus fit only on the subset of respondents answering non-neutrally.

3. **Favor Death Penalty (Gallup)**: Are you in favor of the death penalty for a person convicted of murder?
(P: *Favor*; NP: *Oppose*; N: *No Opinion, Don't Know*)

4. **Prefer Death Penalty (Gallup)**: If you could choose between the following two approaches, which do you think is the better penalty for murder–the death penalty or life imprisonment, with absolutely no possibility of parole?
(P: *Prefer Death Penalty*; NP: *Prefer Life Without Parole*; N: *Neither, Either/It Depends, DK/Refused*)

5. **Favor Death Penalty (ABC)**: Do you favor or oppose the death penalty for persons convicted of murder?
(P: *Favor Death Penalty*; NP: *Oppose Death Penalty*; N: *Don't Know, No Opinion, It Depends (Vol.)*)

6. **Courts Not Harsh Enough (Gallup)**: In general, do you think the courts in your area deal too harshly, or not harshly enough with criminals?
(P: *Not Harsh Enough*; NP: *Too Harsh*; N: *About Right, Don't Know, No Opinion*)

7. **Favor Death Penalty (Time)**: Do you favor or oppose the death penalty for individuals convicted of serious crimes, such as murder?
(P: *Favor*; NP: *Oppose*; N: *Not Sure*)

8. **Use Force (ANES)**: There is much discussion about the best way to deal with the problem of urban unrest and rioting. Some say it is more important to use all available force to maintain law and order – no matter what results. Others say it is more important to correct the problems of poverty and unemployment that give rise to the disturbances. Where would you place yourself on this scale, or haven't you thought much about this? (1. Solve problems of poverty and unemployment ... 7. Use all available force.)
(P: *5 to 7*; NP: *1 to 3*; N: *4*)

9. **More Prisons and Police (Gallup)**: Which of the following approaches to lowering the crime rate in the United States comes closer to your own view–do you think more money and effort should go to attacking the social and economic problems that lead to crime through better education and job training or more money and effort should go to deterring crime by improving law enforcement with more prisons, police, and judges?
(P: *Improving Law Enforcement*; NP: *Social and Economic Problems*; N: *Don't Know/Refused, Both (Vol.), Neither (Vol.)*)

10. **Stop Crimes Regardless (ANES)**: Some people are primarily concerned with doing everything possible to protect the legal rights of those accused of committing crimes. Others feel that it is more important to

stop criminal activity even at the risk of reducing the rights of the accused. Where would you place yourself on this scale, or haven't you thought much about this? (1. Protect rights of accused ... 7. Stop crimes regardless of rights of accused)
(P: *5 to 7*; NP: *1 to 3*; N: *4*)

11. **Death Penalty Deters (Gallup)**: Do you feel that the death penalty acts as a deterrent to the commitment of murder, that it lowers the murder rate, or not?
(P: *It Does Deter*; NP: *It Doesn't Deter*; N: *Don't Know, No Opinion*)
12. **Favor Death Penalty (Roper)**: (Frequently on any controversial issue there is no clear cut side that people take, and also frequently solutions on controversial issues are worked out by compromise. But I'm going to name some different things, and for each one would you tell me whether on balance you would be more in favor of it, or more opposed to it?)... Imposing the death penalty on those convicted of serious crimes such as murder, kidnapping, etc.
(P: *Favor*; NP: *Opposed to*; N: *Mixed Feelings, Don't Know*)
13. **Favor Harsher Sentencing (Roper)**: (Frequently on any controversial issue there is no clear cut side that people take, and also frequently solutions on controversial issues are worked out by compromise. But I'm going to name some different things, and for each one would you tell me whether on balance you would be more in favor of it, or more opposed to it?)... Harsher prison sentences for those convicted of crimes.
(P: *Favor*; NP: *Opposed to*; N: *Mixed Feelings, Don't Know*)
14. **More Important to Punish (Gallup)**: In dealing with those who are in prison, do you think it is more important to punish them for their crimes, or more important to get them started 'on the right road'?
(P: *Punish Them*; NP: *Get Started Right*; N: *No Opinion*)
15. **Prefer Death Penalty (ABC)**: Which punishment do you prefer for people convicted of murder: the death penalty or life in prison with no chance of parole?
(P: *Prefer Death Penalty*; NP: *Prefer Life in Prison*; N: *Don't Know, No Opinion*)
16. **Favor Crime Bill (Gallup)**: Do you favor or oppose the crime bill which Congress recently passed?
(P: *Favor*; NP: *Oppose*; N: *Don't Know/Refused*)

Crime Anxiety

1. **Too Little on Halting Rising Crime (GSS)**: (We are faced with many problems in this country, none of which can be solved easily or inexpensively. I'm going to name some of these problems, and for each one I'd like

you to tell me whether you think we're spending too much money on it, too little money, or about the right amount.) e. Halting the rising crime rate.
(A: *Too Little Money*; NA: *Too Much Money*; N: *About Right, Don't Know*)

2. **Too Little on Law Enforcement (GSS)**: (We are faced with many problems in this country, none of which can be solved easily or inexpensively. I'm going to name some of these problems, and for each one I'd like you to tell me whether you think we're spending too much money on it, too little money, or about the right amount.) e. Law enforcement.
(A: *Too Little Money*; NA: *Too Much Money*; N: *About Right, Don't Know*)

3. **Spend More Halting Crime (Roper)**: (Turning now to the business of the country–we are faced with many problems in this country, none of which can be solved easily or inexpensively. I'm going to name some of these problems, and for each one I'd like you to tell me whether you think we're spending too much money on it, too little money, or about the right amount) Halting the rising crime rate–are we spending too much, too little, or about the right amount on halting the rising crime rate?
(A: *Too Little*; NA: *Too Much*; N: *About Right*)

4. **Spend More on Crime (ANES)**: If you had a say in making up the federal budget this year, for which (1986 and after: of the following) programs would you like to see spending increased and for which would you like to see spending decreased: Should federal spending on dealing with crime be increased, decreased, or kept about the same?
(A: *Increased*; NA: *Decreased or Cut Entirely*; N: *Same, Don't Know*)

5. **Feel Inadequately Protected (Time)**: Do you feel adequately protected by the police from being the victim of a crime?
(A: *Yes*; NA: *No*; N: *Not Sure*)

6. **Worry About Crime (Time)**: Is being a victim of crime something you personally worry about, or not?
(A: *Yes*; NA: *No*; N: *Not Sure*)

7. **Spend More on Police (GSS)**: (Listed below are various areas of government spending. Please indicate whether you would like to see more or less government spending in each area. Remember that if you say "much more," it might require a tax increase to pay for it.) c. The police and law enforcement.
(A: *Spend More, Spend Much More*; NA: *Spend Less, Spend Much Less*; N: *Spend Same, Can't Choose*)

8. **Worry About Crime (CBS)**: How much of the time do you worry about being the victim of a crime — a lot of the time, some of the time, hardly ever, or never?

(A: *A lot of the time, Some of the time*; NA: *Never, Hardly ever*; N: *NA/Don't Know*)

9. **Too Little on Reducing Crime (GSS)**: (We are faced with many problems in this country, none of which can be solved easily or inexpensively. I'm going to name some of these problems, and for each one I'd like you to tell me whether you think we're spending too much money on it, too little money, or about the right amount.) e. Reducing crime.
   (A: *Too Little Money*; NA: *Too Much Money*; N: *About Right, Don't Know*)

Mistrust

1. **No Confidence in Police (Gallup)**: (Now I am going to read you a list of institutions in American society. Please tell me how much confidence you, yourself, have in each one–a great deal, quite a lot, some, or very little?) How about the police?
   (M: *Some, Very Little, None (Vol.)*; NM: *A Great Deal, Quite A Lot*; N: *Don't Know/Refused*)
2. **No Confidence in Criminal Justice (Gallup)**: (Now I am going to read you a list of institutions in American society. Please tell me how much confidence you, yourself, have in each one–a great deal, quite a lot, some, or very little?) How about the criminal justice system?
   (M: *Some, Very Little, None (Vol.)*; NM: *A Great Deal, Quite A Lot*; N: *Don't Know/Refused*)
3. **Cold Toward Police (ANES)**: (There are many groups in America that try to get the government or the American people to see things more their way. We would like to get your feelings towards some of these groups. I have here a card on which there is something that looks like a thermometer. We call it a "feeling thermometer" because it measures your feelings towards groups. Here's how it works. If you don't know too much about a group or don't feel particularly warm or cold toward them, then you should place them in the middle, at the 50 degree mark. If you have a warm feeling toward a group or feel favorably toward it, you would give it a score somewhere between 50 degrees and 100 degrees, depending on how warm your feeling is toward the group. On the other hand, if you don't feel very favorably toward some of these groups–if there are some you don't care for too much–then you would place them somewhere between 0 degrees and 50 degrees.) Policemen/the police.
   (M: *0 to 49*; NM: *51 to 100*; N: *50*)
4. **No Confidence in Legal System (Roper)**: (Now, taking some specific aspects of our life, we'd like to know how confident you feel about them. First, do you feel very confident, only fairly confident, or not at all confident that: we can on the whole depend on the justice of our legal system?

(M: *Not at all confident*; NM: *Very confident*; N: *Only fairly confident, Don't Know*)

5. **No Confidence in Police Protection (Gallup)**: How much confidence do you have in the ability of the police to protect you from violent crime–a great deal, quite a lot, not very much, or none at all?
(M: *None At All, Not Very Much*; NM: *A Great Deal, Quite A Lot*; N: *Don't Know/Refused*)

6. **No Respect for Police (Gallup)**: How much respect do you have for the police in your area – a great deal, some, or hardly any?
(M: *Hardly Any*; NM: *A great deal*; N: *Some*)

7. **Police Have Been Brutal (Gallup)**: In some places in the nation, there have been charges of police brutality. Do you think there is any police brutality in your area, or not?
(M: *Yes*; NM: *No*; N: *Don't Know/Refused*)

8. **Police Not Honest (Gallup)**: (Please tell me how you would rate the honesty and ethical standards of people in these different fields–very high, high, average, low or very low?) How about...police officers?
(M: *Low, Very Low*; NM: *High, Very High*; N: *Average, Don't Know/Refused*)

9. **No Confidence in Courts (GSS)**: (How much confidence do you have in...) e. Courts and the legal system
(M: *Very Little Confidence, No Confidence At All*; NM: *Complete Confidence, A Great Deal of Confidence*; N: *Some Confidence, Don't Know*)

10. **No Confidence in Conviction (Gallup)**: How much confidence do you have in the ability of courts to convict and properly sentence criminals?
(M: *None, Not Much*; NM: *Great Deal, Quite A Lot*; N: *Don't Know*)

11. **Police Not Honest (NBC)**: How would you rate the honesty and ethical standards of police officers?
(M: *Low, Very Low*; NM: *High, Very High*; N: *Average, Don't Know/Not Sure*)

Table 6: Questions in Public Opinion Dataset

	Polls	Resp.	Black Resp.	Coverage	Neutrals	Pr(P)	Pr(NN)
Courts Not Harsh Enough (GSS)	30	53,548	7,325	1972-2014	19.4	C	S
Favor Death Penalty (GSS)	30	55,216	7,666	1972-2014	6.62	S	S
Favor Death Penalty (Gallup)	28	43,059	4,069	1956-2013	8.95	C	S
Prefer Death Penalty (Gallup)	13	12,476	1,510	1985-2014	10.4	C	S
Favor Death Penalty (ABC)	8	9,884	930	1981-2006	5.75	S	μ
Courts Not Harsh Enough (Gallup)	7	13,911	1,478	1965-1993	32.8	S	μ
Favor Death Penalty (Time)	6	4,781	569	1989-2003	5.75	S	S
Use Force (ANES)	6	8,388	848	1968-1992	23.4	S	S
More Prisons and Police (Gallup)	5	5,224	370	1989-1994	7.58	S	S
Stop Crimes Regardless (ANES)	5	9,815	949	1970-1978	30.6	C	S
Death Penalty Deters (Gallup)	3	4,081	595	1985-1991	7.38	S	S
Favor Death Penalty (Roper)	3	5,960	652	1978-1984	17.5	C	S
Favor Harsher Sentencing (Roper)	3	5,958	653	1978-1984	14.5	C	S
More Important to Punish (Gallup)	3	4,117	367	1955-1989	9.55	C	S
Prefer Death Penalty (ABC)	3	3,091	381	2003-2006	33.2	S	S
Put Criminals Away (LAT)	3	4,433	421	1993-1995	14.5	N/A	μ
Criminals Cannot Rehabilitate (LAT)	2	3,007	284	1993-1994	4.02	N/A	μ
Favor Crime Bill (Gallup)	2	2,033	142	1994-1994	19.2	M	S
More Prisons and Police (LAT)	2	2,942	276	1994-1995	8.09	N/A	μ
Too Little on Halting Rising Crime (GSS)	29	34,527	4,677	1973-2014	29.7	M	S
Too Little on Law Enforcement (GSS)	20	21,456	3,102	1984-2014	39.0	C	S
Spend More Halting Crime (Roper)	15	28,925	3,167	1971-1987	29.3	S	S
Spend More on Crime (ANES)	9	20,454	3,064	1984-2012	32.2	S	S
Feel Inadequately Protected (Time)	5	4,336	330	1989-1997	3.04	M	μ
Worry About Crime (Time)	5	4,336	330	1989-1997	0.71	S	S
Spend More on Police (GSS)	4	4,744	601	1985-2006	39.5	M	S
Worry About Crime (CBS)	2	2,067	182	1994-2012	0.24	S	μ
Too Little on Reducing Crime (GSS)	1	484	55	1984-1984	29.1	S	S
No Confidence in Police (Gallup)	22	22,618	1,905	1993-2014	0.43	S	S
No Confidence in Criminal Justice (Gallup)	21	21,607	1,818	1993-2014	0.99	M	S
Cold Toward Police (ANES)	7	11,988	1,207	1966-1992	9.69	C	S
No Confidence in Legal System (Roper)	7	13,161	1,328	1973-1983	52.7	C	μ
No Confidence in Police Protection (Gallup)	6	6,533	763	1985-1999	1.18	C	S
No Respect for Police (Gallup)	6	10,900	1,095	1965-1999	25.7	S	S
Police Have Been Brutal (Gallup)	6	11,247	1,820	1965-1999	10.9	C	S
Police Not Honest (Gallup)	5	5,111	461	2009-2013	31.9	S	S
No Confidence in Courts (GSS)	3	4,008	517	1991-2008	51.5	C	S
No Confidence in Conviction (Gallup)	2	2,244	185	1985-1989	2.36	S	S
Police Not Honest (NBC)	2	2,426	400	1985-1995	47.9	N/A	μ

[1] Questions are ordered by the three dimensions we identify: punitiveness, anxiety, and mistrust.
[2] ‘Pr(P)’ and ‘Pr(NN)’ describe the models we fit to predict the probabilities of a punitive, anxious or mistrustful response and a non-neutral response, respectively. C refers to the complex model, M to the middling model, and S to the simple model (N/A means that none of these models fit, and so the question is omitted from our estimates). μ refers to the mean (only applicable when estimating the probability of giving a non-neutral response. ‘Neutrals’ gives the proportion of respondents giving neutral responses out of all those in the sample.

Table 7: Results of Panel Unit Root Tests

	T_i	Test	Unit Root
Incarceration Rate	51	LLC	No (0.00)
		IPS	No (0.00)
		Madwu	Yes (0.98)
		Hadri	Yes (0.00)
Officer Rate	42	LLC	No (0.00)
		IPS	No (0.00)
		Madwu	No (0.00)
		Hadri	Yes (0.00)
Corrections Spending	49	LLC	No (0.00)
		IPS	No (0.00)
		Madwu	No (0.01)
		Hadri	Yes (0.00)
Police Spending	49	LLC	No (0.00)
		IPS	Yes (0.71)
		Madwu	No (0.00)
		Hadri	Yes (0.00)
Black Political Representation (%)	41	LLC	No (0.00)
		IPS	No (0.00)
		Madwu	No (0.00)
		Hadri	Yes (0.00)
Violent Crimes per 100,000	51	LLC	No (0.00)
		IPS	No (0.00)
		Madwu	No (0.00)
		Hadri	Yes (0.00)
Black Population (%)	42	LLC	No (0.00)
		IPS	No (0.00)
		Madwu	No (0.00)
		Hadri	Yes (0.00)
GDP per capita (Log)	51	LLC	No (0.00)
		IPS	No (0.00)
		Madwu	Yes (0.07)
		Hadri	Yes (0.00)
Growth Rate	51	LLC	No (0.00)
		IPS	No (0.00)
		Madwu	Yes (0.07)
		Hadri	Yes (0.00)
Income Inequality	51	LLC	Yes (0.24)
		IPS	No (0.00)
		Madwu	Yes (0.41)
		Hadri	Yes (0.00)
Tax Collection	39	LLC	Yes (0.35)
		IPS	No (0.00)
		Madwu	Yes (0.53)
		Hadri	Yes (0.00)
Punitiveness (Enns)	51	LLC	Yes (0.39)
		IPS	Yes (0.35)
		Madwu	No (0.00)
		Hadri	Yes (0.00)

Table 8: Test Details

Abbreviation	Reference	H_0	H_A
Hadri	Hadri 2001	All panels are stationary	Panels are stationary
IPS	Im, Pesaran and Shin 2003	All panels contain unit roots	Some panels contain unit roots
LLC	Levin, Lin and Chu 2002	Panels contain unit roots	Some panels are stationary
Madwu	Maddala and Wu 1999	All panels contain unit roots	At least one panel is stationary

Table 9: Results from Panel Regressions: Unit Root Specifications

	Incarceration Rate	Officer Rate	Corrections Spending
Lagged Dep. Vars			
Δ Incarceration Rate$_{t-1}$	0.116*		
	(0.057)		
Δ Incarceration Rate$_{t-2}$	0.031		
	(0.028)		
Officer Rate$_{t-1}$		0.568**	
		(0.071)	
Officer Rate$_{t-2}$		0.235**	
		(0.021)	
Officer Rate$_{t-3}$		0.116^{+}	
		(0.067)	
Officer Rate$_{t-4}$		-0.069*	
		(0.030)	
Corrections Spending$_{t-1}$			0.820**
			(0.020)
Short-Run Impact			
Black Political Representation (%)$_{t-1}$	0.001	0.863*	-0.006^{+}
	(0.502)	(0.425)	(0.004)
Democratic Control$_{t-1}$	0.549	-0.004	-0.004
	(0.439)	(0.261)	(0.003)
Violent Crimes per 100,000$_{t-1}$	1.591**	0.897**	0.006
	(0.582)	(0.294)	(0.004)
Δ GDP per capita (Log)$_{t-1}$	-2.432	-2.094	0.054
	(2.134)	(2.033)	(0.034)
Δ Growth Rate$_{t-1}$	-0.574	2.237	0.037
	(2.864)	(2.086)	(0.024)
Δ Income Inequality$_{t-1}$	-1.462	1.152	0.008
	(2.065)	(1.550)	(0.012)
Δ Tax Collection$_{t-1}$	1.378**	0.594*	0.014**
	(0.435)	(0.235)	(0.003)
Black Population (%)$_{t-1}$	-0.800	-0.509	0.001
	(0.570)	(0.361)	(0.003)
Δ Punitiveness (Enns)$_{t-1}$	-5.774	-14.896	-0.059
	(8.301)	(9.313)	(0.103)
Long-Run Multiplier			
Black Political Representation (%)	0.009	5.749*	-0.033^{+}
	(0.591)	(2.589)	(0.020)
Violent Crimes per 100,000	1.887**	5.971**	0.033
	(0.697)	(1.722)	(0.022)
Δ Tax Collection	1.600**	3.972*	0.076**
	(0.528)	(1.813)	(0.020)
Δ Punitiveness (Enns)	-6.432	-99.940^{+}	-0.347
	(9.866)	(67.248)	(0.568)
Model Info			
Observations	1802	1764	1813
States	49	49	49
Range	1972-2008	1973-2008	1972-2008
Avg. N_i	36.8	36	37
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Lags of DV	2	4	1
Adj. R^2	0.184	0.875	0.911