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The Franchise, Policing, and Race: Evidence from Arrests Data and the Voting Rights Act[†]

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This paper investigates the relationship between the franchise and policing. We find that, following the Voting Rights Act of 1965, Black arrest rates in counties that both had more newly enfranchised Blacks and were covered by the legislation fell, compared to similar Southern counties that were not covered. We document no corresponding patterns for White arrests. Our results are driven by arrests by sheriffs, who are always elected, and by less serious offenses. These results indicate that voting rights, when combined with electoral accountability, lead to improved treatment of minority groups by police. (*JEL* D72, J15, K16, K42, N32, N42)

> So as opposed to a sheriff being appointed by a mayor or city council and being beholden to that city council, we are beholden to the people. We see our bosses as the citizens that elect us.
>
> Greg Champagne, president of the National Sheriffs' Association[1]

The treatment of minority groups by police in the United States is a perennially controversial issue, and the long shadow of Jim Crow laws still looms large over the public debate on policing and race.[2] After the end of Reconstruction in the late 1870s, African Americans were gradually deprived of their voting rights, and institutionalized discrimination and segregation became dominant features of race relations in the US South (see Calderon, Fouka, and Tabellini 2023; Margo 2008). Law enforcement was one important domain. With roots in the seventeenth and eighteenth century slave patrols (Hadden 2001), police forces in southern states

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[1] Source: https://www.usnews.com/news/politics/articles/2016-11-04/joe-arpaio-david-clarke-and-why-the-us-still-elect-sheriffs.

[2] Jim Crow laws were introduced in Southern states between the end of the nineteenth century and the beginning of the twentieth century to enforce racial segregation. Upheld in the 1896 Supreme Court ruling on *Plessy v. Ferguson*, they began to be repealed in the 1950s starting with the 1954 ruling on *Brown v. Board of Education*, declaring the segregation of public schools unconstitutional.

have played a crucial role in the enforcement of Jim Crow laws.[3] The systematic violation of African American rights at their hand did not go unnoticed, leading to a series of federal inquiries, starting with the Wickersham Commission in 1931. The Civil Rights Commission (1961) provides a particularly vivid snapshot, chronicling baseless arrests of "uppity" Blacks, whose only fault was defying the existing racial social order. Significantly, it was the police brutality in the 1965 "Bloody Sunday" beatings of civil rights activists to trigger federal intervention, which led to the enactment of the Voting Rights Act (VRA) of 1965.

The VRA removed obstacles to Black voter registration.[4] Covered jurisdictions in the US South were placed under federal oversight and required preclearance of any changes to their voting laws by the federal government.[5] The passage of the VRA marked a watershed moment (Cascio and Washington 2014), bringing about significant gains to Black communities in a number of areas (Wright 2013). Yet, not much is known on whether this cornerstone of civil rights legislation affected police–minority relations, as most of the literature focuses on contemporary law enforcement (Persico 2009).

To what extent could Black enfranchisement improve the police treatment of African Americans? Policing is an important public service and, thus, it is natural to conjecture that its delivery can be addressed at the ballot box. Yet, the link between minorities political and civil rights is far from obvious, given the potential risk of tyranny by the majority (Zakaria 1997; Mukand and Rodrik 2020). Anecdotal evidence indicates that a number of chief law enforcement officers (CLEOs) known for their harsh treatment of Blacks were removed from office by voters in the South following the enactment of the VRA.[6] At the same time, as the Black vote became crucial in many electoral races, White sheriffs began to change their behavior toward this newly enfranchised group.[7]

In this paper, we attempt to move beyond this anecdotal evidence via a systematic causal analysis of the effects of the franchise on arrest rates by race using data from the FBI Uniform Crime Reports (UCR) (Federal Bureau of Investigation 2018), spanning the period 1960–1981. If the extension of the franchise has an effect, areas with a larger share of African Americans should experience a change in Black arrest rates after the passage of the VRA. However, as the racial composition of arrests in those areas might have changed independently of the federal intervention, the comparison of arrest patterns by race over time is combined in our analysis with the

[3] As pointed out by Myrdal (1944, 535), "The Negro's most important public contact is with the policeman. He is the personification of white authority in the Negro community."

[4] The main obstacle involved literacy test provisions.

[5] Covered jurisdictions were those that both imposed a test or device restricting the right to vote and experienced a turnout below 50 percent in the 1964 presidential election.

[6] One particularly emblematic example involves Jim Clark—the sheriff of majority-Black Dallas County, Alabama—who gained notoriety in the "Bloody Sunday" events of 1965, when a group of civil right activists in Selma were brutally beaten by his infamous sheriff posse. In the subsequent election of 1966—which saw a record number of African Americans registering to vote for the first time—he lost office. Clark was replaced by Wilson Baker, a White moderate overwhelmingly supported by the Black electorate.

[7] For example, as pointed out by Peirce (1974, 189), "The white sheriff in heavily black Holmes county [Mississippi] won … by doing an about-face from the old Southern stereotype. 'He is terrific,' Robert G. Clark, a prominent black politician, acknowledged, 'He treats everybody like a man. If he comes to your house with a search warrant, he is polite, and if he has to arrest you, he apologizes for this.'"

addition of a control group—that is, jurisdictions in the former Confederacy with a similar history of discrimination and Black disenfranchisement that were not covered by the VRA.

Our identifying assumption is that, in the absence of federal intervention, covered and noncovered jurisdictions would have experienced the same trends in the racial patterns of arrests. Using an event-study approach, we find no evidence of pre-trends for either Black or White arrests within covered counties with a larger share of African Americans relative to comparable counties not covered by the VRA. Likewise, we rule out the existence of pre-trends in crime rates and other outcomes capturing the long-term evolution in racial attitudes and political participation.

Turning to the effect of the VRA on police–minority relations, our main result is that arrest rates for Blacks fell in counties that were both covered and had a high concentration of Black residents. We find no corresponding patterns for White arrest rates.

How do institutional features of police departments affect these patterns? Law enforcement in the United States is hyperlocalized, with significant variation in the selection of CLEOs, who shape departmental culture through their leadership and are in charge of policing practices with direct effects on the treatment of minorities (Hailes and Manalili 2000).[8] Importantly, while many police chiefs in the United States are appointed by city mayors, nearly all sheriffs, whose officers patrol counties, are elected, potentially making them more responsive to voters.[9] Consistent with this idea, we document that our results are driven by arrests carried out by sheriffs rather than by police chiefs. Hence, the direct election of CLEOs is an important channel through which the effects of the VRA played out. Furthermore, we show that the evolution of Black arrests is driven by less serious misdemeanor offenses—over which police have more discretion—rather than more serious felonies. Thus, Black enfranchisement led to an improvement in the police treatment of minorities, helping to address concerns about baseless arrests for minor crimes.

Elections can affect arrest rates by changing the behavior of incumbent CLEOs or by affecting the identity of CLEOs voted into office. For example, the enfranchisement of African Americans could bring into power Black CLEOs or more moderate Whites. Analyzing the race of CLEOs, we document the near complete absence of Blacks 20 years after the passage of the VRA. This allows us to rule out the identity politics channel for sheriffs or police chiefs. Furthermore, analyzing sheriff incumbency patterns over time, we find no evidence of an increase in sher-

[8] Leadership within organizations can fundamentally shape outcomes (Bertrand and Schoar 2003), and the implications of different methods of selection of chief executives have been studied in a variety of domains (Besley and Coate 2003; Iaryczower, Garrett, and Shum 2013; Nowacki and Thompson 2021). In the context of law enforcement, related research has examined police oversight (Devi and Fryer Jr. 2020; Rivera and Ba, forthcoming), officer motivation (Mas 2006), and the relationship between crime and low-level arrests (Cho, Goncalves, and Weisburst 2023).

[9] Broadly speaking, policing at the local level falls under the authority of sheriff offices, which have county-wide jurisdiction, and municipal police departments, providing police services only within incorporated municipal limits. Despite their county-wide powers, sheriffs mainly focus on policing unincorporated areas and, thus, police functions tend to be shared by the two bodies according to a rural-urban divide. Besides policing different areas, the two bodies also differ in terms of functions and in the degree of independence from other local governments. In particular, the sheriffs' duties are broader since—besides law enforcement—they are also in charge of county jails and correctional facilities, carry out bailiff duties, and, in some instances, are responsible for the collection of county fees and taxes and the sale of licenses and permits.

iff turnover in covered counties with large Black populations. These findings are consistent with anecdotal evidence about sheriffs changing their behavior toward African Americans in the aftermath of the VRA.

Even if the VRA did not lead to the election of Black CLEOs, it paved the road to Black office holding in other important local governments, such as county commissions and city councils, which might play a role in developing policing guidelines and practices, which in turn might affect arrest rates. Overall, we do not find evidence that increased Black representation in other local offices led to changes in racial patterns of arrests.

We then address four alternative explanations for our results. First, if certain jurisdictions, such as municipalities, are more likely to have unionized police forces, CLEOs might be less able to discipline officers in case of misconduct. This could explain differences in arrest patterns between sheriffs and police chiefs. We rule out this potential mechanism by showing that unionization rates are similar in the treatment and control groups for both police and sheriff offices. Second, our results could be due to changes in the supply of crime by Blacks resulting from other developments brought about by the VRA, such as migration, improvements in schooling, or improvements in labor market conditions. Given that the latter materialized many years after the VRA, whereas the decline in Black arrests occurred soon after, we conclude that these factors are unlikely to have played an important role in driving our results. Third, our findings could be driven by changes in the supply of crime in response to new policing practices. Yet, more lenient law enforcement toward African Americans should lead to a decline in the supply of crime, implying that, if anything, our estimates would be downward biased. Fourth, our results could be due to the presence of elevated arrests of Blacks at civil rights protests during the pre-VRA era. To address this potential explanation, we show that our results are similar when controlling for Black activism. Moreover, they are driven by arrests carried out by sheriffs, whereas protests were concentrated in more urban areas under the jurisdiction of municipal police chiefs.

The civil rights era brought about a massive realignment of political allegiances in the US South (Kuziemko and Washington 2018). As barriers to voter registration were removed, Black voter registration and turnout soared (Bernini et al., forthcoming; Cascio and Washington 2014; Wright 2013), bringing about an expectation of fundamental changes in policy areas in which the Black electorate did not have any voice. A recent and growing literature has exploited the geographic variation in the VRA provisions to analyze the effect of enfranchisement on the distribution of state funds (Cascio and Washington 2014), White backlash (Ang 2019; Bernini et al., forthcoming), Black elected officials in local governments and government spending (Bernini, Facchini, and Testa 2023), and labor market outcomes (Aneja and Avenancio-Leon 2019).

We contribute to this literature by studying the VRA in the context of the administration of justice. In 1961, the report of the US Civil Rights Commission (1961) widely acknowledged the issues of widespread police brutality against African Americans in the US South, advocating for changes in leadership to address discrimination in the administration of justice. In his iconic "I Have a Dream" speech of 1963, Dr. Martin Luther King, Jr. denounced the unspeakable horrors of police

brutality against Blacks, while also advocating for their right to vote.[10] The VRA, giving voice to previously disenfranchised Blacks, provided the tools to increase police leadership accountability. Whether it led to tangible improvements in policing practices in Black communities remains an open empirical question that we address, focusing on arrests by race within the US South.

I. Context and Data

Our goal is to study the effect of the VRA on racial patterns of law enforcement. To do so, we focus on the eleven states of the former Confederacy, sharing a similar history of slavery and Black disenfranchisement, and exploit geographic variation in coverage, one of the key VRA provisions. As shown in the top panel of Supplemental Appendix Figure A1, 6 of the 11 Confederate states (Alabama, Georgia, Louisiana, Mississippi, South Carolina, and Virginia) were fully covered, and 1 (North Carolina) was partially covered.[11]

Our analysis will rely on arrests data by local police offices. Although we do not have detailed information on the context around the arrest, such as police use of force, arrests and incarceration have been used by White elites in the South as a tool to exert social control over African Americans (Mazumder 2019). White sheriffs were often the most powerful local officials and the " … principle enforcers of the social and legal convention of the Jim Crow Society … the sheriff sent a signal to the Black community: any Black citizen entertaining thoughts of challenging the system had only to walk by the local jail to see the hierarchy of race" (Moore 1997, 53).

We have assembled a database of arrests by local police offices from the UCR (Federal Bureau of Investigation 2018) spanning the period 1960–1981.[12] Our initial unit of observation is an agency year. However, due to voluntary filing and limited reporting during the pre-VRA era, an analysis based on annual data is not feasible. We thus average annual arrests data for each race (i.e., Black arrests and White arrests) by agency over time, using different time windows depending on the type of analysis we carry out. For our long-run measure of growth in arrest rates, we start by averaging data on pre-VRA (1960–1965) and post-VRA (1975–1981) years, and obtain a balanced panel by dropping agencies that do not report either pre- or post-VRA.[13] Then, for each agency type (police and sheriff), we collapse arrests

[10] "We can never be satisfied as long as the Negro is the victim of the unspeakable horrors of police brutality..." (King, Jr. 1963).

[11] More precisely, of the 100 North Carolina counties, 39 were covered. See the Supplemental Appendix for more details. In the 1975 reauthorization of the VRA, coverage was extended to take into account potential discrimination against language minorities. As a result, the provision was extended to any jurisdiction where a single language minority group comprised more than 5 percent of the voting age population in 1970 in addition to the turnout threshold and required ballots to be bilingual. Given that our focus is on the effect of the VRA on the arrest rates of African Americans, we adopt the 1965 definition. We address this issue later in a robustness check.

[12] See the Supplemental Appendix for details. Our analysis stops in 1981 because the 1982 reauthorization of the VRA introduced major amendments in the legislation.

[13] The frequency of reporting by agency in the pre- and post-VRA period is summarized in Supplemental Appendix Figure A3, illustrating the number of years for which data are available (up to five years before and six years after the VRA) and the corresponding shares of reporting agencies. As shown, there is limited reporting during the pre-VRA period, especially for sheriff offices.

to the county level and create race specific rates by dividing arrests by the relevant county population (i.e., Black population or White population using the 1960 census for the pre-VRA and the 1980 census for the post VRA periods). Finally, we construct the growth rate of arrests as the difference of log arrest rates, between the post- and pre-VRA periods.[14] These data are then merged with county-level characteristics obtained from the census and other sources. See the Supplemental Appendix for the definitions of variables and sources.

Given the voluntary nature of UCR filing, a natural concern involves sample selection, and we address this issue in several ways. First, as noted above, filing was voluntary both before and after the VRA, and we only focus on jurisdictions that reported during both eras. Second, as shown in the bottom panel of Supplemental Appendix Figure A1, which displays the geographic coverage of our sample, the number of reporting agencies appears to be scattered throughout the region, not clustered in any particular area and, as shown in Supplemental Appendix Figure A2, the likelihood of reporting does not vary differentially in covered counties, relative to uncovered ones, depending on their racial composition. Third, as shown in Supplemental Appendix Table A1, the key sociodemographic characteristics of the counties included in our analysis are broadly comparable to those of the overall South, even if they have larger populations and tend to be more urban. [15]

Table A2 in the Supplemental Appendix presents summary statistics. Before the introduction of the VRA, the overall average arrest rate is 6.37 per thousand for Blacks, whereas the corresponding figure for Whites is 1.99. After the VRA, the number of arrests increases for both groups, reaching on average 6.69 per thousand for Blacks, and 3.12 for Whites. Thus, the growth in arrest rates is much higher for Whites than for Blacks. Municipal police carry out more arrests than sheriff offices for both Blacks and Whites. This is consistent with the sheriff sample being, on average, less populated and more rural than the police sample. To address this difference, our specifications, as described below, include controls for share rural, with some specifications estimated separately for sheriff and police samples. On the other hand, they are broadly comparable in terms of other important characteristics, such as the Black population share and the unemployment rate.

II. Extension of the Franchise and Arrests

We develop our main hypotheses in the context of a probabilistic voting model, with a full presentation in the Supplemental Appendix. In this model, the key policy involves police treatment of Blacks, with White voters preferring harsher treatment than Black voters. Two White candidates, who care about winning, but

[14] Thus, the unit of observation is the agency type–county, with some counties having both police and sheriff observations, some having only one sheriff observation, some having only one police observation, and some counties having neither type and, thus, not represented in the data.

[15] In addition, using data on homicides derived from the National Center for Health Statistics multiple causes of death database, as described in the Supplemental Appendix, we do not find different regression coefficients on the interaction between covered county status and Black share when comparing our sample to the overall South. See Supplemental Appendix Figure A5.

are also citizens with preferences over policy, announce credible platforms prior to the election.

Before the VRA, as only Whites vote, equilibrium policies reflect their preferences. After the VRA, Black voters are allowed to cast ballots, and candidates moderate their platforms toward the preferences of this group. The effect of the franchise extension is more pronounced in areas with a larger share of Black voters.

We next extend the model to consider appointed officials, with voters choosing an elected leader who then appoints a CLEO. Given that voters now choose a bundle of officials, the link between voters' preferences and platforms is weakened. As a result, the effect of the franchise extension is stronger with directly elected rather than appointed officials.

Our test of the first hypothesis—that is, treatment of Blacks by police should improve more in areas with a larger share of African Americans—is based upon a three-fold comparison: (i) before and after the VRA; (ii) in counties covered by the VRA versus comparable counties in the South; and (iii) in counties with significant Black populations, which are more affected by the VRA, versus less Black counties, which are less affected by it. The first two comparisons are discrete in nature, while the final comparison is based upon a continuous measure: the fraction of African Americans in the jurisdiction.[16] To test the second prediction, we introduce an additional comparison between sheriffs, which are always elected, and police chiefs, which are typically appointed.

Figure 1 displays the variation in the growth of arrest rates and pre-VRA Black population shares that we will exploit in our analysis. As shown in the upper left panel, Black arrest rates in noncovered counties are growing more quickly in areas with a larger share of African Americans.[17] But, importantly, the growth in Black arrest rates is less pronounced in covered areas, as shown in the upper right panel. White arrest rates (bottom panel), by contrast, appear to be essentially independent of the Black population in the county, and this is the case for both noncovered (lower left panel) and covered areas (lower right panel). Figure 1 thus provides prima facie evidence of differences in racial patterns of arrests depending on county racial composition and coverage. These comparisons will be critical to our empirical strategy.

Our identifying assumption is that, in the absence of federal intervention, covered and noncovered jurisdictions—sharing a similar history of discrimination and disenfranchisement—would have experienced the same trends in the racial patterns of arrest rates.[18] To corroborate this assumption, we investigate pre-trends using an

[16] Our limited sample sizes unfortunately do not permit a comparison of counties along state borders—an approach that is utilized in related studies, such as Aneja and Avenancio-Leon (2019) and Bernini, Facchini, and Testa (2023).

[17] Among other possible explanations, this increase in Black arrest rates could reflect either efforts by local officials in majority Black areas to reduce crime starting in the 1970s (Forman Jr. 2017) or efforts by the federal government, including funding local police departments, to combat crime in urban areas during the 1960s and 1970s (Hinton 2016).

[18] Although our pre-VRA period is relatively short, 1963 represents a watershed moment for the salience of civil rights (Kuziemko and Washington 2018) as the media turned their attention to the ongoing boycott of segregated shops in Birmingham, Alabama. The violent repression of civil rights activists by the local police chief was aired on live television, drawing the attention of the public and of the Kennedy administration. As a result, different trends could have emerged already in the 1963 and 1964 window compared to the earlier period.

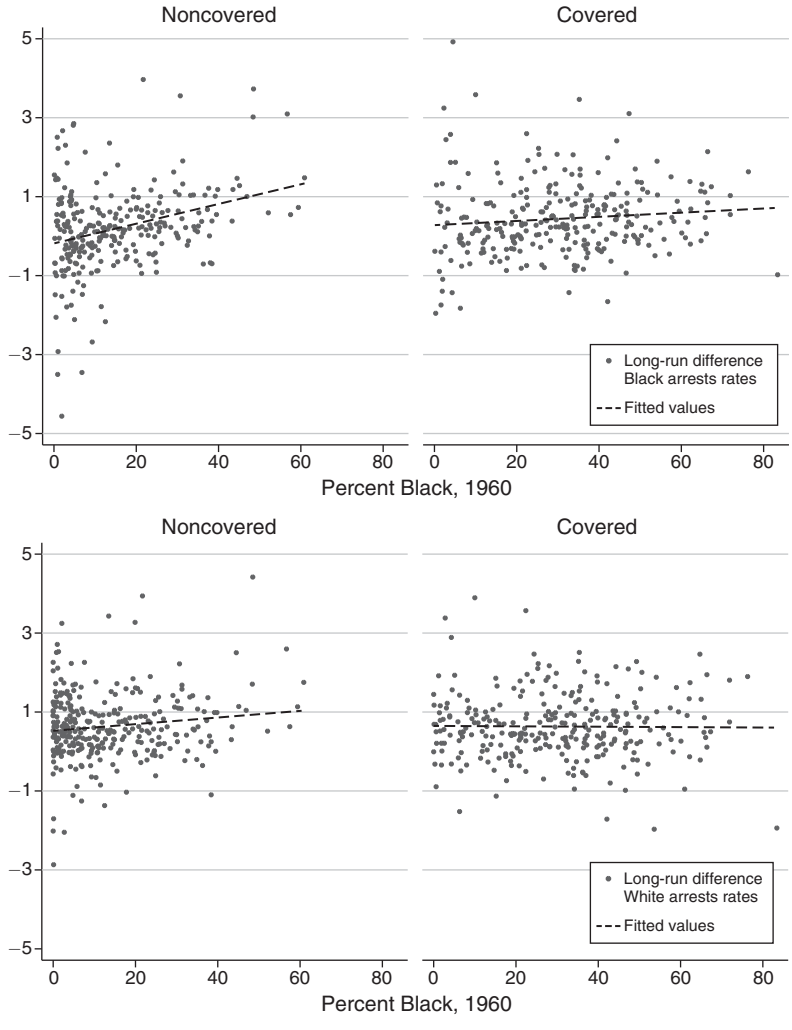

FIGURE 1. CHANGE IN ARREST RATES (1960–1981), BY COVERAGE

Notes: Estimated parameter: Black share 1960. Bars represent 95 percent confidence intervals.

event-study approach, where we average data over five time intervals and estimate the following specification:

$$(1) \qquad y_{ct} = \sum_{n \neq 1960} \gamma_n D_n^t Black_{1960} + \sum_{n \neq 1960} \theta_n D_n^t Black_{1960} \times Cov_c + \mathbf{X}_c' \boldsymbol{\beta} + I_{st} + I_c + \epsilon_{ct},$$

where y_{ct} is the natural log of arrest rates (of either race) in county c at time t; $Black_{1960}$ is the 1960 Black population share in the county; Cov_c is an indicator equal to one for counties covered by the policy in 1965 and zero otherwise; and D_n^t is an indicator taking a value of one if $n = t$. In our model, we include county fixed effects, I_c, as

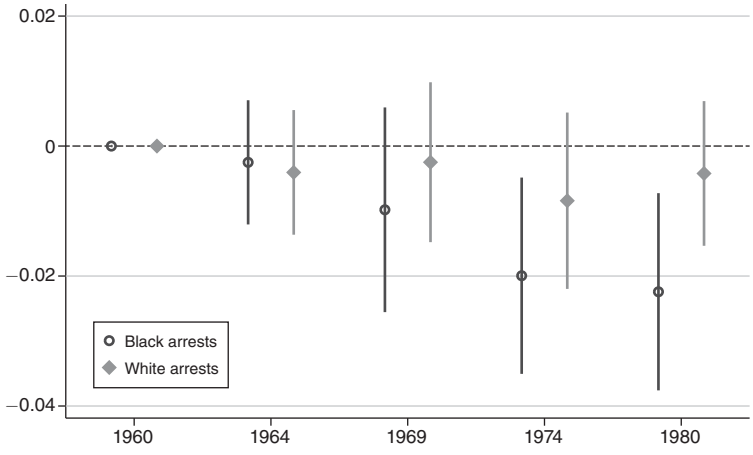

Figure 2. Difference in the Gradient in Black and White Arrest Rates in 1960 Percent Black between Covered and Noncovered Counties

Notes: Estimated parameter: Black share 1960 × coverage. Controls: population, population × coverage, unemployment rate, unemployment rate × coverage, state-year interactions, and county fixed effects. Omitted interaction: 1960. Bars represent 95 percent confidence intervals.

well as state-year interactions, I_{st}, capturing time varying state specific shocks. Finally, $\mathbf{X}_c'$ is a vector of pre-VRA county-level controls that include population and unemployment, both interacted with the Cov_c indicator. To identify the model, we omit the first data point in our sample—that is, the average of log arrest rates over the period 1960–1962.[19] Given that district courts played a key role in enforcing the coverage provisions of the VRA, in line with Bernini, Facchini, and Testa (2023), we cluster standard errors in all our specifications by judicial divisions, accounting for potential correlation at this level.

Figure 2 reports the difference in the gradient of average arrests rates (ln) in the 1960 share of Blacks between covered and noncovered counties, by race. As shown, during the pre-VRA period, covered counties with larger shares of African Americans do not exhibit different patterns in arrest rates by race. After the passage of the act, on the other hand, Black arrests in covered counties grew less than in comparable counties not covered by the federal legislation. This pattern is already visible in the first five years after the passage of the VRA, but the difference in the gradient becomes larger and statistically significant over the subsequent decade. For White arrests, by contrast, there is not a significant decline.

The lack of pre–trends is reassuring as it lends support to our identifying assumption that treatment and control groups do not display differences in racial patterns of arrests before the passage of the VRA. At the same time, patterns in arrest rates by race may not capture the evolution of more extreme forms of police violence against

[19] We use separate indicators for the covered and noncovered portions of North Carolina. To simplify the notation, the reference period has been labeled as 1960.

African Americans, such as deadly use of force or lynching. Moreover, our pre–trends analysis only covers a short period of time before the passage of the VRA, but important differences in racial attitudes and political participation between the deep and the outer South started to emerge much earlier, potentially raising the issue of selection into treatment (Bernini, Facchini, and Testa 2023). We address all these concerns in Supplemental Appendix Table A3, where we analyze pre-trends using data on police use of force, lynching, KKK membership, NAACP membership, voter turnout, and Republican vote shares. Overall, we find little evidence of pre-trends in these outcomes.[20]

III. Baseline Analysis

We next turn to a long differences analysis, where we focus on the growth in average (ln) arrests rates between the pre- (1960–1964) and post-VRA (1975–1981) periods. This approach allows us to build a meaningful sample to carry out the analysis by agency, for which a higher frequency study is not feasible due to limited reporting over time, particularly for sheriffs. Moreover, as the UCR survey starts in 1960, averaging over the entire pre-VRA period mitigates potential measurement error in the earlier years in which the survey was administered.

Our long-run empirical analysis is based upon the following regression specification:

$$(2) \quad \Delta ArrestRates_c = \gamma Black_{1960} + \theta Black_{1960} \times Cov_c + \mathbf{X}_c' \boldsymbol{\beta} + I_s + \epsilon_c,$$

where $\Delta ArrestRates_c$ measures the change, from pre- to post-VRA, in the natural log of arrest rates of either Black or White individuals in county c; $Black_{60}$ is the share of African Americans in the county in the 1960 US Census; and Cov_c indicates that the county was covered by the VRA. $\mathbf{X}_c$ is a set of pre-VRA county characteristics. In some specifications, we also allow the impact of these controls to vary by treatment status. Finally, I_s are state fixed effects, which capture state-specific trends in this long difference specification. The main parameter of interest is θ, which captures the difference in the gradient of the 1960 Black population share between treatment and control counties, after accounting for the main effects of the share of African Americans in 1960 and coverage status. To test our second prediction, we run these regressions separately by sheriff and police agencies.

[20] We do find some evidence of negative pre-trends for lynching, which indicates possible negative selection into treatment, meaning that pre-trends reflected in lynching would work against our hypotheses. It is still possible that other unobserved differences between treatment and control groups could bias our results. Yet, since coverage was targeted toward jurisdictions with the worst record of discrimination, in the absence of federal intervention, those counties would have been likely to experience a worsening—rather than an improvement—of the minority treatment by law enforcement. Hence, if anything, our estimates would provide a lower bound for the actual effects. At the same time, to mitigate these concerns, we will take into account pre-VRA lynching and additional county-level characteristics in our baseline analysis. Supplemental Appendix Table A4 investigates pre-VRA trends in economic and demographic outcomes. We find no changes between covered and noncovered areas in the Black population share, unemployment, the proportion of unskilled, the share poor in the population, and cotton yield (a proxy for agricultural productivity). We find, instead, statistically significant differences in population growth and in the percent rural.

Table 1—OLS Models. Dependent Variable: Sheriffs and Police, Long-Run Difference in ln(Arrests Rates), by Race (1960–1981)

	Black (1)	White (2)	Black (3)	White (4)	Black (5)	White (6)	Black (7)	White (8)	Black vs White (9)
Percent Black, 1960 × coverage	−0.016 (0.007)	−0.009 (0.006)	−0.016 (0.007)	−0.009 (0.006)	−0.017 (0.007)	−0.005 (0.006)	−0.018 (0.009)	−0.001 (0.008)	−0.018 (0.009)
Percent Black, 1960	0.022 (0.006)	0.009 (0.004)	0.022 (0.006)	0.008 (0.005)	0.024 (0.006)	0.007 (0.005)	0.024 (0.007)	0.007 (0.005)	0.024 (0.007)
White × percent Black, 1960 × coverage									0.017 (0.007)
Basic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other controls	No	No	No	No	Yes	Yes	Yes	Yes	Yes
Coverage × controls	No	No	Yes	Yes	No	No	Yes	Yes	Yes
State trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.09	0.01	0.09	0.01	0.12	0.03	0.10	0.02	0.09
Observations	551	589	551	589	486	521	486	521	1,007

Notes: Robust standard errors clustered by judicial divisions in parentheses. Basic controls: population, unemployment rate. Other controls: percent family below the poverty line, percent unskilled, percent rural, cotton suitability, farms with 700 acres or more, pro-Black protests, anti-Black protests, Black police, green book establishments, lynching.

Our baseline regression results are presented in Table 1. Columns 1 and 2 report our most parsimonious specifications, including only the variables of interest and the basic economic and demographic controls we have used in our event study. In columns 3 and 4, we allow the effects of the controls to vary by treatment status. In columns 5–8, we follow the same structure, but we now account also for other potential drivers of the patterns we have uncovered including additional pre-VRA economic and demographic controls (i.e., percent family below the poverty line, percent unskilled, and percent rural), proxies of Black political activism (pro-Black activism), White racial attitudes (anti-Black activism), historic prevalence of slave labor (cotton suitability), land ownership concentration, the presence of Blacks in the police force, episodes of violence toward African Americans (lynching), and green book establishments, capturing the segregation in public accommodation targeted by the Civil Rights Act.

Our results show that Black arrest rates grew more slowly in covered areas with significant Black populations, and these findings are remarkably robust when including interactions between coverage and controls (columns 3 and 7).[21] The estimated effect of coverage is large in magnitude: An increase in the Black share of

[21] They continue to hold also when dropping from the analysis those jurisdictions covered under the 1975 VRA amendments (i.e., all counties of Texas and four counties in Florida) or redefining coverage as a categorical variable taking a value of 1 for jurisdictions covered under the 1965 and a value of 0.5 for those covered in 1975 to account for the fact that the former were exposed to a much longer treatment than the latter, as shown in Supplemental Appendix Table A6. We also investigate possible nonlinearities (see Supplemental Appendix Figure A4) and we find that Black arrest rates are rising more quickly in counties with larger Black populations for counties not covered by the VRA, relative to counties covered by the VRA. We do find some evidence of a flattening of the curve for noncovered jurisdictions around the 50 percent threshold and some evidence of a decline in Black arrest rates for covered counties with a very large Black share, in excess of 70 percent. These findings are noisy, though, and there is no common support in the Black share in uncovered jurisdictions. Note that also, as shown in Supplemental Appendix Table A6, our baseline results continue to hold when focusing on the sample with common support in the Black share.

TABLE 2—OLS MODELS. DEPENDENT VARIABLE: SHERIFF VERSUS POLICE LONG-RUN DIFFERENCE IN ln(ARRESTS RATES), BY RACE (1960–1981)

	Sheriff sample		Police sample		Sheriffs versus police	
	Black	White	Black	White	Black	White
	(1)	(2)	(3)	(4)	(5)	(6)
Percent Black, 1960 × coverage	−0.034	−0.012	−0.005	0.006	−0.034	−0.012
	(0.014)	(0.013)	(0.008)	(0.006)	(0.014)	(0.013)
Police × percent Black, 1960 × coverage					0.029	0.018
					(0.016)	(0.014)
Adjusted R^2	0.05	0.03	0.13	0.18	0.13	0.16
Observations	246	278	354	369	600	647

Notes: Robust standard errors clustered by judicial divisions in parentheses. In all our specifications, we include the full set of controls of columns 7 and 8 of Table 1: their interaction with coverage status and state trends.

the population by 10 percentage points, from, say, 20 percent to 30 percent, leads to a 16–18 percent reduction (columns 3 and 7) in the growth of the arrest rate for Blacks in covered counties, relative to noncovered counties. The corresponding analysis using White arrest rates never produces statistically significant differences.[22] Finally, we show that Black arrest rates, relative to White arrest rates, decline in covered counties with significant populations of newly enfranchised Black voters and that this difference is statistically significant. We establish this in column 9, where we report the coefficients from a regression specification pooling together (i.e., stacking) Black and White arrests and show that the triple interaction coefficient is statistically significant.

We next introduce an additional comparison: county versus municipal governments. Given that sheriffs are always elected in the South and that municipal police chiefs are mostly appointed, we expect our baseline results to be driven by county governments. As shown in Table 2 and across the first four specifications that we consider, Black versus White arrest rates and county versus municipal governments, the only statistically significant effect of coverage involves the growth in arrests of Blacks by sheriffs, as documented in column 1. This effect is, again, large in magnitude.[23] Differences are small and statistically insignificant for Blacks arrested by municipal police (column 3) as well as for Whites arrested by both sheriffs (column 2) and municipal police (column 4). Again, these findings are consistent with our hypothesized electoral mechanism.[24] The

[22] Supplemental Appendix Table A5 reports all coefficients for the specifications without interactions. Our results indicate that the arrest rates—for both Whites and Blacks—grew less in counties with larger initial populations and more episodes of pro-Black activism before the VRA. Furthermore, counties with a larger preexisting Black police force exhibit slower growth in arrest rates for Blacks.

[23] An increase in the Black share of the county population by 10 percentage points, from, say, 20 percent to 30 percent, leads to a 34 percent reduction in the growth of the Black arrest rate in covered counties, relative to noncovered counties.

[24] In the Supplemental Appendix, we explore whether this result could be driven by systematic differences in the degree of urbanization between the police and sheriff samples. In particular, we restrict our analysis to counties in the common support for the share of population living in rural areas for both the sheriff and police sample and our results are unaffected (see Supplemental Appendix Table A6). Additionally, we explore the electoral mechanism using variation in the selection procedures for municipal CLEOs. While we lack definitive information on

Table 3—OLS Models. Dependent Variable: Long-Run Difference in ln(Arrest Rates, by Race and Offense) (1960–1981)

	Felony Black (1)	Nonfelony Black (2)	Felony White (3)	Nonfelony White (4)
Sheriff and police sample				
Percent Black, 1960 × coverage	−0.014	−0.021	−0.006	0.002
	(0.008)	(0.009)	(0.008)	(0.008)
Adjusted R^2	0.07	0.12	0.03	0.02
Observations	457	484	510	520
Sheriff sample				
Percent Black, 1960 × coverage	−0.029	−0.040	−0.017	−0.006
	(0.016)	(0.015)	(0.012)	(0.013)
Adjusted R^2	0.05	0.05	0.04	0.03
Observations	231	244	276	277
Police sample				
Percent Black, 1960 × coverage	0.005	−0.009	0.005	0.004
	(0.011)	(0.008)	(0.009)	(0.007)
Adjusted R^2	0.06	0.18	0.05	0.19
Observations	330	352	357	369

Notes: Robust standard errors clustered by judicial divisions in parentheses. In all our specifications, we include the full set of controls of columns 7 and 8 of Table 1: their interaction with coverage status and state trends.

final two columns pool together (i.e., stack) the police and the sheriff samples. As shown in column 5, when studying Black arrests, the results are driven by sheriffs and there is a statistically significant difference in this effect between police and sheriff departments. When studying White arrests, as shown in column 6, no such patterns appear. This result is consistent with our theoretical model, which predicts that the effect of the VRA should be stronger for elected CLEOs—sheriffs in this case—than for appointed ones.

Our next extension involves the type of offense underlying the arrest. In the United States, crimes are typically classified into less serious misdemeanors and more serious felonies. If our results are driven by changes in policing practices following the VRA, such practices should be mostly driven by misdemeanor crimes, given that police have more discretion over arrests in these cases, relative to more serious felony crimes. As shown in the first panel of Table 3, our baseline results are indeed driven by changes in nonfelony arrest rates for Blacks, and these results are statistically significant. We also uncover a decline in felony arrest rates for Blacks, but this result is

whether CLEOs are elected or appointed in each municipality, we are able to discern, using data from the Census of Government (1957), whether municipalities have the discretion from the state to elect their police chiefs (see the Supplemental Appendix for more details). Given our interpretation that electoral systems changed policing practices, we expect that arrests by municipal police under elective CLEOs should mirror our results regarding arrests by elected sheriffs. Appointed police chiefs should instead be less responsive. Supplemental Appendix Figure A6, where we report coefficient estimates for the interaction between 1960 Black share and coverage status for municipalities with and without discretion in electing their CLEOs, broadly confirms the relevance of the electoral channel.

not statistically significant at conventional levels.[25] For White arrest rates, we again find no systematic patterns with respect to whether or not the jurisdiction is covered and the size of the Black population. In the second and third panel of Table 3, we examine the results separately for sheriffs and police chiefs and for type of crime. As shown, our findings are driven by nonfelony arrests of African Americans by sheriffs (column 2 of the second panel). This effect is the largest that we have documented. Once again, we find instead smaller and not statistically significant effects for all coefficients involving police chiefs (columns 1–4 of the third panel).[26]

IV. Mechanisms

We have documented so far that the enfranchisement of Black voters leads to a reduction in Black arrest rates for minor offenses and that these results are driven by the behavior of sheriffs. In this section, we carry out additional exercises to shed light on the electoral mechanism underlying our findings. We first examine whether our findings are driven by an increase in Black sheriffs and/or a reduction in incumbency rates for White sheriffs. We then examine the role of other Black elected officials, such as county commissioners.

Elections can impact arrest rates by changing the behavior of incumbent CLEOs, or by affecting the identity of the CLEO voted into office. For example, the enfranchisement of African Americans could bring into power Black CLEOs or more moderate Whites. To explore this channel, we start by investigating whether our baseline results are due to a change in the race of elected officials. To this end, we start by using information on the race of elected CLEOs from Bernini, Facchini, and Testa (2023). We find no Black sheriffs or elected police chiefs during the pre-VRA era. In 1980, there are only two Black sheriffs, both in covered counties, and only three elected police chiefs and marshals (two in covered counties and one in a noncovered county). Given these very small counts of Black elected CLEOs, our documented reduction in Black arrest rates following the VRA must be driven by a change in behavior and/or in the type of elected White sheriffs. While we do not have any direct measures of the behavior of White CLEOs, above and beyond our measures of arrest rates, we can investigate whether the VRA increased sheriff turnover. To this end, we have hand-digitized the names of all sheriffs in the US South, exploiting several archival sources reporting information on county elected officials during the period 1956–1981. We then compare sheriff names between adjacent elections to develop an indicator of sheriff incumbency that we average over presidential terms to obtain average incumbency rates.[27]

[25] While the magnitude of the effect is smaller for felonies, these two coefficients are not statistically different from one another, likely because of the small size of our sample.

[26] In the Supplemental Appendix, we also investigate whether our results are driven by arrests of adults versus juveniles. As shown in Supplemental Appendix Table A7, the decline in arrest rates in the sheriff sample is driven by adult arrest rates.

[27] Sheriffs' term lengths are typically two or four years. Using this information, we have built a county average incumbency rate over each presidential term, as to obtain a balanced set of counties throughout the entire period (1956–1980). Since in Mississippi, sheriffs faced a one-term limit until 1972, which was removed thereafter, we dropped this state from the analysis. See the Supplemental Appendix for more details.

Table 4—OLS Models. Dependent Variable: Change in Black Office Holding (1960–1981)

	Police sample		Sheriff sample		Sheriff sample: standardized mean difference		Sheriff sample: nonstandardized mean difference	
	Municipal council	Judicial	Commissioners	Judicial	Commissioners	Black arrests	Commissioners	Black arrests
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Percent Black, 1960 × coverage	−0.017	0.010	0.145	−0.003	0.173	−0.021	0.092	−0.082
	(0.074)	(0.013)	(0.064)	(0.006)	(0.053)	(0.019)	(0.170)	(0.018)
Adjusted R^2	0.40	0.07	0.41	0.32	0.50	0.01	0.20	0.26
Observations	354	354	246	246	164	164	82	82

Notes: Robust standard errors clustered by judicial divisions in parentheses. In all our specifications, we include the full set of controls of columns 7 and 8 of Table 1: their interaction with coverage status and state trends.

Using this measure, we deploy the same triple difference specification as our baseline analysis to carry out an event study. Once again, our key explanatory variable is the interaction between coverage status and percent Black in the country. As shown in Supplemental Appendix Figure A7, which plots the coefficients for the entire South (left panel) and for the UCR sample of reporting sheriff agencies (right panel), covered counties with larger shares of African Americans do not experience any significant change over time in sheriffs incumbency. This finding is consistent with White sheriffs changing their behavior after the passage of the VRA. This could be driven by a change in their leadership style, affecting officers' behavior, or by a change in their hiring practices, resulting in a police force that better reflects the racial composition of the underlying population (see Donohue and Levitt 2001; McCrary 2007; Bulman 2019; and Ba et al. 2021). In the first four columns of Supplemental Appendix Table A8, we investigate this question both in the short run (columns 1 and 2), covering 1959–1969, and in the long run (columns 3 and 4), covering 1959–1987. The dependent variable in all cases is the change in the number of Black police officers per capita. Our key finding is that covered counties with larger shares of African Americans do not experience a different pattern in the racial composition of the police force.[28] This evidence is consistent with the idea that the change in the racial patterns of arrests is primarily driven by changes in police practices implemented by CLEOs elected after the VRA.

Even if the VRA did not lead to Black CLEOs, other Black elected officials in local jurisdictions might play a role in developing policing guidelines and practices, which, in turn, might affect arrest rates. We next investigate the role played by the race of other elected local officials, above and beyond CLEOs. Using data on the share of Black officials elected to county commissions, municipal governments, and judicial bodies between 1962 and 1981, we find, as shown in Table 4, some evidence of a larger increase in the share of Black elected officials at the local level within covered jurisdictions compared to noncovered ones. In particular, we find statistically significant increases in the share of Black commissioners (column 1) at the

[28] This finding is in line with the low turnover and high tenure of officers documented by McCrary (2007).

county level, and these effects are large in magnitude. There is no corresponding increase in either the share of Black judges, in either sample, or the share of Black elected officials in municipal governments.

Given the increase in Black county commissioners in covered jurisdictions with a larger share of African Americans, the estimated reduction in Black arrest rates in our baseline results could be due to this change in Black representation. To explore whether this is the case, we next exploit an additional source of variation, based upon whether local officials were elected by a plurality rule in a unique district covering the entire jurisdiction ("at-large elections") or in multiple districts ("district system"). District-based systems have been shown to be more favorable toward minorities, as documented in Trebbi, Aghion, and Alesina (2008) and Bernini, Facchini, and Testa (2023). As shown in columns 5 and 7 of Table 4, the increase in the share of African American commissioners documented above is driven entirely by counties with district-based elections. However, those counties do not experience a decline in Black arrests (columns 6 and 8), suggesting that our baseline results are not driven by newly elected Black officials.[29]

Overall, our analysis implies that the VRA led to an improvement in the policing practices of White sheriffs. This is consistent with both historical accounts of White sheriffs carrying out baseless arrests for minor crimes to enforce the hierarchy of race in the pre-VRA period (US Commission on Civil Rights 1961), and with a change in their law enforcement culture following the passage of the act (Peirce 1974).[30]

V. Alternative Explanations

In this section, we explore several alternative explanations for the documented reduction in Black arrest rates in covered jurisdictions. First, the ability of CLEOs to change police practices and to discipline their workforce following misconduct might be constrained by the presence of unions, which are more relevant for police chiefs than for sheriffs (Zoorob 2022). As a result, unionization might drive the observed pattern in arrests rates. Note first that trade unions have been historically weak in the South and, before the passage of the VRA, police bargaining rights in this area of the country were severely restricted.[31] Thus, it is unlikely that the effect of elected CLEOs we have uncovered could be driven by patterns of union bargaining power rather than electoral accountability. Nevertheless, in the last two columns

[29] The fact that Black arrests decline more in covered jurisdictions with nondistrict based elections is consistent with the findings of Bernini et al. (forthcoming), documenting White backlash when African Americans are elected into office.

[30] The key role played by CLEOs has been documented by Hailes and Manalili (2000, 30), highlighting that police officers' performance of their duties in the field are " … heavily influenced by the leadership of their department … When incidents of brutality, misconduct or racism occur, the chiefs immediate reaction to these incidents will have a great impact on whether the incident will be repeated in the future." In the same report, the results of a survey of randomly selected law enforcement officers carried out by the police foundation indicate that "approximately 85 percent of the respondents agreed or strongly agreed that a police chief's strong position against the abuse of authority can make a big difference in deterring officers from abusing their authority."

[31] None of the former Confederate states had any collective bargaining laws for police in place pre-VRA, and even after 1965, the South continued to lag behind the rest of the country. In fact, as of 1978, only two states—Florida and Louisiana—had introduced state collective bargaining laws for police forces (Ichniowsky 1982).

of Supplemental Appendix Table A8, we explore this possibility by exploiting information from the 1987 LEMAS survey on agencies covered by collective bargaining agreements to build an indicator of unionization. The latter can be interpreted as a change, given that in the pre-VRA period, the number of such agreements in the former Confederate states is effectively zero. As shown, there is no differential pattern in collective bargaining between the treatment and control groups, and this is true for both the police and the sheriff sample. Hence, while police trade unionism might well play a role elsewhere in the country or in later years, we can rule it out as an important factor in the South during our sample period.

The second alternative explanation involves other changes in the Black experience brought about by the VRA. Improvements in Black education or labor market conditions induced by the VRA, for example, could lead to changes in the propensity to commit crimes for reasons unrelated to policing patterns. This could then mechanically change arrest rates of African Americans for reasons related to the VRA but by a different mechanism, involving changes in the supply of crime.[32]

We attempt to address this second alternative explanation in two ways. First, as documented in Section III, our event-study analysis demonstrates that the reduction in Black arrests in places with a large share of Black voters first appeared in the five years after the implementation of the VRA. By contrast, improvements in Black education and subsequent labor-market outcomes when transitioning into adulthood took many years to materialize.[33] Second, regarding the supply of crime, we use the data on reported crimes (overall, property, and violent crime) to carry out an event study with annual frequency. As summarized in Supplemental Appendix Figure A8, there is no difference in the gradient between covered and noncovered counties for the entire sample or in each of the sheriff and police subsamples. The same holds true when we focus on long-run differences (e.g., between 1960 and 1980) for different types of crime in the first two columns of Table 5.[34] While these measures are not race specific, they document no differential changes in crime in treated areas—that is, those covered by the VRA and with a large share of Black voters. While one might naturally expect that a reduction in arrests should provide incentives for more crime, note that police brutality during the pre-VRA era was often designed to suppress African Americans, rather than to reduce crime, as argued earlier.

Next, we examine whether our findings could be driven by systematic differences between the sheriff and police samples by analyzing changes in the Black experience following the VRA along three dimensions: changes in migration patterns, improvements in education, and improvements in labor market conditions. Regarding

[32] More formally, the probability of arrest can be written as $\Pr(arrest) = \Pr(arrest|crime)\Pr(crime)$. We are interested in the effect of the VRA on $\Pr(arrest|crime)$ but can only measure $\Pr(arrest)$. Thus, any corresponding changes in $\Pr(crime)$ could potentially bias our results. Related to this, to the extent that police officers engage in statistical discrimination, policing patterns might respond to changes in the propensity of African Americans to commit crimes. This statistical discrimination could in turn lead to changes in Black arrests rates, again for reasons related to the VRA but via a different mechanism.

[33] In the first five years following the introduction of the Title I Elementary and Secondary Education Act of 1965, school funding continued to disproportionately benefit White schools, leading to a reduction of school dropout rates for White but not for Black pupils (Cascio, Gordon, and Reber 2013).

[34] Note that our results reflect reported crimes. The null effects found here could be driven by two opposing forces: higher trust in the police, leading to higher reported crime rates, and lower crime due to improved policing.

TABLE 5—OLS MODELS. POST-VRA TRENDS (1960–1981)

	Property crime (1)	Violent crime (2)	Migration (3)	Black migration (4)	White migration (5)
Sheriff sample					
Percent Black, 1960 × coverage	−0.012	−0.011	0.006	0.019	0.004
	(0.013)	(0.016)	(0.003)	(0.008)	(0.003)
Adjusted R^2	0.24	0.10	0.41	0.31	0.35
Observations	201	190	266	254	266
Police sample					
Percent Black, 1960 × coverage	−0.009	−0.019	0.005	0.021	0.006
	(0.010)	(0.015)	(0.003)	(0.005)	(0.003)
Adjusted R^2	0.05	0.11	0.44	0.34	0.38
Observations	304	270	364	360	364

Notes: Robust standard errors in parentheses. In all our specifications, we include the full set of controls of columns 7 and 8 of Table 1: their interaction with coverage status and state trends.

migration, as shown in columns 3–5 of Table 5, covered counties experienced a significant increase in population for both African Americans and Whites, although the inflow of Blacks compared to Whites is more pronounced in the sheriff sample. This pure population change cannot explain our results, however, since we use time-varying and race-specific measures of population when measuring crime rates. Nevertheless, there could be a change in the composition of the African American population, especially given that our sample period overlaps with the second wave of the Great Migration, which ended around 1970 (Calderon, Fouka, and Tabellini 2023; Derenoncourt 2022). If highly educated African Americans in covered counties with a large share of Black voters were less likely to move North, due to other changes brought about by the VRA, this could lead to a relative reduction in Black arrest rates in these places. To tackle this issue, in Table 6, we investigate patterns of sociodemographic outcomes that are likely to be related to the propensity to commit crimes (i.e., educational attainment, unemployment, and poverty). Our analysis indicates the absence of differential effects between covered and uncovered counties both in the sheriff and in the police samples.[35] This is true when we consider characteristics of the entire population (columns 1–3) and those of African Americans alone (columns 4–6), which are separately reported by the census for counties with more than 1,000 African Americans.[36]

[35] Even if educational attainment does not change, in the long run, covered counties have experienced an increase in school funding and school quality (Cascio and Washington 2014). To address this point, in Supplemental Appendix Figure A9, we examine the change in education spending and we find that coverage leads to an overall increase, but no statistically significant differential change between counties for which we have data for only police departments and counties for which we only have data for sheriff departments. Given that our results are driven by sheriff departments, these patterns of school spending cannot explain our baseline results.

[36] While these results suggest that our baseline findings with respect to arrests are not driven by changes in labor market conditions, it of course remains possible that the VRA brought about changes in labor market outcomes for African Americans, as documented in Aneja and Avenancio-Leon (2019), focusing on wages. Even if labor market conditions improved for African Americans in covered counties with a large share of newly enfranchised Black voters, this cannot explain our differential findings for elected sheriffs versus appointed police chiefs.

TABLE 6—OLS MODELS. POST-VRA TRENDS, BY RACE (1960–1981)

	All counties			Counties with # Blacks ≥ 1,000		
	Unskilled	Unemployed	Poor	Black unskilled	Black unemployed	Black poor
	(1)	(2)	(3)	(4)	(5)	(6)
Sheriff sample						
Percent Black, 1960 × coverage	0.024	−0.002	−0.028	−0.008	−0.008	0.120
	(0.050)	(0.021)	(0.043)	(0.133)	(0.066)	(0.155)
Adjusted R^2	0.35	0.45	0.82	0.34	0.35	0.45
Observations	278	278	278	169	172	165
Police sample						
Percent Black, 1960 × coverage	0.061	0.007	0.023	0.053	−0.024	0.147
	(0.043)	(0.018)	(0.042)	(0.073)	(0.045)	(0.115)
Adjusted R^2	0.36	0.53	0.82	0.42	0.29	0.57
Observations	368	369	369	306	304	298

Notes: Robust standard errors in parentheses. In all our specifications, we include the full set of controls of columns 7 and 8 of Table 1: their interaction with coverage status and state trends.

Third, even in the absence of changes in the Black population, the supply of crime could vary in response to changes in policing practices. In particular, to the extent that crime responds to enforcement practices, we might expect an increase in the supply of Black crime in response to a reduction in mistreatment of African Americans by police. Thus, if anything, changes in the supply of crime via this mechanism should work against our identification strategy, leading us to understate the reduction in Black arrests associated with better treatment of African Americans by police in covered areas following the VRA.

Fourth, our results could be driven by elevated levels of Black arrests associated with protests. In particular, during the pre-VRA era, there were widespread reports of arrests of African American protesters, perhaps inflating Black arrest rates during our pre-period in covered areas and contributing to our documented reduction in Black arrests following the VRA. We have attempted to address this issue by controlling for Black activism at the county level and interacting this measure with coverage status. In addition, our results are driven by arrests in the sheriff sample, and, if anything, protests were concentrated in more urban areas—for example, those under the jurisdiction of municipal police chiefs. Thus, arrests of Black protesters during the pre-VRA era also cannot explain our key findings.

VI. Conclusion

In this paper, we investigate the effect of the enfranchisement of Black voters on police practices, as captured by race-specific arrest rates. We document that, following the VRA, Black arrest rates fell in areas both covered by the legislation and with a large number of newly enfranchised African Americans. We find no corresponding patterns for White arrest rates. These results are due to less serious offenses, for which police have more discretion in arrest decisions. Thus, Black enfranchisement brought about by the VRA led to an improvement in the police treatment of minorities, helping to address the Civil Rights Commission's (1961)

concerns about baseless arrests for minor crimes driven by the desire to subjugate African Americans. Importantly, we also find that the decline in Black arrest rates is driven by sheriffs, who are universally elected in our sample. We do not find corresponding differences on the whole for arrests by police chiefs, who are overwhelmingly appointed, yielding credence to our hypothesized electoral mechanism. Taken together, our results indicate that, despite the possibility that White elites might have resisted Black empowerment using incarceration as a tool to disenfranchise African Americans (Alexander 2010; Eubank and Fresh 2022), the VRA led to improved treatment by police in the fifteen years following its enactment, changing law enforcement culture and practices, but only when CLEOs are elected, rather than appointed.

While historical in nature, these findings have significant policy implications today, especially given the ongoing national debates over both race and policing and race and voting. On the one hand, although African Americans continue to be disproportionately targeted by law enforcement, our results indicate that having locally elected CLEOs affects accountability and improves the treatment of minority groups by police. On the other hand, since the franchise matters, should recently enacted changes in the cost of voting, such as voter ID laws, have disproportionate effects on Black voters, this might have adverse effects on the treatment of minorities by police. Taken together, these results emphasize the important link between the administration of justice and the democratic process.

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