# The Econometric Effects of the Voting Rights Act in the U.S. South

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Department of Economics and Merton College
University of Oxford

*A thesis submitted for the degree of*
*Doctor of Philosophy in Economics*

Michaelmas Term 2020

## Abstracts

**Chapter 1**
*The Voice of Radio in the Battle for Equal Rights: Evidence from the U.S. South*
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*JEL classification:* J15, N92, L82, D74
*Keywords:* Minority Rights, Media, Conflict, Enfranchisement

**Chapter 2**
*Race, Representation, and Local Governments in the U.S. South: The Effect of the Voting Rights Act*
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*JEL classification:* D72, H7, J15, N92
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Dedicated to all those who,
in spite of pushbacks,
keep moving forward

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**Disclaimer:** The views expressed in this thesis are solely those of the authors and should not in any way be interpreted as reflecting those of the authors' employers, organizations, committees, other groups or individuals. The authors alone are responsible for any possible errors.

*Andrea Bernini*
Oxford, March 4, 2021

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# Introduction

Since its passage in 1965, the Voting Rights Act (VRA) marked a watershed moment for the advancement of both civil and voting rights of African Americans. In the most disenfranchised counties in the U.S. South, the VRA facilitated the registration of African Americans by supplanting the authority of local registrars. Thus, the Act preceded a period of drastic changes in race relations, with episodes of violence, progress, and inclusion registered across southern counties: patterns with long-lasting and heartfelt impacts even today.

This thesis is comprised of four chapters sharing an interest in the VRA, although each study remains self-standing in terms of its research question, data, and methodology. Different aspects of the Act are analyzed, from social unrest to voter registration, from representation to electoral rules, party support, and public spending. These county-level analyses are all made possible through an extensive data collection effort: a result of thorough consultations across a broad range of archives, from Harvard University to Clark Atlanta University. By providing a robust and consistent view on the most important aspects of the VRA, the overall purpose of this thesis is to contribute to the quantitative assessment of – as President Lyndon B. Johnson bluntly put it – "one of the most monumental laws in the entire history of American freedom."

**Chapter 1** sets the scene for later analyses by describing the social background over which the VRA took place. Profoundly shaken by the civil rights movement of the 1960s, southern counties suddenly experienced a large number of violent and destructive social disorders. With well over 180 riots occuring between 1964 and 1971, these disorders, which almost exclusively took place within African American communities, profoundly affected the socio-economic outcomes of African Americans for many decades to come.

Then, this chapter builds on existing anecdotal evidence to provide the first quantitative assessment of the role of the media in driving both the frequency and the severity of these social disorders across southern counties. In particular, using both cross-sectional and panel datasets, a single, well-identified driver is found to be a highly significant predictor for both the extensive and intensive margins of riots. Radio stations with a programming designed specifically for African American audiences (known as Black-appeal radio stations), are shown to be the channel through which the knowledge of random sparks reached wide enough audiences for riots to emerge. Through their active and ingrained engagement across local African American communities, and for their ability to coordinate subsequent protests and demonstrations, these radio stations also played a key role in affecting the severity of these events.

To quantify the role played by Black-appeal radio stations, a novel and comprehensive list of all stations active in both 1964 and 1968 is collected, along with information on the location, power, and frequency of each antenna. These variables, in conjunction with geographic information system applications, are then used as inputs in a model of electromagnetic signal propagation to estimate the transmission of radio waves across the U.S. South. In the preferred specification, a marginal (1%) increase in the share of the county receiving the signal from a Black-appeal radio station is estimated to lead to a 7% and 15% rise in the mean levels of the extensive and intensive margins of riots, respectively. Several mechanisms behind this result are considered, with the quantity, quality, and the length of exposure to radio programming all being decisive factors.

**Chapter 2** starts from the observation that, while race remains one of the most debated issues in American politics, we still have limited evidence on whether, when, and how did the arguably most important piece of civil rights legislation enacted by U.S. Congress led to gains in African American office-holding. On top of this, evidence on the role that locally elected African American officials had in the domain of policy remains to date essentially anecdotal. This chapter

fills the gap in the empirical literature by setting forth causal evidence on both of these important questions.

In order to evaluate the impact that the VRA had on the racial composition of southern governments, a list of all African American elected officials serving on county governments, municipal governing bodies, and school district boards is assembled for the period between 1962 and 1980. The main identification strategy hinges on the fact that the special measures that were introduced by the VRA in 1965 did not apply equally to all southern counties. In particular, strict federal scrutiny was imposed on the subset of counties that used a test or device restricting the right to vote and that experienced a turnout rate below 50% in the 1964 presidential election. The subset of counties that were subject to the Act had to seek federal approval of any change in voting laws and federal examiners could monitor activities in the polling place. Of the eleven states of the former Confederacy, six were fully covered by the provisions of the Act (Alabama, Georgia, Louisiana, Mississippi, South Carolina, and Virginia), one was partially covered (North Carolina), while four states were not covered (Arkansas, Florida, Tennessee, and Texas). This heterogeneity in treatment across counties holding a similar history of slavery and African American disenfranchisement forms the basis for the causal inference presented in this study.

In the chapter, the VRA is shown to have led to a higher African American representation at the county-level, and it did so soon after its introduction. Importantly, these gains were not limited to minor offices, but they extended to powerful elective bodies, including county commissions, which hold the duty to collect taxes and make spending decisions. This result is found to be strongly dependent on electoral rules, as counties with pre-existing systems more favorable to the election of minorities experienced earlier gains in African American representation.

This chapter further provides systematic evidence of an improvement in local infrastructure following the election of African Americans. Counties electing African American commissioners spent significantly more on capital projects, with this increase being financed predominantly through borrowing and, to some extent,

through intergovernmental transfers. While counties electing more African American commissioners benefited from increased local public goods provision, they did not receive larger individual transfers.

**Chapter 3** assesses whether the 2013 abolition of the VRA is justified based on evidence of African American office-holding gains. With a narrow 5-to-4 vote, the U.S. Supreme Court concurred that the key provisions of the Act were no longer needed, because of the very success of the VRA in eradicating discrimination in counties. The four dissenting Justices expressed a radically different opinion, reiterating that discrimination was not yet extirpated. And although there is no unique metric for evaluating the success of the VRA, this chapter considers an important one: the election of African Americans into office. To speak to this controversy, evidence is collected on all the African American officials serving across county commissions, local judiciary, and law enforcement bodies. In this chapter, the causal effect of the Act on the election of African Americans within the heart of southern county governments is estimated, and the progress in descriptive representation within a quarter century from the VRA's enactment is assessed.

The VRA is found to have causally increased African American representation across all county-level offices. Yet, the journey toward elective bodies proved to be long and arduous, for numerous obstacles remained to the election of African American officials: suffice it to say that, in the year immediately prior to the passage of the VRA, fewer than 60 African Americans were in office across all southern states. In fact, the positive impact of the VRA is found to be mediated by other important factors, such as pre-VRA educational attainment, political organizations, white supremacy, and electoral rules.

Furthermore, assessing the extensive margin of African American office-holding unfolds a strikingly uneven pattern. As of 1990, half of all the counties that were subject to the VRA did elect at least one African American official, compared with less than 20% of the set of southern counties that were not subject to the Act. However, the ability of African Americans to gain representation varied substantially across offices. African American commissioners were in office in 80%

of majority-Black covered counties, yet only 15% of them had an African American sheriff or police chief. African American judges were even less frequent, with 95% of majority-Black counties not electing a single one.

Over the quarter century following the passage of the VRA, these findings highlight that, notwithstanding a clear progress, the journey of African Americans toward elective offices has remained paved with obstacles. Further, they confirm that the 1965 federal intervention has remained vital to overcome long-lasting obstacles to African American representation. With the Supreme Court striking down the heart of the VRA in 2013, discriminatory and disenfranchising policies have once again emerged in the political debate, risking to overturn the progress made so far.

**Chapter 4** presents the first quantitative assessment of the primary goal of the Act: to facilitate the registration of African Americans, by imposing federal scrutiny over the authority of local registrars. In line with Section 2 of the Act, which stated that "no voting qualification or prerequisite to voting, or standard, practice, or procedure shall be imposed or applied by any State or political subdivision to deny or abridge the right of any citizen of the United States to vote on account of race or color," the VRA put in place special measures reducing the ability of sub-federal entities to circumvent the Fifteenth Amendment. Surprisingly, systematic evidence on its direct effect on African Americans' registration and political participation has remained limited. The lack of a robust evaluation is even more striking as the VRA is anecdotally the culprit of both a wide counter-mobilization effort by white voters and the loss of southern support for the Democratic Party observed across presidential elections since Richard Nixon's victory in 1968.

This chapter makes progress on these important questions by carrying out the first quantitative analysis of the VRA's impact on the registration rates of both African American and white voters and on their support for the Democratic Party. To measure the African American mobilization and the white counter-mobilization, a novel dataset on voter registration statistics, by race and at the county-level, is assembled for ten southern states between 1956 and 1988. At this level of granularity,

this dataset constitutes the most complete set of information on voter registration rates for the U.S. South during this important period.

By pre-empting both state and local authorities over any voter qualifications, the Act causally led to a sudden jump in the registration rates of African Americans, and it did so soon after its introduction. The Act also had a long-lasting effect, as its impact is estimated to be strongly present even twenty years after its passage: by 1984, a 10% rise in the 1960 share of African Americans in the county population led to a 3.3% increase in African American registration rates, and this is on top of the 4.5% increase observed across counties not covered by the VRA. As African Americans began entering the political process, a significant white backlash component is also estimated: whites responded by registering more, especially in the set of counties where the threat of more African American office-holding was perceived to be the highest. Due to this substantial white counter-mobilization effort, the Act has not had a significant effect on the gap in the registration rates between African American and white voters.

Total turnout rates also rose significantly following the VRA. The impact on turnout means that the Act had a significant role in shaping the two-party competition that ensued in the U.S. South in the second half of the twentieth century. In counties covered by the VRA, the drop in the white affiliation with the Democratic Party is estimated to grow over time. On the other hand, and in line with anecdotal evidence, southern African Americans have always robustly voted in support for the Democratic Party.

The following four chapters include the analyses outlined above. These are presented as self-contained journal-style articles.

*The masses of African Americans who have been deprived of educational and economic opportunity are almost totally dependent on radio as their means of relating to the society at large. They do not read the newspapers . . . . Television speaks not to their needs, but to upper middle class America. One need only recall the Watts tragedy and the quick adaptation of the "Burn Baby Burn" slogan to illustrate the pervasive influence of the radio announcer on the community.*

— Martin Luther King Jr., *keynote address, National Association of Radio and Television Announcers Convention*, Atlanta, September 28, 1967

# 1

# The Voice of Radio in the Battle for Equal Rights: Evidence from the U.S. South

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## 1.1 Introduction

During the eight-year period between 1964 and 1971, 752 race riots occurred across the United States. These episodes of social unrest led to 228 deaths, almost 70,000 arrests, and 16,000 episodes of arson and other destructive events.[1] For their frequency and destructiveness, the 1960s riots have been studied in depth by both policy-makers and scholars alike and, mainly due to their apparent unpredictability, explaining the causes of such riots has been the primary focus of analysis. Notwithstanding the fact that numerous theoretical and empirical explanations have been put forward in this literature over the past fifty years, no consensus has yet emerged on what drove the race riots of the 1960s.

In this chapter, I build on existing anecdotal evidence to provide the first quantitative assessment of the role that the media has played in driving both the frequency and the severity of the race riots. In particular, using both cross-sectional and panel datasets, a single, well-identified driver is estimated to be a highly significant predictor for both the insurgence and the severity of riots: southern radio stations airing Black-appeal programming, through their active and ingrained engagement in local African American communities, are the channel through which knowledge of random sparks happening within the county's boundaries reached a wide enough audience for riots to emerge. For their ability to coordinate subsequent protests and demonstrations, these radio stations also played a key role in affecting the conflicts' overall destructiveness.

To quantify the role played by Black-appeal radio stations, a novel and comprehensive list of all stations active in 1964 is collected, along with information on the location, power, and frequency of each antenna. These variables, in conjunction with geographic information system applications, are then used as inputs in a model of electromagnetic signal propagation to estimate the transmission of radio waves across the U.S. South. In the main analysis, the exogeneity of the treatment variable is validated by showing that the variation in the reception of the radio signal is

[1] In the U.S. South alone, during 465 days of riots spread across 189 individual events, 30 people lost their lives, while 2,290 got injured, and 8,980 were arrested.

uncorrelated to all those county-level characteristics that might have had a significant effect on riots. Subsequently, the reception of the signal from a Black-appeal radio station is found to be a strong and robust predictor of both more frequent and more severe disturbances. Importantly, this effect remains robust to the inclusion of a wide array of controls, and to the use of either propensity score analysis, regression discontinuity design, instrumental variables specifications, and panel techniques.[2] In the preferred specification, a marginal (1%) increase in the share of the county receiving the signal from a Black-appeal radio station is estimated to lead to a 7% and 15% rise in the mean levels of the likelihood and intensity of riots, respectively.

To shed light on the underlying mechanisms at play, information is also collected on both the type of programming aired by each station and on the share of Black-appeal programming in the weekly schedule. Furthermore, by leveraging on the fact that most of these stations tended to be relatively small and poorly funded, and were thus active for only a handful of years, the list of all Black-appeal radio stations is also assembled for 1968. Firstly, the results are driven by the subset of radio stations that devoted a larger fraction of their weekly schedule to airing Black-appeal programs: the quantity of programming mattered. Secondly, even within the subset of stations exclusively airing Black-appeal programs, only the stations that aired more politically charged content had a meaningful impact on the emergence of riots: the content of programming mattered. Lastly, by exploiting the idiosyncratic survival rate of radio stations, it emerges that only those stations that stayed in operation for longer had a robust effect on the likelihood and severity of riots, while those stations that either quickly shut down or only subsequently opened up had no meaningful impact: the resilience of radio stations mattered.

The contributions of this chapter are three-fold, and span several fields of research. First, by demonstrating that Black-appeal radio stations were important actors in developing ethnic conflicts, this chapter provides a direct empirical application to the theoretical argument stating that individuals prefer to obtain like-minded

[2]Instrumental variables specifications are based on the two-stage least squares (2SLS) regression analysis, while panel techniques on the generalized difference-in-differences – two-way fixed effects – framework.

## 1.1. Introduction

information from media, and that these biased media outlets can exert a non-trivial persuasion effect (Knight and Tribin 2018). In fact, while there exists an extensive literature on the effect played by either the television or the print media, relatively less is known on the impact of radio in affecting people's attitudes toward social and political issues.[3] Among the existing studies finding a robust effect of radio on political participation, Yanagizawa-Drott (2014) provides evidence that historical variation in the hate-radio station RTLM contributed to the Rwandan genocide, by leading to persuasion and imitation by neighboring communities. In post-genocide Rwanda, Blouin and Mukand (2019) instead find that exposure to the government Radio Rwanda leads to higher inter-ethnic trust and cooperation as well as lower ethnic salience. In the context of the Nazi-fascist occupation of Italy between 1943 and 1945, Gagliarducci et al. (2019) show that BBC counter-propaganda, through Radio Londra, significantly affected the intensity of the resistance movement against the Nazi occupation and fascist regime, while Adena et al. (2015) find a strongly positive effect of the Nazi radio propaganda on the popular support to the Nazi regime. Exploiting a similar estimation strategy to the one used in this chapter (based on the variation in radio signal strength resulting from topographic factors), Wang (2020) measures the extent to which charismatic individuals exploit the media to shape political outcomes by focusing on the impact of the first populist radio personality in American history: Father Charles Coughlin.[4]

Although all these studies point to the significant persuasion exerted by radio, they do so by analyzing one single radio station at a time. This chapter adds a layer of complexity by considering the effect of all those radio stations that aired at least 12 hours of Black-appeal programming a week: 200 stations in 1964 and 157 additional ones in 1968. The variation stemming from this large set of stations

[3]Knight and Tribin (2019), Knight and Tribin (2018), Snyder and Strömberg (2010), Gentzkow and Shapiro (2010), and Puglisi and Snyder (2015) all analyze the impact of television or the print media.

[4]Olken (2009) is the first study to assess the effect of heterogeneity of signal transmission on economic outcomes, finding that the Indonesian radio and television both lowered participation in social organizations and self-reported trust. Garcia-Arenas (2016) documents that free media, in the form of an American radio broadcasting in Russia, can play an important role in the political processes of a regime change, while DellaVigna et al. (2014) present evidence on the effect of Serbian public radio on Croatian nationalism.

can be exploited to more precisely pin down the persuasion effect of radio. In particular, in the context under consideration, Black-appeal radio stations could be expected to have exerted a significant effect on riots, by coordinating and pooling local African American communities, when: i) radio stations are more ingrained in the local community (proxied by the share of airtime devoted to Black-appeal programming or by the length of operation of radio stations); and ii) radio stations more openly discuss political topics in their programs. This chapter finds support to both of these hypotheses.

Second, this chapter sheds light on over fifty years of debate seeking to isolate the common drivers of the 1960s race riots. Since the early work by Spilerman (1970, 1971, 1972, and 1976), a wide array of theoretical explanations for both the frequency and destructiveness of riots have been put forward and empirically tested (Bryan 1979, McPhail and Wohlstein 1983, and McPhail 1994). Although these lay across a wide spectrum ranging from structural strain arguments (Smelser 1962) to absolute and relative deprivation (Downes 1968 and Gurr 1968), to lack of political representation (Lieberson and Silverman 1965), only two variables were ever found to satisfactorily explain most of the variation in riots observed across cities: the size of the non-white population and a regional dummy indicator. That is, except for these two variables, both the occurrence and the severity of riots remain uncorrelated to all other indicators of city-level heterogeneity. More recently, starting with the observation that upheavals tend to cluster geographically and temporally, diffusion models assess whether riots are indeed more likely as a result of contagion from similar events, by exploiting functional forms for both spatial and temporal proximity (Myers 1997, 2010).[5] Through quantifying the role exerted by Black-appeal radio stations in channeling the information of single sparks and in coordinating protests, this chapter lends support to this diffusion argument. It does so by directly measuring the relevance of such communication networks through

[5]Instead of diffusion models, Olzak and Shanahan (1996) and Myers (1997) have considered competition models: by focusing on differences in the labor market among cities, they try to explain race riots as a result of changes in the level of competition for jobs. In this chapter, the share of African Americans in the county population, state fixed effects, and several indicators of labor market tightness are included in all specifications.

## 1.1. Introduction

testing the role of radio stations on the emergence and severity of riots. Riots ought not to be treated as unpredictable and random events, for the nexus between isolated sparks and full-blown riots is provided by the presence of Black-appeal radio stations. Analyzing the local media is important, as it further controls for the fact that most of the 1960s riots tended to be relatively small events that likely did not receive neither national nor regional media coverage.

Third, this chapter contributes to the literature studying the social and economic consequences of the 1960s race riots in the short, medium, and long-run. With riots taking place mostly in African American neighborhoods, Collins and Margo (2007) and Collins and Smith (2007) find that these civil disturbances led to a statistically significant and large fall in the value of African American property. The 1960s riots are also associated with long-term changes in the ethnic composition of cities, through their role in the *white flight* observed across urban hubs (Boustan 2010 and Collins and Margo 2007), and in worsening the labor market outcomes of African Americans (Collins and Margo 2004).[6] For a lack of an identifiable common driver, all these studies have resulted in treating race riots as if generating from a black-box when, however, these ought not to be treated as unpredictable or idiosyncratic events. Augmenting these ex-post evaluations with the mechanisms linking single sparks to full-blown civil disturbances might lead to additional insights, that are specific to those communities with access to these radio stations. For instance, for their ingrained presence across African American communities, Black-appeal radio stations might have had a long-lasting social and economic impact above and beyond the insurgence of riots. In communities deeply affected by riots, radio stations could have contributed to increased participation in post-riot reconstruction efforts, while reducing the salience of ethnicity and augmenting inter-ethnic trust.

The remainder of the chapter is organized as follows. Section 1.2 presents the institutional framework, with a particular emphasis on the role of the radio industry, Black-appeal radio stations, and race riots in the U.S. South. Section 1.3 describes the dataset assembled for this study, including a description of the novel

[6]Earlier attempts to quantitatively measure the economic consequences of the 1960s riots are found in Frey (1979) and Kelly and Snyder (1980).

and comprehensive list of radio stations and race riots. Section 1.4 constitutes the main body of analysis. After documenting the exogeneity of radio signals (Section 1.4.1), Section 1.4.2 analyzes the effect of radio stations on the frequency and severity of riots, with these results validated also through the use of instrumental variables in Section 1.4.3. Section 1.4.4 considers the role played by the content of the programming aired by the radio stations in driving the overall results, and panel techniques in Section 1.4.5 allow to both reinforce the main results while highlighting additional underlying mechanisms at play. Section 1.5 concludes.

## 1.2 Institutional context

The 1960s has gone down in history as a decade of radical and extreme changes, with episodes and trends that arose during that era still resonating today. Across civil and voting rights, African Americans made historic gains toward legal equality, while race riots caused persistent divisions within and between cities. The radio represented the dominant mass medium of the age, and Black-appeal radio stations started to establish themselves as the most influential communication tool among African American communities. In this section, an overview of these decisive factors is presented.

### 1.2.1 The pervasiveness of radio in the U.S.

Up until the revolution brought by the television in the early 1970s, radio broadcasting was arguably the cheapest and preferred source of entertainment across American households. As the first electronic mass medium technology, its entry was far from smooth: due to financial crises, wars, high cost of early radio sets and the need for their continuous maintenance, it was not until the 1950s that most households could afford one. In fact, if one had to pay over $1,200 (in constant 2019 dollars) to own a radio receiver in 1925 and $1,150 in 1930, $720 were instead needed in 1940 and "only" $275 in 1950 (Sterling and Kittross 2002). By 1950, 92.6% of all American households did own at least one radio receiver, and by 1960 this figure had risen to 96.3%, remaining stable at 98.6% between 1965 and 1975.

The average number of radio sets owned by each household also rose steadily, from 2.1 in 1950 to 3.7 in 1960, 4.1 in 1965, 5.1 in 1970 and 5.6 in 1975 (U.S. Bureau of the Census 1982). Thus, during the period considered in this chapter, a combination of lower costs of production, higher purchasing power, and improved technology meant that radio sets were available in almost every household: by the early 1960s, radio had successfully ended the print monopoly of mass media.

The key determinant in driving the results of this chapter is whether (and to what extent) Black-appeal radio stations were an important source of knowledge and information for African Americans during the sample period under consideration. Indeed, Black-appeal radio stations could have had an impact on the behavior of African Americans only if: i) African Americans tuned in regularly to listen to these stations, and ii) the messages aired by these Black-oriented stations were well-received by their audiences. As data to empirically prove both of these points have never been systematically collected, in the following subsection, I document the ingrained and pervasive role of Black-appeal radio stations using a broad array of surveys and anecdotal evidence.

**Black-appeal radio stations**

The first radio station to broadcast an all-Black programming schedule was WDIA-AM in Memphis, Tennessee, in 1948. Quickly thereafter, as more (white) entrepreneurs realized the potential for this market, many stations started to include an increasing amount of Black-appeal programming in their schedule, although these stations did very little in addressing controversial topics (Barlow 1999 and Halper 2014). In the 1960s, the scenario radically changed. With race relations becoming more salient, these stations began to more willingly air controversial topics, openly discussing the struggles for civil and voting rights. A larger number of (still almost exclusively white-owned) Black-appeal radio stations were set up, and more general-market (an epithet for white-oriented) radio stations began to shift their content toward Black-oriented formats (Kahlenberg 1966 and Jeter 1981).[7]

[7]This move was driven by two factors: i) advertising revenues from undifferentiated programming started to fade, as the television began to provide the same service, but with images; and ii)

But how relevant were these radio stations in shaping African Americans' attitudes across the U.S. South? First of all, during the period under investigation, radio remained the preferred mass medium among African Americans. From a survey conducted in 1961 and 1962 across twenty cities in the U.S. South, it emerged that 78.2% of African Americans did listen to the radio regularly, while a substantial share did not read any newspapers or magazines (31.0% and 54.6%, respectively).[8] This same survey was further taken by 264 African American students and, in line with expectations, the proportion of those not reading any newspapers or magazines fell to 8.0% and 6.5%, respectively, while those frequently listening to the radio increased to 83.0%. Interestingly, the survey also asked 694 white adults about their behavior in how they access the media. Of these, only 58.4% claimed that they listen to the radio regularly, about 20 (25) percentage points less than the sample of African American adults (students). White adults also read more newspapers and magazines, with only 13.0% and 33.1% not reading any. A nation-wide study conducted by the Opinion Research Corporation found that, while 71% of African Americans listened to the radio, only 57% of whites did so, with the former group also watching less television and reading fewer daily newspapers and magazines. Lastly, a Harvard Business Review study further agreed that African Americans spent most of their media time listening to the radio.[9]

Having established that radio was the most relevant source of information and entertainment within the African American community, it is important to demonstrate that, within the universe of radio stations, African Americans preferred to listen to Black-appeal radio stations and, secondly, that their thoughts and beliefs were significantly shaped by this type of stations. In this respect, a 1963 study

with African Americans' total income quadrupling from $3 billion to $12 billion between 1940 and 1950, the business industry started to selectively target this growing consumer market by advertising through Black-appeal radio stations. However, even in 1968, there are only five radio stations licensed to African Americans (Broadcasting 1972).

[8]The African American adult sample had information on 618 respondents with some coverage on all the 11 states of the former Confederacy (Matthews and Prothro 2006). It included the following counties: Elmore, Montgomery (AL); Clark, Pulaski (AK); Leon, Manatee, Sarasota (FL); Gwinnett, Jeff Davis (GA); East Carroll (LA); Walthall (MS); Forsyth, Pitt (NC); Lexington, Richland (SC); Hickman (TN); Erath, Harris, Taylor (TX); Pulaski (VA).

[9]For the last two studies, see Sponsor (1964) and The New York Times (October 11, 1964).

carried out by KZEY-AM, a Black-appeal radio station located in Beaumont, Texas, showed that 90% of African Americans who responded to the survey had a working radio in the household, and that 78% had listened to it on that given day (Sponsor 1964). Of these, 78% said that the station they listened to was a Black-appeal one, 13% a general-market one, while 9% could not recall the type of radio station that they listened to. Furthermore, 80% of the respondents said that they would most likely buy products or services if they were advertised over a Black-appeal radio station, with only 20% doing so if they were aired on television. In fact, advertising aired on Black-appeal radio stations was able to drive twice as many purchases among African Americans compared to general-market radio stations, with the latter having the same effect on persuading either a white or an African American listener.[10] African Americans were also able to recall more commercials if those were aired by Black-oriented radio stations compared to general-market radio stations, while whites could recall significantly fewer ads.

Besides surveys, there is also a significant amount of anecdotal evidence supporting the view that African Americans were more responsive to the information conveyed by Black-appeal radio outlets, compared to either general-market ones or the television. Interviewed by Sponsor (1964), Bob Dore, who owned a company representing the interests of more than three dozen Black-appeal radio stations in the U.S. South, stated that "when a Negro hears a commercial in general-market radio, he's not sure it's meant for him. When he hears one on a Negro station, he knows it's meant for him. Sure, there may come a day when there's no need for 'Negro radio' as we know it now. But that day, despite all the progress that has been made by U.S. Negroes and all the progress they will make in the near future, is still a long way off." Interviewed by The New York Times in 1964, Graeme Zimmer of Continental Broadcasting, a division of Rollins Broadcasting, Inc., which operated five Black-appeal radio stations, said that "Negro-programmed radio is

[10]This survey was conducted at the height of the civil rights movement by the Center for Research in Marketing, Peekskill, N.Y. It found that 90% of African Americans would support a product boycott if it were backed by one of the major civil rights organizations, such as the NAACP, the Urban League, CORE, SNCC, and SCLC.

the only media that reaches the Negro every day with what he wants to hear and know." Similarly, Harry Novik, general manager of the Black-appeal radio station WLIB-AM in Harlem, said that "the Negro-oriented station has a personal relationship with local Negro communities that is unknown in general radio."

All in all, the above-mentioned evidence, based on surveys and anecdotes, has indicated that, during the 1960s: i) African Americans relied on the radio as the leading source of information and entertainment; ii) within the radio industry, preference leaned strongly toward Black-appeal radio stations; and that iii) these stations had an important influence on the behavior of its audience.

### 1.2.2 The race riots of the 1960s

During the 1960s, the U.S. was shaken by violent and destructive civil outbursts. These disorders, which occurred in years already defined by historic events related to both civil and voting rights (the Civil Rights Act and the Voting Rights Act were passed in 1964 and 1965, respectively), marked a period of drastic changes in race relations across and within American cities. And although ethnic conflicts, demonstrations, and protests already emerged in the first half of the twentieth century, the concentration of the 1960s riots, both in terms of their intensity, frequency, and geographic distribution, made them entirely unique and unprecedented events.

Importantly for this study, which seeks to quantify the effect stemming from Black-appeal radio stations, the 1960s race riots emerged, almost exclusively, within African American communities. And although sociologists tend to classify the 1960s riots with the term *black aggression*, attacking whites was not the aim of these riots; rather, as identified in the Kerner Commission report, "while the civil disorders of 1967 were racial in character, they were not interracial. The 1967 disorders, as well as earlier disorders of the recent period, involved action within Negro neighborhoods against symbols of white American society – authority and property – rather than against white persons." Further, that the 1960s race riots could have been affected by radio stations so entrenched in the local community

is reinforced by the fact that the background of these disorders was characterized by heavily localized and county-specific features. From the report, these include "severely disadvantaged conditions for Negroes, especially as compared with those for whites; a local government often unresponsive to these conditions; Federal programs which had not yet reached a significantly large proportion of those in need; and the resulting reservoir of pervasive and deep grievance and frustration in the ghetto." Black-appeal radio stations are necessary, albeit not sufficient for riots to emerge. Radio stations acted as a coordination device by propagating the knowledge of the first spark, with the latter in itself remaining a random event: "At some point in the mounting tension, a further incident – in itself often routine or even trivial – became the breaking point, and the tension spilled over into violence."[11]

## 1.3 Data

This chapter estimates the persuasion effect that Black-appeal radio stations had on African American communities in the U.S. South, through their role in spurring and coordinating the extensive and the intensive margins of the 1960s race riots. To carry out this analysis, a rich novel dataset has been built, and its key elements are described below.

### 1.3.1 Black-appeal radio stations

A novel and comprehensive dataset on Black-appeal radio stations active in both 1964 and 1968 has been assembled by combining different historical sources of the media industry. To the best of my knowledge, this is the most complete set of information on Black-appeal radio stations that has ever been used in the empirical literature.

To construct the 1964 sample, the primary source is the magazine Sponsor (1964). From the early 1950s until the mid-1960s, this magazine has been the first and most reliable outlet to present detailed statistics and analyses on African Americans' role and impact in shaping the media (chiefly radio) industry, although it did so

[11] See pages 107-109 of the U.S. National Advisory Commission on Civil Disorders (1968). Chaired by Otto Kerner, it is now more commonly known as the Kerner Commission report.

for the sake, and from the eyes, of the advertising industry, which constituted the magazine's key readership. Starting in 1952, it began to publish the once-per-annum report titled *Negro market* and, by early 1960s, it further included the *Directory of major Negro-appeal radio stations in the United States*. However, it is only with the 1964 publication that this list includes all those stations with a significant amount of Black-appeal programming to be safely classified as Black-programmed radio stations. In particular, out of the close to 500 stations that are mentioned to be having some appeal to the African American community in 1964, only 200 southern stations enter this list by airing at least 12 hours of Black-appeal programming a week and, of these, 59 are reported to be 100% Black-appeal stations.[12]

The magazine Sponsor ceased to exist in 1964. Therefore, to construct the 1968 sample of Black-appeal radio stations, the 1968 directory (titled *Radio stations regularly scheduling Negro programs*) by Spot Radio Rates and Data (1968) is instead considered. Besides being published by Standard Rate & Data Service, Inc. – a traditional source of reliable market data in the media industry – this directory is the only one that, to the best of my knowledge, in these immediate post-1964 years, reports not only a list of Black-appeal radio stations but also the total number of hours airing such programs. From the 1968 directory, information on all radio stations that broadcast at least 12 hours of Black-appeal programming has been collected and digitized.[13] In 1968, there were 157 southern stations airing at least 12 hours of Black-appeal programming a week and, of these, 59 were 100% Black-appeal. And although the total number of radio stations that exclusively targeted African Americans is the same for both years in the sample, there is a

[12]For each station, information on the strength of each radio tower (in watts, *w*, or kilowatts, *kw*) and its frequency (in kilocycles per second, *kc*) is obtained. These are both important elements to construct the dispersion of the signal using the Irregular Terrain Model. In the event of gaps in the 1964 directory, supplementary information has been obtained from: i) earlier publications of Sponsor's *Negro market*; ii) the 1964 edition of the Broadcasting Yearbook; and iii) the 1964 edition of the North American Radio-TV Station Guide.

[13]Other lists of Black-appeal radio stations are included in: i) the Broadcasting Yearbook; ii) the magazine Billboard; and iii) the magazine Cash Box. However, in all of these, there is no information on the amount of airtime that is devoted to Black-appeal programming. Further, when cross-checking these directories, it resulted that many of the included stations had very little content of Black-oriented programming to justify their inclusion to the list (for more on this, see Jeter 1981).

## 1.3. Data

considerable degree of variation across counties and states over time. Table A.1.2 presents a summary of the radio stations used in this study.

### 1.3.2 The Irregular Terrain Model

For each station in the dataset, information on the location, frequency, and strength of each radio tower (200 in 1964 and 157 in 1968), have been used to construct the predicted spatial propagation of the radio signal. The model used to predict coverage is the Irregular Terrain Model (ITM), in conjunction with publicly available Shuttle Radar Topography Mission (SRTM) terrain data. To make the estimated coverage from ITM and SRTM operational for this study, three geographic information system (GIS) applications (Google Earth, ArcGIS, and QGIS) have all been used. In particular, these applications have been exploited to geocode and georeference the signal coverage of each radio station with the counties of the U.S. South.[14]

To address the role that topography has in this physical model of electromagnetic signal propagation, variables related to the surface over which the signal travels have been constructed and are also included in the analysis as controls. Furthermore, the predicted free-space (line of sight) coverage signal is included as an additional regressor in this study: for each radio station, it is the expected coverage if there were no obstacles to the propagation of the radio waves. In practice, there are a number of elements (e.g., buildings, mountains, parks, but also the air and the curvature of the earth) that make the signal decay faster. By including the free-space signal as an additional regressor, the role of topography is further taken into consideration, and the estimated effect is obtained only out of the variation in signal reception that is due to topography.

[14]In this study, the tool provided by Nautel, a manufacturer in radio broadcasting, is used to obtain the predicted coverage of each station in the sample. As a reference on the ITM, see Hufford (2002). As for the SRTM, see Farr et al. (2007). The SRTM was flown on an 11-day mission by the Space Shuttle Endeavour in February 2000. It acquired topographic data to build digital elevation models on a near-global scale.

### 1.3.3 The race riots of the 1960s

Starting with the early 1970s, sociologists have started to collect information from media outlets and government agencies in order to systematically study the riots of the 1960s. Among others, the sources exploited for these studies include the Riot Data Review compiled by the Lemberg Center for the Study of Violence at Brandeis University, the Congressional Quarterly's Civil Disorder Chronology of 1967, the Kerner Commission report of 1968, and newsclips from both the New York Times and Washington Post. As all these sources are based on either different definitions of riots, different samples or time horizons, this chapter is based on a combination of all these four different sources, by following the approach firstly carried out by Spilerman (1970), and subsequently refined by Carter (1986).[15] Spilerman's definition of a race riot being a spontaneous event, which involved at least 30 people, some of whom African Americans, resulting in either aggressive behavior, looting, or property damage, has become the standard academic definition in studying the 1960s riots. Importantly, this definition excludes all the protests which could have been resulted from (or being nonetheless directly related to) civil rights activism and protests, by considering only disturbances that occurred outside of school settings and civil rights demonstrations. In this chapter, I exploit Carter's original dataset and focus on both the extensive and the intensive margins of race riots.

As for the extensive margin, I consider the likelihood that a county experiences a riot, by focusing on the share of race riots, defined as the total number of riots in county $c$ in state $s$, standardized by the total number of riots in all southern counties:

$$Extensive\ Margin_{c,s} = 100 \cdot \left[ \frac{\sum_r^R Riot_{r,c,s}}{\sum_c \sum_r^R Riot_{r,c,s}} \right] \tag{1.1}$$

The intensive margin is instead based on a cumulative index of riot severity:

$$Intensive\ Margin_{c,s} = \sum_r^R Severity\ Index_{r,c,s} = 100 \cdot \left[ \sum_{i=1}^{5} \frac{X_{i,r,c,s}}{X_{i,T}} \right] \tag{1.2}$$

where $X_{i,r,c,s}$ is one of the five indicators of severity collected by Carter (1986), for each riot $r$ that occurred in county $c$ and state $s$. These are: i) days of rioting; ii)

[15] Carter (1986) refined the original dataset by including all race riots until 1971. Since then, Carter's refinement over Spilerman's has become the new academic standard.

number of deaths; iii) injuries; iv) arrests; and v) episodes of arson reported by police during the riot. $X_{i,T}$ is the sum of each indicator $i$ across all the riots that occurred in the U.S. South during 1964-1971 (e.g., the 2,290 injuries or the 8,980 arrests). For each riot $r$, the sum of all the five ratios (one for each indicator $i$) is then the severity index associated with that given riot: $Severity\ Index_{r,c,s}$.[16] To allow for the possibility that more than one riot $r$ could have occurred within a given county, the sum of this index across all the riots that occurred in county $c$ and state $s$ is considered to form the cumulative index of severity: $Intensive\ Margin_{c,s}$.

When measuring the impact of media on riots using a panel dimension, I augment the dataset of Carter (1986) with the information on riots collected in Olzak (2015). The latter is similarly based on Spilerman (1970), but considers all race conflicts during the period 1954-1992. Further, it differs from Carter (1986) on the source of information used (Olzak 2015 only considers the New York Times) and on the definition of what constitutes a race riot: a demonstration where at least 50 people participated, with some form of violence, and lasting for at least two hours. In this study, information on the period 1960-1963 available from Olzak (2015) is added to the one previously considered (1964-1971). Before doing so, the correspondence between the two datasets has been carefully cross-checked.[17]

### 1.3.4 Other controls and summary statistics

Summary statistics on all the variables included in this study are presented in Table A.1.1. In line with knowledge on how radio waves propagate over land and water, treated counties (i.e., those receiving the signal of Black-appeal radio stations)

[16]That is, the severity of riot $r$ is the proportion of all days of rioting that occurred during riot $r$, plus the proportion of all deaths, plus the proportion of all injuries, plus the proportion of all arrests, plus the proportion of all episodes of arson. Correspondingly, summed over all riots in the sample, $Severity\ Index_{r,c,s}$ equals 500 to reflect the five indicators of severity, as the index is also multiplied by 100. In constructing this severity index, I closely follow Collins and Margo (2004) and Collins and Margo (2007).

[17]Firstly, while using the definition found in Olzak (2015) leads to fewer and more severe riots during 1964-1971, all the results of the chapter still hold if this sample is considered. Secondly, one of the five indicators of severity (episodes of arson) is not available in Olzak (2015). Reassuringly, in the sample based on the period 1964-1971, the correlation between the episodes of arson and the remaining four other indicators of severity is relatively strong. Then, using four instead of five indicators of severity in the panel version of the intensive margin remains an inconsequential, albeit necessary, adjustment.

are smaller and have both fewer rivers and lakes, while being less mountainous. These counties have larger shares of African Americans in the total population (also measured through the share of non-white households) and are more likely to be covered by the Voting Rights Act (VRA) in 1965. They are also more populous (higher density) and have a larger manufacturing sector, along with a more ingrained presence of historically Black colleges and universities (HBCUs), and with a larger fraction of the population enrolled in school. On the other hand, the two groups are more similar along other indicators of socio-economic development, such as the unemployment rate, as well as poverty and income levels. Birth rates, the age distribution, the average number of members in the households, and median school years are similar between the two groups. Indicators for the standard of living also tend to be comparable: the share of farms with electricity, and the share of households with television sets, radio sets, and cars. Treated counties have smaller agricultural and mining sectors and have fewer farms (measured as either the land covered by farms or through an indicator of agricultural productivity). They also have less per capita expenditures on highways. Regarding indicators of ethnic conflicts pre-1964, the counts of lynchings against African Americans are similar between the two groups, while treated counties experienced more episodes of anti-Black activism, and had more local branches of both the National Association for the Advancement of Colored People (NAACP) during 1957-1964 and the Ku Klux Klan (KKK) during 1925-1940.

Some of these differences are statistically significant and large in magnitude. To address this potential endogeneity threat, which would inevitably lead to biased estimates, several techniques are considered in the rest of the chapter. Among these, propensity score methods are used to balance the sample along observable characteristics; secondly, a regression discontinuity design is deployed, comparing counties within a 15-mile radius from a radio station; and, lastly, the sample is restricted to the set of counties in which African Americans are a minority of the population. Reassuringly, the main results of the chapter remain robust in all these more demanding specifications.

## 1.3. Data

## 1.4 The voice of Black-appeal radio stations

Radio is found to have shaped the behavior of its listeners in an important way: Black-appeal radio stations, especially those more ingrained in the local communities they served, significantly led to more frequent and more severe riots. This result also depends on the length of operation of radio stations and on the amount of coverage of political events. Importantly, a unique driver of riots is uncovered in the data, and robust quantitative evidence to this claim is presented in this section.

### 1.4.1 Media and county characteristics

This study exploits signal reception of Black-appeal radio stations as an exogenous determinant of media bias through radio listenership. The identification strategy is modeled through both cross-sectional and panel frameworks, along with instrumental variables, propensity score matching techniques, and regression discontinuity designs, and it is based on the county-level variation in the signal reception of Black-appeal radio stations. The exposure to the signal received from Black-appeal radio stations is computed either as a continuous statistics (the share of the county being covered) or through a dummy indicator equal to one if the majority of the county is covered.[18]

As a first step in ensuring that the requirements for a successful identification of the effects are met, it remains to be verified that radio coverage (the independent variable in the main analysis) is unrelated to all the county-level characteristics that might also have had an effect on the dependent variables (the extensive and intensive margins of race riots). I hereby propose a specification to formally assess whether the exposure to Black-appeal radio stations is balanced with respect to a wide array of pre-period county-level controls.

Firstly, I include state fixed effects in order to control for the most persistent structural differences across states in the U.S. South. Secondly, in order to capture

[18]The coverage of the radio signal is modeled through the Irregular Terrain Model (ITM), in conjunction with publicly available Shuttle Radar Topography Mission (SRTM) terrain data and with geographic information systems (GIS), where the resulting output of electromagnetic signal propagation inherently depends on the earth surface, topographic characteristics, and the frequency and power of the radio towers.

the reliance of the ITM on geographic features, in all the specifications that follow, I consider a vector of controls that are topographic in nature. In particular, since the electromagnetic signal propagates differently over land and water, as well as deviating when hitting large obstacles (such as mountains or hills), these county-level variables include the size of lakes, the length of rivers, the overall size of the county, the presence of National Parks and of mountains, and a dummy indicator for those counties facing the sea.[19] And besides being key inputs in constructing the expansion and propagation of radio waves trough the ITM, including these topographic variables in the specifications might also help to account for all the unobserved differences in political or social organizations between, say, mountainous counties and their maritime counterparts, or between counties extensively covered by forests or lakes. This further lends support to the validity of the analysis, as these differences might also be important in how riots emerge and evolve.

Then, in order to test for the orthogonality between the radio signal reception and a range of socio-economic county characteristics, I estimate the following regression for the cross-section of southern counties:

$$\textit{Radio Signal}_{c,s} = \alpha_s + \mathbf{X}_{c,s}\gamma_1 + \mathbf{Topography}_{c,s}\gamma_2 + \epsilon_{c,s} \tag{1.3}$$

In this model, the dependent variable, $\textit{Radio Signal}_{c,s}$, measures whether county $c$ in state $s$ is exposed to a Black-appeal radio station (either through a continuous variable or as a dummy indicator of coverage), $\mathbf{Topography}_{c,s}$ is a vector including the full set of geographic controls just discussed, and $\alpha_s$ are state fixed effects. If the exposure to Black-appeal radio stations is to be balanced with respect to pre-period county characteristics, which are represented by the vector $\mathbf{X}_{c,s}$, then the vector of estimated coefficients $\gamma_1$ should be indistinguishable from zero.[20]

[19] I have constructed these topographic variables using the ArcGIS application, extracting information from the shapefiles downloaded from Natural Earth. Data on the list of coastal counties has been constructed from information available from the U.S. Census. To see the differential effect of land and water on the propagation of the radio signal, see the zoomed-in image of the coverage of two Black-appeal stations in Virginia (WBCR-AM in Christianburg and WRAP-AM in Norfolk) presented in Map (c) of Figure A.1.1.

[20] In this and all subsequent analyses, standard errors are robust and adjusted for clustering at the county-level, and the regressions are weighted by 1960 population.

Table 1.1 reports the coefficients $\gamma_1$ of Eq. 1.3 estimated through OLS. The first two columns of Table 1.1 suggest that, after controlling for state fixed effects and for the set of topographic indicators, the degree to which a southern county receives the signal from any Black-appeal radio station, measured as either a dummy indicator, as in column (1), or through a continuous variable, as in column (2), is unrelated to twenty-five of the included variables, with only one control predicting coverage. In particular, the only control reaching significance at conventional levels is the one measuring the share of households with a television handset in 1960. And if any of the included controls were to be relevant in predicting radio signal reception, television ownership is arguably the most plausible one: after controlling for all other county-level socio-economic variables, more television sets (which remained a significant investment in 1960) could be expected to be purchased in counties with better signal reception.[21]

Importantly, whether or not a county receives the radio signal from a Black-appeal radio station is not related to pre-existing measures of ethnic conflicts in the county. The $p$-value from a joint $F$-test of the lynching variable, the counts of episodes of anti-Black activism, and the location of both NAACP branches and of KKK's Klaverns is 0.72 for column (1) and 0.57 for column (2), thus indicating that the estimated coefficients on these variables are jointly indistinguishable from zero. This goes some way forward in addressing the threat of reverse causality by showing that more Black-appeal radio stations were not set up as a response to more conflicts in the first half of the twentieth century (nor in the first half of the 1960s). This, and the fact that no other pre-period control is correlated with the propagation of radio waves, in turn ought to provide support in claiming the exogeneity of radio coverage in the subsequent analyses of this chapter.

As a way to directly assess how each of the twenty-five controls affects the main dependent variables of this chapter, the specification presented in Eq. 1.3 is

[21] Taking Chicago and 1954 as the representative city and year: a television set (table model, VHF) cost $204.39 and, to repair it (replacing the transformer), it cost $23.05. A radio set (table model) cost $24.12 and a newspaper (daily edition, on the street) cost $0.05. In 2020 prices (using changes in the CPI), these correspond to about $1,954, $220, $231, and $0.50. The original prices have been reported in U.S. Bureau of Labor Statistics (1956).

again estimated, this time including as dependent variables either the extensive margin of riots, column (3) of Table 1.1, or the intensive margin, column (4). It is striking to observe the extent to which most of the controls that were not relevant in predicting coverage from the radio signal, columns (1) and (2), now reach significance at conventional levels. In particular, it is reassuring to notice that the pre-1964 measures of race conflicts seem to have a persistent effect on the dependent variables of columns (3) and (4): more lynchings of African Americans and more frequent episodes of anti-Black activism translate into fewer riots during the period 1964-1971, perhaps due to more stringent law enforcement in these counties. On the other hand, localities with a more pervasive presence of the KKK exhibit both more frequent and stronger episodes of race riots, with the presence of NAACP branches working to counteract this effect.[22]

Lastly, and in line with expectations and anecdotal evidence, more race riots are observed in those counties that are more densely populated and that have a larger fraction of African Americans in the total population. The fact that these controls are significant only with respect to race riots and not with the radio signal reception shows just how important it is to include them when seeking to quantify the impact that Black-appeal radio stations had on both the likelihood and on the intensity of race riots. Furthermore, some of these estimated coefficients move in opposite directions between columns (1) and (2) and columns (3) and (4), making their inclusion even more important.[23] Using alternative specifications, the effect of media on riots is tested throughout the rest of the chapter.

### 1.4.2 Media and race riots

In Section 1.4.1, the key independent variable of interest is shown to be orthogonal to the full set of pre-period county characteristics. This, along with the fact that many

[22]The $p$-value from a joint $F$-test of the lynching variable, the counts of episodes of anti-Black activism, the location of NAACP branches, and the location of the Klaverns of the KKK is 0.03 for column (3) and 0.00 for column (4). Consequently, the estimates of the coefficients on these variables are jointly significantly different from zero.

[23]For instance, the pre-period pro-African American activism proxied by the location of NAACP's branches is positively (albeit insignificantly) correlated with Black-appeal radio stations, while having a negative effect on the magnitude and frequency of race riots.

of these controls instead do affect race riots directly, provides support in exploring whether Black-appeal radio stations had an effect in spurring the 1960s race riots in a full specification with state fixed effects, socio-economic and topographic controls, as well as pre-1964 measures of ethnic conflicts. Correspondingly, the following regression is estimated, based on the cross-sectional differences in signal reception across southern counties:

$$Race\ Riots_{c,s} = \alpha_s + \beta_1 Radio\ Signal_{c,s} + \mathbf{X}_{c,s}\gamma_1 + \epsilon_{c,s} \tag{1.4}$$

where now $\mathbf{X}_{c,s}$ includes both the vector of pre-period county-level characteristics considered in Eq. 1.3 and also the the full set of topographic variables. $Race\ Riots_{c,s}$ represents either the extensive or the intensive margins of the 1960s race riots, while the independent variable of interest, $Radio\ Signal_{c,s}$, corresponds to either the dummy indicator or the continuous variable for the coverage of the radio signal from Black-appeal radio stations.[24]

Eq. 1.4 is estimated separately over five different samples of radio stations, according to the share of their weekly programming that is devoted to air Black-appeal content. In the analysis, the following five brackets are considered: i) all the southern stations with at least 12 hours of Black-appeal programming, from as little as 4% of Black-appeal content to 100% (200 stations); ii) all stations with at least 25% of Black-appeal content (148 stations); iii) at least 50% (121 stations); iv) at least 75% (65 stations); and v) the subset of stations exclusively airing programs that are of interest to African Americans (59 stations).[25] Estimating Eq. 1.4 separately for each bracket is motivated by the a priori expectation that, if Black-appeal radio stations mattered for the likelihood and for the severity of race riots, then this effect ought to increase with the extent of being entrenched and ingrained in the local African American communities, as proxied by the percentage of the total airtime that is devoted to programs that are of interest to African Americans.

[24]Similarly to Eq. 1.3, $\alpha_s$ are state fixed effects, while standard errors are robust and adjusted for clustering at the county-level, and the regressions are weighted by 1960 population.

[25]See Table 1.2.1 for a summary of the information collected on Black-appeal radio stations in 1964 and 1968, by state.

This is exactly what is found in Table 1.3, where the coefficient $\beta_1$ from estimating Eq. 1.4 is presented, separately for each bracket, separately for the two definitions of signal coverage (dummy and continuous indicators), and also separately for the two dependent variables of interest: the likelihood of riots, columns (1) to (5), and the intensity of riots, columns (6) to (10). The impact of all 200 stations on the extensive and intensive margins of riots is shown in columns (5) and (10), respectively. The estimated relationship between the dummy indicator of coverage associated with any Black-appeal radio station and both the extensive and intensive margins of riots is positive, although only the latter coefficient reaches (marginal) significance. Importantly, restricting the sample to those radio stations that dedicated more airtime to Black-appeal programming has a marked impact on the estimates, with coefficients that both grow in magnitude and are more precisely estimated: moving from columns (5) and (10) to columns (1) and (6), respectively.

The estimates behave in a similar fashion if radio coverage is measured through a continuous statistics, rather than through a dummy indicator. Significant coefficients on the extensive and intensive margins of riots are obtained only for the top brackets of radio stations (above 75% and 50% of Black-appeal content, respectively), and this further highlights that the persuasion effect of media is channeled through radio outlets that are more deeply rooted in the local communities. As for the magnitude of the effect, Panel B of Table 1.3 indicates that a 1% increase in the county's share that is covered by a 100% Black-appeal radio station translates into 0.6% more frequent and 6.4% more severe riots. However, as there is a substantial amount of southern counties that never experience any race riots, it is interesting to consider mean values. With the mean levels of the likelihood and intensity of riots being 0.084 and 0.413, respectively, the marginal (1%) increase in the share of the county covered by a 100% Black-appeal radio station is then found to lead to a 7% and 15% increase in the frequency and severity of riots.

As presented in Section 1.4.1, radio signal coverage is orthogonal to a wide array of pre-period controls. However, in order to further address any additional concerns on the validity of the identification strategy, a term that ought to truly isolate the

variation in the waves' propagation that is due to the role of topography alone is further included in Eq. 1.4. This indicator, $Unobstructed\ Signal_{c,s}$, measures the counterfactual signal propagation of each radio station had there not been any physical obstacles (i.e., with a direct line of sight) between each station's tower and each receiver.[26] Correspondingly, after the inclusion of $Unobstructed\ Signal_{c,s}$ as an additional regressor, the $\beta_1$ coefficient now captures the effect of topography alone, alleviating any remaining concern on the exogeneity in the signal's idiosyncrasies. And except for $Unobstructed\ Signal_{c,s}$, all terms in Eq. 1.5 reflect those found in Eq. 1.4:

$$\begin{aligned} Race\ Riots_{c,s} &= \alpha_s + \beta_1 Radio\ Signal_{c,s} + \\ &+ \beta_2 Unobstructed\ Signal_{c,s} + \mathbf{X}_{c,s}\gamma_1 + \epsilon_{c,s} \end{aligned} \tag{1.5}$$

Table 1.4 presents the estimated coefficients $\beta_1$. The impact of Black-appeal radio stations, which is now estimated only out of the variation in topography, remains strong and robust.[27] The firm similarity between the $\beta_1$ coefficients estimated using Eq. 1.4 and Eq. 1.5 would suggest that the idiosyncrasy of the topography across southern counties is key in driving the results and, consequently, this should further provide support to the exogeneity of the media variable used. This is relevant also given the fact that the vector of geographic controls (statistics for the size of land, lakes, rivers, mountains, and parks, as well as coastal and state indicators) is always included in all specifications.

These estimates consistently point to a scenario in which Black-appeal radio stations significantly mattered in the likelihood and intensity of riots, and more so as their role in the local community rose. In what follows, for their larger role in spurring civil disorders, the focus is restricted to the 100% Black-appeal radio

[26] In a similar fashion, Olken (2009) also includes the predicted would-be signal had there not been any obstacles.

[27] If anything, the $\beta_1$ coefficient is somewhat larger in the specification with the intensity of riots, leading to an 18% increase in the mean level of the dependent variable in the specification with 100% Black-appeal radio stations. The $\beta_2$ coefficient is positive in the specification with the extensive margin of riots, while it moves in the opposite direction as for the intensity of riots. In both cases the magnitudes are significantly smaller compared to their $\beta_1$ counterparts, and are also estimated with less precision, suggesting that it is indeed the variation due to topography between transmitting and receiving locations that is driving the main results of the effect of media on the 1960s race riots.

stations. As a first check of the main results, in Table 1.5, the robustness of the $\beta_1$ coefficients is assessed in a series of alternative specifications. In columns (1) and (5), Eq. 1.5 is estimated using only state fixed effects, with the exclusion of the $Unobstructed\ Signal_{c,s}$ variable, the full set of pre-period topographic and socio-economic controls, and also the four measures of pre-1964 ethnic conflicts. Across all specifications, the coefficients are highly significant and larger than those obtained from the preferred full specification of Eq. 1.5. This upward omitted variable bias is partially addressed by the inclusion of topographic controls and measures of pre-period ethnic conflicts, columns (2) and (6). Lastly, in both columns (3) and (7) – corresponding to Eq. 1.4 – and columns (4) and (8) – Eq. 1.5 – the four indicators of pre-period conflicts all play a significant and robust role. The standardized count of lynchings against African Americans, the count of anti-Black protests, and the number of local branches of the NAACP all worked against further ethnic conflicts. On the other hand, a more pervasive presence of the KKK is associated with both more frequent and more destructive civil disturbances. A possible explanation for these results is that the former three factors all contributed to improving the underlying adverse conditions and grievances that often constituted the background of disorder in the riot cities (as it emerges from the report of the Kernel Commission), while the latter worsened it. That is, the positive change that was demanded by rioters had already occurred in the cities that experienced pre-riot protests, and this likely led to increased trust between African Americans and whites and increased interracial communication, lowering the chances of future disorders. On the other hand, the presence of white extremist groups (proxied by the presence of a Klavern) increased the likelihood of riots.

Another way to check for the soundness of the main results is to consider propensity score techniques. In particular, propensity scores are constructed for both the subset of treated and untreated counties as a function of the underlying

observables.[28] As shown in Table A.2.2, regardless of which propensity score framework is considered (e.g., stratification, matching, trimming, or even a combination of stratification and trimming), the estimated coefficients maintain a remarkable degree of stability, in terms of sign, magnitude, and significance.[29] The stability of the coefficients is also observed through a regression discontinuity design, where only counties within a 15-mile radius from a radio station are considered, and also by replicating the analysis on the subset of counties in which African Americans are a minority of the population.

The balance obtained from these more demanding specifications highlights that the results presented thus far are not reliant on the heterogeneity of covariates in the sample of southern counties, and are instead picking up the effect of something more deeply rooted: the impact of Black-appeal radio stations. However, in order to address other important remaining issues (such as measurement errors in the construction of the main explanatory variable, and also unobserved or omitted factors), in Section 1.4.3 an instrumental variables approach, based on two-stage least squares, is considered.

### 1.4.3 IV regressions

Measurement errors in the radio signal's coverage could threaten the accuracy of the whole analysis.[30] In fact, this type of error, which is inherently untestable, would lead to OLS estimates on *all* included regressors to be both biased and

[28]Propensity scores allow to test for the stability of the main estimates to varying levels of balance in the data and, importantly, to check whether there are any remaining biases in the more general, and arguably less demanding and less stringent, specifications that have been presented in this section.

[29]For instance, matching treated and untreated counties using a very stringer caliper of 0.1, column (3) of Table A.2.2, which drastically lowers the observations used from 1,022 to 213, only marginally changes the estimates of the dummy indicator's effect on the severity of riots, from 4.9% to 5.5%.

[30]Measurement errors in deriving the main explanatory variable of interest could arise through three likely channels. First, the dataset on Black-appeal radio stations, constructed through the collection of several historical sources on the media industry in the 1960s could have resulted in some inconsistency when these sources were combined. Second, the individual sources themselves could have reported somewhat incorrect statistics. Intrinsically, this is due to the survey nature through which they obtained information on each radio station. Third, the ITM, although being based on a wide array of inputs, might not have precisely estimated the propagation of these radio waves. Every effort to minimize all these potential risks has been carefully followed.

inconsistent. As a first, although arguably informal, attempt to provide support to the accuracy of the radio signal variables, the areas generated through the ITM are shown to well coincide with the ones that radio stations were reporting as their actual coverage. That is, through the direct visual comparison with historical sources, the ITM is shown to be a reasonably good predictor of the actual waves' propagation. However, in order to address any remaining inaccuracy in the process generating the radio signals, or in the measurement of the inputs feeding into the ITM (such as the location of radio towers, their frequency or power), an instrumental variables (IV) method is now considered.

In addition to measurement errors, the use of IV techniques further allows to address the problem of unobservable, as well as omitted, factors. That is, although Table 1.1 showed that radio signals are orthogonal to pre-period county characteristics, latent factors might still affect both the dependent and independent variables. These correlations could then make OLS estimates unreliable and could thus constitute an additional potential cause for concern in the identification of the true effect.

Importantly, in what follows, all of the included instruments affect the proportion of counties exposed to the radio signals, while remaining uncorrelated with all those unobservable factors determining either the extensive or intensive margins of riots.[31] By being correlated with the radio signals while being uncorrelated with the error term, the proposed instruments could be treated as valid ones.[32] Following Strömberg (2004), the instruments exploit natural characteristics of the counties, and ought to gauge the extent to which radio waves propagate through the ground and through the air. As waves propagate differently according to topography, these instruments include: i) a measure of ground conductivity at the county-seat,

[31]The degree to which the instruments are sufficiently correlated with the independent variable is measured through Stock-Yogo tabulations based on the Cragg-Donald statistic, computed by looking at the goodness-of-fit of the first stage regressions relating radio signal's coverage to the entire set of instruments. See Stock and Yogo (2005).

[32]As all the IV equations are overidentified, the independence of the set of instruments from the error process is (partially) tested, through the Sargan test, by regressing the residuals from each two-stage least squares (2SLS) regression on all the included instruments.

available from the U.S. Federal Communications Commission (FCC);[33] and ii) a list of variables affecting the degree to which the signal propagates through the air: the size of the county; the total length of the county covered by rivers; the total area of the county covered by lakes; an indicator for the presence of National Parks in the county; the height of all the mountains above 800m; an indicator for all the coastal counties.[34] An additional instrument takes into account the decisive role played by historically Black colleges and universities (HBCUs) in the often successful setup and running of many Black-appeal radio stations (Barlow 1999). In particular, in order to measure the pervasiveness and relevance of HBCUs in the U.S. South, the instrument includes an indicator based on their enrollment statistics.

The IV results are presented in Table 1.6, separately for the extensive and intensive measures of race riots. For ease of comparability, the OLS coefficients are presented in columns (1) and (3). In columns (2) and (4), the IV coefficients estimated from the two-stage least squares (2SLS) computational method using the instruments on topography and on HBCUs are considered. The IV estimates of the effect of Black-appeal radio stations are larger than the OLS estimates, and this is especially the case for the dummy indicator of coverage (about 3.5 times larger). For the continuous measure, the IV estimates are about 2.5 times larger than the corresponding OLS estimates. These larger IV estimates could mean that OLS specifications are suffering from downward asymptotic bias. This bias could be driven by lower measures of coverage obtained from the ITM in the subset of counties experiencing race riots. Alternatively, it could be due to any unobserved factors that are positively related to radio signal propagation and negatively related

[33]I would like to thank David Strömberg for supplying the dataset on ground conductivity. *Ceteris paribus*, radio waves travel further when ground conductivity, which itself depends on the terrain, is higher. Ground conductivity is used by the FCC in prediciting the propagation of AM radio signals. As estimated by the FCC, throughout the U.S., ground conductivity ranges between 0.5 and 30 millimhos (or millisiemens) per meter, while the conductivity of seawater is estimated at 5,000 millimhos per meter, resulting in the best propagation of AM signals. The reason why radio waves travel further the higher the statistic for ground conductivity is due to the fact that radio waves, when traveling along the surface of the earth, generate a current in the terrain. Thus, if the terrain has poor conductivity characteristics, these currents create resistive heating that dissipates the strength of the signal.

[34]This vector of instruments corresponds to the topographic controls used in the rest of the analysis.

to race riots. Therefore, either working through measurement errors or latent factors, these results suggest that the OLS estimates measure a lower bound for the true effect of receiving radio signals on the extensive and intensive margins of race riots.

### 1.4.4 The political content of Black-appeal radio stations

Radio stations affect the behavior of their audience through the content they air. That is, although the aggregate effect on riots has been shown to be consistently strong and robust across the board, within-group differences could still exist and might depend on the stations' programming.[35] Out of the 59 stations that exclusively aired Black-appeal programs, comprehensive information on the content of their weekly programming has been collected for 57 of them. Then, a statistics measuring the political content of each station's programming is computed as the ratio of the sum of the total weekly hours airing programs of news, interviews, religion, and public service, divided by the total weekly hours of all programs in the station.[36]

To disentangle whether the aggregate effect on riots might vary depending on the political content of radio stations, the following specification is estimated:

$$\begin{aligned} \textit{Race Riots}_{c,s} &= \alpha_s + \delta_1 \textit{High Political Radio}_{c,s} + \\ &+ \delta_2 \textit{Low Political Radio}_{c,s} + \mathbf{X}_{c,s}\gamma_1 + \epsilon_{c,s} \end{aligned} \tag{1.6}$$

where $\mathbf{X}_{c,s}$ includes both the vector of pre-period county-level characteristics and the full set of topographic variables. It further includes the share of the county that is covered by the unobstructed radio signals (i.e., with a free line of sight) of stations with either a high or low level of political content programming. Then, $\delta_1$ measures

[35] This heterogeneity in the effect, if it exists, could be due to the radio stations being either more inclined in talking about the ethnic conflicts happening locally or in neighboring counties, or in simply having the infrastructure to actually monitor the events as they evolved.

[36] The remaining categories include: music, sports, and a category including any other types of programming. The political content variable in these 100% Black-appeal radio stations ranges from 7% to 89% of the total weekly programming. Importantly, with a median at 32% in this spectrum of political content, there is enough idiosyncrasy across stations to estimate the differential effect on race riots stemming from the content of radio programs. In this chapter, the threshold used is set at the $66^{th}$ percentile, and it divides the stations into those that fall in the lower two tertiles and in the upper tertile of the distribution of this political participation statistics. Importantly, all the following results remain robust to alternative choices of the cut-off (e.g., being above or below the $75^{th}$ percentile of the distribution). Reassuringly, moving from the top tertile ($66^{th}$ percentile) to the top quartile ($75^{th}$ percentile) only changes the results marginally.

the effect on riots stemming for a marginal (1%) increase in the county's share covered by the signal from a 100% Black-appeal radio station lying in the top tertile of the distribution of political content ($High\ Political\ Radio_{c,s}$), while $\delta_2$ measures the effect on riots from less politically-charged programming ($Low\ Political\ Radio_{c,s}$).

As depicted on the left-hand side of Figure 1.1, the content of the programming seems to matter as for the extensive margin of riots. In fact, a positive and statistically significant effect is estimated only for the subset of 100% Black-appeal radio stations that have a high level of political content. The coefficient on the other 100% Black-appeal radio stations, albeit positive, does not reach statistical significance at conventional levels. On the other hand, as shown on the right-hand side of Figure 1.1, there is not a meaningful difference on the severity of riots between the two categories of radio stations. The point estimate of the coefficient on the $High\ Political\ Radio_{c,s}$ variable is somewhat higher than the one of $Low\ Political\ Radio_{c,s}$, although not statistically so. Thus, the type of programming does not seem to matter for the intensive margin of riots, while the differential effect on the extensive margin is more evident.

Building on diffusion models, and on anecdotal and qualitative evidence, word of mouth could be thought of as a likely mechanism able to channel the information of a single event to a wide enough audience for a riot to emerge (Myers 1997 and Myers 2010) and, in this chapter, word of mouth is identified with Black-appeal radio stations. As shown in this section, a positive and statistically significant result is obtained only for the subset of radio stations with a meaningful degree of political content in their programming. This might mean that the initial information on the spark (e.g., a protest, an unlawful arrest, or any other unfair violent act) was powerfully and effectively conveyed by those Black-appeal radio stations that were either more interested in political events (i.e., more willing to talk about these civil disorders as they began), or that had the infrastructure to closely follow the upheaval in their infancy. Other types of radio stations, although still exclusively catering an African American audience, were not able to diffuse the information

quickly enough for a riot to emerge, and consequently the ethnic turmoil faded without transforming into a fully developed riot.

This view could also be reconciled with the observation that there seems not be a significant difference in the effect of the content of programming on the intensive margin of riots, with all types of 100% Black-appeal radio stations being associated with more severe disturbances. It could be expected that, once a riot has indeed emerged, Black-appeal radio stations start conveying information on the riot at different levels, according to the degree to which they cater the African American community. And those 100% Black-appeal radio stations, regardless of their predetermined level of political involvement, start to discuss the riot at greater length and more in depth than a station with a smaller fraction of Black-appeal programming. Still, the evidence on the political content of radio stations should be interpreted only as suggestive.

### 1.4.5 Time variation in the exposure to radio

The results presented thus far, using either OLS, IV, propensity score analyses, or a regression discontinuity design, are all based on the cross-sectional variation of the radio signals' coverage across southern counties in 1964. The results paint a consistent picture: Black-appeal radio stations had a significant and sizable effect on both the extensive and intensive margins of the 1960s race riots. In this section, the variability in the survival rate of radio stations is exploited to assess whether their effect on riots varied depending on the length to which stations stayed in operation. That is, it could well be that the length of the exposure matters on the ability of Black-appeal radio stations to have: i) a large enough listenership base to have a meaningful impact on riots; ii) the infrastructure to follow the events as they evolved; or iii) a sufficient revenue stream to be willing to tackle the most controversial topics. To systematically tackle this question, pre-1964 information on riots has been obtained in order to cover the whole period 1960-1971. Considering a longer time series on riots allows to more precisely rule out that the results that have been uncovered in the rest of the chapter could be attributed

to other pre-existing variations across southern counties. In particular, although Black-appeal radio stations were active even before 1964, the ex-ante expectation of no pre-1964 effect is supported by qualitative evidence that these stations did very little in addressing controversial topics, with racism seldom discussed during the 1950s and early 1960s (Barlow 1999 and Halper 2014). If Black-appeal radio stations are found to have an effect on pre-1964 indicators of riots, then this would confound the validity of the whole analysis.

As a first step in this direction, the following event study specification is estimated:

$$\begin{aligned} \textit{Race Riots}_{c,s,t} &= \alpha_{s,t} + \alpha_c + \sum_{n=1960-61}^{1970-71} \beta_{1n} D_n^t \textit{Radio Signal}_{c,s,1964} + \\ &+ \mathbf{X}_{c,s,t}\gamma_1 + \epsilon_{c,s,t} \end{aligned} \tag{1.7}$$

where, compared to Eq. 1.5, the coefficient measuring the effect of radio station is allowed to vary between 1960 and 1971. The dependent variable, $\textit{Race Riots}_{c,s,t}$, represents the time series of either the extensive or intensive margins of riots, where $\alpha_{s,t}$ and $\alpha_c$ are state-year interactions and county fixed effects, respectively. $D_n^t$ is an indicator equal to one whenever $n = t$, $\mathbf{X}_{c,s,t}$ includes the year interactions of all controls (topographic, socio-economic, and the pre-period measures of conflict), as well as the signal's unobstructed propagation for each radio station.[37]

Figure 1.2 plots the coefficient $\beta_{1n}$ for each $n$ from 1960-61 until 1970-71. The estimates indicate that, in the pre-1964 period, the relationship between media and riots is indistinguishable from zero for either treated or untreated counties (those receiving and not receiving the radio signal, respectively). However, after 1964, a different pattern emerges: the impact of media on riots becomes positive and this relationship is steeper in the subset of counties that are exposed to the 100% Black-appeal radio stations. Nevertheless, this larger effect (expressed as the difference in the gradient between treated and untreated counties) is not immediate and seems to slowly build up in 1966-67, peak in 1968-69, before fading somewhat in 1970-71.

[37] To allow for sufficient power, the effect of the $\textit{Radio Signal}_{c,s,1964}$ is estimated separately for six different brackets (1960-61, 1962-63, 1964-65, 1966-67, 1968-69, and 1970-71). Further, as county fixed effects are included, the interaction with the term 1962-63 is omitted (i.e., all coefficients are measured relative to this reference point).

To further study this non-linearity in the effect of media on riots, the complete list of Black-appeal radio stations has been assembled also for 1968.[38] Then, exploiting information on Black-appeal radio stations in both 1964 and 1968, the treatment is analyzed through the lens of a two-way fixed effects (generalized difference-in-differences) model. It is specified as:

$$Race\ Riots_{c,s,t} = \alpha_{s,t} + \alpha_c + \beta_1 Active\ Radio_{c,s,t} + \epsilon_{c,s,t} \tag{1.8}$$

where now the independent variable of interest, $Active\ Radio_{c,s,t}$, includes information on the radio stations from both years and, specifically, it is set equal to one when the county is treated (i.e., when the majority of the county receives the signal from the Black-appeal radio station). Alternatively, using a continuous measure, it is set equal to the exact share of the county receiving the signal in any given year. In both cases, the treatment variable is otherwise set equal to zero.[39] Then, by allowing counties to freely go on/off treatment, Table 1.7 presents the estimated coefficient $\beta_1$. Across all specifications, Black-appeal radio stations are found to have a highly significant and positive effect: the switch toward treatment is associated with a rise of 0.15% in the likelihood of riots and 0.87% in the severity of riots. Alternatively, a marginal rise in the share of the county covered by radio waves is found to lead to a 15% and 28% increase in the mean levels of the extensive and intensive margins of riots. However, by comparing the point estimates of Table 1.7 with the ones estimated in earlier sections, it emerges that the newly estimated effects are smaller in magnitude. One possible explanation for this reduction is that the $Active\ Radio_{c,s,t}$ variable does not allow for the build up of the effect over time. To allow for this non-linear effect, two additional specifications

[38] There were 157 active stations in 1968, compared to the 200 stations that existed in 1964. The turnover was high: in 1968, 80 were newly established stations and only 77 carried over from 1964. Focusing on 100% Black-appeal radio stations, there were 59 such stations in either 1964 or 1968 but, importantly, as shown in Table A.1.2, these 59 stations are not the same, and there is a substantial degree of variation across both southern counties and states to be able to study the effect of the length of the exposure on riots. In particular, of the 59 stations active in 1968, 45 carried over from the previous period.

[39] State-year interactions and county fixed effects are still shown as $\alpha_{s,t}$ and $\alpha_c$, and they automatically control for all the time-invariant characteristics of the county and of the state, including their pre-period mean levels. For more on the generalized difference-in-differences technique, see Bertrand, Duflo, and Mullainathan (2004) and Hansen (2007).

are then considered. The first includes leads in the two-way fixed effects model, while the second directly allows for the effect of media to vary depending on the idiosyncratic survival rate of stations.

In Table 1.8, the generalized difference-in-differences model presented in Eq. 1.8 is augmented with two additional terms: $Lead_{1,c,s,1964}$ and $Lead_{2,c,s,1964}$. That is, the counties that are exposed to Black-appeal radio stations in 1964 (either through the dummy indicator or through the continuous statistics) are now assumed to have received the same treatment also in 1962-63 and in 1960-61, respectively. Performing this placebo test of the effect allows to check for differential outcomes in the pre-treatment period. In all specifications, the estimates on $Lead_{2,c,s,1964}$ are indistinguishable from zero, and the absence of a significant effect on this counterfactual scenario reinforces the validity of the main estimates of the chapter. $Lead_{1,c,s,1964}$ is instead found to be negative: that is, the would-be treated counties were actually experiencing fewer and less severe riots in the two years before 1964. The positive effect found from 1964 onward could then be considered as a lower bound of the true effect of being exposed to Black-appeal radio stations.

To allow for both non-linearities and for the build-up of the effect over time, Eq. 1.9 splits the aggregate effect into three components: the one from the stations that were active both in 1964 and 1968, the one from the stations that closed down in 1968, and the one from those that opened up only in 1968. For ease of comparability, columns (1) and (5) of Table 1.9 show the aggregate effect. The remaining columns allow for three variations of the following specification, estimated on the 1964-1971 sample:

$$\begin{aligned} Race\ Riots_{c,s} &= \alpha_s + \beta_1 Always\ On_{c,s} + \beta_2 Switch\ Off_{c,s} + \\ &+ \beta_3 Switch\ On_{c,s} + \mathbf{X}_{c,s}\gamma_1 + \epsilon_{c,s} \end{aligned} \tag{1.9}$$

And although there is a substantial variability in the number of counties that stop receiving the signal and those that start receiving the signal in 1968, all the effect on riots is concentrated in the subset of counties receiving the radio signal throughout the whole period. That is, the coefficient on $Switch\ Off_{c,s}$ and $Switch\ On_{c,s}$ are

estimated to be indistinguishable from zero, while $\beta_1$ closely corresponds to the coefficient estimated when no distinction is made on the survival rate of stations. The length of exposure is important: Black-appeal radio stations active in both years had a larger impact on the likelihood of riots and on their severity. As they stayed in operation for longer, they likely had a more robust revenue stream, a wider audience, and a better infrastructure and more independence to deal with controversial topics such as civil disorders.

## 1.5 Conclusions

In this chapter, a mechanism able to explain both the location of civil ethnic disturbances, as well as their severity, is proposed and empirically tested. Black-appeal radio stations, through their active and ingrained role within African American communities across southern counties, were the link allowing small, individual, and isolated events to reach a wide enough audience for large-scale race riots to emerge. For their ability to coordinate subsequent protests, these stations also significantly affected the 1960s riots' overall destructiveness. This result lends empirical support to the theoretical argument linking individual preferences to like-minded information from media, with non-trivial persuasion effects exterted by such biased media outlets. This chapter also sheds light on a long-standing debate on what constituted the driving channel behind the 1960s riots by isolating, for the first time, a single and well-identified factor.

The county exposure to the signal of each radio station is measured through a model of electromagnetic signal propagation in conjunction with geographic information system applications exploiting information on each station's antenna. The impact of Black-appeal radio stations on the 1960s race riots is then identified through alternative techniques, based on both cross-sectional and panel datasets. Exploiting county-level variations, a marginal (1%) increase in the share of the county receiving the signal from a Black-appeal radio station increased by 7% and 15% the mean levels of the likelihood and intensity of riots, respectively. A consistent

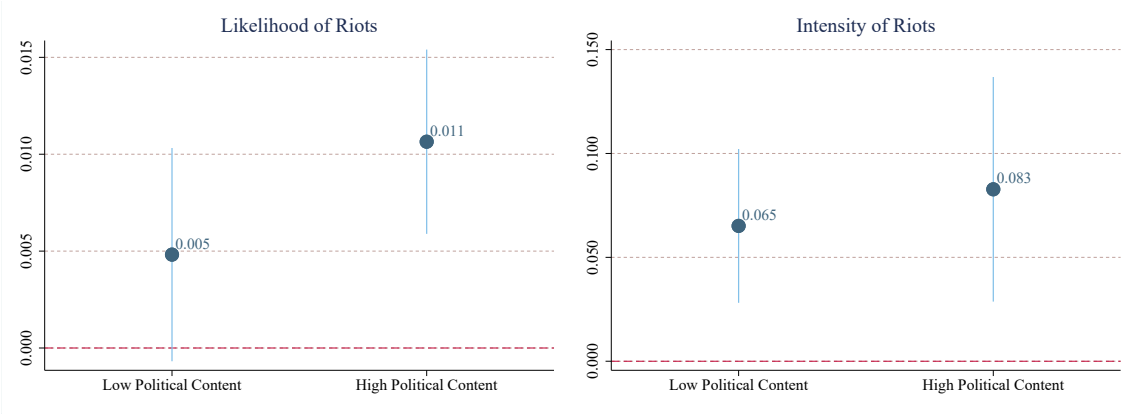
story is found using OLS, propensity score analysis, regression discontinuity design, instrumental variables specifications, and panel techniques.

Black-appeal radio stations significantly changed the fabric of southern counties, through their sizable and robust impact on the likelihood and on the severity of the 1960s race riots. By analyzing the individual features of each station, this chapter has touched upon possible mechanisms at play, based on both the quantity and quality of programming, as well as on the survival rate of stations. Firstly, measuring the relevance of Black-appeal radio stations as deeply important institutions in the local communities they served, all the aggregate results are concentrated in the subset of stations airing a significant amount of Black-appeal content. Secondly, by studying the content of each station's programming, it emerges that not only the amount of airtime, but also the type of programming mattered in an important way: having an apparatus already set up and in place to air more politically charged content, allowed only a subset of stations to make a meaningful impact on the emergence of riots, by effectively conveying information on disturbances as soon as they erupted. Lastly, the length of operation mattered, with those stations that either quickly shut down or only subsequently opened up leading to no meaningful contribution to race riots.

Several additional avenues for research appear worth exploring. First, Black-appeal radio stations in southern counties might be systematically different from those set up elsewhere in the United States. And as large riots have also emerged in the North of the country, an interesting question is whether the impact of radio stations is found to be similar to the one unveiled in this chapter, or whether additional channels become important. Second, as the 1960s riots have been found to have had large and significant consequences on several economic outcomes in the affected cities, it remains to be tested whether such outcomes have also depended on the presence of Black-appeal radio stations, with the latter perhaps acting as a backstop in the otherwise strongly negative effects of riots. Third, Black-appeal radio stations might have had spillover effects into other important dynamics at play during the 1960s. Through their highly effective engagement of the local communities, they could have had important effects on the progress observed across voting and

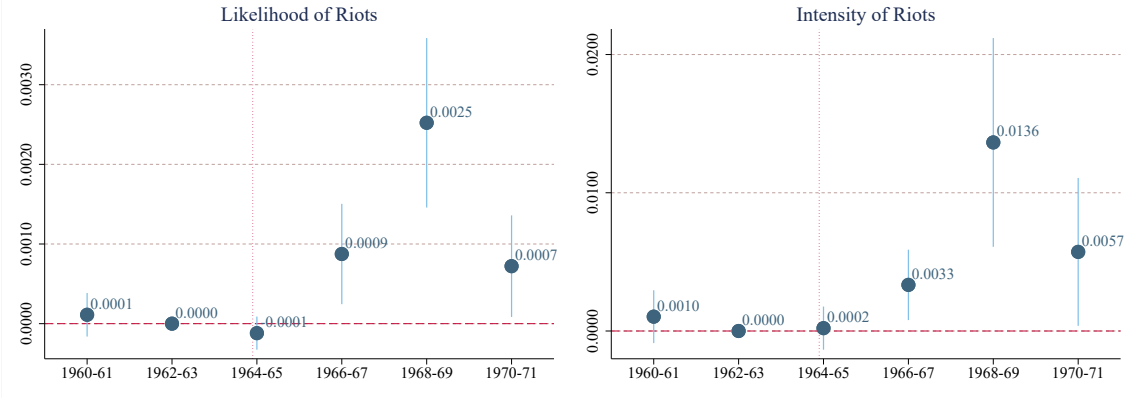
civil rights, by rising voter registrations and leading to more African American candidates in local elections. All these questions are left to further research.

**Figure 1.1:** The political content of radio stations' programming




The two figures plot the $\delta_1$ and $\delta_2$ coefficients estimated from the specification presented in Eq. 1.6. The confidence intervals shown are set at the 90% level of statistical significance.

**Figure 1.2:** Event study of the effect of radio stations on riots




The two figures plot the $\beta_{1n}$ coefficients estimated from the specification presented in Eq. 1.7. The confidence intervals shown are set at the 90% level of statistical significance.

**Table 1.1:** The determinants of radio signals and riots

| | Radio Signal | | Race Riots | |
|---|---|---|---|---|
| | Dummy (1) | Continuous (2) | Likelihood (3) | Intensity (4) |
| Share of African Americans, 1960 | 0.005 | 0.519 | 0.033*** | 0.179** |
| | (0.006) | (0.421) | (0.012) | (0.076) |
| VRA coverage in 1965 | –0.106 | –13.638 | –0.314* | –2.292 |
| | (0.130) | (10.650) | (0.173) | (1.647) |
| Population density, 1960 | 0.238 | 19.068 | 0.755** | 7.943*** |
| | (0.210) | (13.465) | (0.342) | (2.940) |
| Manufacturing size, 1960 | –0.003 | –0.067 | –0.001 | –0.147* |
| | (0.004) | (0.271) | (0.006) | (0.079) |
| Agriculture size, 1960 | 0.000 | 0.191 | –0.002 | –0.063 |
| | (0.004) | (0.244) | (0.006) | (0.046) |
| Mining size, 1950 | –0.002 | 0.121 | 0.013** | –0.031 |
| | (0.004) | (0.280) | (0.006) | (0.066) |
| Agricultural intensity, 1960 | –0.003 | –0.004 | –0.022** | –0.121 |
| | (0.004) | (0.325) | (0.011) | (0.105) |
| Unemployment rate, 1960 | 0.016 | 1.239 | –0.021 | –0.093 |
| | (0.014) | (1.030) | (0.023) | (0.201) |
| Share of families in poverty, 1960 | –0.013 | –0.670 | –0.005 | –0.413* |
| | (0.013) | (1.024) | (0.024) | (0.227) |
| Median family income, 1960 | –0.000 | 0.005 | –0.000 | –0.004* |
| | (0.000) | (0.014) | (0.000) | (0.002) |
| Birth rate, 1960 | 0.000 | 0.266 | –0.003 | –0.121 |
| | (0.004) | (0.303) | (0.007) | (0.082) |
| Household members, 1960 | 0.167 | 3.660 | –0.833*** | –2.258 |
| | (0.168) | (9.516) | (0.317) | (2.559) |
| Size of farms, 1959 | –0.001 | 0.009 | –0.004 | –0.044* |
| | (0.001) | (0.098) | (0.003) | (0.025) |
| Nonwhite households, 1950 | 0.005 | 0.356 | –0.008 | 0.002 |
| | (0.006) | (0.448) | (0.011) | (0.075) |
| Farms w. electricity, 1954 | 0.004 | –0.027 | 0.008 | –0.021 |
| | (0.003) | (0.247) | (0.009) | (0.131) |
| Households w. tv, 1960 | 0.006* | 0.463** | 0.004 | 0.091* |
| | (0.003) | (0.216) | (0.006) | (0.053) |
| Households w. two or more cars, 1960 | 0.001 | 0.349 | 0.014 | 0.147* |
| | (0.005) | (0.313) | (0.009) | (0.075) |
| Older than 65 years, 1960 | 0.023 | 1.560 | –0.098** | –0.266 |
| | (0.015) | (0.977) | (0.043) | (0.296) |
| Expenditure on highways, 1962 | –0.017 | –0.393 | –0.013 | 0.012 |
| | (0.017) | (1.603) | (0.043) | (0.311) |
| Median school years, 1960 | 0.032 | 0.916 | –0.011 | –0.948* |
| | (0.041) | (2.909) | (0.079) | (0.524) |
| Enrolled in school, 1950 | –0.001 | –0.032 | –0.003* | 0.001 |
| | (0.001) | (0.070) | (0.001) | (0.012) |
| Lynchings of Afr. Am., 1902-1964 | –0.168 | –11.652 | –0.695** | –2.791 |
| | (0.129) | (8.518) | (0.290) | (2.761) |
| Anti-black activism, 1960-1964 | –0.001 | –0.036 | –0.038** | –0.616** |
| | (0.012) | (0.746) | (0.019) | (0.239) |
| NAACP branches, 1957-1964 | 0.039 | 3.936 | –0.242** | –1.632** |
| | (0.056) | (3.998) | (0.099) | (0.804) |
| KKK Klaverns, 1925-1940 | 0.031 | 1.741 | 0.489*** | 1.826** |
| | (0.036) | (2.916) | (0.139) | (0.837) |
| State Fixed Effects | Yes | Yes | Yes | Yes |
| Topographic Controls | Yes | Yes | Yes | Yes |
| Mean Dep. Var. | 0.153 | 16.053 | 0.004 | 0.084 |
| Adj. R-Square | 0.44 | 0.50 | 0.65 | 0.46 |
| N | 1022 | 1022 | 1022 | 1022 |

Robust standard errors in parenthesis are adjusted for clustering at the county-level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. All regressions include state fixed effects and topographic variables.

## 1.5. Conclusions

**Table 1.2:** The race riots by year and state

| | 1964 | 1965 | 1966 | 1967 | 1968 | 1969 | 1970 | 1971 | Total |
|---|---|---|---|---|---|---|---|---|---|
| *Panel A. Race Riots in the U.S.* | | | | | | | | | |
| Number of riots | 11 | 11 | 53 | 158 | 289 | 124 | 68 | 38 | 752 |
| Days of riots | 34 | 20 | 109 | 408 | 739 | 284 | 126 | 82 | 1,802 |
| People killed | 2 | 35 | 11 | 83 | 66 | 13 | 13 | 5 | 228 |
| People injured | 996 | 1,132 | 525 | 2,801 | 5,302 | 861 | 710 | 414 | 12,741 |
| People arrested | 2,917 | 4,219 | 5,107 | 17,011 | 31,680 | 4,730 | 2,027 | 1,408 | 69,099 |
| Episodes of arson | 238 | 3,006 | 812 | 4,627 | 6,041 | 369 | 283 | 459 | 15,835 |
| *Intensity of Riots* | *0.16* | *0.50* | *0.28* | *1.35* | *1.96* | *0.37* | *0.23* | *0.15* | *5.00* |
| *Panel B. Race Riots in the U.S. South* | | | | | | | | | |
| Number of riots | 1 | 0 | 10 | 26 | 74 | 35 | 17 | 26 | 189 |
| Days of riots | 3 | 0 | 17 | 47 | 227 | 74 | 39 | 58 | 465 |
| People killed | 0 | 0 | 0 | 3 | 11 | 3 | 10 | 3 | 30 |
| People injured | 15 | 0 | 36 | 164 | 1,463 | 91 | 223 | 298 | 2,290 |
| People arrested | 465 | 0 | 207 | 1,176 | 4,373 | 1,070 | 720 | 969 | 8,980 |
| Episodes of arson | 0 | 0 | 10 | 322 | 1,139 | 81 | 113 | 291 | 1,956 |
| *Intensity of Riots* | *0.07* | *0.00* | *0.08* | *0.57* | *2.56* | *0.46* | *0.65* | *0.61* | *5.00* |
| *Panel C. Race Riots in the U.S. South, by State* | | | | | | | | | |
| Alabama | 0 | 0 | 3 | 1 | 1 | 2 | 1 | 0 | 8 |
| Arkansas | 0 | 0 | 0 | 1 | 8 | 2 | 1 | 0 | 12 |
| Florida | 1 | 0 | 4 | 5 | 16 | 7 | 5 | 12 | 50 |
| Georgia | 0 | 0 | 3 | 4 | 7 | 0 | 3 | 5 | 22 |
| Louisiana | 0 | 0 | 0 | 3 | 1 | 2 | 4 | 1 | 11 |
| Mississippi | 0 | 0 | 0 | 1 | 3 | 1 | 0 | 2 | 7 |
| North Carolina | 0 | 0 | 0 | 2 | 14 | 8 | 2 | 3 | 29 |
| South Carolina | 0 | 0 | 0 | 0 | 4 | 4 | 0 | 0 | 8 |
| Tennessee | 0 | 0 | 0 | 3 | 7 | 3 | 0 | 3 | 16 |
| Texas | 0 | 0 | 0 | 2 | 4 | 4 | 0 | 0 | 10 |
| Virginia | 0 | 0 | 0 | 4 | 9 | 2 | 1 | 0 | 16 |

**Table 1.3:** The effect of radio stations on riots

| | Likelihood of Riots | | | | | Intensity of Riots | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| *Panel A: Black-Appeal Radio Stations: Dummy Variable* | | | | | | | | | | |
| 100% | 0.371** | | | | | 4.814*** | | | | |
| | (0.182) | | | | | (1.177) | | | | |
| > 75% | | 0.366** | | | | | 4.757*** | | | |
| | | (0.181) | | | | | (1.177) | | | |
| > 50% | | | 0.184 | | | | | 2.583*** | | |
| | | | (0.136) | | | | | (0.814) | | |
| > 25% | | | | 0.078 | | | | | 1.580** | |
| | | | | (0.128) | | | | | (0.799) | |
| > 0% | | | | | 0.021 | | | | | 1.393* |
| | | | | | (0.126) | | | | | (0.757) |
| State Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Topographic | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Socio-Economic | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Pre-VRA Conflict | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Adj. R-Square | 0.66 | 0.66 | 0.65 | 0.65 | 0.65 | 0.50 | 0.50 | 0.47 | 0.47 | 0.47 |
| N | 1022 | 1022 | 1022 | 1022 | 1022 | 1022 | 1022 | 1022 | 1022 | 1022 |
| *Panel B: Black-Appeal Radio Stations: Continous Variable* | | | | | | | | | | |
| 100% | 0.006** | | | | | 0.064*** | | | | |
| | (0.003) | | | | | (0.016) | | | | |
| > 75% | | 0.006** | | | | | 0.064*** | | | |
| | | (0.003) | | | | | (0.016) | | | |
| > 50% | | | 0.004 | | | | | 0.036** | | |
| | | | (0.002) | | | | | (0.014) | | |
| > 25% | | | | 0.002 | | | | | 0.021 | |
| | | | | (0.002) | | | | | (0.015) | |
| > 0% | | | | | 0.001 | | | | | 0.017 |
| | | | | | (0.002) | | | | | (0.016) |
| State Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Topographic | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Socio-Economic | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Pre-VRA Conflict | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Adj. R-Square | 0.67 | 0.67 | 0.65 | 0.65 | 0.65 | 0.50 | 0.50 | 0.47 | 0.47 | 0.46 |
| N | 1022 | 1022 | 1022 | 1022 | 1022 | 1022 | 1022 | 1022 | 1022 | 1022 |

Robust standard errors in parenthesis are adjusted for clustering at the county-level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. All regressions include state fixed effects, topographic variables, socio-economic controls, and pre-VRA measures of conflict.

**Table 1.4:** Controlling for free-space radio waves

| | Likelihood of Riots | | | | | Intensity of Riots | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| *Panel A: Black-Appeal Radio Stations: Dummy Variable & Free-Space Dummy Signal* | | | | | | | | | | |
| 100% | 0.324* | | | | | 5.087*** | | | | |
| | (0.183) | | | | | (1.242) | | | | |
| > 75% | | 0.314* | | | | | 4.960*** | | | |
| | | (0.179) | | | | | (1.228) | | | |
| > 50% | | | 0.164 | | | | | 2.778*** | | |
| | | | (0.133) | | | | | (0.790) | | |
| > 25% | | | | 0.078 | | | | | 1.973*** | |
| | | | | (0.122) | | | | | (0.717) | |
| > 0% | | | | | 0.033 | | | | | 1.840*** |
| | | | | | (0.122) | | | | | (0.697) |
| Free-Space Signal | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| State Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Topographic | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Socio-Economic | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Pre-VRA Conflict | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Adj. R-Square | 0.66 | 0.66 | 0.65 | 0.65 | 0.65 | 0.50 | 0.50 | 0.48 | 0.47 | 0.47 |
| N | 1022 | 1022 | 1022 | 1022 | 1022 | 1022 | 1022 | 1022 | 1022 | 1022 |
| *Panel B: Black-Appeal Radio Stations: Continuous Variable & Free-Space Continuous Signal* | | | | | | | | | | |
| 100% | 0.006* | | | | | 0.076*** | | | | |
| | (0.003) | | | | | (0.019) | | | | |
| > 75% | | 0.005* | | | | | 0.073*** | | | |
| | | (0.003) | | | | | (0.019) | | | |
| > 50% | | | 0.004 | | | | | 0.043** | | |
| | | | (0.003) | | | | | (0.017) | | |
| > 25% | | | | 0.003 | | | | | 0.033** | |
| | | | | (0.002) | | | | | (0.016) | |
| > 0% | | | | | 0.002 | | | | | 0.029 |
| | | | | | (0.002) | | | | | (0.018) |
| Free-Space Signal | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| State Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Topographic | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Socio-Economic | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Pre-VRA Conflict | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Adj. R-Square | 0.67 | 0.67 | 0.65 | 0.65 | 0.65 | 0.51 | 0.51 | 0.48 | 0.47 | 0.47 |
| N | 1022 | 1022 | 1022 | 1022 | 1022 | 1022 | 1022 | 1022 | 1022 | 1022 |

Robust standard errors in parenthesis are adjusted for clustering at the county-level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. All regressions include state fixed effects, topographic variables, socio-economic controls, pre-VRA measures of conflict, and the unobstructed radio signals.

**Table 1.5:** The stability of the effect

| | Likelihood of Riots | | | | Intensity of Riots | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| *Panel A: 100% Black-Appeal Radio Stations: Dummy Variable* | | | | | | | | |
| Radio signal | 0.950*** | 0.532*** | 0.371** | 0.324* | 7.472*** | 5.714*** | 4.814*** | 5.087*** |
| | (0.201) | (0.198) | (0.182) | (0.183) | (1.749) | (1.553) | (1.177) | (1.242) |
| Free-space signal | | | | 0.144* | | | | –0.855 |
| | | | | (0.076) | | | | (0.655) |
| Share of Afr. Am. | | | 0.031** | 0.029** | | | 0.149* | 0.163** |
| | | | (0.012) | (0.012) | | | (0.076) | (0.076) |
| VRA coverage | | | –0.227 | –0.219 | | | –1.159 | –1.204 |
| | | | (0.152) | (0.153) | | | (1.318) | (1.326) |
| Lynchings | | –0.835*** | –0.648** | –0.664** | | –6.039** | –2.187 | –2.095 |
| | | (0.318) | (0.257) | (0.258) | | (2.435) | (2.428) | (2.410) |
| Anti-black acts | | 0.010 | –0.042** | –0.039* | | –0.148 | –0.660*** | –0.675*** |
| | | (0.013) | (0.021) | (0.020) | | (0.160) | (0.230) | (0.237) |
| NAACP branches | | 0.039 | –0.271*** | –0.278*** | | 0.037 | –2.003** | –1.961** |
| | | (0.156) | (0.099) | (0.099) | | (0.986) | (0.825) | (0.808) |
| KKK Klaverns | | 0.477*** | 0.468*** | 0.469*** | | 1.841* | 1.556* | 1.548* |
| | | (0.169) | (0.140) | (0.140) | | (1.055) | (0.843) | (0.838) |
| State Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Topographic | No | Yes | Yes | Yes | No | Yes | Yes | Yes |
| Socio-Economic | No | No | Yes | Yes | No | No | Yes | Yes |
| Pre-VRA Conflict | No | Yes | Yes | Yes | No | Yes | Yes | Yes |
| Adj. R-Square | 0.31 | 0.51 | 0.66 | 0.66 | 0.27 | 0.36 | 0.50 | 0.50 |
| N | 1134 | 1134 | 1022 | 1022 | 1134 | 1134 | 1022 | 1022 |
| *Panel B: 100% Black-Appeal Radio Stations: Continuous Variable* | | | | | | | | |
| Radio signal | 0.013*** | 0.008*** | 0.006** | 0.006* | 0.097*** | 0.078*** | 0.064*** | 0.076*** |
| | (0.002) | (0.002) | (0.003) | (0.003) | (0.022) | (0.019) | (0.016) | (0.019) |
| Free-space signal | | | | 0.001 | | | | –0.019** |
| | | | | (0.001) | | | | (0.010) |
| Share of Afr. Am. | | | 0.030** | 0.029** | | | 0.141* | 0.170** |
| | | | (0.012) | (0.012) | | | (0.075) | (0.076) |
| VRA coverage | | | –0.194 | –0.194 | | | –1.015 | –1.003 |
| | | | (0.156) | (0.156) | | | (1.327) | (1.320) |
| Lynchings | | –0.770*** | –0.628** | –0.635** | | –5.736** | –2.083 | –1.892 |
| | | (0.291) | (0.255) | (0.255) | | (2.302) | (2.489) | (2.398) |
| Anti-black acts | | 0.012 | –0.038* | –0.037* | | –0.120 | –0.619*** | –0.647*** |
| | | (0.012) | (0.021) | (0.020) | | (0.152) | (0.227) | (0.229) |
| NAACP branches | | –0.009 | –0.283*** | –0.285*** | | –0.278 | –2.070*** | –2.012** |
| | | (0.148) | (0.099) | (0.099) | | (0.880) | (0.802) | (0.799) |
| KKK Klaverns | | 0.469*** | 0.467*** | 0.468*** | | 1.842* | 1.592* | 1.560* |
| | | (0.168) | (0.142) | (0.142) | | (1.025) | (0.826) | (0.816) |
| State Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Topographic | No | Yes | Yes | Yes | No | Yes | Yes | Yes |
| Socio-Economic | No | No | Yes | Yes | No | No | Yes | Yes |
| Pre-VRA Conflict | No | Yes | Yes | Yes | No | Yes | Yes | Yes |
| Adj. R-Square | 0.33 | 0.53 | 0.67 | 0.67 | 0.27 | 0.38 | 0.50 | 0.51 |
| N | 1134 | 1134 | 1022 | 1022 | 1134 | 1134 | 1022 | 1022 |

Robust standard errors in parenthesis are adjusted for clustering at the county-level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. All regressions include state fixed effects. Topopgrahic variables are included in columns (2)-(4) and (6)-(8). Socio-economic controls are included in columns (3)-(4) and (7)-(8). Pre-VRA measures of conflict are included in columns (2)-(4) and (6)-(8). The unobstructed radio signals are included in columns (4) and (8).

**Table 1.6:** IV (2SLS) estimates of the effect

| | Likelihood of Riots | | Intensity of Riots | |
|---|---|---|---|---|
| | OLS (1) | IV (2) | OLS (3) | IV (4) |
| *Panel A: 100% Black-Appeal Radio Stations: Dummy Variable* | | | | |
| Predicted signal | 0.325* | 1.161* | 5.090*** | 19.059*** |
| | (0.183) | (0.693) | (1.243) | (6.036) |
| Underid. *LM*-stat | | | | |
| $\chi^2$ | | 15.904 | | 15.904 |
| *p*-value | | 0.044 | | 0.044 |
| Weak id. *F*-stat | | | | |
| *F* | | 10.644 | | 10.644 |
| Overid. *J*-stat | | | | |
| $\chi^2$ | | 8.848 | | 6.621 |
| *p*-value | | 0.264 | | 0.469 |
| Free-Space Signal | Yes | Yes | Yes | Yes |
| State Fixed Effects | Yes | Yes | Yes | Yes |
| Topographic | Yes | Yes | Yes | Yes |
| Socio-Economic | Yes | Yes | Yes | Yes |
| Pre-VRA Conflict | Yes | Yes | Yes | Yes |
| Adj. R-Square | 0.66 | 0.58 | 0.50 | 0.18 |
| N | 1021 | 1021 | 1021 | 1021 |
| *Panel B: 100% Black-Appeal Radio Stations: Continuous Variable* | | | | |
| Predicted signal | 0.006* | 0.015* | 0.076*** | 0.196*** |
| | (0.003) | (0.008) | (0.019) | (0.063) |
| Underid. *LM*-stat | | | | |
| $\chi^2$ | | 22.314 | | 22.314 |
| *p*-value | | 0.004 | | 0.004 |
| Weak id. *F*-stat | | | | |
| *F* | | 12.777 | | 12.777 |
| Overid. *J*-stat | | | | |
| $\chi^2$ | | 11.936 | | 9.422 |
| *p*-value | | 0.103 | | 0.224 |
| Free-Space Signal | Yes | Yes | Yes | Yes |
| State Fixed Effects | Yes | Yes | Yes | Yes |
| Topographic | Yes | Yes | Yes | Yes |
| Socio-Economic | Yes | Yes | Yes | Yes |
| Pre-VRA Conflict | Yes | Yes | Yes | Yes |
| Adj. R-Square | 0.67 | 0.61 | 0.51 | 0.36 |
| N | 1021 | 1021 | 1021 | 1021 |

Robust standard errors in parenthesis are adjusted for clustering at the county-level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. All regressions include state fixed effects. All regressions include state fixed effects, topographic variables, socio-economic controls, pre-VRA measures of conflict, and the unobstructed radio signals.

**Table 1.7:** Generalized difference-in-differences estimates of the effect

| | Likelihood of Riots | Intensity of Riots |
|---|---|---|
| | (1) | (2) |
| *Panel A: 100% Black-Appeal Radio Stations: Dummy Variable* | | |
| Black radio in 1964 and 1968 | 0.1525*** | 0.8748*** |
| | (0.0424) | (0.2952) |
| County Fixed Effects | Yes | Yes |
| State-Year Fixed Effects | Yes | Yes |
| Mean Dep. Var | 0.0550 | 0.0141 |
| Adj. R-Square | 0.20 | 0.12 |
| N | 6804 | 6804 |
| *Panel B: 100% Black-Appeal Radio Stations: Continuous Variable* | | |
| Black radio in 1964 and 1968 | 0.0021*** | 0.0153*** |
| | (0.0006) | (0.0057) |
| County Fixed Effects | Yes | Yes |
| State-Year Fixed Effects | Yes | Yes |
| Mean Dep. Var | 0.0550 | 0.0141 |
| Adj. R-Square | 0.20 | 0.13 |
| N | 6804 | 6804 |

Robust standard errors in parenthesis are adjusted for clustering at the county-level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

**Table 1.8:** Generalized difference-in-differences estimates of the effect with leads

| | Likelihood of Riots | | | Intensity of Riots | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *Panel A: 100% Black-Appeal Radio Stations: Dummy Variable* | | | | | | |
| Black radio in 1964 and 1968 | 0.1525*** | 0.1211*** | 0.1481*** | 0.8748*** | 0.6174** | 0.8667*** |
| | (0.0424) | (0.0403) | (0.0425) | (0.2952) | (0.2904) | (0.2736) |
| 1964 radio in 1962 | | –0.0809** | | | –0.6632*** | |
| | | (0.0340) | | | (0.2429) | |
| 1964 radio in 1960 | | | –0.0114 | | | –0.0207 |
| | | | (0.0333) | | | (0.2794) |
| County Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes |
| State Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Year Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Adj. R-Square | 0.20 | 0.20 | 0.20 | 0.12 | 0.12 | 0.12 |
| N | 6804 | 6804 | 6804 | 6804 | 6804 | 6804 |
| *Panel B: 100% Black-Appeal Radio Stations: Continuous Variable* | | | | | | |
| Black radio in 1964 and 1968 | 0.0021*** | 0.0017*** | 0.0020*** | 0.0153*** | 0.0141** | 0.0170** |
| | (0.0006) | (0.0006) | (0.0006) | (0.0057) | (0.0071) | (0.0071) |
| 1964 radio in 1962 | | –0.0010** | | | –0.0028 | |
| | | (0.0004) | | | (0.0050) | |
| 1964 radio in 1960 | | | –0.0003 | | | 0.0041 |
| | | | (0.0005) | | | (0.0053) |
| County Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes |
| State Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Year Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Adj. R-Square | 0.20 | 0.20 | 0.20 | 0.13 | 0.13 | 0.13 |
| N | 6804 | 6804 | 6804 | 6804 | 6804 | 6804 |

Robust standard errors in parenthesis are adjusted for clustering at the county-level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

**Table 1.9:** Early adopters, early exiters, and late adopters

| | Likelihood of Riots | | | | Intensity of Riots | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| All active in 1964 | 0.0065** | 0.0064** | | | 0.0776*** | 0.0774*** | | |
| | (0.0030) | (0.0030) | | | (0.0208) | (0.0208) | | |
| Always on | | | 0.0065** | 0.0064** | | | 0.0773*** | 0.0771*** |
| | | | (0.0029) | (0.0029) | | | (0.0206) | (0.0207) |
| Switch off | | | 0.0178 | 0.0178 | | | 0.0544 | 0.0531 |
| | | | (0.0196) | (0.0196) | | | (0.0616) | (0.0625) |
| Switch on | | –0.0041 | | –0.0043 | | –0.0161 | | –0.0156 |
| | | (0.0033) | | (0.0033) | | (0.0268) | | (0.0267) |
| Free-Space Signal 1964 | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Free-Space Signal 1968 | No | Yes | No | Yes | No | Yes | No | Yes |
| State Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Topographic | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Pre-VRA Conflict | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Mean Dep. Var. | 0.4131 | 0.4131 | 0.4131 | 0.4131 | 0.0845 | 0.0845 | 0.0845 | 0.0845 |
| Adj. R-Square | 0.54 | 0.54 | 0.54 | 0.54 | 0.39 | 0.39 | 0.39 | 0.39 |
| N | 1134 | 1134 | 1134 | 1134 | 1134 | 1134 | 1134 | 1134 |

Robust standard errors in parenthesis are adjusted for clustering at the county-level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. All regressions include state fixed effects, topographic variables, pre-VRA measures of conflict, and unobstructed radio signals in 1964 and 1968.

*Give us the ballot, and we will fill our legislative halls with men of goodwill.*

— Martin Luther King, Jr., *Give Us The Ballot*, May 17, 1957

# 2

# Race, Representation, and Local Governments in the U.S. South: The Effect of the Voting Rights Act

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## 2.1 Introduction

Universal suffrage is a defining feature of modern representative democracies. Yet, the right to vote is often not sufficient to guarantee representation of minorities and traditionally disadvantaged groups. Under the predicament that the identity of elected office-holders substantially contributes to shape policies, provide role models, and weaken stereotypes, many countries have resorted to strong remedial measures, known as political reservation, such as gender quotas or reserved seats for minorities.[1] In no other country perhaps more than in the United States, the issue of minority discrimination in the political sphere has been so central. Yet, the United States has pursued a much less prescriptive approach than mandated representation, relying instead on courts to enforce the anti-discriminatory provisions embedded in the Voting Rights Act (VRA) of 1965. The purpose of this chapter is to study whether such arguably less intrusive measures have been effective in advancing descriptive and substantive representation of African Americans.

Defined by President Lyndon B. Johnson as "one of the most monumental laws in the entire history of American freedom," the VRA abolished obstacles to the registration of African American voters (chiefly literacy test provisions) and put in place special measures, known as coverage, mandating federal scrutiny over states with a history of African American disenfranchisement. As presented in Chapter 4, the elimination of literacy test provisions led to an immediate and sharp increase in both the registration and turnout rates of African American voters, making them a political force to be reckoned with (Donohue and Heckman 1991, Wright 2013, and Cascio and Washington 2014). Yet, whether this fast progress in political participation was paralleled by gains in African American representation could be questioned: as shown in Chapter 3, powerful obstacles to the election of African Americans had remained in place.[2] Unlike mandated representation

[1] Gender quotas are present in more than one hundred countries (Besley et al. 2017) and reserved seats for minorities in more than thirty (Krook and O'Brien 2010). For evidence on their effects on policy making, see Pande (2003) and Chattopadhyay and Duflo (2004).

[2] Despite the special measures introduced by the VRA, a prevailing culture of white supremacy and intimidation discouraged African Americans from running for office (Andrews 1997). Moreover, in the presence of racially polarized voting, majoritarian electoral rules made it difficult to elect

measures used elsewhere, the VRA only provided a legal framework to tackle discrimination in courts. The civil rights movement leadership was clearly aware that the road toward African American office-holding was paved with obstacles.[3] Did the VRA live up to its promise?

While race remains one of the most debated issues in American politics, we still have limited evidence on whether, when, and how did the arguably most important piece of civil rights legislation enacted by the U.S. Congress led to gains in African American office-holding. This chapter makes progress on this important question by carrying out the first systematic assessment of the VRA's impact on the racial make-up of all local governments in the U.S. South between 1962 and 1980. It provides causal evidence that coverage increased African American representation at the local level, and it did so soon after its introduction. Importantly, these gains were not limited to minor offices, but rather extended to powerful elective bodies.

To carry out the analysis, a novel dataset on local African American elected officials in the eleven states of the former Confederacy is assembled. It is based on digitized information from the National Roster of Black Elected Officials (NRBEO) on African Americans serving on county governments, municipal governing bodies, and school district boards between 1962 and 1980. To identify the effect of the VRA on the election of African American officials, this chapter exploits both the fact that the special measures introduced by the VRA only applied to a group of southern states and also that their potential impact was greater in areas with larger African American population shares, because the race of the candidates was highly salient.[4]

minority candidates. Hence, gains in African American office-holding at the federal and state-level have been linked to the increase in minority-majority districts, which took place more than twenty years after the passage of the VRA (Handley and Grofman 1994). Furthermore, even when African Americans were a majority of the electorate, their chances of winning office were hampered by a white political class ready to use African American vote dilution tactics: switches from single-member districts (SMD) to elections at-large (Trebbi, Aghion, and Alesina 2008), substitution of elective with appointive offices, and discriminatory qualifications for African American candidates in order to prevent their empowerment (Parker 1990).

[3]The VRA was met by the fierce resistance of southern white segregationists. Hence, since its inception, it was clear that the enforcement by the courts would have been essential.

[4]Jurisdictions that both imposed a device restricting the right to vote and also experienced a turnout rate below 50% in the presidential election of 1964 were covered under Section 5 of the VRA. As a result, they were subject to pre-clearance by federal authorities of any change affecting the voting process. Six states (Alabama, Georgia, Louisiana, Mississippi, South Carolina, and

In particular, non-covered counties in the former Confederacy – with a similar history of slavery and African American disenfranchisement – form a comparison group. This chapter thus estimates whether covered counties with a larger pre-existing African American population share (treatment group) – that in the pre-VRA period did not exhibit any substantial difference in African American representation compared to those counties in the control group – experienced a larger increase in African American office-holding, from before to after the VRA, when compared to counties that were not covered. One concern in this triple differences research design is that the share of African Americans in the population was on average larger in covered than in non-covered counties. This imbalance poses two potential threats to the identification of the effects. First, the difference between treatment and control group could be problematic if the effect of the pre-existing share of African Americans on electoral outcomes was purely driven by counties in treated states with African American population shares outside of the common support. Second, heterogeneity on observables increases the sensitivity to potential bias due to unobserved covariates. Since matching on pre-treatment variables reduces both bias and heterogeneity (Rosenbaum 2005), these concerns are addressed by deploying a geographic regression discontinuity (GRD) design, where more homogeneous counties spanning the border between covered and non-covered states are compared.

The baseline triple differences results show that the VRA had a sizable impact on the extent to which enfranchisement led to African American office-holding. The increase in representation was essentially driven by the election of African Americans to county governments (county commissions and enforcement bodies). In particular, counties with larger African American population shares in the treatment group experienced an increase in the share of African American commissioners twice as large as that in the control group. These results also hold when the GRD design is considered. Thus, contrary to anecdotal evidence suggesting that African Americans were only elected to minor offices, this chapter shows that the VRA significantly

Virginia) were fully covered and one state (North Carolina) was partially covered. Furthermore, all covered jurisdictions were forced to remove the literacy test provisions that de facto prevented African Americans from registering to vote.

increased African American representation across county commissions, the most important local governing body in the U.S. South.[5] This effect is also found to vary depending on pre-existing electoral rules: gains in African American office-holding took place only in treated counties where commissions were elected by single-member districts (SMD) rather than at-large. This indicates that the enforcement of the VRA provisions tackling vote dilution tactics (the pre-clearance mechanism) was effective.

In the U.S. South, county governing bodies are entrusted with budget authority: they have the power to levy taxes and appropriate funds for county expenditures on essential public services and infrastructure: roads, sanitation, parks and recreation, housing, urban development, education facilities, public utilities, etc. As a result, the election of African American commissioners, besides enhancing descriptive representation of previously disenfranchised African Americans, could have fundamentally changed their substantive representation in the domain of chronically underprovided local public goods. Since the beginning of the century, poor education facilities (Margo 1990 and Jones 1917) and the lack of basic infrastructure (Valelly 2009), such as paved street, drainage, and street lights had become the hallmark of southern African American communities. As a result, the election of local African American officials was hailed as a vehicle for ameliorating the provision of local public goods that were highly salient for the African American electorate.[6] Models of identity politics provide indeed formal arguments for why descriptive representation should matter for substantive representation (Besley and Coate 1997). Despite the theoretical and intuitive appeal of this proposition, the evidence on the effect of African American office-holding on policy in the U.S. South is still

[5]The effectiveness of the VRA in promoting African American representation was often questioned by civil rights activists and the popular press. For example, in 1976, the March edition of Ebony reported that "most of the [B]lack officials holding county offices have been elected as justices of the peace, constables, or school board members rather than to posts of greater policy making authority." See Poinsett (1976).

[6]As pointed out by Wirt (1997), "many [B]lacks had first wanted their local representatives to be symbolic, that is to be [B]lack like themselves. In time, tough, they wanted representatives to provide individual or group services and to secure the public policies that would provide sufficient resources."

limited.[7] The novel dataset on African American elected officials allows this chapter to systematically assess this hypothesis, by analyzing the link between African American commissioners and local public spending.

To this end, taking advantage of the fact that African American office-holding crucially depends on the share of African Americans enfranchised by the VRA, this chapter investigates how the relationship between the pre-existing share of African Americans and spending by local governments changed over time in counties that were subject to the VRA. Since counties with larger shares of African Americans might have seen changes in spending even in the absence of the VRA provisions that fostered African American representation, in order to isolate the effect of African American office-holding, a difference-in-differences design is implemented, exploiting two sources of heterogeneity: coverage of the VRA and pre-existing electoral rules. In particular, since the VRA led to an increase in the share of African American commissioners only within counties with district-type elections (SMD) then, if the identity of the commissioners mattered, this particular group of counties should have experienced a differential change in the relationship between spending and the African American population. Under the assumption that, had they not elected more African Americans, covered SMD counties would have experienced the same spending patterns as the comparison group, the effect of African American office-holding on spending can be identified. The results of this chapter show that covered SMD counties with larger shares of African Americans in the population – that in the pre-VRA period did not display different spending patterns – by the late seventies experienced a faster growth in local capital spending, which is financed mainly through debt. On the other hand, this study does not uncover significantly different patterns for current outlays that include payments for welfare and salaries, and these results continue to hold when the GRD design is considered. Thus, while counties electing more African American commissioners benefited from increased local public goods provision, they did not receive larger individual transfers. The

[7]For example, Button (1989) documents a positive correlation between the share of elected African American officials and the improvement in street paving and recreational facilities across six communities. For an overview, see Wright (2013).

salience of local public goods provision for the African American electorate, as well as the fact that capital investments – unlike current spending – are largely exempt from balanced budget rules and are essentially financed by borrowing, could provide a rationale for this pattern.

Seven days after the enactment of the VRA, Martin Luther King, Jr., highlighted the crucial role that its provisions were likely to play: "We now have a federal law which can be used, and use it we will." The federal law was indeed used, with immediate effects, in the most staunch segregationist states. In the year before the passage of the VRA, none of the 81 counties in Mississippi had any African American elected official. Three years later, African Americans were in office in 12 counties and, fifteen years down the road, they were serving in 52 counties. The federal scrutiny mandated by the VRA worked: in less than two decades, it transformed the racial make-up of local governments and brought tangible gains in the form of public investments across African American communities in the U.S. South.

The remainder of the chapter is structured as follows. Section 2.2 reviews the related literature, whereas Section 2.3 provides background information on the VRA and its enforcement. Section 2.4 introduces the main features of the dataset used in the analysis, and Section 2.5 presents the main results. Section 2.6 assesses the effect of African American representation on local public spending, and Section 2.7 concludes.

## 2.2 Literature review

This chapter contributes to different streams of the literature. Models of identity politics underscore the importance of descriptive representation as a pre-condition for substantive representation (Besley and Coate 1997). The widespread use of political reservation measures to remedy the under-representation of disadvantaged groups has fostered a lively debate on the merits and drawbacks of prescriptive tools, such as quotas, that could distort political competition (Besley et al. 2017). In the United States, the issue of minority representation has been tackled with a different approach, relying on a legal apparatus to protect the rights of re-enfranchised

African Americans. Although the legalistic approach embedded in the VRA had the advantage of being less intrusive, its effectiveness in promoting descriptive representation has been questioned. In fact, in the post-VRA period, the rise in the number of African Americans elected to state and federal offices has been disappointingly slow, and it has been mainly attributed to the increase in minority-majority districts, which took place more than twenty years after the passage of the VRA (Handley and Grofman 1994).

At the local level, where the journey toward African American representation likely began, systematic evidence remains scant, and we still lack a causal analysis of the impact of the VRA on African American office-holding.[8] In this chapter, we fill this gap using a novel dataset on African American elected officials across all levels of local governments in the former Confederacy, before and after the passage of the VRA. This allows us to study the differential effect of the legislation across local offices, including county commissions that exercise control over local public finances. Most importantly, by deploying a difference-in-differences identification strategy and a geographic regression discontinuity design, this analysis is the first to provide causal evidence on the impact of the VRA on local African American representation.

Our chapter also relates to previous work on the effect of the VRA on public spending. Husted and Kenny (1997) have provided evidence of an increase in state spending on social welfare following the removal of literacy tests and poll taxes provisions. Cascio and Washington (2014), focusing instead on electoral turnout and state transfers to localities, have shown that the extension of the voting franchise led to an increase in transfers toward African American communities, even though

[8]Several studies have analyzed the patterns of elected officials by race throughout the seventies, typically focusing on individual cross-sections of cities. Davidson and Grofman (1994) examine a sample of 1,060 cities in covered states, selected on a combination of population and ethnic composition thresholds. Considering an initial period that varies between 1970 and 1980, and a final period in the late eighties or early nineties, they show that the largest gains in African American representation took place in cities that switched from elections at-large to single-member districts. Marschall, Ruhil, and Shah (2013) analyze a panel of city councils using data from the International City/County Management Associations (ICMA) at five-year intervals between 1981 and 2006 and find that the likelihood of having at least one African American councilor in a municipality, and the number of elected African American councilors, are positively correlated with coverage of the VRA. Marschall, Ruhil, and Shah (2010), focusing instead on a sample of 300 school boards and councils, find that the odds of African Americans winning office increase with elections by single-member districts.

state governments did not see tangible gains in African American office-holding. The latter implies that identity politics can be ruled out as a driving factor of state spending decisions. The analysis of this chapter differs from previous work on the VRA because, by shifting the focus from state to local governments, we show that the identity politics channel indeed matters and can explain a substantial fraction of the increase in county government spending within covered jurisdictions. These results could be rationalized by models of identity politics highlighting the importance of elected representatives' characteristics in shaping policies (Corvalan, Querubin, and Vicente 2020), particularly when they belong to traditionally under-represented groups identified by race, ethnicity, or gender (Pande 2003, Chattopadhyay and Duflo 2004, Besley et al. 2004, and Besley et al. 2017).

This chapter also contributes to the literature on the effects of the voting franchise on spending on either public goods or targeted individual transfers.[9] In the standard set up (Meltzer and Richard 1981), the extension of the franchise leads to a greater demand for redistribution, typically in the form of individual transfers. Exploiting the history of Reconstruction after the American Civil War, Logan (2020) estimates the effect of politician race on public finance, finding that counties with African American politicians had much larger county tax revenues and public goods provision than others. Using spatial and temporal variation in federal enforcement of the VRA, Aneja and Avenancio-Leon (2019) document that minority political influence improved African Americans' relative position through increased public employment, fiscal redistribution, as well as through implementation and enforcement of group-favoring labor market policies. Lizzeri and Persico (2004) emphasize how the extension of the voting franchise can drive efficiency-enhancing growth in public goods provision with diffuse benefits. Consistent with the latter prediction, the results of this study indicate that the VRA increased the provision of public goods (capital spending on infrastructure) rather than targeted individual transfers. This finding corroborates the arguments put forward by Wright (2013) that gains achieved by African Americans did not take place at the expense of white Southerners, since

[9]For an overview of the literature on electoral incentives and public spending, see Ting, Snyder, and Hirano (2018).

## 2.2. Literature review

African American political participation facilitated biracial cooperation towards mutually beneficial goals: for instance, investment on growth-enhancing policies.[10] This "biracial coalition for economic growth" (Wright 2013) might have also played a role in explaining why African American elected officials were influential, even though they typically remained a minority group within local governing bodies.

## 2.3 The 1965 Voting Rights Act

Although the Fifteenth Amendment of the U.S. Constitution explicitly prohibited the denial or abridgment of the right to vote on account of race, it was systematically violated in the U.S. South in the years after Reconstruction. Efforts to address this issue in the post-WWII period were, in the words of Attorney General Katzenbach, blocked by "evasion, obstruction, delay, and disrespect," and required the right to vote to be vindicated "in suit after suit, in county after county," with negligible practical effects.[11] Therefore, a need was identified for "a new approach, an approach which goes beyond the tortuous, often ineffective pace of litigation. What is required is a systematic, automatic method to deal with discriminatory tests, with discriminatory testers and with discriminatory threats."

The 1965 Voting Rights Act (VRA) offered such a method. Special provisions were devised for jurisdictions in which the potential for discrimination was believed to be the greatest. Under Section 4 of the VRA, these jurisdictions were identified as those states and political subdivisions that both imposed a test or device restricting the right to vote and that also experienced a turnout rate below 50% in the presidential election of 1964. Seven states of the former Confederacy (Alabama, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, and Virginia) fell in this category.[12] Under Section 5 of the VRA, these covered jurisdictions

[10] As emphasized by Wirt (1997): "Whites reported that [B]lack empowerment had helped them overturn the old powers and the planters who had blocked racial and economic change."

[11] Statement before the House Judiciary Committee, rendered on March 18, 1965, page 4.

[12] More precisely, six states (Alabama, Georgia, Louisiana, Mississippi, South Carolina, and Virginia) were fully covered and one state (North Carolina) was partially covered (i.e., of its 100 counties, 39 fell under the provisions of the VRA). On the other hand, Arkansas, Florida, Tennessee, and Texas were not covered.

were subject to pre-clearance by either the U.S. District Court for the District of Columbia or by the Attorney General of any change in legislation affecting voting. Furthermore, the Attorney General could dispatch federal examiners and request that federal observers would monitor activities within the polling places. All covered jurisdictions were also forced to remove the literacy test provisions that de facto prevented African Americans from registering to vote.

The VRA represented an unprecedented intervention by the federal government on matters typically reserved to the states by the Constitution, and created a new status quo, dramatically reducing the ability of sub-federal entities to circumvent the Fifteenth Amendment.[13] The enforcement of the VRA was far from smooth. At the state-level, strategic redistricting was used to dilute the African American vote. As a result, by the early 1980s, fewer than a third of the majority African American counties elected an African American state representative. At the local level, redistricting was more difficult, and an intense battle focusing on electoral rules – which played a key role in minority representation – was fought in courts throughout the 1970s. Traditionally, the selection of local officials took place either in elections at-large, where the majority at the local level (e.g., county, city, school boards) elected all the representatives, or in district-based systems, where representatives were instead chosen in districts or local wards.[14] As elections by single-member districts (SMD) came under attack, by being more favorable to minorities' candidates, the legal tool of pre-clearance became crucial to block this type of vote-diluting practices. Switches toward elections at-large were successfully challenged in court, and covered states could retain SMD-type elections.

Right after the passage of the VRA, African American elected officials began to serve on county governments, municipalities, and school boards, where virtually no

[13]In fact, its constitutionality was immediately but unsuccessfully challenged in *South Carolina v. Katzenbach* (383 U.S. 301, 1966). Coverage provisions have remained in place until 2013, when the Supreme Court struck down Section 4 as unconstitutional in *Shelby County v. Holder* (570 U.S. 529, 2013).

[14]City councils and school boards were predominantly elected at-large, whereas SMD elections were more common for county commissions. Some local governments also adopted proportional representation systems earlier in the twentieth century, but discarded them shortly thereafter. See Trebbi, Aghion, and Alesina (2008) for more details.

African American had been in office since the times of Reconstruction. Reports of African American office-holding gains started to regularly make the headlines of local newspapers, suggesting that the enfranchisement was fundamentally changing the make-up of locally elected bodies.[15] While bringing African Americans into office was one of the most important objectives of the VRA, the evidence on its immediate effect remains largely anecdotal: the goal of this chapter is to address this gap.

## 2.4 Data

The objective is to study the link between the enfranchisement of African American voters in the U.S. South and their ability to elect officials at the local level. In particular, this chapter empirically assesses whether counties with larger African American population shares experienced a significant change in the racial composition of their local governing bodies. Additionally, it investigates whether electoral rules played a role in the election of officials and whether representation also affected public finances. To carry out this analysis, a rich dataset has been built.

### 2.4.1 Local African American elected officials

While biographical data on members of the U.S. House of Representatives and Senate are easily available from the congressional directories, similar information on local elected officials at the county, municipality, and school board level is much more difficult to obtain, and has not been systematically collected for the period covered in this study.

In the context of a broad effort to promote African American political engagement, in the eve of the VRA, the Southern Regional Council's Voter Education Project started to record data on African Americans elected to public office, both at the national and at the local level. As a result, the National Roster of Black

[15]For instance, just two years after the passage of the VRA, the Voter Education Project reported: "A little over four years ago, not a single Negro was registered to vote in West Feliciana Parish in Louisiana. ... Today Negro registration totals over 2,000, or 56% of the parish's total registration. ... Last year, two Negroes were elected to the school board. This year, challenging political control of a parish in which violence and intimidation have not been uncommon, 17 Negro candidates awaited the November primary. When the results were in, 6 of the candidates had won. Having the vote obviously makes a difference in West Feliciana Parish."

Elected Officials (NRBEO) began to be published as a directory in 1969, reporting the name, the office held, and the address of all African American elected officials. The NRBEO has been subsequently updated at a yearly frequency by the Joint Center for Political Studies, becoming the most authoritative source of data on African American elected officials in the United States.[16]

The directory of the NRBEO has been built with information obtained from a variety of sources. First, questionnaires were mailed to all known African Americans holding local offices (i.e., those listed in the previous year's directory), asking them to verify the records available to the Joint Center for Political Studies. In addition, they were requested to give the expiration date of their current term in office and to provide the names of any other African American officials they knew. All newly acquired names of African American elected officials were then verified via phone calls to the appropriate local jurisdiction. Approximately 60% of the questionnaires were returned in a typical year. When questionnaires were not returned, phone calls to verify the existing data were made to a variety of local administrative sources. Furthermore, the research staff of the Joint Center for Political Studies regularly reviewed news clippings to gather information on African American office-holders. In addition, government offices, associations of public officials, state offices of major political parties, and organizations concerned with African American political participation periodically provided names of new office-holders. Even if the goal of the NRBEO has been to provide comprehensive information on African American elected officials at the national, state, and local levels, the data collection methodology has improved over time.[17] As a result, under-counting of African

[16]The NRBEO directory has routinely been used in U.S. Census publications (e.g., the Statistical Abstract of the United States), since the U.S. Census began collecting information on African American elected officials only starting in 1987, through the Census of Government.

[17]As reported in the 1971 publication: "As of March 1971, according to our records, the Roster is up to date and comprehensive. However, in our effort to compile the information, we became aware of the possibility that [B]lack elected officials in some parts of the nation might not have come to our attention and, therefore, could be missing from the Roster. Such may be the case even though we have checked and double-checked the sources and have received timely assistance from many individuals and organizations in amassing the voluminous data."

American elected officials is more likely to have occurred during the first years of data collection when the system for gathering information was still being perfected.[18]

The NRBEO is only available in paper format. Thus, to carry out the analysis of this chapter, county-level counts of African American elected members of county governments (commissions, judicial and law enforcement bodies, and other local offices), municipalities, and school districts have been constructed and digitized for the eleven states of the former Confederacy.[19] For the period prior to the introduction of the VRA, no systematic effort to identify local African American elected officials had instead been carried out, "the chief reason being that such a phenomenon was virtually unknown" (Voter Education Project 1969). Still, in some constituencies, African Americans did run for office and were elected before 1965. Using information from documents written by the Southern Regional Council and from local newspapers' archives, data have been collected also for 1962 and 1964. While some measurement error is unavoidable, these figures are consistent with the aggregate counts that were published at the time (Voter Education Project 1969). Thus, combining these different sources, information on African American elected officials across all local offices between 1962 and 1980 is available for the eleven states of the former Confederacy.

The data indicate that, in 1964 (the year immediately prior to the passage of the VRA), a total of 67 African American elected officials were in power: 56 held positions at the local level, whereas 11 had been elected to State Houses and Senates. By 1980, this number had increased more than thirty-folds. As a result, there were 2,265 African American elected members of county governments, municipalities, and

[18]To verify the quality of the information from the NRBEO, we have compared it with the Census of Government 1987, which is the official publication closest to our period of study reporting a breakdown of local elected officials by race. Restricting our attention to county-level officials (where both the NRBEO and the Census use the same geographical unit of observation), and to the 1987 directory of the NRBEO (which matches the survey period used in the Census), we find that, out of 1,023 entries (the Census did not include information for the 75 counties in Arkansas), an exact match between the two sources is obtained 894 times (or 87.4%). Importantly, differences between unmatched observations by coverage status are not statistically significant at conventional levels.

[19]The NRBEO does not report information on the members of school boards in Virginia, where school systems are considered agencies of city and county governments and are administered by appointed boards.

school boards, and 142 African American state representatives and senators. Figures 2.1 and 2.2 illustrate the geographic patterns of African American representation at the county-level in 1964 and 1980. In 1964, only 23 counties out of a total of 1,102 (or 2% of the total) had at least one African American elected official. However, no clear geographic pattern could be identified. On the other hand, by 1980, 488 counties (or 44% of the total), had at least one African American elected official. By the end of the sample period, counties electing African Americans were also clearly concentrated in covered states (65% of the total). Furthermore, as shown in Figure B.1.1, by 1980, the geographic distribution of African American representation closely followed the distribution of African Americans in the total population.

Focusing on the year prior to the passage of the VRA and on the last year of the sample, Table B.1.1 reports summary statistics for the variables used in this analysis, distinguishing between counties that were covered or not covered in 1965.[20] The main variable of interest is the share of African Americans elected to each local office, which is defined as:

$$ShareBlackElected_{c,o,t} = \frac{BlackElected_{c,o,t}}{Elected_{c,o,t}} \tag{2.1}$$

where $c$, $o$, and $t$ denote county, office, and year, respectively. For the numerator the information collected from the NRBEO is considered, while for the denominator the Census of Governments of 1967 and 1977 is used.[21] The latter reports the total number of elected officials for county governments (commission, judiciary and law enforcement, and other local offices), municipalities, and school boards, aggregated at the county-level. Unfortunately, there is no separate information available for each county government category. As a result, to compute the share of African

[20]The set of covered counties includes all the counties in the fully covered states (Alabama, Georgia, Louisiana, Mississippi, South Carolina, and Virginia) and the 39 counties in North Carolina that were also subject to the VRA in 1965.

[21]We use the 1967 figure as the denominator for the congressional years up to 1972, and the 1977 figure thereafter. The average number of all local elected officials slightly declined between 1967 and 1977. In particular, it decreased from 25 to 22 for county governments and from 18 to 17 for school districts, whereas it increased from 26 to 27 for municipalities. The variation in the overall number of all local elected officials is modest, and follows the same pattern in covered and non-covered states.

Americans in each different type of county government, we use the total number of elected officials in county governments as the denominator.[22]

As shown in the top panel of Table B.1.1, in covered counties, the proportion of African Americans across all local government levels rose from 0.1% immediately before the VRA to 7.0% by 1980. The increase was much more modest in counties that were not covered, where the average share of African American elected officials in the last year of our sample was only 2.2%. As for specific levels of government, the data indicate that the largest increase is to be observed across school districts in covered counties. From a situation in which there were no African American elected officials in 1964, there were 5.8% by 1980; this change was much larger than the one observed in counties not covered by the VRA, where the increase was from 0.1% in 1964 to 1.4% in 1980. As for municipalities in covered counties, there was an increase in African American representation from 0.2% in 1964 to 5.4% in 1980; in non-covered counties, the corresponding change was from close to 0% to 1.9%. Lastly, African American representation across county governments in covered counties went from 0% in 1964 to 4.1% by 1980, while the corresponding figure in 1980 was only 0.6% for non-covered counties. Of the increase in representation across county governments, 60% is accounted for by county commissions, 30% by judicial and law enforcement bodies, and 10% by other local county officials.

### 2.4.2 Other control variables

The control variables that are used in the analysis are summarized in the bottom panel of Table B.1.1. The set of counties subject to the VRA were characterized by a much higher African American population share than those counties that were not covered (32.4% v. 13.2%). Covered counties were also smaller, less urban, and had a lower agricultural productivity.[23] As for the counties' economic characteristics, they shared similar unemployment rates (5.0% in covered counties, 4.9% in non-covered

[22]For example, the share of African Americans in county commissions is given by the ratio between the number of African American commissioners and the total number of elected officials in county governments.

[23]As cotton was the dominant crop in the South, we use county cotton suitability as a measure of agricultural productivity.

ones), and poverty was very widespread: 46% of the population in covered counties lived in households falling below the poverty line, and the corresponding figure for non-covered states was only slightly lower at about 44%. The vast majority of the population was unskilled: 74% and 71% in covered and non-covered counties, respectively.[24] Finally, counties in covered states were characterized by greater racial tensions, as measured by the number of episodes of both pro-African American and anti-African American demonstrations that occurred between 1960 and 1964.

Summing up, the counties in covered and non-covered states differed in terms of several observable characteristics, and some of these differences (in particular, the share of African Americans) are large and statistically significant. For this reason, besides directly accounting for these differences in the empirical specifications, in Section 2.5.4, the robustness of the findings is assessed by implementing a geographic regression discontinuity (GRD) research design. GRD takes advantage of the fact that counties spanning the border between a covered and a non-covered state are more similar than a pair of counties taken at random, also with respect to the share of African Americans in the population.

## 2.5 The VRA and electoral outcomes

This section analyzes the effect of the VRA on electoral outcomes, starting with a discussion of the main identification strategy. Then, it turns to the presentation of both an event study and a long-run difference specification. The GRD design is further considered in order to address potential threats to the identification of the effects. The role of electoral rules is investigated last.

### 2.5.1 Estimation strategy

Before 1965, seven states of the former Confederacy (Alabama, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, and Virginia) adopted restrictions to the franchise (in particular, literacy tests), which were administered in a

[24]This variable is measured as the share of 25 years old or more with less than a high school diploma.

discriminatory fashion to disproportionately affect African American voters. The VRA, together with the removal of literacy tests, brought in special provisions (known as coverage), which were targeted at those jurisdictions where the potential for discrimination was believed to be the greatest. As the VRA was meant to protect African American voters from the infringement of their political rights, its impact is expected to be greater in areas where the share of African Americans in the total population was higher.

In principle, the special measures introduced by the VRA did not need to result in an increase in support for African American candidates, as the newly enfranchised African American voters did not have to cast their ballot along racial lines. However, the VRA came into place in a context of extreme racial tensions, where the election of African American candidates had a highly symbolic value. The importance of the race dimension in the electoral context of the post-VRA period is epitomized by the words of the first African American running for office in Edgefield County, South Carolina: "There's an inherent value in office-holding. ... A race of people who are excluded from public office will always be second class."[25] Thus, given the salience of race, the effect of the VRA on African American office-holding at the local level should depend on the county pre-existing share of African Americans. Hence, a straightforward way to estimate the effect of the VRA on African American representation would be to investigate how the relationship between the pre-existing share of African Americans in the county population and the share of African Americans elected across local governments changed over time within states that were subject to the VRA. In particular, if the legislation had an effect, the slope of this relationship ought to increase around the time of the passage of the Act.

One important problem with this type of strategy is that areas with larger shares of African Americans in the population might have seen increases in African American representation even in the absence of the specific provisions included in the VRA. For example, civil rights activism could have led to greater African American efforts to cast a ballot in local elections, even in the absence of coverage,

[25]See Wright (2013).

and this could have led to an increase in African American representation. For this reason, following Cascio and Washington (2014), the strategy discussed above is augmented with the creation of a comparison group, which includes the four states of the former Confederacy (Arkansas, Florida, Tennessee, and Texas) and the 61 counties in North Carolina, with a similar history of slavery and African American disenfranchisement, which were not subject to the VRA in 1965. In other words, this chapter estimates whether covered counties with a larger pre-existing African American population (treatment group) experienced a larger increase in African American representation – from before to after the VRA – when compared to the counties of the other former Confederate states that were not covered (control group). The main identifying assumption is that, in the absence of coverage, the two groups – that in the pre-VRA period did not exhibit any substantial difference in African American representation – would have experienced the same trends in the election of African American officials.

### 2.5.2 Event study

To illustrate the evolution of the relationship between the share of African American elected officials and the pre-existing share of African Americans in the county population, Figure 2.3 plots the coefficients obtained by regressing the share of African American office-holders in the congressional election years on the 1960 share of African Americans, separately by year and treatment status. State fixed effects – so that the coefficients capture within-state effects – and a number of other pre-VRA county characteristics – the unemployment rate, the share of families below the poverty line, the share of unskilled workers, a measure of agricultural productivity, the population and the share of its urban component, and occurrences of both pro- and anti-African American activism – are also included across all specifications. The top-left panel of Figure 2.3 presents the results for all local elected officials; turning clockwise, it focuses on county governments, municipal governments, and school boards, respectively.

The estimates indicate that, in the period before the passage of the VRA, the relationship between the share of African American elected officials and the 1960 share of African Americans in the county population was not different from zero for either the treatment or the control group. After the passage of the VRA, a different pattern emerges: the relationship becomes positive, and for counties in the treated group it is steeper than for those in the control group. The differential change in slope is evident already in the 1968 election, where a 10% increase in the 1960 share of African Americans is associated with a 0.6% increase in the share of African American elected officials in covered counties and a 0.4% increase in those counties that were not subject to the VRA. By 1980, a 10% increase in the 1960 share of African Americans is associated with an increase of around 2.5% (1.5%) in the share of African American elected officials in covered (non-covered) counties. The change in the slope of this relationship is more striking when considering county governments, whereas the pattern is less clear for school boards and municipalities.

To investigate the significance of this differential pattern for the various offices, the following model is estimated:

$$\begin{aligned} ShareBlackElected_{c,s,t} &= \sum_{n>1962} \gamma_n D_n^t ShareBlack_{1960} + \\ &+ \sum_{n>1962} \theta_n D_n^t \, ShareBlack_{1960} \times VRA_{c,s} + \\ &+ \mathbf{X'}_{c,s}\beta + I_{s,t} + I_c + \epsilon_{c,s} \end{aligned} \tag{2.2}$$

where $ShareBlackElected_{c,s,t}$ is the share of African American elected officials, $ShareBlack_{1960}$ is the share of the African American population in 1960, $VRA_{c,s}$ is an indicator taking a value of one if the county was covered in 1965, $D_n^t$ is an indicator taking a value of one if $n = t$, $\mathbf{X'}_{c,s}$ is the vector of pre-VRA controls introduced above, $I_{s,t}$ are state-year interactions, and $I_c$ are county fixed effects. In all the specifications, standard errors are clustered at the county-level. And since county fixed effects are included, the interactions with the first year of the sample are omitted in order to identify the model. That is, year 1962 is used as the reference for evaluating how the slope of the relationship between the share of

African American officials and the 1960 share of African Americans in the total population changed over time.

In Figure 2.4, the estimated coefficients $\theta_n$, which capture the difference in the gradient between covered and non-covered counties, are plotted. As shown in the top-left panel, which considers all local elected officials, the treatment-control difference is broadly positive, statistically significant, and increasing over time. This overall pattern is very clearly driven by county governments. On the other hand, across school boards and municipal governments, significant differences by treatment status are not observed.

### 2.5.3 Long-run difference specification

The evidence presented so far is consistent with the idea that coverage had an effect on the election of African Americans at the local level, in particular across county governments. To summarize the main findings, and later on to assess the robustness of the results, this chapter now turns to a triple differences model à la Cascio and Washington (2014), in which data from two periods are used, one before the introduction of the VRA (1964) and one following the policy (1980). Data for the years in between is omitted mainly because, as pointed out before, the collection of information on African American elected officials carried out through the NRBEO significantly improved over time.

More precisely, a long-run difference model is considered:

$$\begin{aligned} \Delta ShareBlackElected_{c,s} &= \gamma ShareBlack_{1960} + \\ &+ \theta\, ShareBlack_{1960} \times VRA_{c,s} + \mathbf{X'}_{c,s}\beta + I_s + \epsilon_{c,s} \end{aligned} \tag{2.3}$$

where the dependent variable $\Delta ShareBlackElected_{c,s}$ is the change in the share of African American elected officials in county $c$ of state $s$ between 1964 and 1980, and $I_s$ are state fixed effects, which capture a state specific trend in this long-run difference specification. $\mathbf{X'}_{c,s}$ is a vector of pre-VRA county characteristics, including the unemployment rate, the share of families below the poverty line, the share of unskilled workers, a measure of agricultural productivity, the population

and the share of its urban component, and occurrences of both pro- and anti-African American activism. The coefficient $\gamma$ captures the within-state change in the gradient of the 1960 African American population share in the control group, whereas $\gamma + \theta$ does the same in the treatment group. As before, the interest is in the difference between the two: the coefficient $\theta$.

The main identifying assumption is that, in the absence of coverage, African American representation would have changed in the same way across treated and non-treated counties. In the event study, the absence of significantly different pre-VRA trends in African American office-holding between the two sets of counties is reassuring for the identification strategy. At the same time, southern engagement with civil rights issues had been growing since the aftermath of WWII. Thus, despite the absence of pre-VRA trends in African American elected officials, covered counties with larger shares of African Americans might have experienced different long-run trends in other important outcomes related to the civil rights movement, which could cast doubts on the suitability of the chosen non-covered counties as the control group. To assuage these concerns, Table B.2.1 presents a series of placebo exercises focusing on long-run pre-VRA trends in African American political activism, anti-civil rights sentiment, and turnout rates.[26] While it is generally the case that, in the pre-VRA period, counties with larger shares of African Americans did indeed experience a significant increase in African American political activism, anti-civil rights sentiment, and turnout rates, this pattern is the same for both covered and non-covered counties.

Turning now to the long-run analysis of African American office-holding, Table 2.1 presents the baseline regressions, focusing on the change in the share of all African American officials elected across county governments, municipalities, and

[26]The measure of African American political activism is given by the change in the National Association of Colored People (NAACP) local branches between 1964 and 1940. For the same period, the change in the number of Ku Klux Klan (KKK) klaverns is used as a proxy for the long-run trend in anti-civil rights sentiment. Furthermore, since in the 1964 presidential election, the republican candidate Barry Goldwater ran on a largely anti-civil rights platform, the change in support for Goldwater in 1964 compared to Eisenhower in 1952 is used as an alternative proxy for the evolution of anti-civil rights sentiment. Lastly, the change in turnout rates between the presidential elections of 1964 and 1952 is used to capture the overall long-run trend in political participation in the pre-VRA period.

school districts. Column (1) reports the findings from a parsimonious specification, which controls only for the share of African Americans in 1960 and its interaction with the coverage dummy. Counties with a larger pre-VRA African American population elected a larger proportion of African American officials, and the change from before to after the VRA is significantly larger in the treatment group than in the control group. In column (2), the specification is augmented with economic and demographic controls, as well as state specific trends to account for unobserved, state-specific time-varying shocks. Counties with higher pre-VRA unemployment rates and a more urban population tend to elect a greater share of African American officials, whereas the opposite is true for counties with larger shares of unskilled workers. On the other hand, poverty, population, and agricultural productivity do not display significant effects. Importantly, including these additional controls does not affect the sign and significance of the main findings on the effects of the VRA.

Lastly, in order to account for the increase in racial tensions recorded in the years prior the the passage of the VRA, two measures, which are respectively based on the counts of pro- and anti-African American events that occurred between 1960 and 1964, are included. The results reported in column (3) indicate that a larger occurrence of pro-African American demonstrations in the immediate pre-VRA period is correlated with a higher share of African Americans subsequently elected to local public office. At the same time, counties characterized by more anti-African American protests between 1960 and 1964 tend to subsequently elect a lower number of African Americans to office, even though this effect is not statistically significant. In the last specification of Table 2.1, the impact of the control variables is allowed to vary between covered and non-covered counties, in order to rule out the possibility that the patterns that have been identified might be driven by other sources of heterogeneity among counties in covered states. Importantly, the main results are robust.

How large is the effect of the VRA on the change in the share of local African American elected officials fifteen years after its introduction? The preferred specification in column (4) of Table 2.1 indicates that a 10% increase in the 1960

share of African Americans leads to a 1.9% increase in the share of African American elected officials in non-covered counties and an additional 1.3% in covered counties.

In the upper panel of Table 2.2, the effect of the VRA on African American representation across different types of local offices is investigated using the same specification as in column (4) of Table 2.1. County governments are considered in columns (1)-(3), municipal governments in column (4), and school boards in column (5). The estimation results in Table 2.2 confirm the findings that were uncovered in the event study using all years between 1964 and 1980. As shown in columns (1)-(3), the VRA led to an increase in African American representation across all county governments, although the effect is much larger for law enforcement bodies and county commissions. On the other hand, the VRA did not produce larger gains in African American office-holding across school boards, column (4), and municipalities, column (5).

Thus, analyzing African American office-holding in all local elected offices across southern states, the following conclusions are reached. First, while the gains among judicial and law enforcement bodies are consistent with anecdotal evidence of African American officials being elected to relatively minor offices (including justices of the peace, constables, and clerks), this chapter finds that African American representation also increased in the most powerful local government in the U.S. South: county commissions. Importantly, the VRA doubled the extent to which African American enfranchisement led to office-holding among commissioners. Second, while in the first fifteen years, the VRA was effective in increasing African American representation in county governments, the same is not true for municipalities and school boards. This finding is consistent with previous accounts of African American representation attributing the slow progress in African American office-holding in covered municipalities and school boards to the fact that both inherited from the Progressive Era electoral rules unfavorable to minorities, which remained in place throughout the 1970s.[27]

[27] Although systematic data for the elections of school boards and municipalities are not available, surveys on selected samples indicate that the majority of school districts and municipalities held at-large, non-partisan, and off-cycle elections, usually associated with low turnout rates (Marschall,

### 2.5.4 Geographic regression discontinuity research design

The baseline results show that the VRA significantly increased the extent to which the enfranchisement of African Americans led to African American office-holding across county governments. The main identifying assumption is that, in the absence of coverage, counties in all former Confederate states would have followed the same pattern in the election of African Americans. In fact, in the pre-VRA period, conditioning on African American population shares, treatment and control group – which shared a similar history of discrimination and African American disenfranchisement – did not display different patterns in either African American office-holding or other measures of political participation and civil society's engagement with racial issues. However, the fact that the two groups differ along important economic and demographic characteristics may raise further concerns.

In particular, the 1960 share of African Americans in the total population was substantially larger in covered counties than in non-covered ones. And although there is enough variation in the share of African Americans to ensure the existence of a sufficient degree of common support for the identification of the effects, this imbalance poses two potential threats to the identification. First, the difference between treatment and control groups could be problematic if the effect on electoral outcomes was purely driven by counties in covered states with African American population shares outside the common support: for instance, in the top quintile of the distribution. Second, heterogeneity on observables increases the sensitivity to potential bias due to unobserved covariates. Since matching on pre-treatment variables reduces both bias and heterogeneity (Rosenbaum 2005), these concerns are addressed by deploying a geographic regression discontinuity (GRD) research design that compares more homogeneous contiguous counties spanning the border between

Ruhil, and Shah 2010). These rules persisted over time since legal challenges to elections at-large during the 1970s focused mainly on the most important county governments. In particular, using data on challenges to elections at-large in covered states from Davidson and Grofman 1994, we find that, between 1965 and 1980, the average incidence of challenges to elections at-large – computed as the ratio between the number of cases and the units of local governments affected – is 15% for counties but only 5% for municipalities. After 1980, legal actions at the municipal level increased, with an average incidence of cases of 13%, suggesting that the effect of the VRA on African American representation at the municipal level did take longer to materialize.

covered and non-covered states. In fact, as shown in Figure 2.5, the difference in average African American population shares between treatment and control group is not statistically significant for border counties (those whose centroid is located at less than 25 miles from the state boundary). Counties sharing a border between covered and non-covered states are more similar to each other also with respect to the other covariates used in the model. To see this point, Figures 2.6 and 2.7 plot the coefficients of the treatment variable obtained from within regressions run on the 295 county-pairs in the border sample. County-pairs do not exhibit statistically significant differences either in the values of pre-VRA covariates (Figure 2.6) nor in their pre- and post-VRA trends (Figure 2.7).

Thus, combining the difference-in-differences estimation strategy with a GRD research design, this chapter estimates whether covered counties with a larger pre-existing share of African Americans experienced a larger increase in African American office-holding than non-covered counties with which they share a border, from before to after the passage of the VRA:

$$\begin{aligned} \Delta ShareBlackElected_{c,p,s} &= \gamma ShareBlack_{1960} + \\ &+ \theta\, ShareBlack_{1960} \times VRA_{c,p,s} + \mathbf{X'}_{c,s}\beta + I_{c,p} + \epsilon_{c,p,s} \end{aligned} \tag{2.4}$$

where $\Delta ShareBlackElected_{c,p,s}$ indicates that county $c$ can be repeated for all pairs $p$ it belongs to, straddling a common state boundary. The specification includes county-pair fixed effects $I_{c,p}$ (i.e., county-pair specific trends in the long-run specification). Hence, the effect of the VRA on African American representation is identified out of the variation within county-pairs spanning the border between covered and non-covered states. The bottom panel of Table 2.2 presents the GRD estimation results. Despite the significant reduction in sample size, the main results are broadly robust: compared to the neighboring non-covered counties, covered counties experience a significantly larger increase in the share of African American county commissioners, judiciary, and law enforcement officers. As for school boards, the effect of the VRA is only marginally significant, while African American representation in municipalities is not affected by the VRA.

The robust finding on county commissions is particularly meaningful because they represent the most important unit of local government in the U.S. South. Thus, in the remainder of the chapter, the focus is restricted to this office. First, the mechanism by which the VRA increased African American representation is further explored, by focusing on the role of electoral rules. Then, the chapter assesses whether coverage, by increasing African American representation in county commissions, brought tangible gains to African American communities in the form of increased public spending.

### 2.5.5 Electoral rules and African American representation

As discussed in Section 2.3, covered counties displayed significant differences in the rules shaping the election of county governing bodies. In particular, at the time of the passage of the VRA, slightly more than half of covered counties chose their governing bodies by single-member districts (SMD), while the others adopted either elections at-large or a combination of the two systems. Theoretically, the effect of electoral rules on minority representation depends on the size of the group (Trebbi, Aghion, and Alesina 2008). Elections at-large are less favorable to minorities when these represent a small share of the total population, because their vote tends to be diluted by this system. In this context, SMD-based elections are instead more desirable. As the share of minority voters increases, confining them to minority-majority districts will instead tend to reduce their overall ability to gain representation, making elections at-large preferable. In practice, as turnout rates are significantly lower for minorities than for the rest of the population – and consistently with the insights from Trebbi, Aghion, and Alesina (2008) – SMD-based elections have come to be seen as more likely to promote African American representation than at-large systems (Davidson and Grofman 1994).

Given these arguments, it is not surprising that the VRA paid particular attention to electoral rules and the potential changes to them. In particular, through the pre-clearance requirement, covered counties with existing electoral systems favorable to minorities had a powerful tool to retain them. That the fate

of the VRA would largely depend on the enforcement of its key provisions was very clear to the civil rights leadership.[28] At the same time, more skeptical views of the VRA have questioned its effectiveness pointing out that "democracy can not be achieved simply by formal legal manipulations of the sort embodied in the Voting Rights Act" (Salamon and Evera 1973).

To shed light on the importance of electoral rules and the enforcement of the VRA surrounding them, this chapter explores whether covered counties with pre-existing electoral rules favorable to the election of minorities (SMD) experienced larger gains in African American representation. To address this question, information has been collected on the electoral rules of county governing bodies as reported by the 1957 Census of Governments and the 1980 National Roster of Black Elected Officials.

Focusing on the electoral rules that were in place when the VRA was passed, this chapter distinguishes between counties belonging to states that elected their county governing bodies by single-member districts (Lousiana, Mississippi, and Virginia) as opposed to at-large (Georgia) or mixed systems (Alabama, North Carolina, and South Carolina). By 1980, thanks to the enforcement of pre-clearance, all three covered states that adopted SMD before the VRA continued to do so, whereas three covered states (Alabama, Georgia, and South Carolina) were on a transition path toward SMD and one (North Carolina) retained a mixed system.[29]

Column (1) of Table 2.3 analyzes whether covered counties that retained their pre-VRA SMD system displayed a different pattern compared to the other counties. The main coefficient of interest is the triple interaction between the share of African Americans in 1960, the VRA indicator, and the SMD indicator. The estimates show

[28]As pointed out by Martin Luther King, Jr.: "To become a major turning point in American life, extensive and dynamic enforcement by the Justice Department is indispensable. ... by bold enforcement, the recalcitrance of the segregationists can be made as impractical as it is illegal and immoral" (King 1965).

[29]The transition toward SMD was prompted by the interpretation of the VRA by courts, which broadened the scope for legal action against vote diluting practices, thus creating the grounds for a shift toward electoral systems more favorable to the election of African Americans. Two influential court cases played a particularly important role. In 1973, the Supreme Court ruling on *White v. Regester* (412 U.S. 755, 1973), and its subsequent application by the Fifth Circuit Court of Appeals in *Zimmer v. McKeithen* (485 F.2 d1297), laid out the standards for evaluating whether at-large elections diluted minority voting strength. For an overview of court cases, see Kousser (1991).

that the interplay between VRA and SMD is key to the election of African American officials since the policy led to an increase in African American office-holding only when county commissioners were elected by SMD.[30]

As court action in the post-VRA period also led to the transition toward SMD in three covered states, column (2) presents the results of an additional specification that further decomposes the effect of the VRA, distinguishing between the early adopters of SMD and those that switched toward it. The results show that covered counties that switched toward SMD also experienced additional gains in African American office-holding, albeit smaller than earlier adopters, suggesting that court action behind the switch played an important role. One concern with this interpretation is that court cases might have taken place in states with a more favorable attitude toward minorities, implying that, even without court action, counties with larger African American population shares would still have elected more African Americans. Yet, the only state that lagged behind in legal challenges to elections at-large was North Carolina, by far the most progressive among the set of covered states. Thus, in the absence of a change in electoral rules, the pattern in African American representation should be the opposite than the one we observe: everything else equal, counties in the most progressive covered state (North Carolina) should have experienced larger gains in African American office-holding than those in the less progressive ones. Still, the evidence from the switch toward SMD should be interpreted only as suggestive.

The results on the heterogeneous effect of the VRA based on pre-existing electoral rules imply that pre-clearance provisions, protecting pre-existing SMD arrangements, helped to promote growth in African American representation. Thus, contrary to the more pessimistic views that the VRA could not address the subtle and powerful obstacles to African American political representation entrenched in a culture of white supremacy, this chapter indicates that the legal tools it provided were crucial to advance African American political empowerment. To be clear, non-legal barriers to African American political participation were formidable. As "the notion that

[30]The results are also robust to trimming the sample to its common support (Table B.2.2) and to restricting the analysis to the border sample (Table B.2.3, Panel B).

politics was a white folks business had been prevalent in [B]lack communities since the turn of the century" (Lewis and Allen 1972), the challenge that civil rights organizations faced went beyond simply enrolling African American voters on electoral registers or bringing about legal challenges to insure the implementation of the VRA. Thus, the enforcement of pre-clearance, beyond its mechanical effect, might have been crucial to give African American voters confidence in their ability to change the racial make-up of elected offices.

## 2.6 Local public spending

The results presented so far have shown that the VRA, by fundamentally changing the make-up of the electorate in the U.S. South, produced an important shift in the characteristics of individuals elected to local offices. The analysis has also highlighted that the specific measures put in place by the VRA were crucial to its success. The fact that, in just a little over a decade, a minority group, previously banned from the voting booth, was able to elect minority candidates into office is per se an important achievement. The election of African Americans had a high symbolic value as, in the words of civil rights activist Laurence Guyot, it represented "a bit of [B]lack authority, a gradual return to respect for those accustomed to having their lives manipulated by white hands."[31]

Over time, African American office-holding brought bigger expectations in the domain of policy toward deprived African American communities, suffering from chronic under-provision of local public goods.[32] In fact, as pointed out by Button (1989), "[B]lack citizens often had high expectations that their [B]lack representatives would improve their streets and parks quickly and measurably." This section provides a first systematic attempt at determining whether electing more African Americans to county governments led to an improvement in the provision of local public goods.

[31] See Wright (2013).

[32] Poor African American communities in the South often lacked basic infrastructure, such as paved street, were deficient in the areas of sanitation, drainage, sidewalks, and street lights (Valelly 2009), and their school facilities have been described as "miserable beyond all description" (Jones 1917). They also suffered from limited access to parks, playgrounds, and recreational facilities, which were mostly reserved to whites (Button 1989).

In the U.S. South, county commissions play a dominant role among local governments as they preside over a number of very important functions (Wager 1950).[33] In particular, they have the power to levy taxes and appropriate funds for expenditures on essential public services and infrastructure (roads, sanitation, parks and recreation, housing, urban development, public utilities, etc.), with county governments accounting for more than half of the overall local spending.[34]

Hence, to study the relationship between African American representation and local public spending, this chapter considers information from the Census of Government on current and capital spending by county governments, which are available at five-year intervals since 1957. As shown in Table B.1.2, real spending per capita (in 2000 USD) in 1957 is about 30 USD higher in non-covered counties than in covered ones. Most of this gap (equivalent to about 10% of the average total spending across non-covered counties) is due to current spending, since the difference in capital spending is less than one dollar. Interestingly, by the early 1980s, the spending differential in total and current spending had virtually disappeared, whereas capital spending per capita is about 4 USD higher in covered than in non-covered counties. These changes may be due to a variety of factors and the objective is to isolate the effect of African American representation.

Since African American office-holding essentially depends on the share of African Americans enfranchised by the VRA, a straightforward way to estimate the effect of the VRA on spending through the African American representation channel is to investigate how the relationship between the pre-existing share of African Americans in the county population and spending by local governments changed over time within covered states. In particular, if African American representation

[33]Historically, the southern model for local governments was inspired by the so-called Virginia Plan, where towns and townships were absent, and nearly all power resided in county courts. Townships made their appearance in the U.S. South when they were forced upon North Carolina and South Carolina after the Civil War. However, as pointed out by Wager 1950, "the rural South never became accustomed to a unit of government smaller than the county."

[34]For instance, 56% in the first year of our sample (1957) and 52% in the end of our sample period (1982). For an overview of expenditure functions at the county-level, see Advisory Commission on Intergovernmental Relations (1971).

had an effect, the slope of this relationship would be expected to increase after the passage of the Act.

As pointed out before, one important problem with this type of strategy is that areas with larger shares of African Americans in the population might have seen increases in spending even in the absence of the specific provisions included in the VRA, whether or not these provisions affected African American representation. To overcome this identification problem, a difference-in-differences design is again implemented, exploiting two sources of heterogeneity: the VRA indicator and the SMD indicator. Once again, this chapter studies whether covered counties with a larger pre-existing share of African Americans in the population (treatment group) experienced a larger increase in spending – from before to after the VRA – when compared to counties in the other former Confederate states that were not covered (control group). Moreover, as gains in African American office-holding depended on pre-existing electoral rules, a quadruple difference design is considered in order to disentangle the effect of the African American representation channel. In particular, since, as shown before, coverage leads to an increase in the share of African American commissioners only within counties with pre-existing SMD, then – if the VRA affects spending through the representation channel – this particular group of counties should experience a different spending pattern from before to after the VRA. Thus, the main coefficient of interest is the interaction between the share of African Americans in 1960, the VRA indicator, and the SMD indicator.

The results of an event study illustrating the difference over time in the relationship between the natural log of real county spending per capita and the 1960 share of African Americans is presented in Figure 2.8. In the pre-VRA period, counties that were both subject to the VRA and that had larger pre-existing shares of African Americans did not display a significantly different spending pattern compared to non-covered counties, and this is true for both current and capital expenditure. However, by the late 1970s, while current expenditure is still comparable between the two groups, there is evidence that capital spending begins to increase at a faster rate across covered counties. Then, the bottom panel of Figure

2.8 focuses on capital spending and presents the estimates of a quadruple interaction model allowing the coefficients to vary depending on pre-existing electoral rules. A clearer pattern emerges: the faster growth in capital spending in the post-VRA period is indeed concentrated in covered counties with SMD-based elections, which, if anything, further display a slightly lower growth before 1965. In other words, the event study indicates that, following the passage of the VRA, covered counties with larger shares of African Americans experience a faster growth in capital spending compared to those counties that were not covered, and this growth is concentrated in counties with pre-existing electoral rules favorable to the election of African Americans. And since the enfranchisement led to gains in African American office-holding only within covered counties with pre-existing SMD electoral rules then, under the assumption that, had they not elected more African Americans, covered counties with both SMD systems and larger African American population shares would have experienced the same spending pattern compared to the control group, a causal interpretation to these findings could be given.

While it seems unlikely that the 1960 share of African Americans in the total population might have a differential impact in covered SMD counties through different mechanisms other than representation, it is still possible that other county characteristics could be driving the spending patterns that have been uncovered. To address these concerns, the robustness of the findings is assessed in a series of alternative specifications, focusing on the long-run. Table 2.4 presents the estimation results using the difference in the natural log of real spending per capita between the beginning (1957) and the end (1982) of the sample period as the dependent variable. Columns (1) and (2) report the estimated coefficients accounting for additional economic and demographic characteristics. Importantly, to rule out the possibility that the identified patterns might be driven by other sources of heterogeneity present in covered counties with SMD systems, the impact of each control variable is allowed to vary by both coverage status and pre-existing electoral rules.[35] The results confirm the absence of a differential pattern for current

[35]That is, all control variables are interacted with the VRA indicator, the SMD indicator, and their interaction. The estimated coefficients are available upon request.

expenditure, whereas capital spending is again found to grow more rapidly within covered counties with larger African American population shares that elected county governing bodies through SMD systems.

As local public spending responds to changes in economic and demographic characteristics, there could still be a concern that different trends in the explanatory variables (not fully captured by their 1960 level) could drive the increase in spending in the group of counties that are both subject to the VRA and that had SMD systems.[36] To address this concern, the analysis is restricted to the border sample. As seen in Figures 2.6 and 2.7, the border sample is balanced on both pre-existing values and trends. The same holds true when looking at coverage by electoral rule: covered counties with SMD systems do not display statistically significant differences in either their 1960 characteristics or their pre- and post-VRA trends (Figures 2.9 and 2.10). Thus, restricting the attention to the border sample ought to give more confidence that the results are not driven by other sources of heterogeneity. The estimation results on the border sample reported in columns (3) and (4) of Table 2.4 show that the sign, magnitude, and significance of the main coefficients are not affected.

Lastly, in order to rule out the possibility that the findings might reflect a general pattern in local spending within covered counties with both SMD systems and larger shares of African Americans, rather than the effect of African American office-holding, columns (5) and (6) of Table 2.4 carry out a placebo exercise using spending by other local governments (municipalities, townships, special districts, and independent school districts) as the dependent variable. If the change in spending by county governments was due to a general pattern driven by any other factor (for example, a change in the economic structure or in the political participation), then an increase in spending by other local bodies should also be observed. However, there is no differential effect of the initial share of African

[36]Directly accounting for changes in our explanatory variables would be problematic because of endogeneity concerns due to reverse causality: changes in spending could drive trends in economic and demographic characteristics.

Americans on spending by other local governments within covered counties electing their county commissioners through SMD systems.[37]

What is the magnitude of the effect on spending? The estimates presented in column (2) indicate that a 10% increase in the 1960 share of African Americans in covered counties, electing commissioners by SMD, led to an additional 3.6% increase in capital spending.[38] Given that, in 1960, one-third of the population in covered counties was African American, this implies an additional 10.8% increase in capital spending over the sample period. This is a notable shift, considering that public spending, particularly on infrastructural projects, is typically characterized by significant inertia (Larcinese, Snyder, and Testa 2012).

How was this increase in spending financed? Local county governments raise revenues through both local taxes and intergovernmental transfers, with the latter amounting to about 40% of county tax revenues at the beginning of the sample period and 35% at the end. While current expenditures are essentially financed by tax revenues, capital spending by local governments is most commonly financed by borrowing (U.S. Bureau of the Census 2000) and, to a much more limited extent, by tax revenues.[39] The ability to borrow to finance infrastructure spending – combined with its salience to the African American electorate – could provide a rationale for the differential patterns observed between capital outlays and current expenditures. To shed light on this issue, Table 2.5 considers a quadruple difference design in order to analyze the long-run changes in tax revenues. Focusing on the difference in the natural log of real revenues per capita between the beginning (1957) and the

[37]Since spending by other local governments is essentially determined by municipalities and school boards, a placebo exercise on the effect of county commissions' electoral rules on other African American elected officials is also carried out. It considers the same specification as the one used in column (2) of Table 2.3, but using the share of African American officials in other local governments (instead of county commissions) as dependent variable. The estimation results, reported in Table B.2.3, show that the share of other elected officials does not display any significantly different pattern within covered counties electing county commissioners through SMD systems.

[38]Since current spending includes many diverse items (e.g., welfare spending, salaries, payments for supplies, and contractual services), it could also be the case that African American office-holding might affect some specific item, but not the aggregate measure. Unfortunately, data limitations do not allow us to investigate this possibility.

[39]Capital spending is typically exempt from balanced budget requirements that instead apply to current spending.

end (1982) of the sample period, columns (1) and (2) summarize the main patterns in intergovernmental transfers and own revenues. The results show that covered counties with both SMD systems and larger shares of African Americans experienced an increase in intergovernmental transfers but not in own revenues. However, the increase in intergovernmental transfers amounts to only about one-third of the total increase in capital spending, implying that about two-thirds of the increase in local expenditures is financed through debt. These results also remain robust when the GRD design is considered, as presented in columns (3) and (4) of Table 2.5.

To conclude, the analysis provides systematic evidence consistent with historical accounts of an improvement in local infrastructure following the election of African Americans (Button 1989). Covered counties electing African American commissioners spent significantly more on capital projects. This increase was financed predominantly through borrowing and, to some extent, through intergovernmental transfers. On the other hand, no evidence is found of African American office-holding leading to an increase in direct payments to individuals. These patterns of local spending are consistent with the idea that the gains achieved by African Americans did not take place at the expense of white Southerners (Wright 2013). In the aftermath of the VRA, the U.S. South appears to have embraced growth-enhancing policies with limited redistribution.

## 2.7 Conclusions

The Voting Rights Act (VRA) of 1965 restated the prohibition against the denial or abridgment of the right to vote on account of race contained in the Fifteenth Amendment, while passing some drastic measures to pre-empt its violation. As a result, seven of the eleven former Confederate states were subject to the VRA's special provisions preventing a re-enactment of the sort of discriminatory practices that, in the post-Reconstruction period, de facto banned African Americans from elective offices. In particular, these states were subject to pre-clearance by federal authorities of any change affecting the voting process. This chapter has assessed

whether the VRA was effective in changing the racial make-up of local governments in the U.S. South.

The results have shown that, while before 1965, African American office-holding across all states of the former Confederacy was unrelated to the racial composition of counties, in the immediate aftermath of the VRA, African American representation increased more in counties with larger shares of African Americans, and the gradient of this relationship was steeper for counties subject to the VRA. Pre-existing electoral rules played an important role in this process. In particular, the VRA led to larger gains in African American office-holding in counties that elected county commissioners by single-member districts (SMD), a system more favorable to the election of minority candidates. As court battles over the enforcement of the VRA's special provisions were instrumental to preserving pre-existing SMD-based elections, pre-clearance provided an effective tool to promote the journey toward African American office-holding across important governing bodies, responsible for the provision of local public goods. As a result, capital spending grew more rapidly within counties electing African American commissioners.

In less than two decades, the VRA significantly changed the racial make-up of local governments in the U.S. South. This chapter has touched upon one of the possible consequences, namely the change in local spending, but several additional avenues for research appear worth exploring. First, as racial perceptions and role models for minorities may affect their economic outcomes, an interesting question is whether the emergence of African American elected officials has improved economic outcomes of African Americans via those channels. Second, as racial attitudes affect political behavior (Kuziemko and Washington 2018), another relevant issue is whether African American office-holding might have changed the way in which southern whites cast their ballot, leading to a polarization along racial lines. While the former question is left to further research, the latter is tackled in Chapter 4.

**Figure 2.1:** Number of African American elected officials in 1964

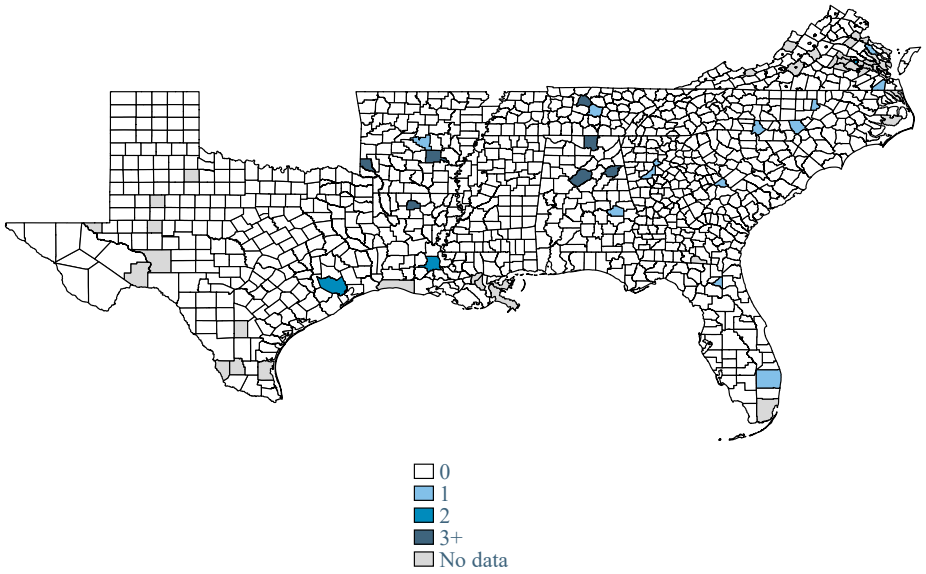



**Figure 2.2:** Number of African American elected officials in 1980

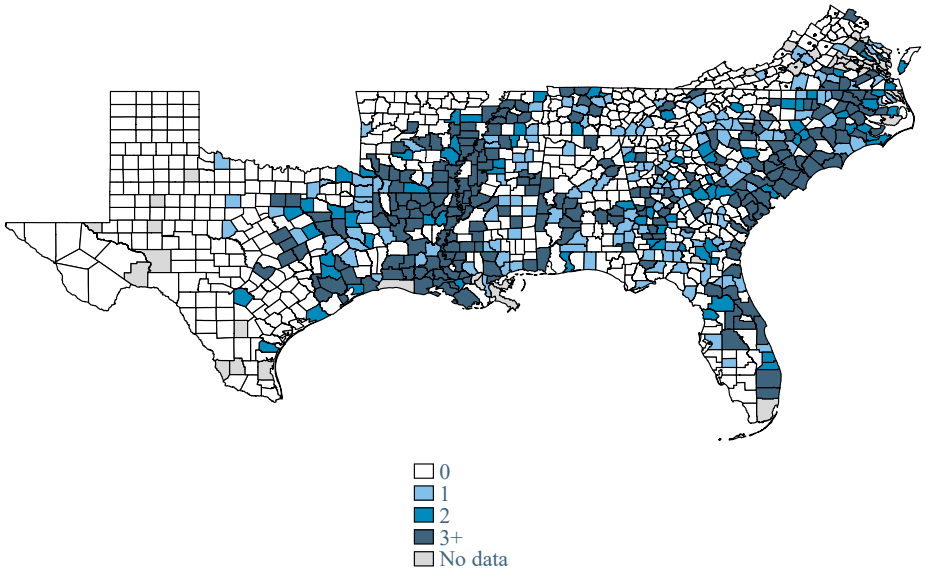
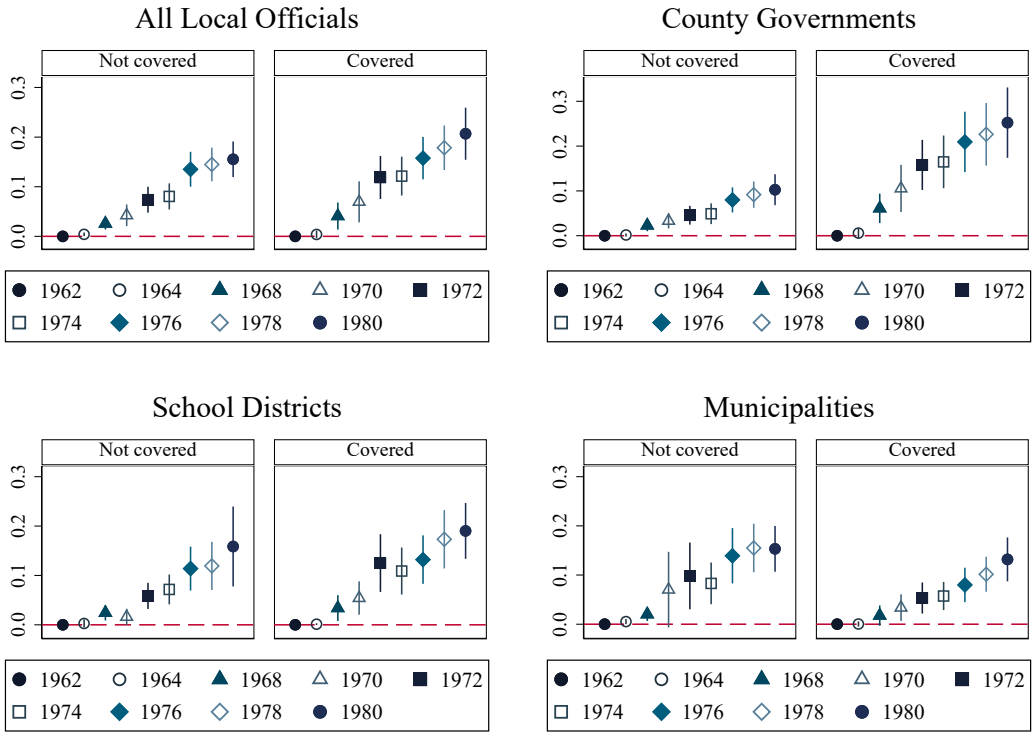
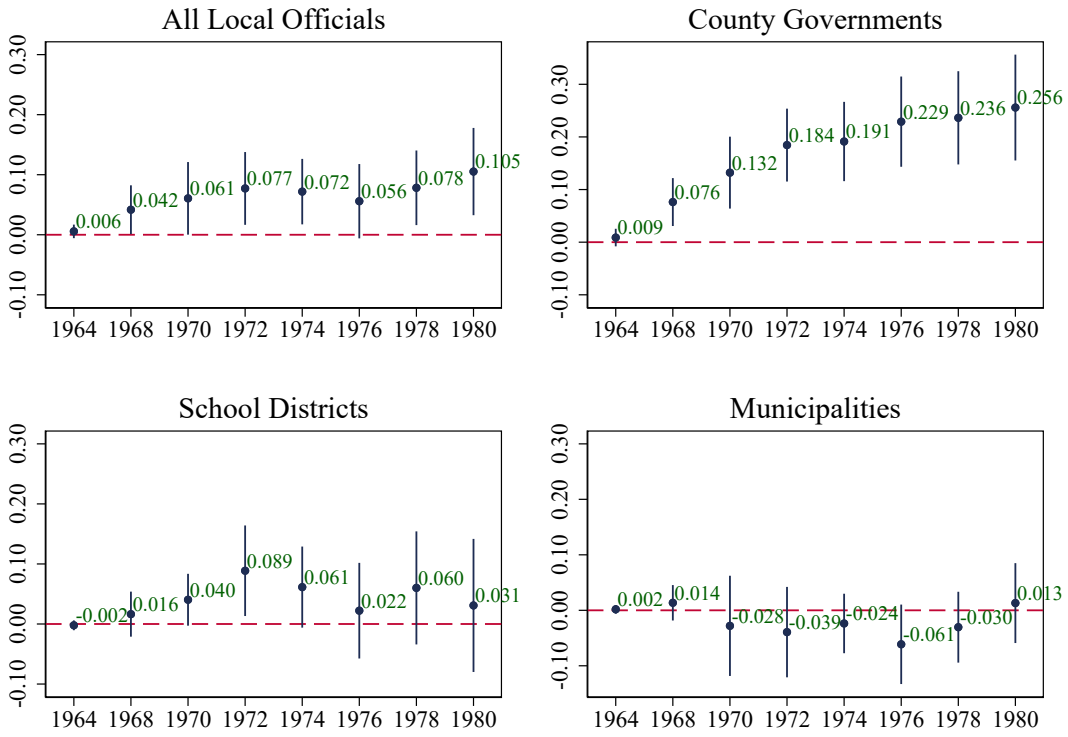


**Figure 2.3:** Trends in the gradient by treatment status and type of office



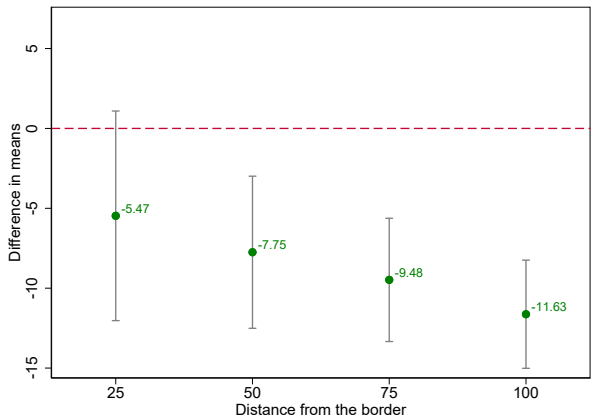

Bars represent 95% confidence intervals. In all the specifications, the controls include: the unemployment rate, the share of families below the poverty line, the share of unskilled workers, a measure of agricultural productivity, the population and the share of its urban component, occurrences of both pro- and anti-African American activism, and state fixed effects.

**Figure 2.4:** Differences in the gradient by treatment status and type of office



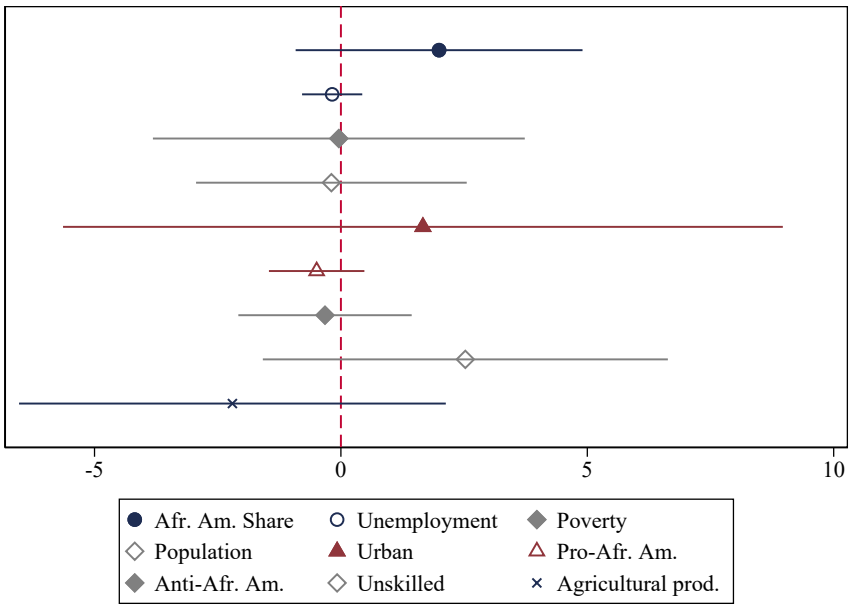

Bars represent 95% confidence intervals. In all the specifications, the controls include: the unemployment rate, the share of families below the poverty line, the share of unskilled workers, a measure of agricultural productivity, the population and the share of its urban component, occurrences of both pro- and anti-African American activism, state-year interactions, and county fixed effects. Omitted interaction: 1962.

**Figure 2.5:** Differences in the share of African Americans on the border sample



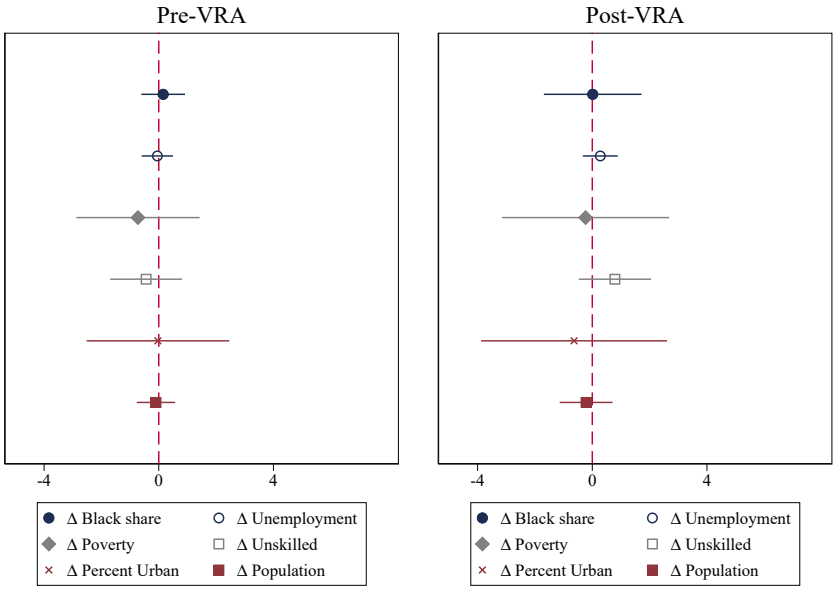

Bars represent 95% confidence intervals. The distance is measured in miles from the border.

**Figure 2.6:** Balance in the values of the covariates on the border sample




Bars represent 95% confidence intervals. Estimated coefficients obtained regressing each variable on the VRA indicator and county-pair fixed effects.

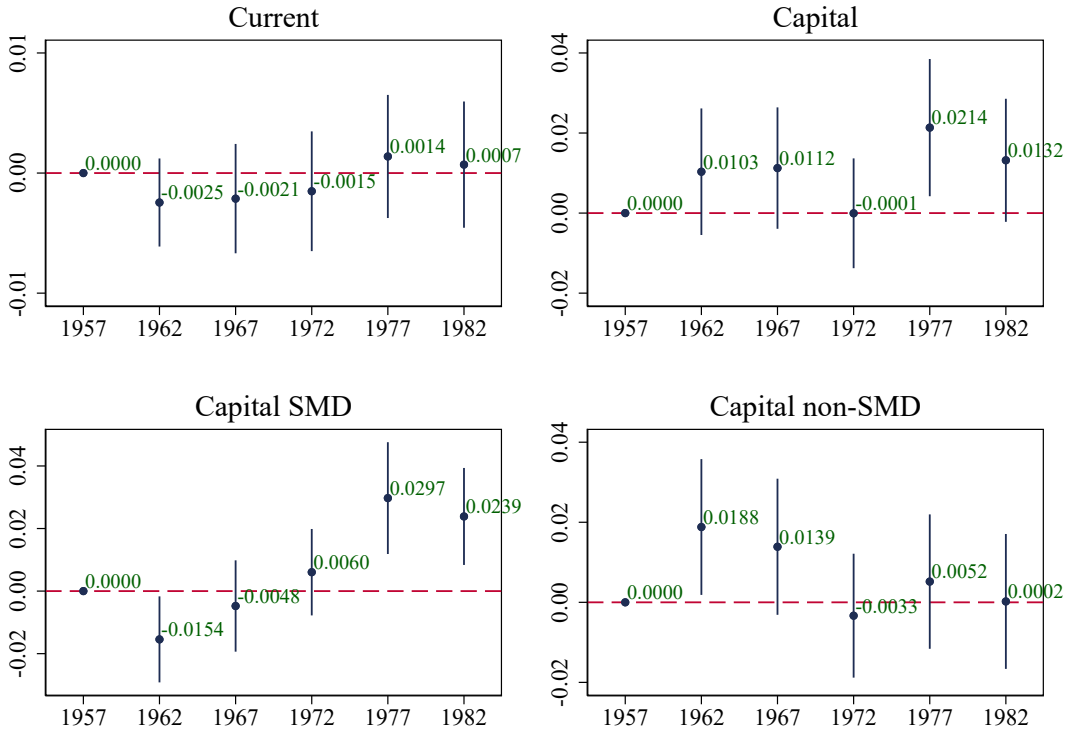
**Figure 2.7:** Balance in the trends of the covariates on the border sample




Bars represent 95% confidence intervals. Estimated coefficients obtained regressing each variable on the VRA indicator and county-pair fixed effects.

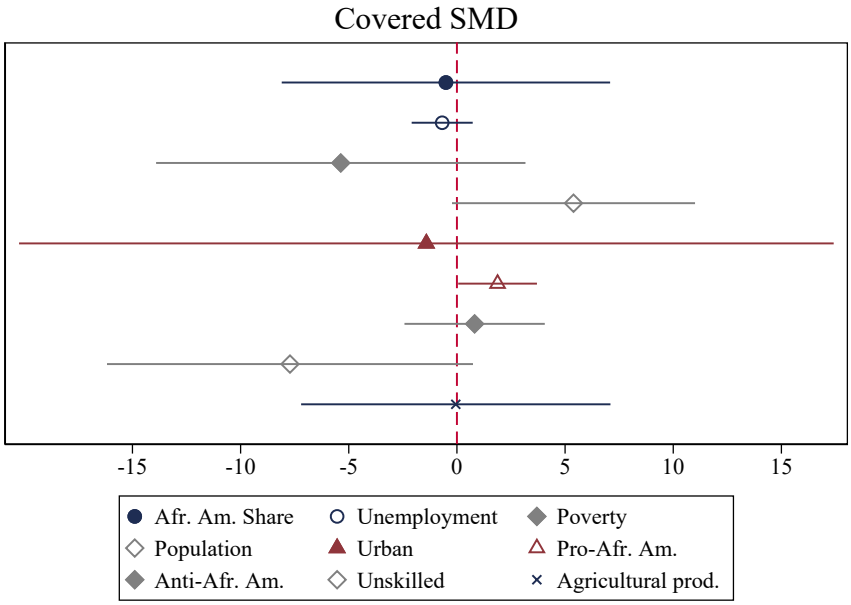
## 2.7. Conclusions

**Figure 2.8:** Differences in the gradient of spending per capita



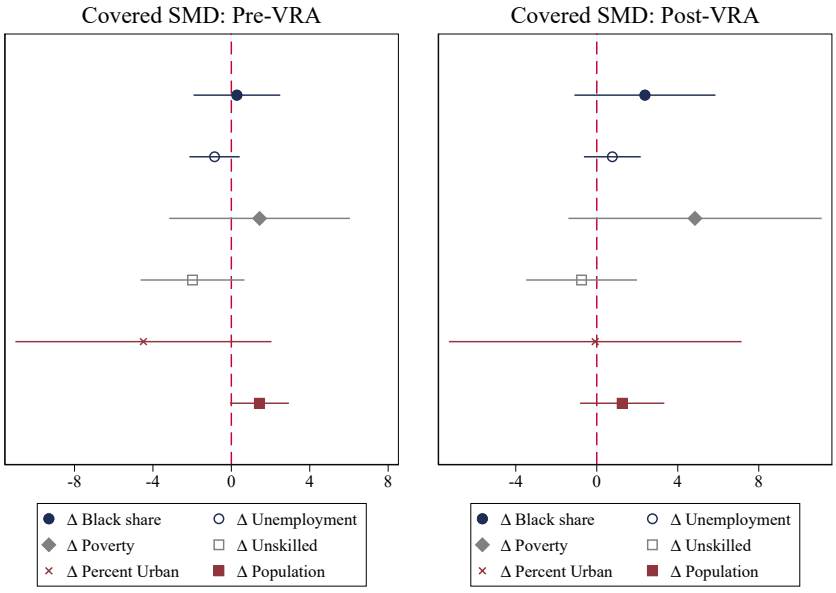

Bars represent 95% confidence intervals. In all the specifications, the controls include: the unemployment rate, the share of families below the poverty line, the population, state-year interactions, and county fixed effects. Omitted interaction: 1957.

**Figure 2.9:** Balance in the values of the covariates on the border sample and SMD




Bars represent 95% confidence intervals. The figure plots the estimated coefficient of the interaction between the VRA indicator and the SMD indicator, obtained by regressing each variable on the VRA indicator, the SMD indicator, their interaction, and including county-pair fixed effects.

**Figure 2.10:** Balance in the trends of the covariates on the border sample and SMD




Bars represent 95% confidence intervals. The figure plots the estimated coefficient of the interaction between the VRA indicator and the SMD indicator, obtained by regressing each variable on the VRA indicator, the SMD indicator, their interaction, and including county-pair fixed effects.

## 2.7. Conclusions

**Table 2.1:** Change in African American elected officials between 1964 and 1980

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Afr. Am. Share, 1960 X VRA | 0.05** | 0.07** | 0.07** | 0.13*** |
| | (0.023) | (0.036) | (0.037) | (0.048) |
| Afr. Am. Share, 1960 | 0.18*** | 0.22*** | 0.21*** | 0.19*** |
| | (0.016) | (0.023) | (0.023) | (0.021) |
| Unemployment, 1960 | | 0.22* | 0.26** | 0.03 |
| | | (0.130) | (0.129) | (0.080) |
| Poverty, 1960 | | –0.02 | –0.02 | –0.02 |
| | | (0.028) | (0.028) | (0.019) |
| Population, 1960 | | 0.00 | –0.00 | 0.00 |
| | | (0.003) | (0.002) | (0.002) |
| Unskilled, 1960 | | –0.04* | –0.04* | 0.05 |
| | | (0.021) | (0.021) | (0.037) |
| Urban, 1960 | | 0.04*** | 0.03** | 0.02*** |
| | | (0.013) | (0.012) | (0.007) |
| Agricultural productivity | | 0.42 | 0.29 | –0.10 |
| | | (0.835) | (0.822) | (0.515) |
| Pro-Afr. Am. protest, 1960-64 | | | 0.30*** | 0.38 |
| | | | (0.065) | (0.259) |
| Anti-Afr. Am. protest, 1960-64 | | | –0.37 | –1.39 |
| | | | (0.312) | (0.965) |
| State Trends | No | Yes | Yes | Yes |
| Controls X VRA | No | No | No | Yes |
| Adj. R-Square | 0.45 | 0.50 | 0.51 | 0.52 |
| N | 1050 | 973 | 973 | 973 |

Robust standard errors in parenthesis are adjusted for clustering at the county-level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

**Table 2.2:** Change in African American elected officials between 1964 and 1980

| | County Governments | | | Other Governments | |
|---|---|---|---|---|---|
| | Commission | Judiciary & Enforcement | Other | Municipality | School Districts |
| | (1) | (2) | (3) | (4) | (5) |
| *Panel A: Overall Sample* | | | | | |
| Afr. Am. Share, 1960 X VRA | 0.100*** | 0.103*** | 0.061*** | 0.009 | 0.039 |
| | (0.024) | (0.022) | (0.017) | (0.039) | (0.056) |
| Afr. Am. Share, 1960 | 0.048*** | 0.008 | –0.000 | 0.148*** | 0.168*** |
| | (0.009) | (0.005) | (0.000) | (0.025) | (0.039) |
| State Trends | Yes | Yes | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes | Yes | Yes |
| Controls X VRA | Yes | Yes | Yes | Yes | Yes |
| Adj. R-Square | 0.39 | 0.25 | 0.15 | 0.40 | 0.42 |
| N | 1019 | 1016 | 1019 | 979 | 830 |
| *Panel B: Border Sample* | | | | | |
| Afr. Am. Share, 1960 X VRA | 0.057** | 0.051*** | 0.010 | 0.022 | 0.122* |
| | (0.027) | (0.018) | (0.008) | (0.062) | (0.065) |
| Afr. Am. Share, 1960 | –0.051 | 0.036 | 0.010 | 0.200** | 0.109 |
| | (0.060) | (0.025) | (0.011) | (0.092) | (0.111) |
| County-Pair Trends | Yes | Yes | Yes | Yes | Yes |
| Adj. R-Square | 0.47 | 0.26 | –0.02 | 0.60 | 0.52 |
| N | 295 | 295 | 295 | 290 | 281 |

Robust standard errors in parenthesis are adjusted for clustering at the county-level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

**Table 2.3:** Commissioners and electoral rules

| | (1) | (2) |
|---|---|---|
| Afr. Am. Share, 1960 X VRA X SMD | 0.103** | 0.139*** |
| | (0.045) | (0.028) |
| Afr. Am. Share, 1960 X VRA | 0.042 | |
| | (0.038) | |
| Afr. Am. Share, 1960 X SMD | –0.029 | |
| | (0.027) | |
| Afr. Am. Share, 1960 | 0.071*** | 0.049*** |
| | (0.025) | (0.009) |
| Afr. Am. Share, 1960 X VRA X Switch | | 0.070** |
| | | (0.034) |
| Afr. Am. Share, 1960 X VRA X Non-switch/Mixed | | –0.004 |
| | | (0.030) |
| Controls | Yes | Yes |
| Controls X VRA | Yes | Yes |
| State Trends | Yes | Yes |
| Adj. R-Square | 0.40 | 0.40 |
| N | 1019 | 1019 |

Robust standard errors in parenthesis are adjusted for clustering at the county-level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

**Table 2.4:** Change in spending between 1957 and 1982

| | Overall Sample | | Border Sample | | Placebo | |
|---|---|---|---|---|---|---|
| | Current | Capital | Current | Capital | Current | Capital |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Afr. Am. Share, 1960 X VRA X SMD | –0.0034 | 0.0346** | 0.0018 | 0.0378*** | 0.0102 | 0.0009 |
| | (0.0039) | (0.0146) | (0.0054) | (0.0119) | (0.0105) | (0.0180) |
| Afr. Am. Share, 1960 X VRA | 0.0011 | –0.0098 | –0.0039 | –0.0236*** | –0.0009 | 0.0142 |
| | (0.0029) | (0.0105) | (0.0034) | (0.0080) | (0.0090) | (0.0153) |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Controls X VRA | Yes | Yes | No | No | Yes | Yes |
| Controls X VRA X SMD | Yes | Yes | No | No | Yes | Yes |
| State Trends | Yes | Yes | No | No | Yes | Yes |
| County-Pair Trends | No | No | Yes | Yes | No | No |
| Adj. R-Square | 0.79 | 0.07 | 0.82 | 0.25 | 0.85 | 0.27 |
| N | 960 | 862 | 281 | 283 | 622 | 886 |

Robust standard errors in parenthesis are adjusted for clustering at the county-level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

**Table 2.5:** Change in revenues between 1957 and 1982

| | Overall Sample | | Border Sample | |
|---|---|---|---|---|
| | Intergov. Revenues | County Own Revenues | Intergov. Revenues | County Own Revenues |
| | (1) | (2) | (3) | (4) |
| Afr. Am. Share, 1960 X VRA X SMD | 0.0106** | –0.0032 | 0.0116* | –0.0017 |
| | (0.0051) | (0.0052) | (0.0070) | (0.0074) |
| Afr. Am. Share, 1960 X VRA | –0.0101** | 0.0024 | –0.0204*** | –0.0006 |
| | (0.0043) | (0.0036) | (0.0059) | (0.0055) |
| Controls | Yes | Yes | Yes | Yes |
| Controls X VRA | Yes | Yes | No | No |
| Controls X VRA X SMD | Yes | Yes | No | No |
| State Trends | Yes | Yes | No | No |
| County-Pair Trends | No | No | Yes | Yes |
| Adj. R-Square | 0.69 | 0.74 | 0.62 | 0.77 |
| N | 976 | 975 | 291 | 291 |

Robust standard errors in parenthesis are adjusted for clustering at the county-level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

*I know you are asking today: "How long will it take?"*
*Not long, because the arc of the moral universe is long, but it bends toward justice.*

— Martin Luther King, Jr., *Our God Is Marching On*, March 25, 1965

# 3

# Uneven Roads to Office? African Americans in Southern County Governments

## Contents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## 3.1 Introduction

On June 25, 2013, in a deeply controversial ruling – *Shelby County v. Holder* (570 U.S. 529, 2013) – the U.S. Supreme Court struck down Section 4b of the Voting Rights Act (VRA), which compelled jurisdictions – identified by a coverage formula – to seek federal pre-clearance for any change in their voting procedures. With a narrow majority, five of the Supreme Court Justices concurred that the key provisions of the arguably most important piece of civil rights legislation in U.S. history were no longer needed, because of the very success of the VRA in eradicating discrimination. The four dissenting Justices expressed instead a radically different opinion, reiterating the views of Congress in the 2006 re-authorization of the VRA that "the scourge of discrimination was not yet extirpated."[1]

How does the evidence speak to this controversy? Although there is no unique metric for evaluating the success of the VRA, the election of African Americans into office has traditionally been an important one. Yet, even as the VRA has been voided of its key legal weapon, our understanding of its causal impact on African American office-holding remains rather incomplete. A large body of literature has assessed the progress of African American descriptive representation in the U.S. Congress (Swain 1993 and Tate 2003) and state legislatures (Handley and Grofman 1994), while Chapter 2 has considered municipalities and school boards (Engstrom and McDonald 1981, Marschall, Ruhil, and Shah 2010 and 2013). Yet, very little is known on important county-level elective offices, including county commissioners, judges, sheriffs, and police chiefs. This chapter fills this gap by assembling a novel dataset on African American elected officials between 1962 and 1990 in order to provide causal evidence of the effect of the VRA on descriptive representation across southern county governments.

[1]The Supreme Court ruling stated that: "50 years later, things have changed dramatically. Voter turnout and registration rates in covered jurisdictions approach parity; blatantly discriminatory evasions of federal decrees are rare. Minority candidates hold office at unprecedented levels." On the other hand, Justice Ginsburg noted that: "In the Court's view, the very success of the Voting Rights Act demands its dormancy. Congress was of another mind. Recognizing that large progress has been made, Congress determined, based on a voluminous record, that the scourge of discrimination was not yet extirpated."

As discussed in Chapter 2, the special provisions of the VRA, which were known as coverage, represented an unprecedented shift in the balance of power between federal and state governments. Counties that imposed a test or device restricting the right to vote and that experienced a turnout below 50% in the 1964 presidential election were placed under strict federal monitoring. In particular, six of the eleven states of the former Confederacy – Alabama, Georgia, Louisiana, Mississippi, South Carolina, and Virginia – were fully covered by the Act's special provisions and one state – North Carolina – was partially covered.[2] Covered counties had to seek pre-clearance by the U.S. District Court for the District of Columbia or by the Attorney General of any change in legislation that affected voting. Furthermore, the Attorney General could dispatch federal examiners to monitor these counties' polling places, and all covered counties were forced to eliminate literacy test provisions. With these barriers being removed, the Act fundamentally changed the make-up of the electorate in the U.S. South (Filer, Kenny, and Morton 1991 and Cascio and Washington 2014).

One of the key expectations of the VRA was that it would radically transform southern politics by bringing African Americans into elective offices. Besides carrying a high symbolic value, African American office-holding was hailed as the primary vehicle for policies that would improve the conditions of African American communities across the South. Yet, the journey of African Americans toward elective offices proved to be long and arduous. The entry of African American candidates in the political arena was discouraged by a culture of white supremacy and intimidation (Andrews 1997). Even when African American candidates were running, both majoritarian electoral rules and polarization along racial lines made it difficult for them to win office in majority-white constituencies (Handley and Grofman 1994).[3] And when African Americans were a majority of the electorate, as in several local jurisdictions in the South, the white political class tried to build new barriers using vote dilution tactics such as switches from single-member districts

[2]Of the 100 counties in North Carolina, 39 were covered by the VRA.

[3]As a result, gains in the number of African Americans elected to federal and state offices have been linked to the increase in the so-called minority-majority districts, which took place only more than twenty years after the passage of the VRA.

## 3.1. Introduction

(SMD) to elections at-large, substitution of elective with appointive offices, and discriminatory qualifications (Parker 1990, Davidson and Grofman 1994, and Trebbi, Aghion, and Alesina 2008). Whether the extraordinary measures introduced by the VRA allowed African Americans to win key offices across county governments in the South remains an open empirical question.

To address this important issue, a novel dataset on African Americans elected across all county governments is assembled for the eleven states of the former Confederacy and for the period between 1962 and 1990. Using information from the National Roster of Black Elected Officials (NRBEO) serving in county commissions, local judiciary, law enforcement, and other governing bodies, this chapter: (a) estimates the causal effect of the VRA on the election of African American officials, and (b) measures the progress in descriptive representation at the county-level within a quarter century from the VRA's enactment.

With covered counties displaying substantial variation in their racial composition, and with race being highly salient for voters, the impact of the VRA could be expected to be larger in those counties with greater shares of African Americans in the population. That is, African American office-holding ought to be related to the pre-VRA share of African Americans and, crucially, if the policy had a significant impact, the slope of this positive relationship should also increase around the time of the passage of the Act. However, as covered counties with larger pre-existing shares of African Americans might have experienced increases in descriptive representation even in the absence of coverage, the remaining counties in the former Confederacy, which were not subject to the Act but which held a similar history of slavery and disenfranchisement, are used to form a comparison group. Then, this chapter estimates whether covered counties with larger pre-existing African American population shares (treatment group) experienced a larger increase in African American representation – from before to after the VRA – when compared to the counties that were instead not covered (control group). The identifying assumption is that, in the absence of coverage, the two groups would have experienced the same patterns in the election of African Americans. Parallel

trends in pre-VRA African American office-holding, political participation, political activism, and white racial attitudes support the suitability of the control group.

The results show that the VRA increased African American representation across county offices. These effects remain robust to the use of various matching techniques (including geographic regression discontinuity, coarsened exact matching, and trimming the sample to its common support), which improve the similarity of treatment and control groups as for their observable characteristics. This chapter also reveals that the gains in the representation of African Americans vary by type of office. Focusing on the most important positions – county commissioners, judges, sheriffs, and police chiefs – this study finds evidence of significant early progress across county commissions, whereas gains were much weaker across both the judiciary and law enforcement.

The results also indicate that the effect of the VRA is mediated by other factors, including the pre-VRA levels of educational attainment of African Americans and their political organizations, a white supremacy culture, and the type of electoral rules (Matthews and Prothro 1963, Karnig 1979, Engstrom and McDonald 1981, and Salamon and Evera 1973). In particular, both the pre-existing share of African Americans with a college-level education and the existence of political organizations magnified the positive effect of the VRA, whereas white supremacy decreased it. Consistent with both Chapter 2 and the existing literature (Engstrom and McDonald 1981 and Marschall, Ruhil, and Shah 2010), this chapter finds that the VRA led to larger African American office-holding gains in county commissions that inherited elections by single-member districts (SMD) from the pre-VRA period.

Overall, this analysis shows that the VRA had a positive effect on African American descriptive representation across county governments in a region where, pre-VRA, African American office-holding was virtually unknown. But to what extent did it change that grim pre-VRA picture? To answer this question, this chapter further considers the patterns in the extensive margin of African American office-holding, measured as the share of counties with at least one African American elected official. Despite a clear progress, a strikingly uneven pattern is found. As

of 1990, African American commissioners were in office in 80% of majority-Black covered counties, yet only 15% of these counties had an African American sheriff or police chief. African American judges were even less frequent, with 95% of majority-Black counties not electing a single one. These findings deliver a mixed picture on the effect of the VRA across county governments as they suggest that, notwithstanding progress, the journey of African Americans toward elective offices had remained paved with obstacles.

## 3.2 County governments and the VRA

Within days from the passage of the VRA, the civil rights leadership greeted the momentous federal legislation as a turning point that could lead to African American representatives. Yet, the initial enthusiasm toward what federal intervention could achieve was soon met by skepticism as African Americans were eventually gaining office "as justices of the peace, constables, or school board members rather than to posts of greater policy making authority"(Poinsett 1976). As discussed in Chapter 2, the election of African Americans carried a highly symbolic value since it represented "a bit of [B]lack authority, a gradual return to respect for those accustomed to having their lives manipulated by white hands."[4] At the same time, as pointed out by Wirt (1997), "many [B]lacks had first wanted their local representatives to be symbolic, that is to be [B]lack like themselves. In time, tough, they wanted representatives to provide individual or group services and to secure the public policies that would provide sufficient resources." To this end, electing African Americans to positions of higher authority was of paramount importance.

Although there is no doubt that the VRA had a positive effect on African American office-holding (Bullock and Gaddie 2014), the existing literature – echoing the concern of civil rights activists on the slow progress in the more powerful elective positions – shows that considerable gains in offices at the state and federal level only materialized more than twenty years after the passage of the VRA (Handley

[4]Wright (2013), page 202.

and Grofman 1994).[5] As presented in Chapter 2, progress within city councils had also been similarly slow (Marschall, Ruhil, and Shah 2010). Hence, the consensus emerging from the existing evidence is that, two decades after the passage of the VRA, African Americans still struggled to be elected into positions of power within covered jurisdictions. Yet, an important piece of evidence is missing to reach a forgone conclusion: county governments, traditionally the most important form of local authority in the U.S. South, have been largely overlooked.

Historically, the southern model of government was inspired by the so-called Virginia Plan, in which towns and townships were absent and nearly all power resided in local county courts that, in addition to judicial functions, levied taxes and appropriated funds for expenditures on essential public services and infrastructure (Wager 1950). Over time, the organization of county governments evolved to assign judiciary, law enforcement, and fiscal powers to the distinct elective offices of judges, sheriffs and police chiefs, and county commissioners, respectively. Thus, in the U.S. South, a number of very important functions were carried out by county-level elective officers that, up to the passage of the VRA, poorly represented the interests of African Americans. As a result, African American communities lacked basic infrastructure, such as paved street, drainage, and street lights (Valelly 2009), and their education facilities were described "miserable beyond all description" (Jones 1917). African American neighborhoods were poorly patrolled by white enforcement officers and serious crimes against African Americans, such as lynchings, took place under the indifference of local sheriffs and their deputies (Rudwick 1962). On some occasions, law enforcement officers were even guilty of conspiracy with the perpetrators of these hideous crimes, which were rarely indicted by courts (Raper 1933).

The passage of the VRA was expected to put an end to this state of affairs. As shown in Chapter 4, white office-holders could no longer ignore African American voters (Cascio and Washington 2014). Furthermore, African Americans did not have to rely on white politicians, since they could now seek office themselves, opening new avenues to their empowerment. Descriptive representation has been shown to

[5] *Thornburg v. Gingles* (478 U.S. 30, 1986) set the basis for minority-majority districts.

have important positive effects for minorities. On the one hand, it can increase the chance that their preferred policies are implemented (Broockman 2014 and Pande 2003). On the other, minority office-holding in positions of authority could provide role models, weaken stereotypes, and build expertise crucial for their advancement (Mansbridge 1999 and Beaman et al. 2009).

Did the VRA foster African American office-holding across county governments in the U.S. South? If so, to what extent did it do so? To the best of our knowledge, this chapter makes progress on these important questions by providing the first systematic assessment of the effects of the VRA on African American office-holding in county governments in the states of the former Confederacy.

## 3.3 Data

To analyze the effect of the VRA, a rich dataset on African Americans elected to county-level offices in the eleven states of the former Confederacy is assembled for the period between 1962 and 1990.

Although county-level information for the pre-VRA period has never been systematically collected before, our figures (obtained from several sources, including both the Southern Regional Council and local newspapers) are consistent with the only aggregate count that exists for that time period (published by the Roster of Black Elected Officials in the Southern States in 1965), which lists a total of 72 African American elected officials at the local, state, and national levels. For the period between the VRA and 1968, the Roster of Black Elected Officials in the Southern States has been considered: a directory firstly assembled in preparation for the Southwide Conference of Black Elected Officials (convened in Atlanta in 1968) by the Voter Education Project (VEP).[6] For all the post-1968 years, the information has been digitized from the National Roster of Black Elected Officials (NRBEO), a special report which is available for almost every year between 1971

[6]The VEP, active between the 1960s and the 1980s, collected data from official publications (e.g., from the Secretary of State or from the Department of Justice) and undertook county-level surveys to produce reports on the process of African American enfranchisement in the U.S. South. Due to a lack of funding, the VEP ceased to operate in the early 1990s.

and 1990.[7] As all the above-mentioned publications are only available in paper format, these directories have been hand-digitized to obtain the total counts of African Americans elected in each county in the U.S. South.

Figure C.1.1 displays aggregate counts of African American elected officials in 1964, 1970, 1980, and 1990. In the year before the VRA, only 5 African Americans held county-wide offices in the entire U.S. South. Three of these officers (two justices of the peace and one member of the Board of Revenue) were based in Macon County, Alabama; then, there was a judge in Shelby County, Tennessee, and a county commissioner in Suffolk City, Virginia. As a result, fewer than 0.3% of the counties in the South had at least one African American elected to county governments. In contrast, by 1970, there were 178 officials in 69 distinct counties: 6.0% of all counties had elected at least one African American. By 1980, this figure rose to 553, with close to a fifth of all counties having at least one African American official (215 counties, or 18.6% of the total). A quarter century after the VRA, the total number of officers rose to 974, resulting in a third of counties (391, or 33.9%) having elected at least one African American officer.

The counts of African Americans elected to county governments have been combined with information on the total number of office-holders in every county, obtained from the Census, to calculate the shares of African Americans in office that are used as the main outcomes of interest in the analysis.[8] The top panel of Table C.1.1 provides summary statistics, by coverage status, for the key dependent variables (i.e., the share of African American elected officials across several offices in county governments), whereas the bottom panel displays summary statistics for all other control variables that are used in the analysis.

[7] The last edition of this compilation was published in 1993. To align this information with the decennial Census, data up to 1990 is considered.

[8] The shares of African American elected officials are computed as:

$$ShareBlackElected_{c,o,t} = \frac{BlackElected_{c,o,t}}{Elected_{c,t}} \tag{3.1}$$

where $BlackElected_{c,o,t}$ is the number of African Americans elected in county $c$, to county-level office $o$, at time $t$, and $Elected_{c,t}$ is the overall number of county elected officials in county $c$, at time $t$. The latter is an aggregate measure as there is no separate information available for each category of county government.

As shown in the table, African American office-holding increased over the period of this analysis for all types of office. The rise has been significantly larger for covered counties, where the overall share of African American elected officials jumped from less than 0.1% before the VRA to 8.5% a quarter century later. In the non-covered sample, the corresponding change is much more modest at 1.8%. County governing bodies account for most of the increase in both sub-samples, reaching, by 1990, 5.8% and 1.3%, respectively. The shares of African Americans in judicial and law enforcement bodies also rose significantly from close to nihil to about 2.1% in covered and 0.5% in non-covered counties (with the rise spread roughly equally between the two types of office) as did representation in other county-level offices (tax assessor, tax collector, coroner, prosecutor, clerk, register of deeds, treasurer, and commissioner of revenue), albeit to a smaller extent.

As for other county characteristics, covered counties were on average smaller, more rural, and displayed larger shares of African Americans in 1960.[9] On the other hand, the two groups were more similar across other indicators of socio-economic development such as the unemployment rate, the share of families living in poverty, and the share of workers without a high school diploma. They were also comparable in terms of racial attitudes, proxied by the counts of African American lynchings that took place in the first half of the twentieth century, whereas the incidence of pro- and anti-African American activism registered in the first half of 1960 was larger across covered jurisdictions.

## 3.4 The effect of the VRA

### 3.4.1 Estimation strategy

Before 1965, seven states of the former Confederacy – Alabama, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, and Virginia – adopted restrictions to the access to the franchise (literacy tests and poll taxes, among others), which

[9]To address these imbalances, different matching techniques are considered (geographic regression discontinuity (GRD) design, trimming the sample to common support, and the coarsened exact matching algorithm) to probe the robustness of these results. See Section 3.4.2 for details.

were administered in a discriminatory fashion to disproportionately affect African Americans. The VRA, together with their removal, brought in special provisions, known as coverage, targeted at these historically more discriminatory jurisdictions. The goal is to assess the effect of such provisions on the representation of African Americans across county governments.

Based on the empirical strategy of Chapter 2, this study exploits an important source of heterogeneity in the exposure to the federal intervention in order to identify the causal effect of the VRA: covered counties displayed substantial variation in their racial composition. Since race was highly salient for voters at the time, the potential impact of coverage is expected to be greater in areas with larger African American population shares. In other words, the increase in African American office-holding should be positively related to the pre-VRA share of African Americans and, crucially, if coverage had an impact, this slope should increase around the time of the passage of the Act. However, counties with larger pre-existing shares of African Americans might have experienced increases in the representation of African Americans even in the absence of coverage. Hence, to isolate the effect of these special provisions, the remaining counties in the former Confederacy (that were not covered by the VRA but that nonetheless held a similar history of slavery and disenfranchisement) are considered to form a suitable comparison group. In particular, this group includes Arkansas, Florida, Tennessee, and Texas, as well as 61 counties in North Carolina.

Thus, following Cascio and Washington (2014), a triple difference research design is implemented, estimating whether covered counties with larger African American population shares (treatment group) experienced a larger increase in African American representation – from before to after the VRA – when compared to counties that were not covered (control group). The main identifying assumption is that, in the absence of the VRA, the two groups – that in the pre-VRA period did not exhibit any significant difference in African American representation – would have experienced the same patterns in the election of African Americans. Under this assumption, non-covered counties provide a valid counterfactual to causally

identify the effect of coverage.[10] Since different pre-VRA patterns in the main outcomes of interest could cast doubts on the main identification strategy, an event study examining pre-trends in African American office-holding within treatment and control groups is presented first. Then, a triple difference specification focusing on long-run changes in African American office-holding is considered.

Starting with the event study, the focus is on how the relationship between the share of African American elected officials and the share of African Americans in 1960 changed over time depending on coverage. This is done by estimating the following specification:

$$\begin{aligned} ShareBlackElected_{c,s,t} &= \sum_{n>62} \gamma_n D_n^t ShareBlack_{1960} + \\ &+ \sum_{n>62} \theta_n D_n^t ShareBlack_{1960} \times VRA_{c,s} \\ &+ \mathbf{X'}_{c,s}\beta + I_{s,t} + I_c + \epsilon_{c,s} \end{aligned} \tag{3.2}$$

where $ShareBlackElected_{c,s,t}$ is the share of African American elected officials across county governments, $ShareBlack_{1960}$ is the share of African Americans in the county population in 1960, $VRA_{c,s}$ is a dummy variable indicating whether the county was covered by the VRA in 1965, $D_n^t$ is a dummy variable equal to one whenever $n = t$, and $\mathbf{X'}_{c,s}$ is the vector of pre-VRA controls. Furthermore, both state-year interactions ($I_{s,t}$) and county fixed effects ($I_c$) are included in the model, and standard errors are clustered at the county-level. As county fixed effects are included, year 1964 is omitted from the sample in order to estimate the model. In other words, the year immediately before the VRA is used as the reference point to evaluate how the relationship between the outcome of interest (the share of African American elected officials) and the key explanatory variable (the share of African Americans in the county population in 1960) changed over time.

Figure 3.1 plots the estimated coefficients of interest: $\theta_n$. These coefficients capture the difference in the relationship between the pre-VRA African American population shares and the share of African American elected officials between

[10] In addition to African American office-holding, parallel trends in pre-VRA political participation, African American political activism, and white racial attitudes support the suitability of the chosen control group. These results are discussed in Section 3.4.2.

covered and non-covered counties. While before the passage of the VRA, no differential pattern between treatment and control groups could be observed, after the introduction of the Act, the difference becomes positive, statistically distinguishable from zero, and growing over time, reaching 2.6% in 1980 and 3.0% in 1990.

To provide a more rigorous framework of analysis to subsequently assess the robustness of the results, the estimation of long-run specifications is considered, where the dependent variable is the change in the shares of African American elected officials between 1964 and 1980 (or 1990). The following specification is estimated:

$$\begin{aligned} \Delta ShareBlackElected_{c,s} &= \gamma ShareBlack_{1960} + \\ &+ \theta ShareBlack_{1960} \times VRA_{c,s} + \mathbf{X'}_{c,s}\beta + I_s + \epsilon_{c,s} \end{aligned} \tag{3.3}$$

where $\Delta ShareBlackElected_{c,s}$ is the change in the share of African American elected officials in county $c$, within state $s$, between 1964 and 1980 (or 1990), and $I_s$ are state fixed effects (a state specific trend in this long difference specification). In this model, the coefficient $\gamma$ captures how the change in African American office-holding relates to the pre-VRA share of African Americans in the control group, whereas $(\gamma + \theta)$ does the same for the treatment group. As before, the interest is in the difference between the two, which is measured by $\theta$. Under the identifying assumption that, in the absence of coverage, African American representation would have changed in the same way within treated and non-treated counties, $\theta$ provides an unbiased estimate of the effect of coverage.

The estimation results are reported in Table 3.1, where the robustness of the main coefficients of interest using a series of alternative specifications is assessed. Column (1) regresses the long-run change in the share of African American elected officials on the share of African Americans in 1960 and its interaction with the VRA indicator and state trends. Column (2) additionally accounts for socio-economic controls, column (3) for racial attitudes and political activism, and column (4) interacts all controls with the VRA indicator. African American office-holding grew significantly more in the treatment group than in the control group, both in the medium-run (1964-1980) and in the long-run (1964-1990). As the model becomes

increasingly more complex and demanding, the coefficients on the interaction of interest remain robust and, if anything, grow in magnitude.[11]

As for the other control variables, African American office-holding grew more in larger counties with less skilled workers. On the other hand, poverty and agricultural productivity were, respectively, negatively and positively correlated with African American office-holding. As for racial attitudes and political activism, the counts of African American lynchings and the number of anti-African American protests were both associated with fewer African American elected officials, while the opposite was true for the counts of pro-African American activism, although some of these variables tend to lose explanatory power in the most demanding specifications.

### 3.4.2 Threats to identification

The results presented so far indicate that the VRA fostered the election of African Americans across county governments. Under the assumption that non-covered counties provide a valid counterfactual, a causal interpretation could be given to these results. As discussed before, covered and non-covered counties shared a very similar history of racial discrimination and disenfranchisement and the event study shows that, before the VRA, the two groups displayed parallel trends in African American office-holding. At the same time, the control group might still exhibit different pre-trends in other important outcomes that could be correlated with subsequent gains in African American office-holding. Hence, to corroborate the validity of the counterfactual, whether the assumption of parallel trends holds is investigated for other important outcomes: political participation, political activism, and white racial attitudes.[12] The results, which are reported in Table C.2.1, show that this was indeed the case.

Although the absence of different pre-trends supports the suitability of the control group, differences in observable characteristics may raise additional concerns.

[11]The fact that the estimated coefficients become larger as more controls are added would indicate that omitted variables are driving the estimates downwards in the simpler specification.

[12]These are measured, respectively, by the change in turnout rates between the presidential elections of 1964 and 1952, the difference in counts of NAACP local branches between 1964 and 1940 and the change in the number of Ku Klux Klan (KKK) klaverns for the same period.

In particular, covered counties were characterized by substantially larger African American population shares. And even if the two samples display a large degree of common support that is sufficient to identify the effect of coverage, this imbalance could remain problematic for two reasons. First, the results could still be driven by observations outside the common support. Second, differences on observables might be associated with an imbalance in the unobservables, leading to biased estimates. To address these concerns, three robustness checks are carried out, using matching techniques that reduce the imbalance in the observable covariates. These results are summarized in Table 3.2: columns (1)-(4) for the medium-run, and columns (5)-(8) for the long-run. To simplify the comparison with the baseline results, columns (1) and (5) report the findings of the most demanding specification from Table 3.1.

By deploying a geographic regression discontinuity (GRD) design, focusing on counties whose centroid is located within 25 miles from a state border, 295 valid county-pairs could be obtained on counties sharing a border between covered and non-covered states. As shown in Figure 3.2, counties sharing a border are balanced in terms of African American population shares, and the same holds true for the full set of control variables used in the specification. Columns (2) and (6) of Table 3.2 present the results for the GRD design, confirming the positive and significant effect of coverage on the change in the share of African American elected officials. These results continue to hold when two additional methods to reduce the imbalance in the covariates are considered: in columns (3) and (7), the overall sample is trimmed to its common support and, in columns (4) and (8), the coarsened exact matching algorithm is used instead. With both methods, and for both sample periods, the estimated coefficients are positive and statistically significant, and they grow in magnitude over time.

## 3.5 Representation by type of office

Having provided robust evidence on the effect of coverage on overall African American representation in county governments, the focus is now on the impact across different types of office. In fact, while the VRA clearly led to an overall

increase in the share of African Americans elected, concerns have been raised on the possibility that this federal intervention might have merely had an effect on minor positions, while leaving offices of greater authority largely unaffected. To shed light on this important question, the previous analysis is augmented to include different elective positions: commissioners, judicial and law enforcement officers, and other minor county-level posts (including, among others, the offices of the tax assessor, tax collector, coroner, prosecutor, clerk, register of deeds, treasurer, and commissioner of revenue).

Figure 3.3 presents the differences in the gradients of the initial share of African Americans between covered and non-covered counties, obtained from event studies carried out separately for county commissioners, judiciary and law enforcement, and other county-level offices. The VRA had a positive and significant impact across the board. In fact, for all types of office, the coefficients $\theta_n$, obtained by estimating Eq. 3.2, are statistically different from zero from 1971 onwards.

These results provide evidence that the VRA led to an increase in African American office-holding. Yet, as the judiciary and law enforcement category also includes several minor posts, in order to study the extent to which the VRA fostered the election of African Americans to positions of greater authority, the focus is now restricted on the most important county-level offices: commissioners, judges, and sheriffs. First, the impact of the VRA on African American representation in these offices is analyzed. Then, it is explored whether its effect varies depending on other pre-existing county-level characteristics (human capital, political organizations, racial attitudes, and electoral rules) that might have enhanced or abated the effect of the federal legislation. Starting with the baseline long-run difference specification, Table 3.3 presents results distinguishing between two changes: 1964-1980 and 1964-1990. The long-run results presented in columns (4)-(6) confirm the findings of the event study of Figure 3.3. The VRA had a positive and statistically significant effect on the electoral outcomes of African Americans across these three offices. At the same time, while for commissioners and sheriffs this effect is already evident by 1980, as shown in columns (1) and (3), respectively, the VRA did not increase the odds of

African Americans being elected as county judges until 1990, as shown in column (2). The VRA's impact is socially and politically meaningful: by 1990, a 10% rise in the 1960 share of African Americans in the county population led to an additional 1.2% increase in the share of African Americans elected as commissioners, and this is on top of the 1.3% effect registered in non-covered counties: column (4); for sheriffs, the additional gain above and beyond what is found in non-covered jurisdictions is estimated at 1.7%: column (6); lastly, the effect on judges is comparatively smaller, at about 0.2%: column (5).

Next, the role that other pre-existing characteristics might have played in enhancing (or abating) the overall positive effects of coverage is considered. In fact, while the VRA put in place a powerful legal apparatus to foster political participation among African Americans, the federal intervention came into place against the backdrop of several elusive obstacles that could have hampered its effectiveness. First, the disadvantaged socio-economic conditions of African Americans did act as a key driver of low African American turnout rates (Matthews and Prothro 1963). Second, limited access to organizational resources and individual skills to run professional political campaigns made it difficult for African Americans to enter the political arena (Karnig 1979). Lastly, pre-existing electoral rules, such as elections at-large (less favorable to the election of minorities), were also standing in the way of progress in African American office-holding (Engstrom and McDonald 1981). Although civil rights organizations where actively trying to overcome these barriers, they were battling against an enduring culture of white supremacy, discouraging the political participation of African Americans (Salamon and Evera 1973). To disentangle the conditional effect of the VRA from these pre-existing characteristics, a quadruple difference design is considered, which augments the baseline specification with a triple interaction between the share of African Americans in the county population in 1960, the VRA indicator, and either pre-VRA individual and social characteristics (human capital, political organizations, and white supremacy) or electoral rules.

### 3.5. Representation by type of office

Human capital is measured as the share of African Americans who had attended at least one year of college by 1960.[13] To quantify the roles of political organizations and white supremacy, historical data is considered in order to build relevant proxies. Under Jim Crow laws, African Americans were systematically excluded from many civil society institutions, and African American churches filled this void becoming "the organizational hub of [B]lack life" (Morris 1984). Not surprisingly, they played a crucial role in the civil rights movement by providing leadership, financial resources, and infrastructure for mass mobilization and political campaigns. Hence, to capture the strength of pre-existing political organizations, memberships in African American churches (as collected in the Census of Religious Bodies of 1936) are considered.[14] To capture county-level differences in the extent of white supremacy, counts of episodes of lynchings of African Americans in the first half of the twentieth century (between 1902 and 1964) are considered. Lynchings, although publicly condemned at the federal level, were often left unpunished by local authorities, and for this reason they provide a good proxy for the negative racial attitudes across the U.S. South.

As for the role played by electoral rules, since judges and sheriffs are always elected at-large, the focus is on county commissioners, for which meaningful institutional variation in the system used to elect them could be exploited. In particular, some covered states elected commissioners by single-member districts (SMD) before the passage of the VRA, and retained this system throughout the entire period of this analysis. Other states that held elections at-large pre-VRA, either started to switch toward SMD during the 1970s, or began their transition in the 1980s and 1990s. Then, three categorical variables allocate counties to three groups: SMD (holding elections by single-member districts throughout the entire period), and early switchers and late switchers (that started to transition toward SMD before and after 1980, respectively).

[13]This measure, obtained from the 1960 Census of Population, has been standardized by the total counts of African Americans in the county. The number of available counties in the sample is reduced to 740 because the information on the numerator is available only for those counties with at least 1,000 nonwhites.

[14]This is the most recent year in which the Census of Religious Bodies was carried out.

Figure 3.4 presents the findings on the role of individual and social characteristics, plotting the estimated coefficient of their interaction with the share of African Americans in the county population in 1960 and with the VRA indicator.[15] Starting with human capital, the positive and statistically significant coefficients reported in the top panel indicate that African American office-holding gains in covered counties with larger shares of African Americans were larger, the higher the pre-existing level of human capital among African Americans. This is consistent with theories highlighting the crucial effect of human capital on both turnout (Matthews and Prothro 1963) and on decisions to run for office (Shah 2014 and Dal Bó et al. 2017). Considering next the role of political organizations, the membership with African American churches had a positive effect on the election of judges and sheriffs, but not on the election of commissioners. One important difference among these offices is that, while judges and sheriffs are typically single-member bodies elected for the whole county, commissioners are members of boards and often represent smaller communities within each county. Thus, for the first two types of office, the organizational infrastructure provided by African American churches appears to be more important, while its role tends to be diluted in the election of the more geographically-fragmented county commissioners. As for the role of racial attitudes (proxied by the lynchings of African Americans), white supremacy reduced the effectiveness of the VRA in the election of both commissioners and sheriffs.

Lastly, the impact of electoral rules is analyzed, and the estimation results are illustrated in Figure 3.5. The top panel, in which covered counties with the SMD system are compared with those counties that subsequently adopted this system, shows that that the positive gains from the VRA are concentrated in the former group, both in the medium-run and in the long-run. The bottom panel further distinguishes between counties that always had SMD, those that switched toward SMD early (before 1980), and those that switched late (after 1980): early switchers catch up with the former group, whereas late switchers lag behind, with no significant gains in African American office-holding. Consistent with Chapter 2

[15]For the full estimation results of the triple difference specifications, see Tables C.2.2, C.2.3, C.2.4, and C.2.5.

and with earlier studies focusing on other types of elected bodies (Engstrom and McDonald 1981 and Marschall, Ruhil, and Shah 2010), these results confirm that elections by SMD were indeed more favorable to African American representation.

In conclusion, this analysis indicates that, while the VRA led to an increase in African American office-holding, its effect was influenced by other important pre-existing characteristics with an impact lasting a quarter century after the passage of the VRA. In particular, even in the long-run, (a) the VRA continued to play a crucial role in the election of African Americans, and (b) the level of human capital and political organizations among African Americans in the early 1960s, as well as electoral rules, still influenced the effect of the federal intervention. By the same token, the vestiges of a white supremacy culture continued to act as an obstacle to the election of African Americans. Taken together, these results deliver, on the one hand, a positive picture on the effectiveness of legislative tools in opening elective offices to minorities. On the other hand, they also provide a stark warning on the persistence of barriers to African American representation in the U.S. South.

## 3.6 How much progress? The extensive margin

The results presented so far all point toward the same conclusion: the VRA had a meaningful, positive, and significant impact on the share of African Americans elected across southern county governments. But to what extent did the VRA manage to break the lack of representation observed in the pre-VRA years? To answer this question, the focus is now on the extensive margin of African American office-holding, defined as the share of counties that elected at least one African American official.

Figure 3.6 plots the evolution over time of the share of counties that have at least one African American elected official. The top-left panel considers all county governments, to then turn to the three sub-categories of commissioners, judiciary and law enforcement, and other elected officials. Consistent with the previous findings, covered counties exhibit faster progress in African American representation compared to those counties that are not covered by the Act. Despite the clear

progress in the aggregate measure, which indicates that, by 1990, almost one in two covered counties had an African American elected official, the extensive margin of African American office-holding across different types of offices reveals a strikingly uneven pattern. While the evolution of representation in county commissions mirrors the aggregate one, considering judicial and law enforcement bodies, only about one in ten covered counties had elected an African American by 1990. For all other locally-elected officials, the figure is even lower, at 7%. Importantly, these findings indicate that the aggregate gains in representation are driven by the election of African Americans as county commissioners. Moreover, whenever an African American was appointed to either judicial and law enforcement bodies or other minor county offices, this typically occurred in counties where an African American also sat in the county commission, whereas the reverse is not true.

Since counties differ substantially in the shares of African Americans in the total population, the somewhat limited progress observed in some offices could be driven by the limited size of the African American constituency. To address this concern, Figure 3.7 repeats the previous analysis, now focusing on the sub-sample of majority-Black counties. As shown in the top-left panel, the share of majority-Black counties with at least one African American elected official is significantly larger compared to the overall sample. In fact, by 1990, almost 80% of these counties had an African American sitting in a county office. As before, the overall pattern for county commissioners mirrors the aggregate one. Interestingly, the similar progress in covered and non-covered counties also suggests that, for this type of office, the VRA plays an important role only when African Americans are a minority within the county. The picture for judicial and law enforcement bodies is instead very different: only about 30% of covered majority-Black counties elected at least one African American official in this type of office and the VRA plays a very important role. Moreover, Figure 3.8 shows that progress is even smaller for the top elective positions in this group: by 1990, only about 15% of covered counties have an African American sheriff, and this figure is halved in the non-covered sample. For African American judges, the statistics is even grimmer, with 95% of majority-Black

counties not electing a single one. Importantly, the gap in the extensive margin of African American office-holding between covered and non-covered counties remained large and tended to widen over time. This suggests that, however small the effect of the VRA might have been, its presence was vital to bring African Americans to these types of positions, since in non-covered majority-Black counties progress among judicial and law enforcement bodies was extremely slow.

In conclusion, while the VRA led to sizable gains across county governments, progress varied substantially by type of office. African Americans were more likely to be elected to multi-member bodies, such as county commissions, rather than to county-wide offices with a single member: judges and sheriffs. These differences might be explained by the fact that, when multi-member bodies are elected by SMD, minority candidates are more likely to be elected. However, since striking differences continue to hold even when African Americans constitute a majority of the population in the county, the electoral system alone cannot explain why, a quarter century after the passage of the VRA, judicial and law enforcement offices in majority-Black counties remained overwhelmingly white. Cultural factors and selection procedures into judicial and law enforcement careers are likely to have played an important role. As pointed out by Moore (1997): "Sheriffs and their deputies were principal enforcers of the social and legal convention of the Jim Crow Society." They played a key role in the disenfranchisement of African Americans, making sure that "[B]lacks voters didn't get into the polling booth at all," whereas in the administration of justice, Black-on-Black crime was usually ignored, white-on-Black crime was not punished, and Black-on-white crime was addressed by the extra-legal practice of lynching. Recruitment of African Americans as police officers was also limited, and even when African American officers were employed, they typically did not have arrest powers over whites (Rudwick 1962). Likewise, the racist culture permeating southern Law Schools discouraged African Americans from choosing the legal practice, limiting their opportunities to reach those higher offices requiring a law degree (Cook 1971).

Although the practices of the Jim Crow era no longer have a place in the contemporary U.S. South, this analysis indicates that African Americans remained at a considerable disadvantaged level when elective offices required qualifications or experience in professions where minorities are typically under-represented. At the same time, the fact that – even twenty-five years after the passage of the VRA – African Americans were more likely to gain office in covered compared to non-covered counties, indicates that the federal intervention remained vital to overcome obstacles to African American representation.

## 3.7 Conclusions

The VRA was heralded by President Lyndon B. Johnson as "one of the most monumental laws in the entire history of American freedom." Yet, our understanding of its effect on African American descriptive representation has remained limited. In particular, little is known on its impact across those important bodies (county commissions, sheriffs, and judges) that are at the heart of local governments in the U.S. South. This chapter has provided the first systematic assessment of this question, offering causal evidence on its positive impact.

The results indicate that, over the quarter century following the VRA's enactment, significant progress was made: half of all covered counties did elect at least one African American official, compared to less than 20% in the set of non-covered counties. At the same time, this chapter has found striking differences in the ability of African Americans to gain representation across different types of office. While progress has been swift and substantial across county commissions, the same was not true for judicial and law enforcement bodies. This imbalance is particularly noticeable in majority-Black counties: while almost 80% of covered counties had some African American representation across county commissions, only 15% of them had an African American sheriff and less than 5% had an African American judge.

This chapter has also investigated the role played by important factors, including African American human capital, African American political organizations, and white racial attitudes. The analysis has highlighted that pre-existing levels of human

capital and political organizations magnified the positive impact of the VRA, whereas a culture of white supremacy lowered it. Interestingly, African American political organizations played a meaningful role only for county-wide single-member offices (judges and sheriffs), suggesting that the lack of resources to run effective campaigns might have particularly penalized African American candidates in this type of races.

Descriptive representation has been shown to affect outcomes in policy areas particularly relevant for minorities and under-represented groups. Hence, the patterns of African American office-holding across county governments that have been uncovered in this chapter call for an assessment on their effects. Chapter 2 has shown that the gains in African American office-holding led to an increase in public investment and local spending across African American communities. Given the centrality of the racial dimension in law enforcement and in the administration of justice, further research would instead be needed on the causes and consequences of under-representation of minorities in these offices.

**Figure 3.1:** African American elected officials in county governments

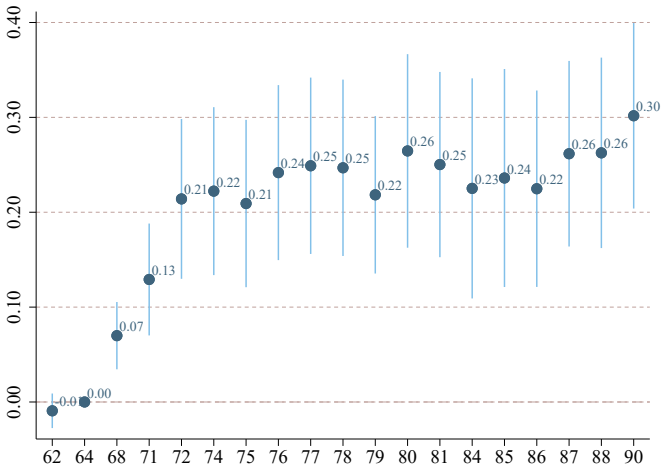
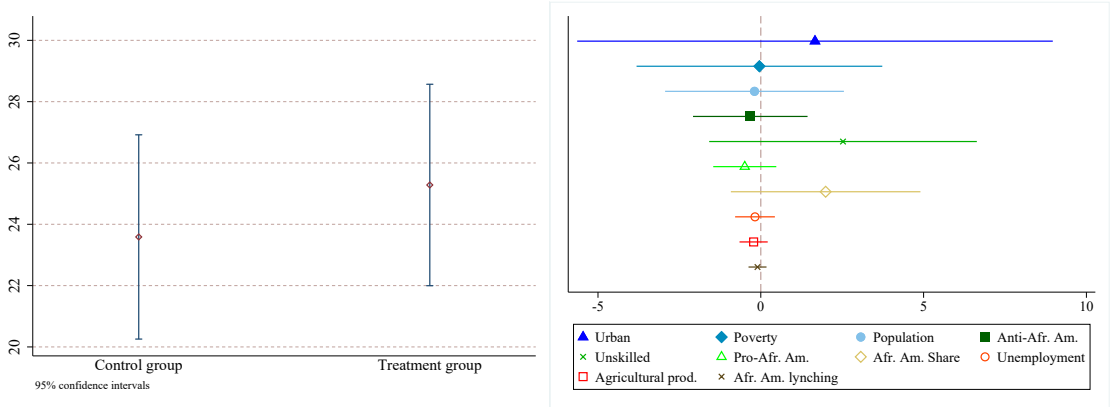
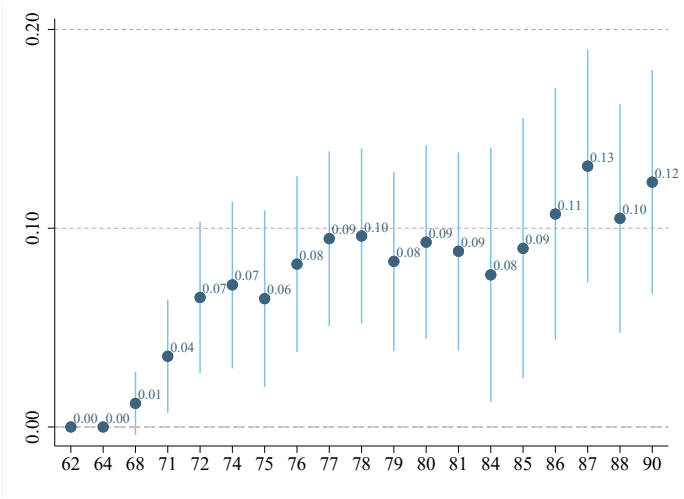



**Figure 3.2:** GRD design: Balance in the sample

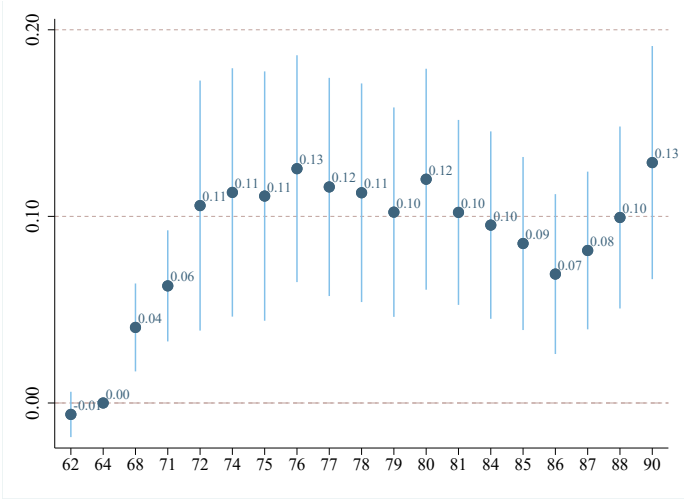



**(a)** Means in the Share of African Americans **(b)** Balance in the Values of the Covariates

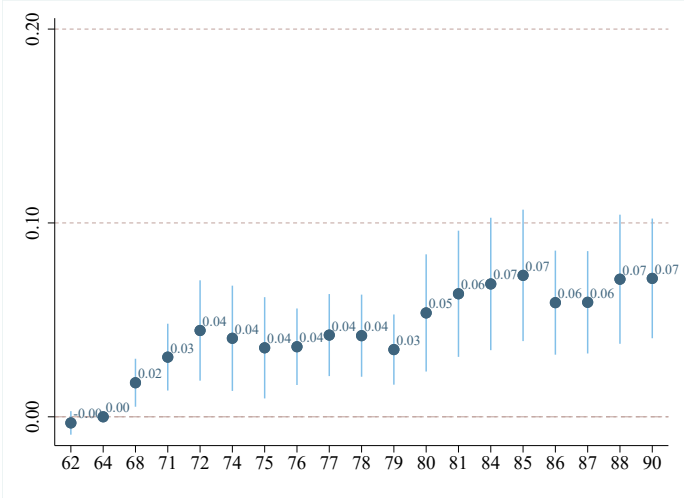
**Figure 3.3:** Differences in the gradient by type of office




**(a)** County Commissioners

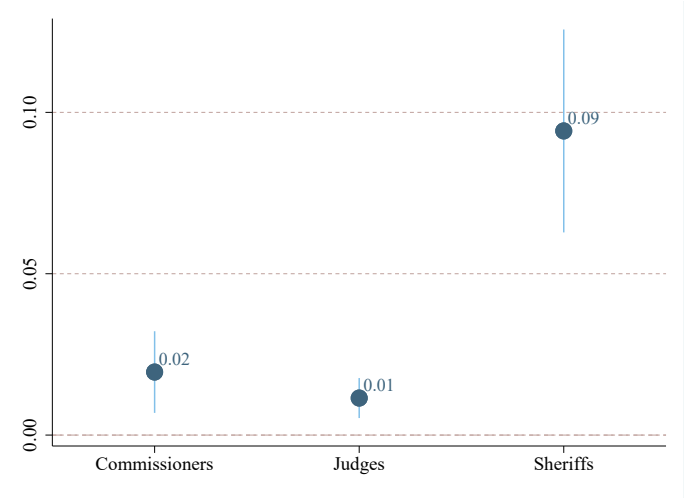



**(b)** Judiciary and Law Enforcement

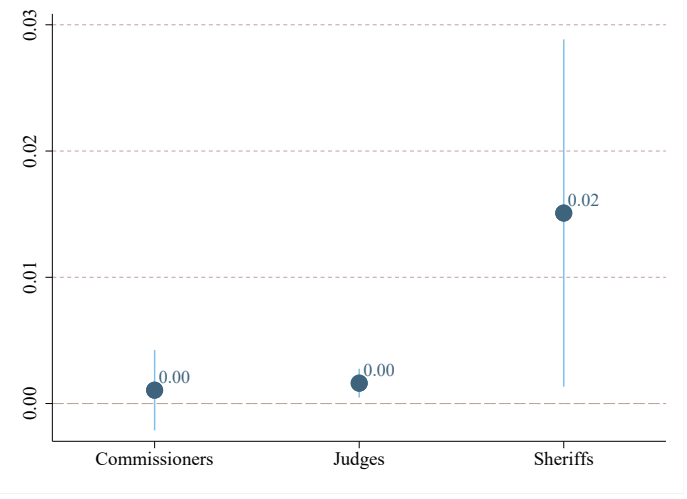



**(c)** Other County Officials

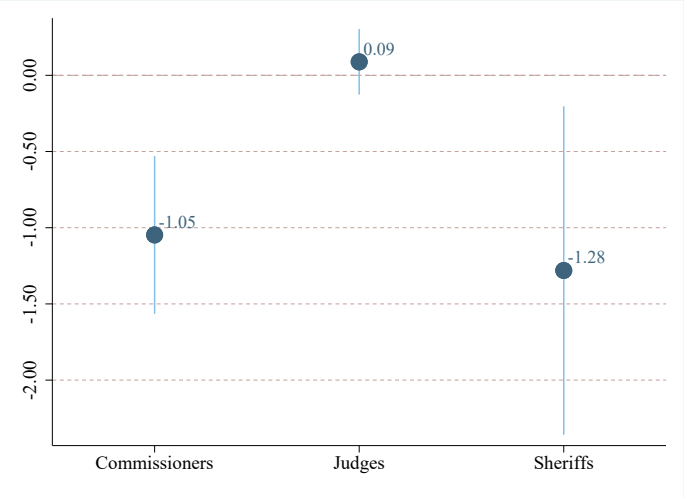
**Figure 3.4:** Human capital, political organizations, and racial attitudes




**(a)** African Americans with College-Level Education

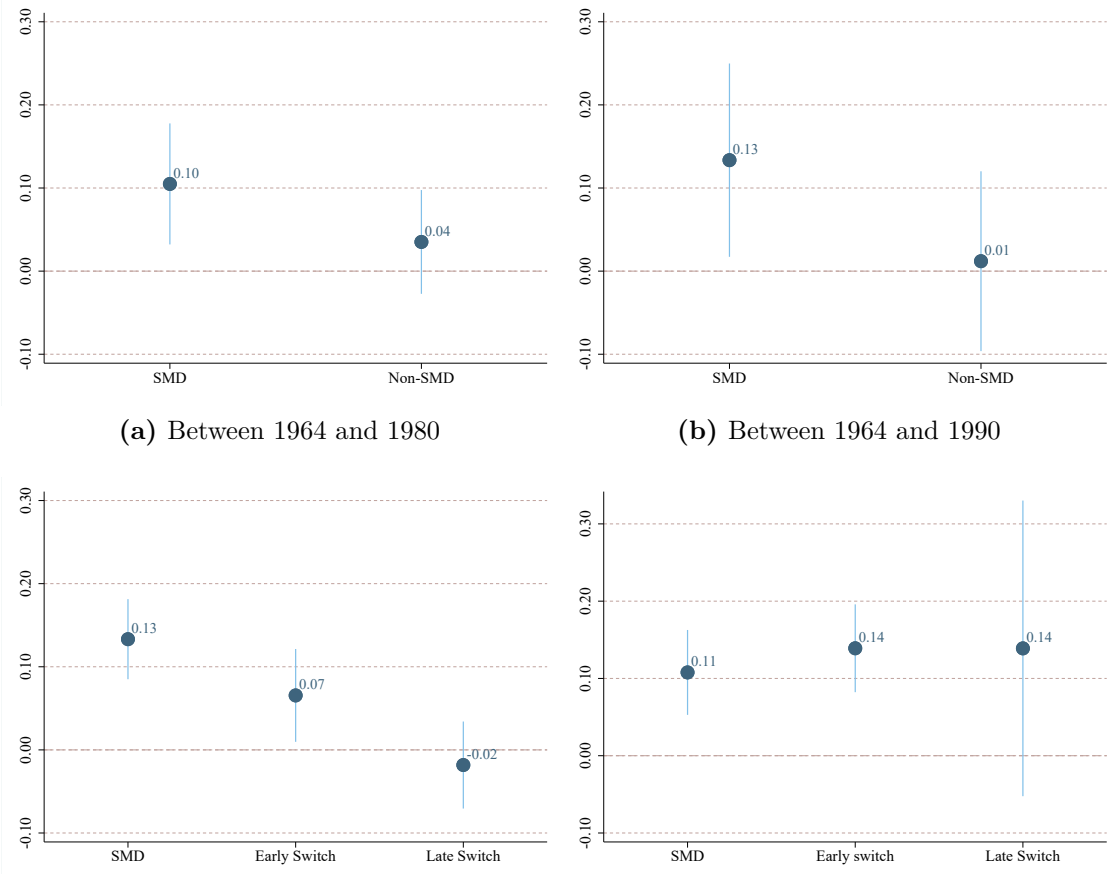



**(b)** African American Churches




**(c)** Lynchings of African Americans

**Figure 3.5:** Electoral rules



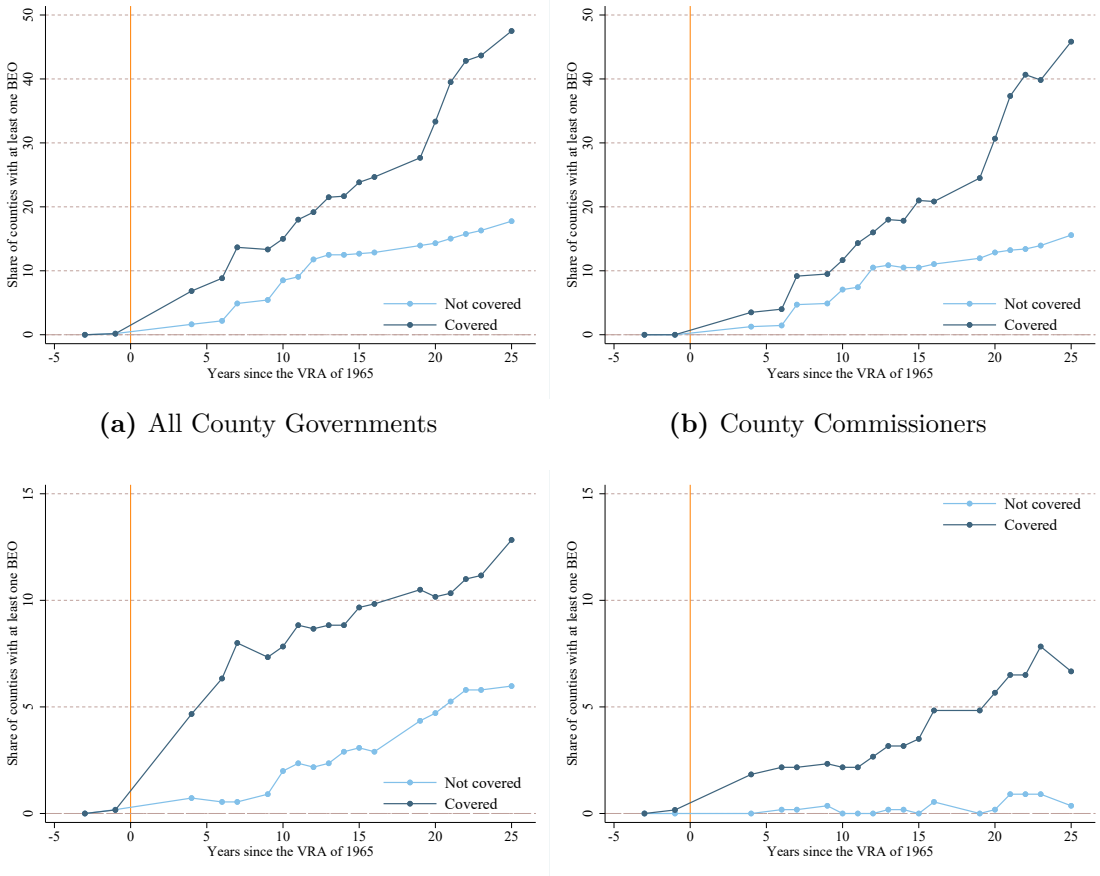

**(a)** Between 1964 and 1980

**(b)** Between 1964 and 1990

**(c)** Between 1964 and 1980

**(d)** Between 1964 and 1990

**Figure 3.6:** Progress in African American office-holding



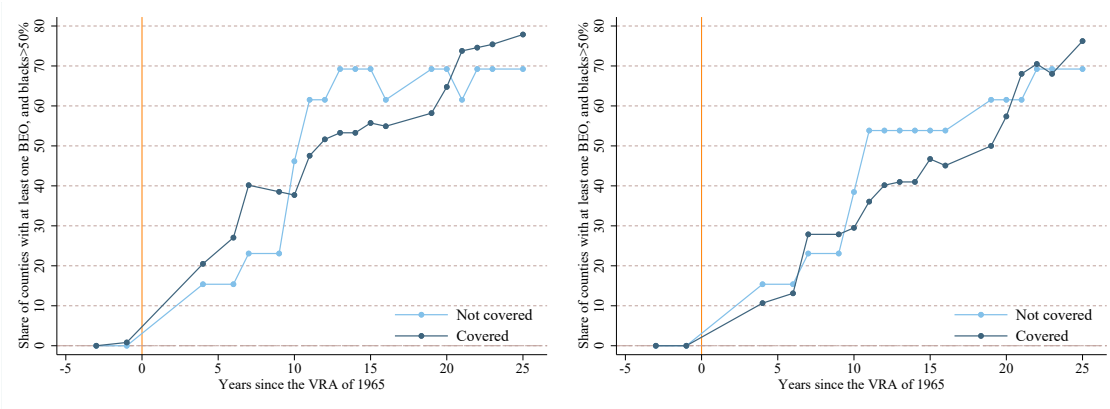

**(a)** All County Governments

**(b)** County Commissioners

**(c)** Judiciary and Law Enforcement

**(d)** Other County Officials

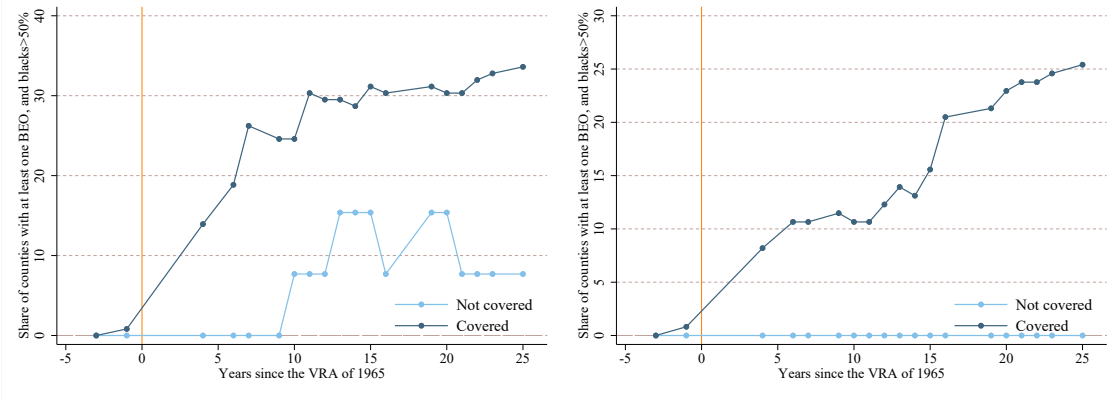
**Figure 3.7:** Progress across majority-Black counties




**(a)** All County Governments




**(b)** County Commissioners

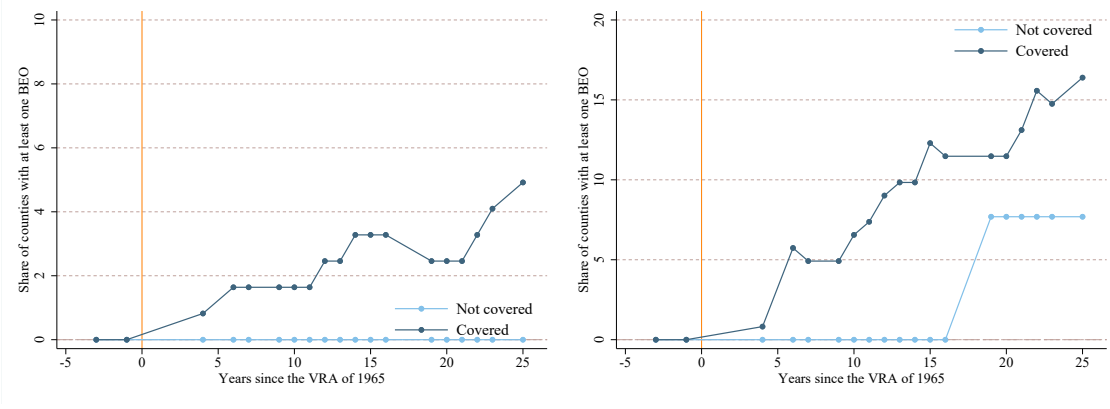



**(c)** Judiciary and Law Enforcement




**(d)** Other County Officials

**Figure 3.8:** African American judges and sheriffs




**(a)** Judges




**(b)** Sheriffs

**Table 3.1:** The effect on the change in the share of elected officials

| | 1964-1980 | | | | 1964-1990 | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Share of Afr. Am. in 1960 X VRA | 0.155*** | 0.173*** | 0.175*** | 0.265*** | 0.235*** | 0.258*** | 0.258*** | 0.303*** |
| | (0.037) | (0.038) | (0.038) | (0.052) | (0.043) | (0.044) | (0.044) | (0.050) |
| Share of Afr. Am. in 1960 | 0.092*** | 0.104*** | 0.100*** | 0.061*** | 0.168*** | 0.168*** | 0.159*** | 0.148*** |
| | (0.014) | (0.017) | (0.017) | (0.012) | (0.027) | (0.029) | (0.029) | (0.025) |
| Unemployment, 1960 | | 0.072 | 0.094 | –0.019 | | 0.039 | 0.115 | 0.065 |
| | | (0.130) | (0.130) | (0.046) | | (0.148) | (0.139) | (0.081) |
| Poverty, 1960 | | 0.014 | 0.016 | –0.056*** | | –0.018 | –0.012 | –0.075*** |
| | | (0.029) | (0.029) | (0.016) | | (0.032) | (0.031) | (0.026) |
| Total population, 1960 | | 0.005* | 0.002 | 0.006*** | | 0.027** | 0.015*** | 0.013*** |
| | | (0.003) | (0.002) | (0.002) | | (0.011) | (0.006) | (0.005) |
| Unskilled, 1960 | | –0.037 | –0.037 | 0.121*** | | 0.030 | 0.026 | 0.124** |
| | | (0.025) | (0.025) | (0.034) | | (0.032) | (0.030) | (0.049) |
| Agricultural productivity | | –0.980 | –1.209 | 0.155 | | –1.042 | –1.458* | –0.935* |
| | | (0.801) | (0.795) | (0.284) | | (0.889) | (0.871) | (0.517) |
| Rural farms, 1960 | | –0.008 | –0.003 | 0.011 | | –0.045 | –0.031 | 0.028 |
| | | (0.027) | (0.027) | (0.011) | | (0.031) | (0.030) | (0.019) |
| Afr. Am. lynching, 1902-1964 | | | –1.211* | –0.116 | | | –2.075* | –0.241 |
| | | | (0.667) | (0.350) | | | (1.122) | (0.483) |
| Pro-Afr. Am. protest, 1960-1964 | | | 0.220*** | 0.130 | | | 0.588*** | 0.624 |
| | | | (0.048) | (0.112) | | | (0.194) | (0.389) |
| Anti-Afr. Am. protest, 1960-1964 | | | –0.462*** | –0.560 | | | –0.529 | –3.010** |
| | | | (0.134) | (0.419) | | | (0.341) | (1.274) |
| Controls | No | Yes | Yes | Yes | No | Yes | Yes | Yes |
| Controls X VRA | No | No | No | Yes | No | No | No | Yes |
| State Trends | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Adj. R-Square | 0.31 | 0.32 | 0.32 | 0.35 | 0.48 | 0.52 | 0.55 | 0.57 |
| N | 1152 | 1021 | 1021 | 1021 | 1152 | 1021 | 1021 | 1021 |

Robust standard errors in parenthesis are adjusted for clustering at the county-level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

**Table 3.2:** The stability of the effect

| | 1964-1980 | | | | 1964-1990 | | | |
|---|---|---|---|---|---|---|---|---|
| | Entire Sample (1) | Border Sample (2) | Common Support (3) | CEM (4) | Total Effect (5) | Border Sample (6) | Common Support (7) | CEM (8) |
| Share of Afr. Am. in 1960 X VRA | 0.265*** | 0.122*** | 0.107*** | 0.191*** | 0.303*** | 0.113** | 0.162*** | 0.232*** |
| | (0.052) | (0.027) | (0.031) | (0.047) | (0.050) | (0.046) | (0.042) | (0.046) |
| Share of Afr. Am. in 1960 | 0.061*** | –0.001 | 0.068*** | 0.061*** | 0.148*** | 0.112 | 0.166*** | 0.148*** |
| | (0.012) | (0.046) | (0.012) | (0.012) | (0.025) | (0.125) | (0.027) | (0.024) |
| Controls | Yes | Yes | Yes | Yes | No | Yes | Yes | Yes |
| Controls X VRA | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| State Trends | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Adj. R-Square | 0.35 | 0.61 | 0.41 | 0.33 | 0.57 | 0.53 | 0.57 | 0.56 |
| N | 1021 | 295 | 944 | 1014 | 1021 | 295 | 944 | 1014 |

Robust standard errors in parenthesis are adjusted for clustering at the county-level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

**Table 3.3:** The effect across county governments

| | 1964-1980 | | | 1964-1990 | | |
|---|---|---|---|---|---|---|
| | Commission. (1) | Judges (2) | Sheriffs (3) | Commission. (4) | Judges (5) | Sheriffs (6) |
| Share of Afr. Am. in 1960 X VRA | 0.094*** | 0.006 | 0.236*** | 0.124*** | 0.015** | 0.170* |
| | (0.025) | (0.004) | (0.075) | (0.029) | (0.007) | (0.100) |
| Share of Afr. Am. in 1960 | 0.053*** | 0.001 | –0.012 | 0.128*** | –0.003 | 0.097 |
| | (0.011) | (0.001) | (0.012) | (0.018) | (0.004) | (0.063) |
| Controls X VRA | Yes | Yes | Yes | Yes | Yes | Yes |
| State Trends | Yes | Yes | Yes | Yes | Yes | Yes |
| Adj. R-Square | 0.39 | 0.06 | 0.09 | 0.55 | 0.22 | 0.15 |
| N | 1021 | 1021 | 1021 | 1021 | 1021 | 1021 |

Robust standard errors in parenthesis are adjusted for clustering at the county-level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

*The tight grip of a long dead hand is hard to break. More than one summer may pass before that grip is broken and the effect of its clasp on the present completely undone.*

— Judge John Minor Wisdom, *The Mississippi Freedom Democratic Party v. The Democratic Party of the State of Mississippi*, June 6, 1966

# 4

# The Impact of the Voting Rights Act on Political Mobilization and Counter-Mobilization

## Contents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## 4.1 Introduction

Between the full disenfranchisement achieved by the turn of the century and the passage of the Voting Rights Act (VRA) in 1965, African Americans were deterred from voting by a concurrent combination of administrative devices and threats of retaliation (Wright 2013). This was far more evident in the U.S. South where, to circumvent the Fifteenth Amendment, poll taxes and literacy tests had been first institutionalized as the prerogative to voting to be then widely administered in a discriminatory fashion by local registrars. As shown in Chapters 2 and 3, the lack of political participation of African Americans was also directly reflected by a complete lack of political representation: in the year immediately prior to the passage of the VRA, only 56 African Americans had been elected in local offices across all southern states.

At the height of both civil and voting rights movements and five months after the outrage at Selma's Bloody Sunday swept the country, Congress passed the VRA, which President Lyndon B. Johnson then signed into law on August 6, 1965. And although the VRA was expected to soon lead to many social and political gains, with the election of African American representatives, "the primary thrust of this new law was to facilitate [B]lack registration by supplanting the authority of local registrars" (Rodgers and Bullock 1972). Surprisingly, evidence on its direct effect on African American registration rates and political participation in the U.S. South remains scant. The lack of a robust evaluation is even more striking as the VRA is anecdotally the culprit of both a wide counter-mobilization effort by white voters and the loss of southern support for the Democratic Party observed across presidential elections since Richard Nixon's victory in 1968.

This chapter makes progress on these important questions by carrying out the first systematic assessment of the VRA's impact on the registration rates of African American and white voters and on their support for the Democratic Party between 1956 and 1988, by focusing on the counties belonging to ten states of the former Confederacy. With the VRA pre-empting both state and local authorities over voter qualifications, this chapter provides causal evidence of a direct and immediate effect

of the Act on the registration of African Americans. These large gains were not limited in time, and a robust effect is also found twenty years after the passage of the Act: by 1984, a 10% rise in the 1960 share of African Americans in the county population led to a 3.3% increase in African American registration rates, and this is on top of the 4.5% increase observed across counties not covered by the Act.

As African Americans began entering the political process, whites responded by registering more. By 1984, this indirect effect of the Act is estimated to reach 2.3% for a 10% increase in the share of African Americans in the county population. And in contrast with the registration of African Americans, this positive effect of the VRA is on top of a negative pattern that is observed across counties not subject to the Act, where whites were less likely to register. Furthermore, while the gain in African American representation was not a significant driver in ensuring that more African Americans registered to vote, whites are estimated to be registering more in the set of counties where the threat of African American office-holding was perceived to be the highest. That is, both the political mobilization of African-American citizens, and their subsequent gains across local elections, were matched by a robust white backlash.

In the aggregate, total voter registration rates rose by an additional 3.8% for a 10% increase in the share of African Americans in the county population, and this is on top of the 2.7% increase found in counties not covered by the Act. However, the large white counter-mobilization effort found in counties covered by the VRA meant that the Act did not have a significant effect in closing the gap in the registration rates between African American and white voters. On the other hand, this gap is estimated to narrow down in non-VRA counties, as the local white response had been absent.

Matching the rise in total registration rates, total turnout rates also increased significantly following the VRA by an estimated incremental effect of 1.7% twenty years following the passage of the Act. Consequently, the VRA also had a significant role in shaping the two-party competition that ensued in the U.S. South during the second half of the twentieth century. In counties covered by the VRA, the

drop in white affiliations with the Democratic Party is estimated to grow over time, reaching 3.1% for a 10% increase in the 1960 share of African Americans in the county population. On the other hand, and in line with anecdotal evidence, southern African Americans robustly voted in support for the Democratic Party.

As the VRA was meant to protect the rights of African Americans, a direct way to estimate the effect of this federal intervention would be to investigate how the relationship between the pre-existing share of African Americans in the county population and the outcomes of political participation changed over time within counties that were subject to the legal requirements of the Act. However, such an empirical strategy would ignore the fact that areas with larger shares of African Americans might have experienced changes in voter registration, turnout, and party support even in the absence of the specific provisions included in the VRA. For this reason, the above-mentioned strategy is combined with the creation of a suitable comparison group that includes all the remaining southern counties that were not subject to the VRA but that, nonetheless, maintained a similar history of African American disenfranchisement before 1965. Then, this chapter estimates whether covered counties with larger pre-existing African American population shares (the treatment group) experienced differential changes in the outcomes of political participation – from before to after the VRA – when compared to the other southern counties that were not covered by the Act (the control group). Throughout this study, the identifying assumption is that, in the absence of coverage from the VRA, the two groups, that in the pre-VRA period did not exhibit a statistically significant difference in the outcomes of political participation, would have experienced the same trends in voter registration, turnout, and party support.

To quantify the role played by the VRA in the surge of African American mobilization and white counter-mobilization efforts, a novel dataset on county-level voter registration statistics, differentiated by race, on ten southern states between 1956 and 1988 is assembled. At this level of granularity, this dataset constitutes, to the best of our knowledge, the most complete set of information on registration rates for the U.S. South during this important period. It includes five southern

states that were subject to the Act: Alabama, Georgia, Louisiana, Mississippi, and South Carolina. In addition, voter registration rates have been collected for two states not covered by the Act: Florida and Tennessee. As for North Carolina, this chapter includes information on both the 39 counties falling under the provisions of the Act as well as the 61 counties that instead faced no legal requirements. And while a complete panel could be assembled for these eight states, two additional southern states are included in a long-run specification analysis: Arkansas (not covered) and Virginia (covered by the VRA).

The contributions of this chapter are three-fold. First, although the VRA has had an undeniable and long-lasting impact upon American politics, the precise extent of this effect has long been debated. On one side of the argument, the registration and subsequent political participation of African Americans is attributed entirely to the VRA and its legal apparatus. This formulation has found support in the works of Lawson (1976), Thernstrom (1987), Herring (1990), Grofman, Handley, and Niemi (1992), Rosenberg (2008), and Wright (2013). The opposing side of this debate posits that additional elements, such as the civil rights movement (along with its voter registration drives) and the salience of the racial discourse during the presidential elections, have been the leading factors behind the observed rise in the registration of southern African Americans (Watters and Cleghorn 1967, Thompson 1986, Black and Black 1987, Arrington and Mead 1992, and Davidson and Grofman 1994). Timpone (1995) statistically tests these opposing sides and finds that, although the VRA had a meaningful effect in the most discriminatory states, the overall increase in the registration of African Americans occurred primarily through the efforts of the civil rights movement and the impetus from the 1964 presidential election. Then, mass mobilization and motivational factors stemming from race-conscious politics must also be taken into consideration when assessing the influence of the VRA. In this chapter, this insight informs the empirical strategy as both registration drives and other confounding factors are all accounted for. Compared to the existing studies – which assumed that pre-1965 trends would have remained the same without the VRA, and are thus based on an historical

counterfactual – this chapter builds a more robust counterfactual using a suitable control group, while exploiting more granular data on voter registration rates and party support on ten states of the former Confederacy.[1]

Second, considerable disagreement in the literature exists over the extent of the white backlash and its defining features. Two opposing arguments could be identified. One side states that the political participation of whites depends on the share of African Americans in the local population. Thus, it posits that it is the *social density* of African Americans that matters the most in explaining the counter-mobilization efforts, as it is the threat of mobilization, rather than the actual participation per se, that informs the behavior of whites. The second formulation argues that the participation of whites depends on the actual political mobilization of African Americans: more value is put on the *behavioral density* of African Americans. Exploiting survey data on the presidential elections that occurred between 1952 and 1984, Stanley (1987) finds that whites voted less in counties with larger shares of African Americans. Similarly, Carmines, Huckfeldt, and McCurley (1995) estimate very modest white counter-mobilization effects in either presidential or congressional elections between 1952 and 1988. Opposite to these results, and based on a period where full disenfranchisement was the norm, Key (1950) finds that whites participated more in the political process across Black Belt counties. By exploiting more granular data and an estimation strategy that is based on the geographic variation in the application of the VRA, this chapter finds support to both theories. In particular, whites are estimated to register more only in counties subject to the VRA, while less registration is observed across counties outside the jurisdiction of the Act. In addition, significantly higher rates of registration are found in the subset of counties where the racial threat was the highest, as measured by the gains in the political representation of African Americans.

Third, it is often assumed that, as a result of the push for more equal civil and voting rights, the Democratic Party "handed the South to the Republicans for a

[1]Fresh (2018) also measures the counterfactual from a control group, although the focus is restricted to the evaluation of the VRA in North Carolina.

generation."[2] With southern politics deeply revolving around race, the Democratic Party secured the overwhelming support of the newly enfranchised African American voters. However, the Democrats have failed to win in four out of five presidential elections that occurred between 1968 and 1988. The success of the Republican Party across presidential elections could only be explained by an increasing polarization of the vote, and Carmines, Huckfeldt, and McCurley (1995) indeed show that white counter-mobilization was not a bipartisan phenomenon: after 1964, whites were more likely to vote for the Republican Party if they lived in a county with a larger concentration of African Americans. Fresh (2018) also suggests that the VRA led to a change in party affiliation through the reactionary political behavior of white voters. Other studies (Wright 1976, Wright 1977, and Huckfeldt and Kohfeld 1989) further attest to the political polarization that has emerged across racial boundaries. This chapter builds on the existing literature by providing the first detailed account of the causal effect that the VRA had on party support by race. This is analyzed through the lens of county-level party affiliation data that is collected for Florida and Louisiana between 1960 and 1988.

The remainder of the chapter is organized as follows. Section 4.2 describes the dataset assembled for this study. Section 4.3 constitutes the main body of analysis. After presenting the estimation strategy in Section 4.3.1, Section 4.3.2 illustrates the evolution of the relationship between the outcomes of political participation and the share of African Americans in 1960, through the lenses of an event study analysis. Then, Section 4.3.3 isolates the variation in the effect stemming from the racial threat, while Section 4.3.4 checks the robustness of the results by considering a long-run specification and a regression discontinuity design that is based on the threshold rule for coverage from the VRA. Section 4.4 concludes.

[2]This remark is attributed to Lyndon B. Johnson, supposedly saying it to his aide Bill Moyers after signing the Civil Rights Act in 1964 (Wright 2013).

## 4.1. Introduction

## 4.2 Data

The goal of this chapter is to empirically assess whether the VRA had a significant impact on voter registration rates, turnout rates, and party support. In this section, the dataset used to carry out this analysis is discussed.

### 4.2.1 Voter registration

While both total counts of voter registration at the county-level and voter registration by race at the state-level are easily available from official publications, accounts on the registration by race and at the county-level is a much more challenging task. This information has never been systematically collected for the period and for the geographic extension considered in this study.

To conduct this analysis, a novel dataset on voter registration statistics is assembled. It includes information, by race and at the county-level, on eight southern states between 1956 and 1988. In particular, complete and detailed information is assembled for five southern states that were subject to the VRA: Alabama, Georgia, Louisiana, Mississippi, and South Carolina. In addition, voter registration rates are collected for two states, Florida and Tennessee, that were not covered by the VRA. Information on the voter registration in North Carolina is also included: in this state, 39 counties fell under the provisions of the Act, while 61 other counties faced no legal requirements. And while a complete panel could be assembled for these eight states, a long-run analysis allows to include two additional southern states – Arkansas and Virginia – with information on one pre-VRA and one post-VRA year.

This dataset is mainly based on the documents found in the archives of the Southern Regional Council's Voter Education Project (VEP). As part of a broader effort to promote African American political engagement, in the eve of the VRA, the VEP started to record data at the local level on both voter registration statistics and election results. It collected and then securely stored official publications (e.g., from the Secretary of State or from the Department of Justice), and often

supplemented these with its own surveys.[3] In order to construct a consistent time series for all the dependent variables, additional sources have also been considered. These include the United States Commission on Civil Rights (1959 and 1961) and the Inter-university Consortium for Political and Social Research (1992).

### 4.2.2 Turnout, party support, and voting age population

Information on the total votes cast in the presidential elections has been obtained from the Inter-university Consortium for Political and Social Research (2013). However, this information is only useful insofar the focus remains on the effect of the VRA on the total turnout (i.e., not differentiated by race) or on the total support for either the Democrats or the Republicans. This has been a serious limitation in many previous empirical studies that tried to shed light on the nexus between the VRA and the political participation of African Americans, since the latter information could only be indirectly inferred from patterns observed out of aggregate variables. However, from the Voter Education Project's archives, key information has been found on the party affiliation of both white and African American voters in Louisiana and Florida. For these two states of the former Confederacy, this chapter exploits detailed county-level panel information of the registration, by party and by race, during each presidential election since 1960. Importantly, while the former state was subject to the legal requirements of the VRA, the latter was not covered by the Act: this variation is then exploited to run a difference-in-differences specification to estimate the effect of the VRA on party support by race.

Lastly, in order to construct rates for both voter registration and turnout, county-level information on the voting age population by race, available from Manson et al. (2019), is considered. Due to changes in the legal requirements to vote, age 21+ are used for 1970 and prior years, and age 18+ for 1980 and later years.[4]

[3]The VEP published official reports and statistics with the ultimate goal of documenting the process of African American enfranchisement in the U.S. South between the 1960s and the 1980s. It also promoted voter education schemes and registration drives across southern states.

[4]On June 22, 1970, President Nixon signed into law *H.R. 4249*, lowering the voting age requirement to 18 starting on January 1, 1971. Official information on voting age population is available every 10 years. Then, in order to obtain information on each intercensal year, a linear interpolation is considered between 1950, 1960, 1970, 1980, and 1990.

### 4.2.3 Control variables

The control variables used in this analysis are summarized in the bottom panel of Table D.1.1. In 1960, the counties subject to the VRA were characterized by a significantly higher share of African Americans in the population compared to the counties not subject to the Act (37.6% v. 19.5%). They were also smaller (38,200 v. 48,400 citizens in each type of county, respectively), poorer (measured as either the share of families below the poverty line or as the share of child poverty), and more densely populated (10.5% v. 7.1%). Further, they had more unskilled workers (74.6% v. 72.0%), and a slightly more important manufacturing sector (25.1% v. 22.0%).

The two sets of counties were instead more similar across other characteristics, such as the unemployment rate (5.3% for those covered by the Act v. 5.4% for those not covered), the share of the urban population (28.4% v. 28.8%, respectively), the size of farms and the share of rural farms (50.1% and 20.9% v. 47.1% and 19.0%, respectively), and a measure of agricultural productivity.[5] Lastly, and in line with expectations, more episodes of African American activism occurred between 1960 and 1964 in those counties that would be subject to the VRA in 1965. In particular, there were 0.6 more counts of pro-African American activism and 0.5 more counts of anti-African American activism, showing a greater ethnic tension among counties covered by the VRA.

Summing up, southern counties differed across several observable characteristics, and some of these differences are statistically significant. This is particularly relevant for the share of African Americans in the county population. The imbalance in the distribution of African Americans is potentially problematic for the identification strategy proposed in this chapter. To address this concern, Section 4.3.4 tests the robustness of the main estimated coefficients against varying levels of the sample size. In particular, a regression discontinuity design is implemented, taking advantage of the fact that counties just above and just below the 50% threshold in the 1964 turnout rate, which defined coverage, are more similar in terms of county characteristics than a pair of counties taken at random. Reassuringly, all

[5] As cotton was the dominant crop in the U.S. South, an index of cotton suitability is considered.

the estimates remain robust as the sample size is gradually increased from a 10% to a 50% bandwidth around the 50% threshold.

## 4.3 The VRA and political participation

This section estimates the effect of the VRA on several outcomes of political participation: voter registration, turnout, and party support. The identification strategy is first discussed in Section 4.3.1. Then, in Section 4.3.2, an event study analyzes the evolution over time in the relationship between the outcomes of political participation and the share of African Americans in 1960, allowing estimates to vary by treatment status. To further assess both the mobilization and counter-mobilization theories of political participation, Section 4.3.3 disentangles the effect stemming from the racial threat component, as proxied by larger gains across African American representation. In Section 4.3.4, a long-run specification is considered: this allows to include two additional states – Arkansas and Virginia – to the eight states that are used in the event study. Lastly, the robustness of the results presented in this chapter is assessed through a regression discontinuity design around the 50% threshold for coverage.

### 4.3.1 Estimation strategy

The VRA, together with the removal of any form of restriction to the franchise (such as literacy tests or poll tax requirements), brought in special measures meant to protect African Americans from the infringement of their political rights. With the direct objective of the VRA being the registration of previously disenfranchised African Americans, the effect of the Act on political participation ought to depend on the pre-existing share of African Americans in the total population. In particular, the a priori expectation is that the VRA's impact is greater in counties with a larger share of African Americans.

Hence, a straightforward way to estimate the effect of the VRA on voter registration rates, turnout rates, and party support, would be to investigate how the relationship between the pre-existing share of African Americans in the county

population and these outcomes of political participation changed over time within states that were subject to the legal requirements of the Act. One important problem with this type of strategy is that areas with larger shares of African Americans in the population might have seen changes in these outcomes even in the absence of the specific provisions included in the VRA. For example, civil rights activism could have led to greater African American registration rates even without the Act. For this reason, the above-mentioned strategy is combined with the design of a suitable comparison group including the remaining southern counties that were not covered by the VRA but that nonetheless maintained a similar history of African American disenfranchisement before 1965.

In other words, this chapter estimates whether covered counties with larger pre-existing African American population shares (the treatment group) experienced differential changes in the outcomes of political participation – from before to after the VRA – when compared to the other southern counties that were not covered (the control group). Throughout this study, the identifying assumption is that, in the absence of the VRA, the two groups, that in the pre-VRA period did not exhibit any statistically significant difference in the outcomes of political participation, would have experienced the same trends in voter registration, turnout, and party support.

### 4.3.2 Event study

To illustrate the evolution of the relationship between the outcomes of political participation and the share of African Americans in 1960, separately by treatment status, the following regression model is estimated:

$$\begin{aligned} PoliticalParticipation_{c,s,t} &= \sum_{n>1956} \gamma_n D_n^t ShareBlack_{1960} + \\ &+ \sum_{n>1956} \theta_n D_n^t \, ShareBlack_{1960} \times VRA_{c,s} + \\ &+ \mathbf{X'}_{c,s}\beta + I_{s,t} + I_c + \epsilon_{c,s} \end{aligned} \tag{4.1}$$

where $PoliticalParticipation_{c,s,t}$ is one of the outcomes of political participation considered in this chapter, measured for each county $c$, state $s$, and year $t$, while $ShareBlack_{1960}$ is the share of African Americans in the county population in 1960.

$VRA_{c,s}$ is an indicator taking a value of one if the county was subject to the legal requirements of the VRA in 1965, $D_n^t$ is an indicator taking a value of one if $n = t$, and $\mathbf{X'}_{c,s}$ is a vector of pre-VRA controls. $I_{s,t}$ and $I_c$ are state-year interactions and county fixed effects, respectively, while $\epsilon_{c,s}$ is the idiosyncratic error term.[6]

The coefficient $\theta_n$ captures the difference in the gradient between the counties in the treatment and the control groups, and the variation of this coefficient over time is then plotted in Figures 4.1 and 4.2. As all specifications include county fixed effects, year 1960 and year 1964 (for Figures 4.1 and 4.2, respectively) are used as the reference point for evaluating how the slope of the relationship between the outcomes of political participation and the 1960 share of African Americans changed over time.[7] In all specifications, state-year interactions – so that the coefficients represent within-state effects – and a vector of pre-VRA county characteristics are included.

The effect of the VRA on the registration rates of African Americans and whites is considered in the top two panels of Figure 4.1. The treatment-control difference (i.e., the estimated coefficient $\theta_n$) in the post-VRA period is strongly positive, statistically significant, and broadly increasing over time for both of these statistics. Interestingly, an immediate effect of the VRA is observed as for the registration rates of African Americans, soon after the enactment of the Act: as early as the presidential election of 1968, a 10% rise in the 1960 share of African Americans in the county population led to an additional 2.2% increase in African American registration rates, and this is on top of the increase that is observed across counties not subject to the Act. By the end of the sample in 1988, the estimate of the impact of the VRA on registration rates further rises to 3.7%.

On the other hand, white voters have reacted with a significant lag to this change in policy. It is estimated that the VRA had no impact on white voter registration rates until the presidential election of 1972, seven years after the enactment of the VRA: the policy was meant to primarily address the registration rates of the

[6]Standard errors are clustered at the county-level and all specifications are weighted by the county population in 1960.

[7]The choice of 1964 as the reference year for Figure 4.2 is dictated by data availability when party support is considered as the dependent variable.

disenfranchised African Americans, while white voters had remained mostly free from literacy tests and any other form of discriminatory voting restrictions. However, the large jump observed in 1972, and the positive and stable coefficients observed until the end of the sample, points to the fact that counter-mobilization could indeed be at play in those counties that were covered by the VRA. To empirically test this claim, Section 4.3.3 allows for the effect of the VRA to vary according to the racial threat in the county.

The estimates also indicate that, in the period before the passage of the Act (i.e., in the three presidential elections between 1956 and 1964), the difference in the relationship between both African American and white voter registration rates and the 1960 share of African Americans was not different from zero. In the panels of Figure 4.1, this is shown by statistically insignificant coefficients $\theta_n$ in all the years up to the presidential election of 1968. This finding supports the suitability of the chosen control group: the set of southern counties not subject to the legal requirements of the VRA appropriately captures what would have happened to the registration rates in the set of treated counties had the Act not been signed into law.

Building on the results just presented, the middle two panels of Figure 4.1 consider the effect of the VRA on total voter registration rates and on the gap between the registration rates of African American and white voters. The former picks up the positive and statistically significant patterns observed for the rates of both whites and African Americans, with a trend that is clearly increasing over time: for a 10% rise in the 1960 share of African Americans in the county population, the estimate increases from 2.4% just after the enactment of the VRA to 3.9% by the end of the sample. On the other hand, no effect is found on the gap in voter registration rates, with a more noisy estimate that is not significantly different from zero throughout all the years of the sample. African Americans did register significantly more following the signing of the VRA (top-left panel), but this rise was matched by higher white registration rates (top-right panel): an indirect effect of the VRA that, similarly to the discussion above, points to the fact that both mobilization and counter-mobilization might be at play.

In the bottom two panels of Figure 4.1, the focus is on two aggregate measures not differentiated by race: total turnout rates and the share of the Democratic vote in each presidential election. As for total turnout rates, while the coefficients cannot be distinguished from zero in any of the pre-VRA years, a different pattern does emerge after the passage of the Act: for the counties in the treated group, the relationship becomes much steeper compared to the ones in the control group, resulting in positive and statistically significant coefficients $\theta_n$. The change in the slope is present already in the 1968 election, where a 10% increase in the 1960 share of African Americans in the county population is associated with an incremental rise of 1.0% in total turnout rates. By the election of 1988, a similar increase in the 1960 share of African Americans is associated with an impact of 1.3%. And similarly to the gap in voter registration rates, there does not seem to be an effect on the share of the votes cast for the Democratic Party. However, as shown in the following analysis, this aggregate result is confounded by additional factors, and a statistically significant relationship does emerge when controlling for the racial threat.

**Party support by race**

In the existing literature, the effect of the VRA on party support had been assessed by analyzing the total votes cast for either the Democratic Party or the Republican Party, and the variation in the support by race could have only been indirectly estimated from these aggregate variables. In this chapter, by narrowing the focus to Florida and Louisiana, the direct effect of the VRA on party support by race could instead be quantified, as reliable information on the total number of African American and white voters affiliated with any given party has been collected.[8] To the best of our knowledge, this is the first systematic attempt to assess the change in the support for the Democratic Party, separately by race, as a result of the VRA.

Figure 4.2 plots the $\theta_n$ coefficient estimated from Eq. 4.1, using the affiliation of both African American and white voters as outcome variables. The VRA is

[8]Louisiana and Florida are the only two states in the U.S. South with closed primary elections. This system allows voters to participate in a party's primary only if registered with that party prior to the election day, excluding both non-partisan and unaffiliated voters.

estimated not to have had a differential impact on the support for the Democratic Party from the newly enfranchised African American voters. However, it did have a sizable and statistically significant effect on the Democratic support from the white voters in the aftermath of the passage of the Act. In fact, this relationship is much steeper in the subset of southern counties that are subject to the legal requirements of the VRA, resulting in a negative and statistically significant coefficient $\theta_n$.

In other words, the African American support for the Democratic Party does not depend on whether the county is covered by the VRA. The affiliation with the Democratic Party, which signed the VRA along with openly promoting other civil rights bills, did not deferentially vary by coverage status. On the other hand, the behavior of white voters systematically depended on whether the county was treated. The VRA led to a reduction in the support for the Democratic Party of 1.0% in 1968 (for a 10% increase in the 1960 share of African Americans in the county population), and this fall in party alignment widened throughout the sample period, reaching 3.9% by the last election of 1988. These estimates further signal that both mobilization and counter-mobilization indeed seem to have occurred across the U.S. South following the passage of the VRA. To disentangle and empirically test these hypotheses, the effect of the racial threat on the political outcomes of interest is now considered.

### 4.3.3 Political mobilization and counter-mobilization

In the previous analysis, the political participation of both whites and African Americans is found to depend on the share of African Americans in the local population. To further disentangle whether the behavior of white voters depended on either the threat of mobilization or on the actual participation of African Americans, the effect of the VRA in the subset of counties that were mostly exposed to gains in African American representation is now considered. The election of African Americans to local county governments might also have an additional effect on the registration of African Americans, as minority office-holding in positions of

local authority could provide role models, weaken stereotypes, and build expertise crucial for their advancement (Beaman et al. 2009).

As shown in Chapters 2 and 3, county-level gains in African American office-holding directly depended on electoral rules. In particular, the legal framework of the VRA contributed to a sizable increase in African American representation only when local elections were carried out through a single-member districts (SMD) system, as opposed to an at-large system. Then, in this section, the extent of the racial threat component is proxied by the variation of electoral systems across southern counties. As a result, both the VRA and pre-existing electoral rules are used as the source of heterogeneity in the analysis.

As before, the focus is on whether covered counties with a larger pre-existing African American share in the population (the treatment group) experienced a significantly different change in the outcomes of political participation – from before to after the VRA – when compared to the remaining southern counties that were not subject to the Act (the control group). In addition to this, as gains in African American office-holding depended on pre-existing electoral rules, the availability of SMD systems is used as a further interaction term and a quadruple difference-in-differences design is then considered to disentangle the effect of the racial threat component on the outcomes of interest. In fact, if the VRA had any effect on political participation through this channel, the subset of counties electing officers through SMD (rather than at-large) ought to experience a significantly different pattern in the outcome variables when compared to the remaining southern counties, from before to after the enactment of the VRA. If the election of African Americans to local county governments was an important mechanism in the response of both whites and African Americans to the VRA, then this would be captured by the $\lambda_n$

coefficient from the following quadruple difference-in-differences design:

$$\begin{aligned} PoliticalParticipation_{c,s,t} &= \sum_{n>1956} \gamma_n D_n^t ShareBlack_{1960} + \\ &+ \sum_{n>1956} \theta_n D_n^t \, ShareBlack_{1960} \times VRA_{c,s} + \\ &+ \sum_{n>1956} \psi_n D_n^t \, ShareBlack_{1960} \times SMD_{c,s} + \\ &+ \sum_{n>1956} \lambda_n D_n^t \, ShareBlack_{1960} \times VRA_{c,s} \times SMD_{c,s} + \\ &+ \mathbf{X'}_{c,s}\beta + I_{s,t} + I_c + \epsilon_{c,s} \end{aligned} \tag{4.2}$$

That is, Eq. 4.1 is augmented with the interaction between the 1960 share of African Americans in the county population ($ShareBlack_{1960}$) and $SMD_{c,s}$ (an indicator taking a value of one if the county elected its local officers through a SMD mechanism), as well as the triple interaction between the share of African Americans, the VRA indicator, and the SMD indicator. The latter is the term disentangling the effect of the VRA on the political outcomes of interest that is generated as a response to the election of African Americans to local county governments.

The coefficient $\lambda_n$ is plotted in Figures 4.3 and 4.4, with the top four panels of Figure 4.3 presenting the effect on registration rates. As shown in the top-left panel, African Americans registered to vote irrespective of the gains in political representation. That is, the VRA led to higher registration rates of African Americans in the counties subject to the Act (as shown in the top-left panel of Figure 4.1), but a differential pattern in registration is not observed as a response to higher representation: having more African Americans elected to county governing bodies was not key in driving more African Americans to the polls. Abolishing the obstacles that prevented voting registration was the single most important factor affecting the registration patterns of African Americans.

On the other hand, the behavior of white voters did vary systematically with the racial threat component. As shown in the top-right panel of Figure 4.3, when faced with a larger threat of African American representation in the local government, white registration rates rose significantly. Furthermore, while counter-mobilization efforts only started in 1972 when considering the whole universe of southern counties

covered by the VRA, this backlash is observable since the very first presidential election of 1968 when focusing on the subset of counties electing local officers through SMD systems: whites responded immediately to the racial threat.

Total registration rates also differed meaningfully for the subset of counties that were both covered by the VRA and that elected the local politicians through SMD: the relationship is steeper from mid-1970s onward and the estimated coefficient $\lambda_n$ is both positive and statistically significant. Furthermore, the gap between the registration rates of African Americans and whites turned negative as soon as the VRA was enacted. That is, when the racial threat is included in the analysis, whites have been more responsive than African Americans in registering to vote, widening the gap in registration. Counter-mobilization is again striking here: when looking at the aggregate effect between covered and non-covered counties, the estimated coefficient on the indicator measuring the gap in registration rates is indistinguishable from zero in all years. However, in the set of counties with more African American representation, the white response has been much more robust than the one of African Americans.

As for both total turnout rates and the support for the Democratic Party, the relationship is strongly positive and significantly different from zero in the post-VRA sample. Interestingly, in the subset of counties with SMD, the support for the Democratic Party seems to be concentrated in the first two presidential elections, while from mid-1970s onward the effect, although positive, remains steady at around two-thirds of the immediate impact. In Figure 4.2, Florida and Louisiana were considered in order to quantify the effect of the VRA on party support by race. As the former state used at-large electoral systems, while the latter elected officials through SMD, the triple interaction of Eq. 4.2 would deliver the same results of its counterpart with fewer interactions. Then, the insights from that analysis could be used to (at least partly) explain the jump in the support for the Democratic Party that is observed during the first ten years following the enactment of the VRA. The African American franchise uniformly voted for the Democrats, while white voters, although starting to reduce their support for the Democratic Party,

took longer to have a sizable impact on overall party support. In other words, there were some moderate white voters who still voted for the Democratic Party in the immediate aftermath of the VRA. An alternative explanation could be that the surge in white registration rates – concentrated in counties subject to the VRA and, to a larger extent, in counties electing officials through SMD – was likely aimed at stopping African Americans from entering local governing bodies. However, when voting in presidential elections, other considerations were more salient, at least in the first ten years after the passage of the VRA.

### 4.3.4 Long-run and regression discontinuity specifications

Restricting the focus of the analysis to the long-run effect of the VRA has several benefits. First, it allows to include one additional control state (Arkansas) and one treated state (Virginia), for which only one pre-VRA and one post-VRA data point exists. This brings the total count of states included in the analysis to ten.[9] Then, the robustness of all previous results – which are based on a panel framework – could be tested through a richer cross-section. Furthermore, considering the impact of the VRA on political outcomes twenty years following its enactment allows to reduce measurement errors in the calculation of registration rates that might have occurred during the first years following the passage of the Act.

In the long-run specification, the dependent variable is the change in the outcomes of political participation in county $c$, within state $s$, between the presidential elections of 1960 and 1984, while $I_s$ are state fixed effects (a state specific trend in this long difference specification):

$$\begin{aligned} \Delta PoliticalParticipation_{c,s} &= \gamma ShareBlack_{1960} + \\ &+ \theta\, ShareBlack_{1960} \times VRA_{c,s} + \\ &+ \mathbf{X'}_{c,s}\beta + I_s + \epsilon_{c,s} \end{aligned} \tag{4.3}$$

[9] Of these, six states are subject to the legal requirements of the VRA (Alabama, Georgia, Louisiana, Mississippi, South Carolina, and Virginia), one is partially covered by the Act (North Carolina), and three are not covered (Arkansas, Florida, and Tennessee).

In this model, the coefficient $\gamma$ captures how the change in the outcomes of political participation relate to the pre-VRA share of African Americans in the control group, whereas $(\gamma + \theta)$ does the same for the treatment group. The focus is on the difference between the two, which is measured by $\theta$. Under the identifying assumption that, in the absence of coverage, political participation would have changed in the same way within treated and non-treated counties, $\theta$ provides an unbiased estimate of the effect of the VRA.

The estimated coefficients from Eq. 4.1 are plotted in Figure 4.4, and all the earlier results remain valid. In particular, by 1984, African American, white, and total registration rates are significantly higher as a result of the VRA. Total turnout rates also remain higher in counties subject to the legal requirements of the Act when compared to those counties left untreated, while the gap between the registration rates of African Americans and whites, as well as the overall support for the Democratic Party, remains indistinguishable from zero.

The results from the long-run specification are also presented in Tables 4.1 and 4.2, where the robustness is assessed using a series of alternative specifications. First, the long-run change in the outcomes of political participation is regressed on the share of African Americans in 1960 and its interaction with the VRA indicator and state trends in column (1). Then, socio-economic controls are additionally accounted for in column (2), with measures of pro- and anti-African American activism included in column (3).

Considering the preferred specification of column (3), a 10% rise in the 1960 share of African Americans in the county population led to a 3.3% increase in African American registration rates, and this is on top of the 4.5% increase observed across counties not covered by the Act. By 1984, the indirect effect of the VRA on the registration rates of whites is estimated to reach 2.3% for a 10% increase in the share of African Americans in the county population, while whites were less likely to register in counties not subject to the Act with a larger African American population. In the aggregate, total voter registration rates rose by an additional 3.8% for a 10% increase in the share of African Americans in the county population,

and this is on top of the 2.7% increase found in counties not covered by the Act. Due to the observed white backlash, the Act has not had a significant effect on closing the gap in the registration rates between African American and white voters by 1984. On the other hand, this gap is estimated to narrow down in non-VRA counties, as the local white response had been absent. Matching the rise in total registration rates, total turnout rates also increased significantly following the VRA by an estimated incremental effect of 1.7% twenty years following the passage of the Act. In counties covered by the VRA, the drop in the white affiliation with the Democratic Party is estimated to grow over time, reaching 3.1% for a 10% increase in the 1960 share of African Americans in the county population. On the other hand, and in line with anecdotal evidence, southern African Americans robustly voted in support for the Democratic Party.

The counties subject to the VRA were selected based on a threshold rule. In particular, counties were covered by the VRA if they administered any type of voting restrictions (for instance, literacy tests or poll tax requirements), and if the total turnout rate in the presidential election of 1964 was below 50%. In this chapter, the average treatment effect (the effect for the average county) has been estimated, by including all southern counties with available registration statistics. However, as emerged from the discussion in Section 4.2.3, treated and untreated counties differed along important dimensions. Chiefly, the distribution of African Americans differed across the two sub-samples, with significantly larger shares of African Americans in the set of counties that were subject to the VRA. Then, in order to balance the sample on its observable characteristics, the analysis is restricted to the set of counties that are just below and just above the 50% threshold for coverage. Furthermore, in order to check the sensitivity of the main results to changes in the sample size, rolling specifications are estimated as the window around the 50% turnout rate is incrementally expanded until the full sample is again obtained.

The coefficients estimated from gradually extending the window around the threshold rule are presented in Figures 4.5 and 4.6. First, a sample bandwidth of $\pm 10\%$ around the 50% threshold is considered. This accounts to about 800

observations when considering the ten states and to about 190 when considering only Florida and Louisiana. By controlling for all the unobservable confounders that vary over time, these counties might do a better job in estimating the true effect of coverage. Reassuringly, as the bandwidth is gradually expanded from ±10% to ±50%, all the difference-in-differences estimates on all the outcomes of political participation remain remarkably stable. Importantly, all the coefficients that were estimated to be statistically (in)significant in the full sample maintain their (in)significance also in these specifications. If anything, by comparing more similar counties that only differed in their coverage status, the effect of the VRA on total turnout rates is slightly smaller, whereas the effect on the voter registration rates of both African Americans and whites is higher.

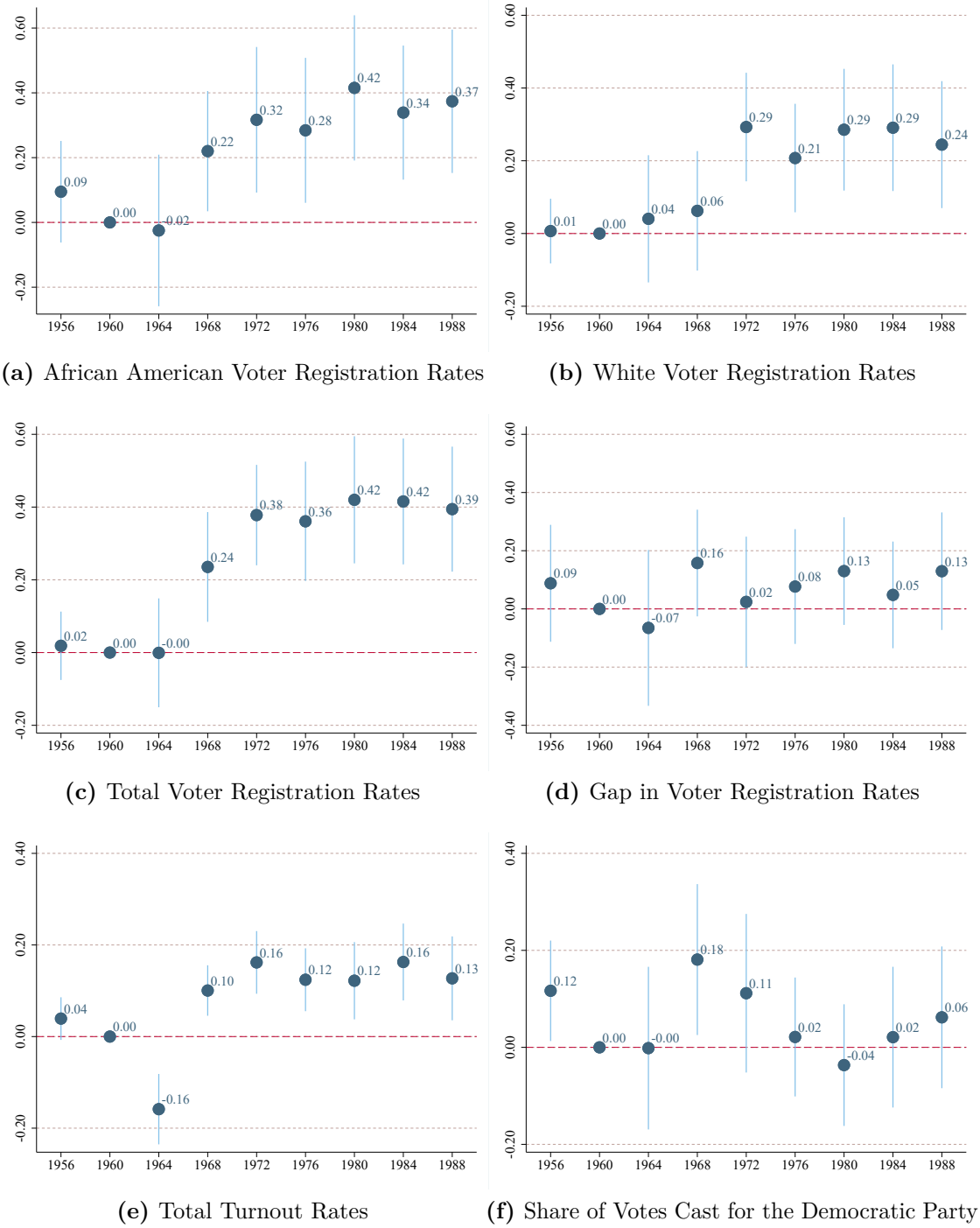
## 4.4 Conclusions

Following a century of disenfranchisement in the U.S. South, the VRA ensured that the right to vote could not be denied on account of race. This was achieved by imposing strict federal monitoring over the authority of local registrars in the most discriminatory counties. By abolishing any type of device restricting the right to vote, the VRA is recognized as the leading factor behind the surge in the registration rates of African Americans. Indirect effects could also be attributed to the VRA, including white counter-mobilization efforts and a loss of support for the Democratic Party.

This chapter has exploited geographic variation in the implementation of the Act across southern counties to empirically estimate the causal effect of the VRA on these political participation outcomes. By pre-empting both state and local authorities over voter qualifications, the VRA had an immediate and long-lasting effect on the registration rates of African Americans. By 1984, a 10% rise in the 1960 share of African Americans in the county population led to a 3.3% increase in African American registration rates, and this is on top of the 4.5% increase observed across counties not covered by the Act. As African Americans began entering the political process and started winning local offices, whites responded by registering more. In the aggregate, both registration rates and turnout rates

rose significantly following the VRA. The impact on turnout means that the Act had a significant role in shaping the two-party competition that ensued in the U.S. South in the second half of the twentieth century, with voting becoming polarized along racial lines. In counties covered by the VRA, the drop in white affiliations with the Democratic Party is estimated to grow over time. On the other hand, and in line with anecdotal evidence, southern African Americans have robustly voted in support for the Democratic Party.

**Figure 4.1:** Event study of the effect on political participation




**(a)** African American Voter Registration Rates

**(b)** White Voter Registration Rates

**(c)** Total Voter Registration Rates

**(d)** Gap in Voter Registration Rates

**(e)** Total Turnout Rates

**(f)** Share of Votes Cast for the Democratic Party

The figures plot the $\theta_n$ coefficients estimated from the specification presented in Eq. 4.1. The confidence intervals shown are set at the 90% level of statistical significance. Omitted interaction: 1960.

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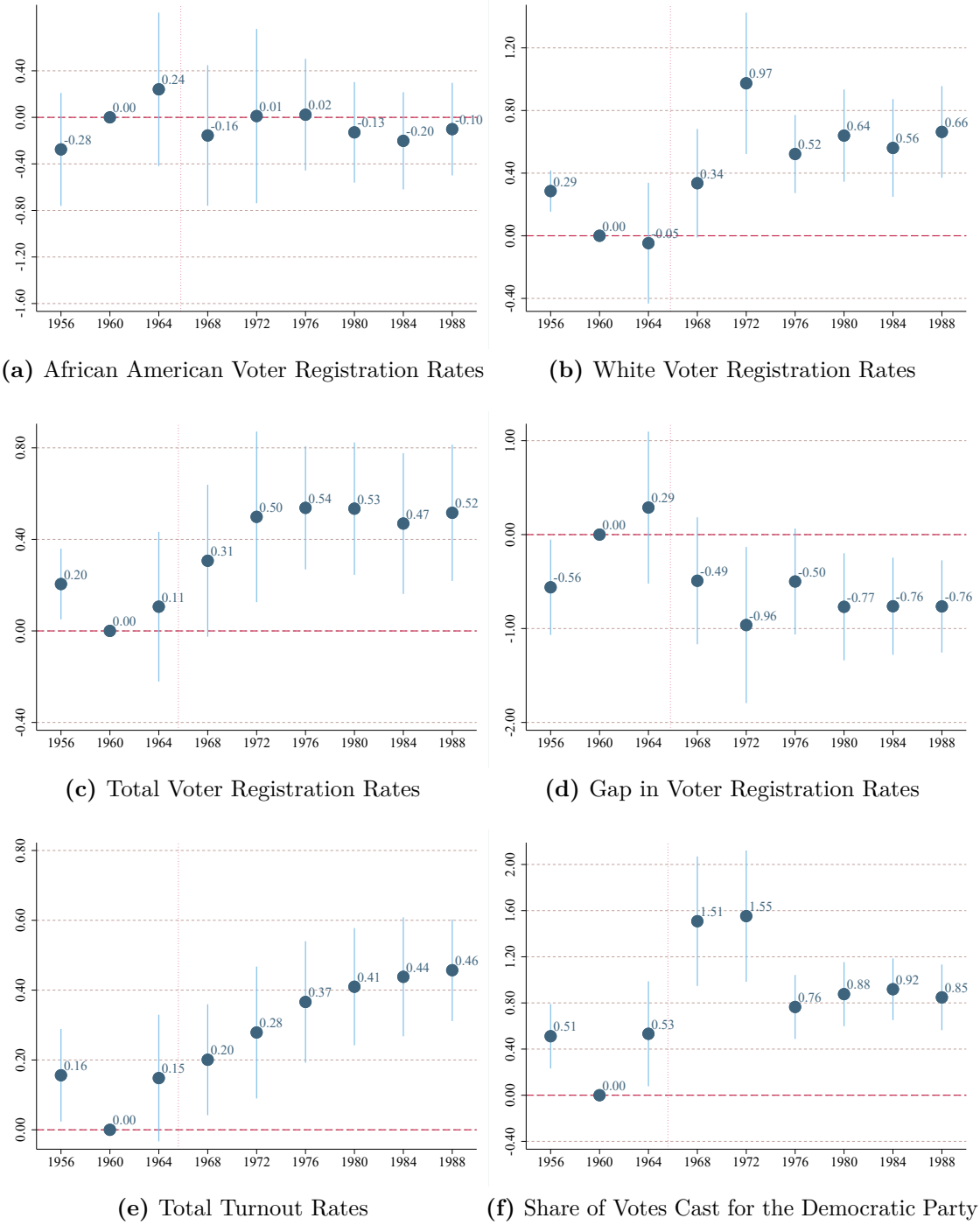
**Figure 4.2:** Event study of the effect on the support for the Democratic Party




**(a)** Afr. Am. Support for the Democratic Party **(b)** White Support for the Democratic Party

The figures plot the $\theta_n$ coefficients estimated from the specification presented in Eq. 4.1. The confidence intervals shown are set at the 90% level of statistical significance. Omitted interaction: 1964.

**Figure 4.3:** Electoral rules and political participation




**(a)** African American Voter Registration Rates

**(b)** White Voter Registration Rates

**(c)** Total Voter Registration Rates

**(d)** Gap in Voter Registration Rates

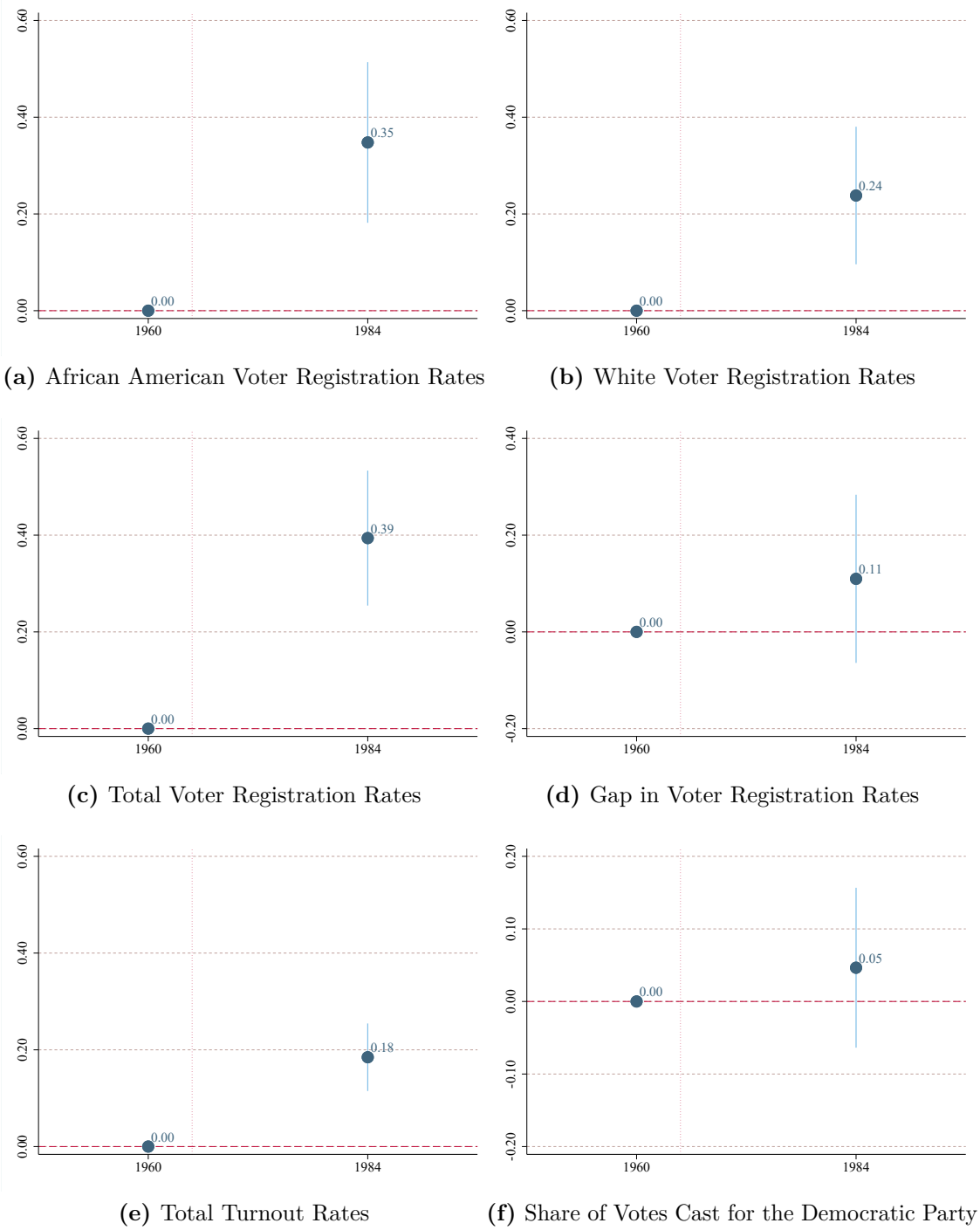
**(e)** Total Turnout Rates

**(f)** Share of Votes Cast for the Democratic Party

The figures plot the $\lambda_n$ coefficients estimated from the specification presented in Eq. 4.2. The confidence intervals shown are set at the 90% level of statistical significance. Omitted interaction: 1960.

## 4.4. Conclusions

**Figure 4.4:** Long-run difference-in-differences




**(a)** African American Voter Registration Rates

**(b)** White Voter Registration Rates

**(c)** Total Voter Registration Rates

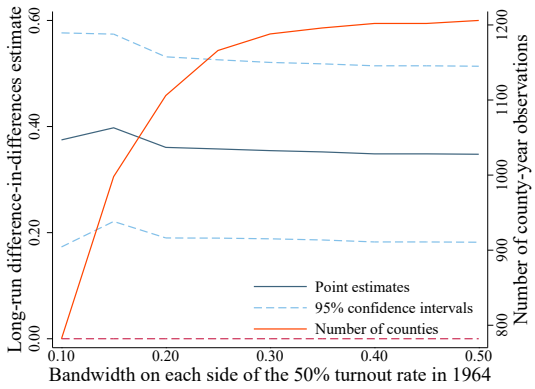
**(d)** Gap in Voter Registration Rates

**(e)** Total Turnout Rates

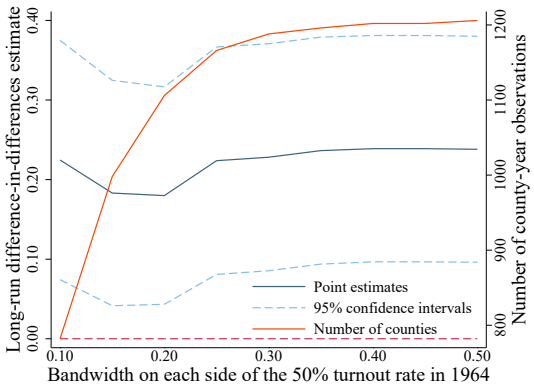
**(f)** Share of Votes Cast for the Democratic Party

The figures plot the $\theta$ coefficients estimated from the specification presented in Eq. 4.3. The confidence intervals shown are set at the 90% level of statistical significance. Omitted interaction: 1960.

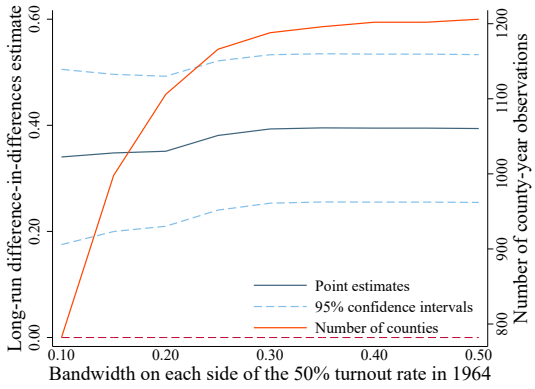
**Figure 4.5:** Regression discontinuity around the 50% threshold for coverage



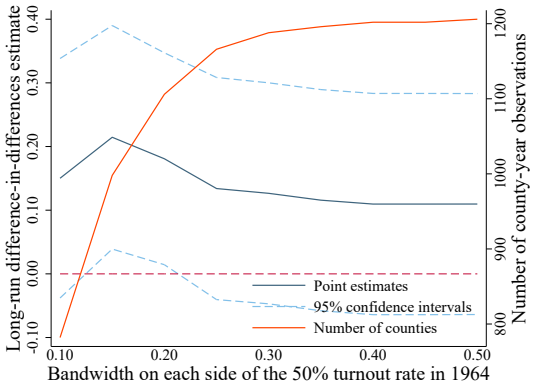

**(a)** African American Voter Registration Rates



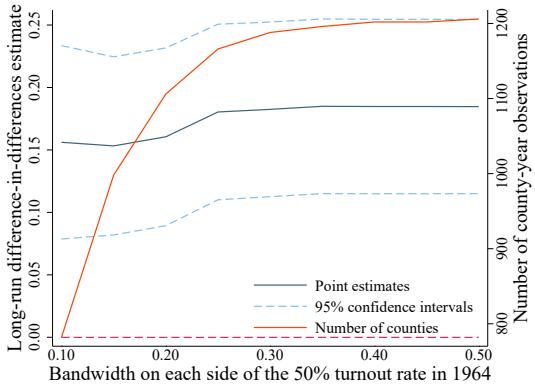

**(b)** White Voter Registration Rates



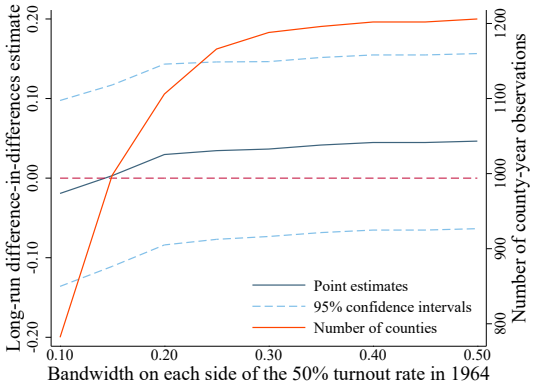

**(c)** Total Voter Registration Rates




**(d)** Gap in Voter Registration Rates




**(e)** Total Turnout Rates




**(f)** Share of Votes Cast for the Democratic Party

The figures plot the $\theta$ coefficients estimated from the specification presented in Eq. 4.3, starting with a bandwidth of 10% around the 50% threshold of the turnout rates in the 1964 presidential election, and with additional increments of 5% until the full sample is considered. The confidence intervals shown are set at the 90% level of statistical significance. Omitted interaction: 1960.

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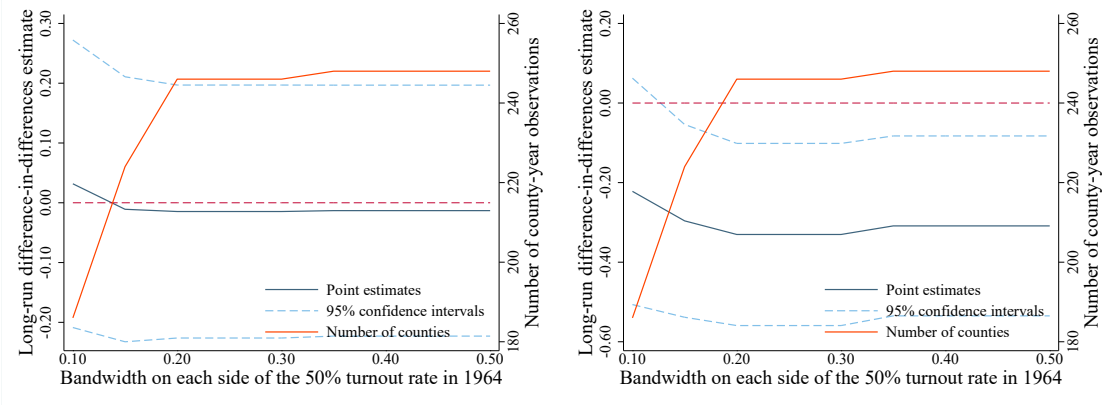
**Figure 4.6:** Regression discontinuity on the support for the Democratic Party




**(a)** Afr. Am. Support for the Democratic Party **(b)** White Support for the Democratic Party

The figures plot the $\theta$ coefficients estimated from the specification presented in Eq. 4.3, starting with a bandwidth of 10% around the 50% threshold of the turnout rates in the 1964 presidential election, and with additional increments of 5% until the full sample is considered. The confidence intervals shown are set at the 90% level of statistical significance. Omitted interaction: 1960.

**Table 4.1:** The effect on the change in the outcomes of political participation

| *Panel A* | African American Voter Registration Rates | | | White Voter Registration Rates | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Share of Afr. Am. in 1960 X VRA | 0.503*** | 0.334*** | 0.334*** | 0.284*** | 0.275*** | 0.226** |
| | (0.117) | (0.102) | (0.102) | (0.104) | (0.097) | (0.089) |
| Share of Afr. Am. in 1960 | 0.581*** | 0.450*** | 0.452*** | –0.071 | –0.075 | –0.110 |
| | (0.117) | (0.104) | (0.100) | (0.089) | (0.081) | (0.079) |
| County Characteristics | No | Yes | Yes | No | Yes | Yes |
| African American Activism | No | No | Yes | No | No | Yes |
| State Trends | Yes | Yes | Yes | Yes | Yes | Yes |
| Adj. R-Square | 0.75 | 0.79 | 0.79 | 0.26 | 0.43 | 0.47 |
| N | 602 | 602 | 602 | 602 | 602 | 602 |
| *Panel B* | Total Voter Registration Rates | | | Gap in Voter Registration Rates | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Share of Afr. Am. in 1960 X VRA | 0.409*** | 0.418*** | 0.381*** | 0.219* | 0.059 | 0.109 |
| | (0.094) | (0.092) | (0.088) | (0.116) | (0.109) | (0.106) |
| Share of Afr. Am. in 1960 | 0.303*** | 0.294*** | 0.274*** | 0.651*** | 0.524*** | 0.562*** |
| | (0.086) | (0.075) | (0.075) | (0.113) | (0.110) | (0.105) |
| County Characteristics | No | Yes | Yes | No | Yes | Yes |
| African American Activism | No | No | Yes | No | No | Yes |
| State Trends | Yes | Yes | Yes | Yes | Yes | Yes |
| Adj. R-Square | 0.57 | 0.64 | 0.65 | 0.72 | 0.76 | 0.76 |
| N | 602 | 602 | 602 | 602 | 602 | 602 |
| *Panel C* | Total Turnout Rates | | | Share of Votes Cast for the Democratic Party | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Share of Afr. Am. in 1960 X VRA | 0.192*** | 0.205*** | 0.173*** | –0.088 | 0.007 | 0.059 |
| | (0.048) | (0.048) | (0.045) | (0.072) | (0.069) | (0.069) |
| Share of Afr. Am. in 1960 | 0.344*** | 0.311*** | 0.321*** | 0.139** | 0.233*** | 0.171** |
| | (0.044) | (0.040) | (0.041) | (0.066) | (0.062) | (0.067) |
| County Characteristics | No | Yes | Yes | No | Yes | Yes |
| African American Activism | No | No | Yes | No | No | Yes |
| State Trends | Yes | Yes | Yes | Yes | Yes | Yes |
| Adj. R-Square | 0.77 | 0.80 | 0.79 | 0.52 | 0.69 | 0.66 |
| N | 602 | 602 | 602 | 602 | 602 | 602 |

Robust standard errors in parenthesis are adjusted for clustering at the county-level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

**Table 4.2:** The effect on the change in the support for the Democratic Party

| | African American Support for the Democratic Party | | | White Support for the Democratic Party | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Share of Afr. Am. in 1960 X VRA | 0.142 | –0.005 | –0.013 | –0.016 | –0.320** | –0.309** |
| | (0.162) | (0.142) | (0.130) | (0.244) | (0.137) | (0.140) |
| Share of Afr. Am. in 1960 | –0.089 | –0.032 | 0.102 | –0.031 | –0.073 | –0.002 |
| | (0.162) | (0.128) | (0.120) | (0.231) | (0.146) | (0.132) |
| County Characteristics | No | Yes | Yes | No | Yes | Yes |
| African American Activism | No | No | Yes | No | No | Yes |
| State Trends | Yes | Yes | Yes | Yes | Yes | Yes |
| Adj. R-Square | 0.50 | 0.60 | 0.68 | 0.16 | 0.41 | 0.42 |
| N | 124 | 124 | 124 | 124 | 124 | 124 |

Robust standard errors in parenthesis are adjusted for clustering at the county-level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

# Appendices

# A

# Appendix to Chapter 1

## Contents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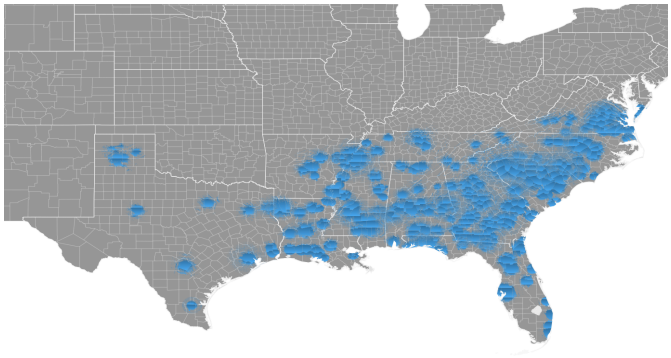
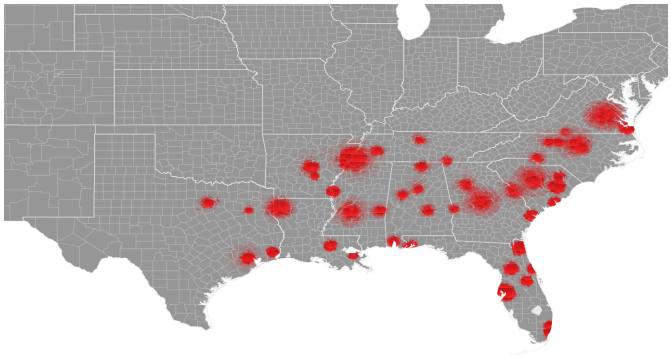
# A.1 Summary statistics

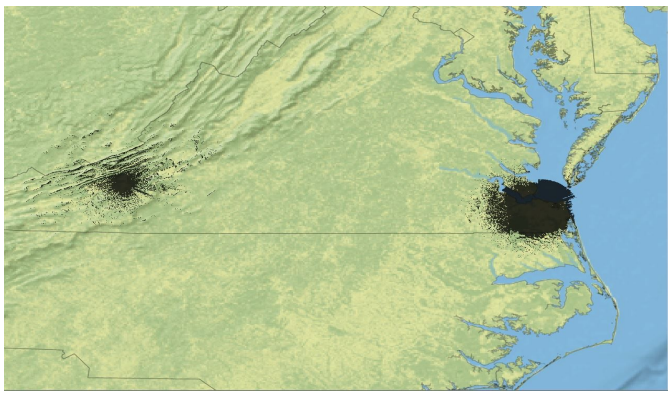
**Figure A.1.1:** Coverage of Black-appeal radio stations



**(a)** All Black-appeal Radio Stations



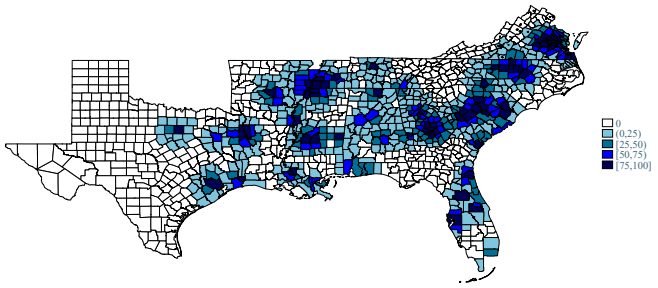
**(b)** 100% Black-appeal Radio Stations



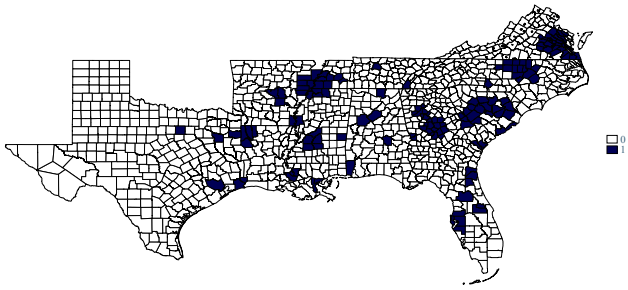
**(c)** The Role of Topography on the Propagation of Waves

Map (a) shows the propagation of the radio signal for all 200 stations reporting at least 12 hours of Black-appeal programming a week, while Map (b) does the same for the 59 stations that exclusively aired Black-appeal content. Map (c) considers two Black-appeal radio stations in Virginia (WBCR-AM in Christianburg and WRAP-AM in Norfolk) to show the impact of topography on the propagation of the radio waves.

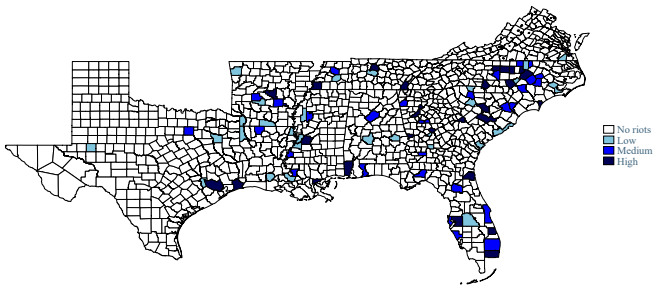
**Figure A.1.2:** Coverage of radio stations and race riots



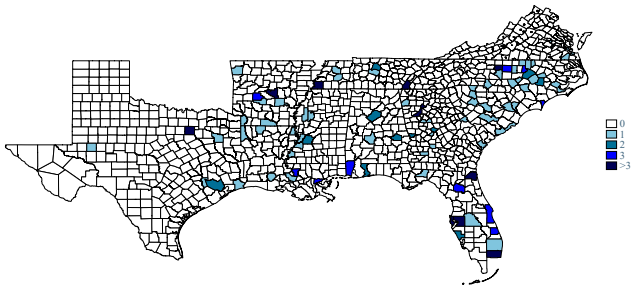

**(a)** Black-appeal Radio Stations: % of County




**(b)** Black-appeal Radio Stations: Dummy Variable




**(c)** Intensity of Race Riots




**(d)** Number of Race Riots

The top two maps present the coverage of the radio signal from the 59 stations exclusively airing Black-appeal programming in 1964, with Map (a) showing the continuous indicator (the share of the county covered by the radio signal) and Map (b) showing a dummy indicator taking the value of 1 when the share of the county covered by the signal is at least 50%. The bottom two maps present the intensity and the number of race riots in the U.S. South between 1964 and 1971.

**Table A.1.1:** Summary statistics

| | With Radio | | Without Radio | |
|---|---|---|---|---|
| | Mean | St. Dev. | Mean | St. Dev. |
| *Race Riots* | | | | |
| Likelihood of riots, 1964-1971 | 0.15 | 0.51 | 0.03 | 0.21 |
| Intensity of race riots, 1964-1971 | 0.83 | 4.04 | 0.06 | 0.84 |
| *Topographic Variables (Also Instruments)* | | | | |
| County area (*squared miles*) | 523.44 | 275.38 | 737.08 | 531.36 |
| Length of rivers (*miles*) | 17.51 | 17.35 | 22.57 | 22.74 |
| Area of lakes (*squared miles*) | 4.72 | 13.29 | 9.48 | 24.17 |
| National Parks (*dummy variable*) | 0.06 | 0.24 | 0.05 | 0.22 |
| Height of mountains above 800m (*meters*) | 0.00 | 0.00 | 20.73 | 201.56 |
| Coastal county (*dummy variable*) | 0.11 | 0.32 | 0.10 | 0.30 |
| *Socio-Economic Variables* | | | | |
| Share of Afr. Am. (%), 1960 | 33.77 | 18.43 | 14.50 | 16.45 |
| VRA coverage (*dummy variable*), 1965 | 0.77 | 0.42 | 0.31 | 0.46 |
| Population density (%), 1960 | 0.20 | 0.70 | 0.12 | 0.58 |
| Size of manufacturing (%), 1960 | 23.49 | 11.29 | 18.96 | 13.20 |
| Size of agriculture (%), 1960 | 18.05 | 13.67 | 20.30 | 12.81 |
| Size of mining (%), 1960 | 0.77 | 2.34 | 3.46 | 7.54 |
| Agricultural intensity | 0.70 | 0.65 | 1.09 | 5.02 |
| Unemployment rate (%), 1960 | 4.92 | 1.76 | 4.95 | 2.23 |
| Share of families in poverty (%), 1960 | 45.93 | 15.21 | 44.80 | 15.25 |
| Median family income (*1960 $*), 1960 | 3358.73 | 1167.83 | 3420.05 | 1187.47 |
| Birth rate (%), 1960 | 18.86 | 5.79 | 17.54 | 6.27 |
| Members of the households (*counts*), 1960 | 3.72 | 0.49 | 3.46 | 0.52 |
| Size of farms (%), 1959 | 53.29 | 23.08 | 61.08 | 29.50 |
| Non-white households (%), 1950 | 32.06 | 17.72 | 13.76 | 15.48 |
| Farms with electricty (%), 1954 | 85.64 | 19.63 | 87.88 | 16.93 |
| Households with televisions (%), 1960 | 71.91 | 13.48 | 71.85 | 14.23 |
| Households with two or more cars (%), 1960 | 18.05 | 6.23 | 19.75 | 8.70 |
| Population above 65 years (%), 1960 | 8.46 | 2.47 | 10.16 | 3.69 |
| Expenditure on highways (*1960 $*), 1962 | 1.47 | 1.31 | 2.04 | 1.68 |
| Median school years, 1960 | 8.54 | 1.41 | 8.78 | 1.50 |
| Enrolled in school (%), 1950 | 70.32 | 20.70 | 62.96 | 30.16 |
| *Pre-Radio Conflicts* | | | | |
| Lynchings of African Americans (%), 1902-1964 | 0.03 | 0.08 | 0.03 | 0.16 |
| Anti-black activism (*counts*), 1960-1964 | 0.34 | 2.13 | 0.05 | 0.62 |
| NAACP branches (*counts*), 1957-1964 | 0.53 | 0.67 | 0.27 | 0.51 |
| KKK Klaverns (*counts*), 1925-1940 | 0.17 | 0.51 | 0.10 | 0.37 |
| *Instruments* | | | | |
| Enrollment HBCUs (*dummy variable*), 1976 | 0.22 | 0.69 | 0.04 | 0.31 |
| Households with radio (%), 1950 | 86.25 | 11.33 | 88.43 | 11.42 |
| Ground conductivity | 4.41 | 3.71 | 9.05 | 8.56 |
| Counties | 524 | | 616 | |

The *With Radio* category includes those counties with at least 50% of the county being covered by the radio signal in either 1964 or 1968. Conversely, *Without Radio* includes those counties where less than 50% of the total land received the radio signal.

**Table A.1.2:** The characteristics of radio stations in 1964 and 1968

| | Power (avg., $w$) | Black-appeal (avg., %) | Number of Stations, by Black-appeal Content | | | | |
|---|---|---|---|---|---|---|---|
| | | | >0% | >25% | >50% | >75% | 100% |
| *1964* | | | | | | | |
| Alabama | 1420 | 61 | 22 | 19 | 18 | 7 | 7 |
| Arkansas | 1821 | 49 | 7 | 4 | 4 | 2 | 2 |
| Florida | 2655 | 64 | 21 | 15 | 14 | 11 | 10 |
| Georgia | 2018 | 49 | 28 | 17 | 14 | 9 | 7 |
| Louisiana | 1525 | 55 | 20 | 16 | 13 | 5 | 5 |
| Mississippi | 1903 | 44 | 18 | 12 | 7 | 3 | 3 |
| North Carolina | 1298 | 49 | 21 | 15 | 7 | 6 | 6 |
| South Carolina | 2426 | 56 | 17 | 13 | 12 | 5 | 4 |
| Tennessee | 7850 | 66 | 15 | 13 | 12 | 5 | 5 |
| Texas | 1633 | 53 | 15 | 10 | 9 | 5 | 5 |
| Virginia | 1938 | 65 | 16 | 14 | 11 | 7 | 5 |
| *1968* | | | | | | | |
| Alabama | 5321 | 65 | 14 | 9 | 8 | 8 | 8 |
| Arkansas | 9625 | 57 | 6 | 3 | 3 | 3 | 3 |
| Florida | 2817 | 60 | 15 | 8 | 8 | 8 | 6 |
| Georgia | 2489 | 47 | 23 | 10 | 8 | 8 | 8 |
| Louisiana | 2818 | 49 | 11 | 5 | 4 | 4 | 4 |
| Mississippi | 7500 | 34 | 16 | 6 | 3 | 3 | 3 |
| North Carolina | 1283 | 42 | 23 | 9 | 7 | 7 | 6 |
| South Carolina | 4750 | 37 | 18 | 6 | 4 | 4 | 4 |
| Tennessee | 13750 | 84 | 8 | 8 | 7 | 6 | 5 |
| Texas | 1269 | 55 | 13 | 6 | 6 | 6 | 6 |
| Virginia | 2200 | 67 | 10 | 7 | 6 | 6 | 6 |

## A.2 Robustness checks

An important threat for the identification of the results is that the two sets of counties (with and without Black-appeal radio stations) might be significantly different along several observable characteristics. By leading to biased estimates, this would then inevitably invalidate the analysis. To address this concern, three methods are considered assessing the stability of the results presented in Chapter 1.

### A.2.1 Propensity score analysis

The first set of results are obtained from propensity score stratification and matching techniques. These methods are both based on constructing, for each county and from the set of available controls, a propensity score measuring the likelihood of receiving the treatment. Then, restricting the analysis to the comparison of those counties receiving the radio signal with those not receiving it, but that nonetheless hold very similar propensity scores, delivers unbiased estimates of the true effect of treatment (Rosenbaum and Rubin 1983). Varying degrees in the balancing of the propensity scores then allows to test for the stability of the estimates to different levels of balance in the data and, consequently, to check whether there are indeed biases in the more general specifications of Chapter 1.[1]

Table A.2.1 lists the ten covariates with the largest standardized difference between the two sets of counties. Importantly, the two variables with the largest imbalance include those summarizing the ethnicity of the population: those counties with Black-appeal radio stations had, on average, significantly more African Americans in the population. Furthermore, they had a larger presence of the NAACP and they were more likely to be covered by the Voting Rights Act (VRA).

Propensity scores are firstly calculated through a logistic regression and then, in order to move from skewed to normal distributions, the log of the odds of the propensity scores (also known as the linear predictor) is computed. The distribution of this statistics, which is plotted in Figure A.2.1, highlights that treated counties

[1]The exposure to the radio signals is based on the dummy indicator, but similar results are obtained if the continuous measure (reflecting the actual share of coverage) is used instead.

tend to have higher propensity scores and are more densely concentrated around the mean (i.e., lower variance).[2] Stratification and matching techniques are then applied to the main specification of the chapter after balancing the sample on its propensity scores. In particular, stratification allows to estimate the effect of being exposed to the radio signal by comparing treated and untreated counties within each stratum. Stratifying the sample into quintiles reaches a high level of balance in the covariates: as shown in column (6) of Table A.2.1, there is no significant imbalance in the standardized difference between the two groups, with all the controls deviating by 0.15 standard deviations or less.[3] Besides stratification, propensity scores could also be exploited to balance the sample through matching techniques. The chosen algorithm ensures that the observations in matched pairs hold propensity scores falling within an arbitrarily small range (known as caliper).[4] Inevitably, this method throws away all those treated counties for which a valid match with untreated counties lying within the specified caliper could not be found. However, by exploiting only the relatively best matches, the balance between the two sets of counties is of a much superior quality: with a demanding caliper of 0.1, the standardized difference reported in column (9) of Table A.2.1 perform slightly better than stratification, and markedly better than the original sample, with the difference in the means across all covariates ranging at around 0.08 standard deviations or less.

Column (1) of Table A.2.2 reports the main coefficient presented in Chapter 1 (Table 1.4). Then, in columns (2) and (3), the same specification is run after either stratifying the sample into quintiles or matching counties by their propensity scores (and imposing a caliper of 0.1). And although the sample size is reduced

[2] A Hosmer-Lemeshow test to check the quality of the logistic method used to construct the propensity score highlights that the model is well-specified: an Hosmer-Lemeshow $\chi^2(8)$ value of 12.58 validates the goodness-of-fit of the model.

[3] Dividing the distribution into five parts (quintiles) is the smallest level of stratification that allows to have at least one observation in each stratum. As shown by Cochran (1968), balancing the sample using five strata is expected to eliminate, on average, around 90% of the overall confounding.

[4] Matching could also be carried out by ensuring that a match is found for each and every treated county. This section considers the more stringent type of matching based on setting a caliper, constraining the difference in the propensity scores of treated and untreated counties to lie within a predetermined range. However, similar and robust results still hold with the less stringent type of matching algorithm.

by about 80% in the latter model, the estimates are very robust across both specifications. Furthermore, the regression is also estimated by either trimming the sample to its common support, column (4), or by excluding, on top of the stratification into quintiles, the largest outliers in the sample, column (5).[5] And although both techniques lead to a considerably smaller sample size (reduced by about 20%), the estimated coefficients remain broadly robust, in terms of both magnitude and significance.

### A.2.2 Geographic regression discontinuity design

The choices leading to the set up of a Black-appeal radio station in any given county could still be driven by factors that are both unobservable and county-specific, and the propensity score analysis might not entirely abate these confounding factors. To correct for this omitted variable bias, the analysis is carried out using the sample of southern counties lying within a 15-mile radius from any of the radio stations exclusively airing Black-appeal programming. This is in the spirit of a regression discontinuity design. The county where each station lies is also excluded from this analysis to address the concern that specific factors leading to the set up of the station might exist, and that these factors could also be correlated with the riots. On the other hand, whether neighboring counties are able to receive the radio signal is entirely dependent on exogenous features, such as the topography and the characteristics of the terrain.

Then, comparing treated and untreated counties spanning the border with the county having the 100% Black-appeal radio station (or by lying nonetheless within a 15-mile radius from such radio station) allows to drastically reduce the heterogeneity in the covariates, while estimating the effect of radio on riots by exploiting the exogenous variation in the propagation of radio waves. In fact, as shown in Figure

[5]In the former model, the included counties are those whose propensity scores lie within the range at which both sets are observed. In the latter model, the specification excludes both those untreated counties that have very high values of propensity scores (above the $100 - \sigma^{th}$ percentile) and those treated counties that have extremely low propensity scores (below the $\sigma^{th}$ percentile). This section considers $\sigma$ to be one and thus trims at the $1^{st}$ percentile, removing those observations whose propensity scores are below the $1^{st}$ percentile of the treated sample or above the $99^{th}$ percentile of the untreated sample.

A.2.2, the difference between treated and untreated counties is indistinguishable from zero in all but three variables. Importantly, the share of African Americans in the population is found to be strongly balanced between the two sets of counties.

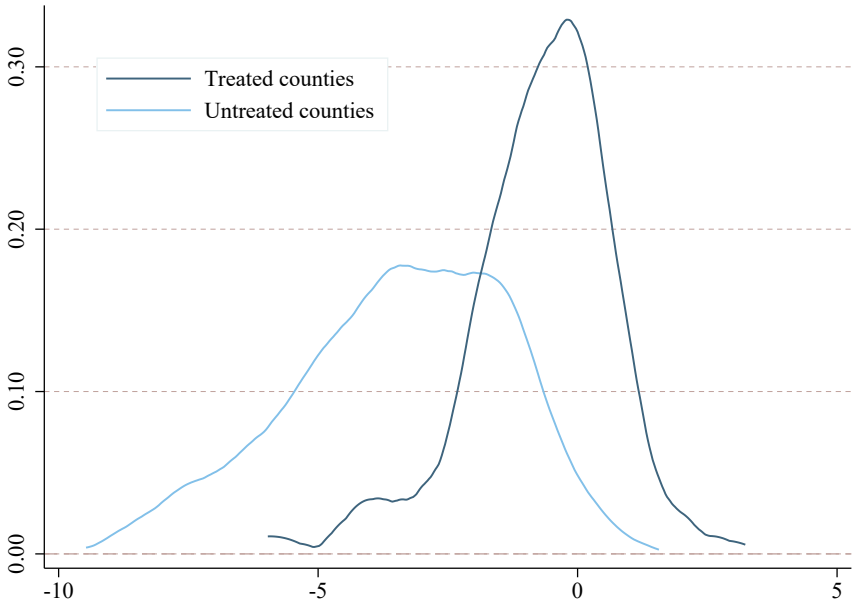
Table A.2.3 presents the estimation results using this regression discontinuity design. Despite the significant reduction in the sample size (comprising 150 southern counties), all the main results remain broadly robust. This provides further evidence that the main estimates of Chapter 1 are isolating the true effect of Black-appeal radio stations on the 1960s race riots.

### A.2.3 Minority-Black counties

Although all the results have remained stable to different degrees of balance in the data, the location of Black-appeal radio stations might still be endogenous and might depend on the share of African Americans in the population. In order to address this threat, the specifications of Chapter 1 are estimated over the subset of counties with an African American population in 1960 that is below 50%. If radio stations are found to have a robust effect even when the analysis is restricted to this subset of counties, then this would provide an additional check of the validity of the treatment effect uncovered in Chapter 1.

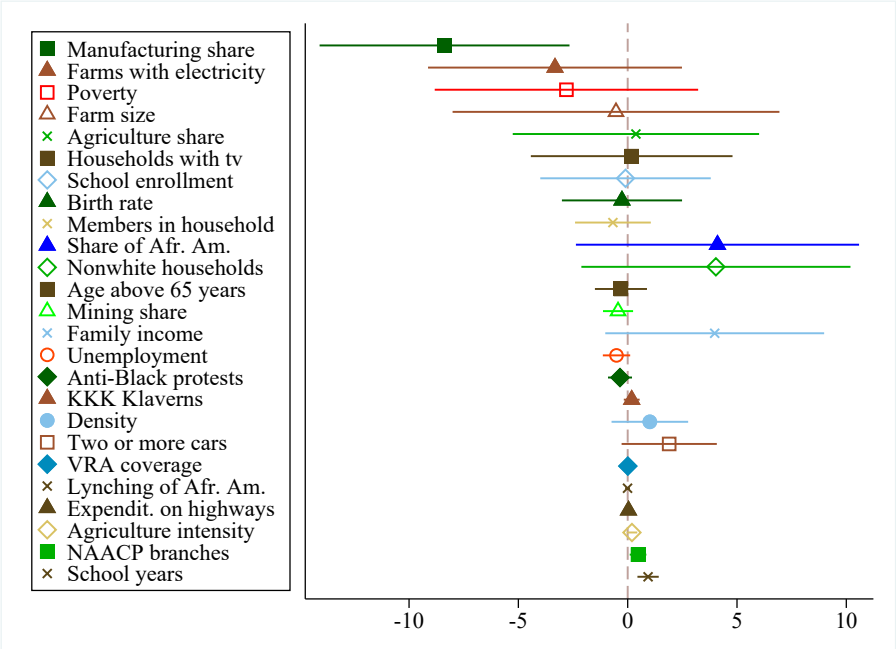
Table A.2.4 presents all the main results when the sample is restricted to those counties where African Americans represented a minority of the population. Across all panels, the estimates are very similar in both magnitude and significance to the ones found in the more general specifications reported in Chapter 1.

**Figure A.2.1:** Distribution of the log of the odds of the propensity score




The figure presents the distribution of the log of the odds of the propensity score, with the latter statistics computed – separately for treated and untreated counties – from a logistic regression based on the full set of covariates.

**Figure A.2.2:** Balance in the values of the covariates: 15-mile radius




The figure presents the estimated coefficients obtained by regressing each variable on the VRA indicator, topographic variables, and state fixed effects. Robust standard errors are adjusted for clustering at the county-level, and confidence intervals are set at the 95% level of statistical significance.

**Table A.2.1:** Standardized differences in the means

| | Sample | | | Stratification | | | Matching | | |
|---|---|---|---|---|---|---|---|---|---|
| | Mean Treat (1) | Mean Untreat (2) | Stand. Diff. (3) | Mean Treat (4) | Mean Untreat (5) | Stand. Diff. (6) | Mean Treat (7) | Mean Untreat (8) | Stand. Diff. (9) |
| Nonwhite households, 1950 | 34.96 | 20.29 | 0.84 | 34.96 | 34.16 | 0.05 | 33.91 | 33.81 | 0.01 |
| Share of Afr. Am., 1960 | 36.27 | 21.47 | 0.81 | 36.27 | 35.42 | 0.05 | 35.07 | 34.99 | 0.00 |
| NAACP branches, 1957-1964 | 0.81 | 0.32 | 0.75 | 0.81 | 0.74 | 0.12 | 0.77 | 0.72 | 0.07 |
| Median income, 1960 | 3968.29 | 3322.05 | 0.54 | 3968.29 | 3812.20 | 0.13 | 3873.15 | 3891.36 | -0.01 |
| Older than 65 years, 1960 | 7.99 | 9.65 | -0.54 | 7.99 | 8.14 | -0.05 | 8.03 | 8.21 | -0.06 |
| VRA coverage | 0.72 | 0.48 | 0.50 | 0.72 | 0.71 | 0.02 | 0.72 | 0.68 | 0.07 |
| Household poverty, 1960 | 39.1 | 46.57 | -0.49 | 39.10 | 40.87 | -0.12 | 40.22 | 40.15 | 0.01 |
| Population density, 1960 | 0.45 | 0.11 | 0.38 | 0.45 | 0.31 | 0.15 | 0.33 | 0.37 | -0.03 |
| Expenditure on highways, 1962 | 1.35 | 1.85 | -0.31 | 1.35 | 1.38 | -0.02 | 1.41 | 1.29 | 0.08 |
| Household members, 1960 | 3.70 | 3.58 | 0.26 | 3.70 | 3.71 | -0.02 | 3.71 | 3.69 | 0.04 |

This table lists the ten covariates with the largest standardized difference in the means between treated and untreated counties. The standardized difference is also reported after the sample is balanced on the propensity score.

**Table A.2.2:** Propensity score analysis

| | | Propensity Score Analysis: Intensity of Riots | | | |
|---|---|---|---|---|---|
| | Not Balanced (1) | Stratification in quintiles (2) | Matching caliper 0.1 (3) | Common support (4) | Stratification & trimming (5) |
| Predicted signal | 5.087*** | 4.247*** | 3.254* | 5.176*** | 3.519*** |
| | (1.242) | (1.182) | (1.673) | (1.358) | (0.943) |
| Free-Space Signal | Yes | Yes | Yes | Yes | Yes |
| State Fixed Effects | Yes | Yes | Yes | Yes | Yes |
| Topographic | Yes | Yes | Yes | Yes | Yes |
| Socio-Economic | Yes | Yes | Yes | Yes | Yes |
| Pre-VRA Conflict | Yes | Yes | Yes | Yes | Yes |
| Adj. R-Square | 0.50 | 0.44 | 0.49 | 0.44 | 0.49 |
| N | 1022 | 1022 | 225 | 871 | 873 |

Robust standard errors in parenthesis are adjusted for clustering at the county-level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. All regressions include state fixed effects, topographic variables, socio-economic controls, pre-VRA measures of conflict, and the unobstructed radio signals.

**Table A.2.3:** The effect in neighboring counties

| | Likelihood of Riots | | Intensity of Riots | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| *Panel A: 100% Black-Appeal Stations* | | | | |
| Dummy | 0.329* | | 0.666** | |
| | (0.184) | | (0.290) | |
| Continuous | | 0.008** | | 0.011** |
| | | (0.003) | | (0.004) |
| Adj. R-Square | 0.53 | 0.60 | 0.30 | 0.29 |
| N | 150 | 150 | 150 | 150 |
| *Panel B: 100% Black-Appeal Stations & Free-Space Signal* | | | | |
| Dummy | 0.329* | | 0.669** | |
| | (0.185) | | (0.292) | |
| Continuous | | 0.008** | | 0.012** |
| | | (0.003) | | (0.005) |
| Adj. R-Square | 0.53 | 0.60 | 0.29 | 0.29 |
| N | 150 | 150 | 150 | 150 |
| *Panel C: IV (2SLS)* | | | | |
| Dummy | 0.822** | | 0.834** | |
| | (0.359) | | (0.423) | |
| Continuous | | 0.013*** | | 0.012** |
| | | (0.004) | | (0.005) |
| Adj. R-Square | 0.38 | 0.56 | 0.30 | 0.30 |
| N | 148 | 148 | 148 | 148 |
| *Panel D: Generalized Difference-in-Differences* | | | | |
| Dummy | 0.057 | | 0.111* | |
| | (0.038) | | (0.057) | |
| Continuous | | 0.001* | | 0.001 |
| | | (0.001) | | (0.001) |
| Adj. R-Square | 0.52 | 0.52 | 0.25 | 0.25 |
| N | 900 | 900 | 900 | 900 |

Robust standard errors in parenthesis are adjusted for clustering at the county-level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. All regressions include state fixed effects and all the controls that are not balanced in the sample.

**Table A.2.4:** The effect in minority-Black counties

| | Likelihood of Riots | | Intensity of Riots | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| *Panel A: 100% Black-Appeal Stations* | | | | |
| Dummy | 0.409** | | 5.339*** | |
| | (0.199) | | (1.253) | |
| Continuous | | 0.006** | | 0.067*** |
| | | (0.003) | | (0.016) |
| Adj. R-Square | 0.67 | 0.68 | 0.52 | 0.52 |
| N | 897 | 897 | 897 | 897 |
| *Panel B: 100% Black-Appeal Stations & Free-Space Signal* | | | | |
| Dummy | 0.370* | | 5.752*** | |
| | (0.203) | | (1.333) | |
| Continuous | | 0.006* | | 0.081*** |
| | | (0.003) | | (0.020) |
| Adj. R-Square | 0.67 | 0.67 | 0.52 | 0.53 |
| N | 897 | 897 | 897 | 897 |
| *Panel C: IV (2SLS)* | | | | |
| Dummy | 1.195* | | 16.707*** | |
| | (0.666) | | (5.183) | |
| Continuous | | 0.014* | | 0.164*** |
| | | (0.008) | | (0.056) |
| Adj. R-Square | 0.60 | 0.63 | 0.33 | 0.43 |
| N | 896 | 896 | 896 | 896 |
| *Panel D: Generalized Difference-in-Differences* | | | | |
| Dummy | 0.152*** | | 0.875*** | |
| | (0.042) | | (0.295) | |
| Continuous | | 0.002*** | | 0.015*** |
| | | (0.001) | | (0.006) |
| Adj. R-Square | 0.20 | 0.20 | 0.12 | 0.13 |
| N | 6804 | 6804 | 6804 | 6804 |

Robust standard errors in parenthesis are adjusted for clustering at the county-level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. All regressions include state fixed effects, topographic variables, socio-economic controls, and pre-VRA measures of conflict.

## A.3 Alternative data on conflicts

Olzak (2015) collected information on all ethnic conflicts that occurred during the period 1954-1992, including riots, meetings, rallies, spontaneous disruptions, boycotts, conflicts at the workplace, and episodes of picketing and ethnic vandalism. Compared to Carter (1986), Olzak (2015) considered the *New York Times* as the main source of information, and defined a race riot as a demonstration involving at least 50 people, with some form of violence, and lasting for at least two hours. In Chapter 1, Carter (1986)'s dataset forms the basis to construct the main dependent variables, but Olzak (2015) is also considered when panel techniques, rather than cross-sectional analyses, are used on a time series spanning 1960-1971.

However, before augmenting the information from Carter (1986) with the one from Olzak (2015), the similarity between these two datasets has been considered. First, a summary of all ethnic conflicts is presented in Table A.3.1, separately by year, type of conflict, and state. Focusing on race riots (Panel B), the bulk of episodes occurred within the period 1964-1971 even according to the definition used by Olzak (2015). Still, there is a non-trivial amount of riots happening between 1960 and 1963 to justify extending the time series to cover the full period 1960-1971. In Table A.3.2, the impact of the full array of twenty-five controls on the likelihood and intensity of both all ethnic conflicts, columns (1) and (2), and race riots, columns (3) and (4), is presented. Similarly to the results shown in columns (3) and (4) of Table 1.1, based on Carter (1986), a large set of controls (including the share of African Americans in the county population, as well as the indicators of pre-period conflicts) are found to be important determinants of ethnic disorders.[6]

Figure A.3.1 plots both datasets. And although Olzak (2015) might understate the actual number of riots (by considering only the *New York Times* and by imposing a threshold of minimum 50 participants), while Carter (1986) might overstate them (by including more sources and by allowing for a slightly weaker definition of riots), the overlap between the two sources (especially in the years 1964-1966) is visible.

[6]Replicating the main specifications presented in the chapter using dependent variables based on Olzak (2015) do not modify the strong effect of being exposed to Black-appeal radio stations.

**Table A.3.1:** The race riots between 1960 and 1975

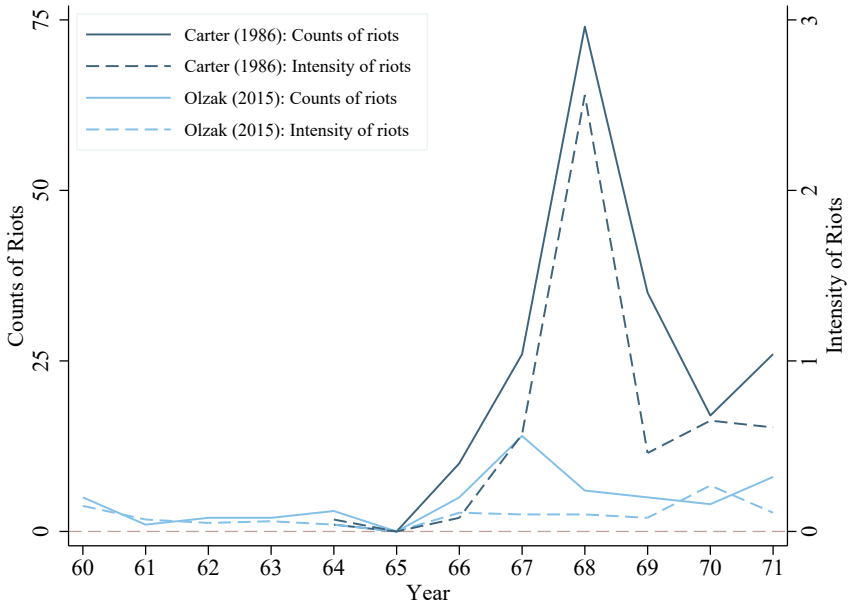
| | 60 | 61 | 62 | 63 | 64 | 65 | 66 | 67 | 68 | 69 | 70 | 71 | 72 | 73 | 74 | 75 | Total |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| *Panel A. Aggregate Race Conflicts in the U.S. South* | | | | | | | | | | | | | | | | | |
| Total Number | 163 | 74 | 24 | 27 | 31 | 43 | 26 | 31 | 27 | 25 | 17 | 22 | 9 | 0 | 4 | 3 | 526 |
| Days of conflict | 277 | 548 | 95 | 30 | 39 | 67 | 40 | 956 | 37 | 35 | 30 | 49 | 14 | 0 | 4 | 3 | 2224 |
| People killed | 1 | 0 | 1 | 404 | 1 | 1 | 1 | 2 | 5 | 1 | 11 | 1 | 5 | 0 | 0 | 0 | 434 |
| People injured | 181 | 30 | 5 | 113 | 131 | 23 | 51 | 84 | 80 | 47 | 140 | 37 | 49 | 0 | 0 | 2 | 973 |
| People arrested | 1473 | 1147 | 378 | 254 | 310 | 201 | 060 | 93 | 291 | 400 | 326 | 772 | 479 | 22 | 0 | 36 | 510045 |
| Size of conflict | 382 | 174 | 68 | 96 | 89 | 112 | 78 | 89 | 87 | 85 | 56 | 65 | 30 | 0 | 12 | 10 | 1433 |
| *Severity Index* | *0.73* | *0.51* | *0.14* | *1.38* | *0.32* | *0.24* | *0.14* | *0.61* | *0.21* | *0.16* | *0.30* | *0.16* | *0.09* | *0.00* | *0.01* | *0.01* | *5.00* |
| *Panel B. Race Riots in the U.S. South* | | | | | | | | | | | | | | | | | |
| Total Number | 5 | 1 | 2 | 7 | 3 | 0 | 5 | 14 | 6 | 5 | 4 | 8 | 3 | 0 | 0 | 0 | 63 |
| Days of conflict | 24 | 1 | 2 | 7 | 3 | 0 | 7 | 23 | 15 | 7 | 14 | 26 | 5 | 0 | 0 | 0 | 134 |
| People killed | 1 | 0 | 0 | 1 | 0 | 0 | 1 | 2 | 4 | 1 | 11 | 0 | 4 | 0 | 0 | 0 | 25 |
| People injured | 60 | 5 | 1 | 68 | 14 | 0 | 35 | 81 | 25 | 46 | 138 | 22 | 48 | 0 | 0 | 0 | 543 |
| People arrested | 412 | 48 | 56 | 223 | 19 | 0 | 83 | 273 | 250 | 198 | 100 | 286 | 22 | 0 | 0 | 0 | 1970 |
| Size of conflict | 21 | 5 | 8 | 32 | 10 | 0 | 16 | 44 | 19 | 18 | 13 | 26 | 11 | 0 | 0 | 0 | 223 |
| *Severity Index* | *0.63* | *0.06* | *0.08* | *0.47* | *0.10* | *0.00* | *0.27* | *0.74* | *0.53* | *0.36* | *0.91* | *0.50* | *0.35* | *0.00* | *0.00* | *0.00* | *5.00* |
| *Panel C. Meetings and Rallies in the U.S. South* | | | | | | | | | | | | | | | | | |
| Total Number | 105 | 45 | 15 | 15 | 11 | 20 | 3 | 8 | 12 | 9 | 8 | 3 | 6 | 0 | 3 | 1 | 264 |
| Days of conflict | 124 | 52 | 28 | 18 | 12 | 37 | 3 | 9 | 13 | 11 | 10 | 3 | 9 | 0 | 3 | 1 | 333 |
| People killed | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 1 |
| People injured | 0 | 1 | 0 | 7 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 9 |
| People arrested | 9031 | 051 | 307 | 1601 | 357 | 1046 | 0 | 11 | 18 | 53 | 670 | 0 | 0 | 0 | 24 | 0 | 6041 |
| Size of conflict | 247 | 100 | 42 | 51 | 30 | 69 | 12 | 22 | 41 | 31 | 27 | 12 | 19 | 0 | 9 | 5 | 717 |
| *Severity Index* | *0.87* | *0.58* | *0.19* | *1.17* | *0.14* | *0.38* | *0.03* | *0.06* | *0.10* | *0.09* | *0.18* | *0.03* | *1.16* | *0.00* | *0.03* | *0.01* | *5.00* |
| *Panel D. Race Riots in the U.S. South, by State* | | | | | | | | | | | | | | | | | |
| Alabama | 1 | 0 | 0 | 5 | 1 | 0 | 0 | 3 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 10 |
| Arkansas | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 |
| Florida | 1 | 0 | 0 | 0 | 0 | 0 | 3 | 2 | 1 | 3 | 3 | 3 | 0 | 0 | 0 | 0 | 16 |
| Georgia | 0 | 0 | 2 | 0 | 0 | 0 | 2 | 2 | 0 | 0 | 1 | 3 | 0 | 0 | 0 | 0 | 10 |
| Louisiana | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 2 | 0 | 1 | 0 | 0 | 1 | 0 | 0 | 0 | 5 |
| Mississippi | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 |
| North Carolina | 0 | 1 | 0 | 1 | 0 | 0 | 0 | 2 | 1 | 0 | 0 | 1 | 2 | 0 | 0 | 0 | 8 |
| South Carolina | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Tennessee | 1 | 0 | 0 | 1 | 0 | 0 | 0 | 2 | 1 | 1 | 0 | 1 | 0 | 0 | 0 | 0 | 7 |
| Texas | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 2 |
| Virginia | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 2 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 3 |

**Table A.3.2:** The determinants of conflicts and riots using alternative datasets

| | All Race Conflicts | | Race Riots | |
|---|---|---|---|---|
| | Likelihood (1) | Intensity (2) | Likelihood (3) | Intensity (4) |
| Share of African Americans, 1960 | 0.065*** | 0.209* | 0.072*** | 0.254* |
| | (0.023) | (0.109) | (0.028) | (0.132) |
| VRA coverage in 1965 | –0.548** | 0.347 | 0.041 | 1.624 |
| | (0.245) | (1.456) | (0.431) | (2.946) |
| Population density, 1960 | –0.263 | 0.518 | –0.185 | –1.347 |
| | (0.704) | (3.355) | (0.762) | (2.688) |
| Manufacturing size, 1960 | –0.015 | –0.062 | –0.006 | –0.073 |
| | (0.011) | (0.051) | (0.015) | (0.068) |
| Agriculture size, 1960 | –0.014 | –0.117 | –0.014 | –0.072 |
| | (0.011) | (0.082) | (0.014) | (0.064) |
| Mining size, 1950 | 0.006 | 0.025 | 0.007 | 0.005 |
| | (0.010) | (0.053) | (0.012) | (0.061) |
| Agricultural intensity, 1960 | –0.015 | –0.002 | –0.036 | –0.203* |
| | (0.020) | (0.111) | (0.028) | (0.120) |
| Unemployment rate, 1960 | 0.006 | –0.514 | 0.033 | –0.112 |
| | (0.051) | (0.314) | (0.072) | (0.293) |
| Share of families in poverty, 1960 | –0.006 | 0.138 | –0.004 | –0.033 |
| | (0.044) | (0.242) | (0.049) | (0.191) |
| Median family income, 1960 | –0.000 | –0.000 | –0.000 | –0.001 |
| | (0.001) | (0.003) | (0.001) | (0.003) |
| Birth rate, 1960 | –0.002 | –0.016 | –0.022 | –0.117 |
| | (0.015) | (0.070) | (0.022) | (0.083) |
| Household members, 1960 | –0.695 | –5.129* | –0.975 | –7.225** |
| | (0.554) | (2.727) | (0.754) | (3.535) |
| Size of farms, 1959 | –0.005 | –0.028 | –0.004 | –0.039 |
| | (0.006) | (0.032) | (0.008) | (0.037) |
| Nonwhite households, 1950 | –0.035* | –0.117 | –0.033 | –0.108 |
| | (0.020) | (0.111) | (0.023) | (0.115) |
| Farms w. electricity, 1954 | –0.030 | –0.282 | 0.006 | 0.059 |
| | (0.026) | (0.204) | (0.024) | (0.078) |
| Households w. tv, 1960 | 0.024** | 0.251* | 0.013 | 0.068 |
| | (0.011) | (0.141) | (0.014) | (0.073) |
| Households w. two or more cars, 1960 | 0.030* | 0.157* | 0.048** | 0.134 |
| | (0.017) | (0.083) | (0.023) | (0.095) |
| Older than 65 years, 1960 | –0.098 | –0.754** | –0.126 | –0.918* |
| | (0.073) | (0.383) | (0.105) | (0.491) |
| Expenditure on highways, 1962 | –0.097* | –0.494 | –0.073 | 0.200 |
| | (0.053) | (0.457) | (0.083) | (0.555) |
| Median school years, 1960 | 0.097 | –0.092 | 0.067 | –0.721 |
| | (0.132) | (0.713) | (0.231) | (0.858) |
| Enrolled in school, 1950 | –0.004 | –0.022* | –0.005 | –0.013 |
| | (0.002) | (0.013) | (0.003) | (0.013) |
| Lynchings of Afr. Am., 1902-1964 | –1.284** | –6.838** | –1.765** | –7.938** |
| | (0.577) | (3.231) | (0.699) | (3.144) |
| Anti-black activism, 1960-1964 | 0.113*** | 0.183 | –0.001 | –0.164 |
| | (0.042) | (0.301) | (0.045) | (0.154) |
| NAACP branches, 1957-1964 | –0.372** | –1.747** | –0.468** | –2.012** |
| | (0.169) | (0.756) | (0.205) | (0.895) |
| KKK Klaverns, 1925-1940 | 0.747*** | 3.484*** | 0.710** | 4.341*** |
| | (0.260) | (1.052) | (0.316) | (1.457) |
| State Fixed Effects | Yes | Yes | Yes | Yes |
| Topographic Controls | Yes | Yes | Yes | Yes |
| Adj. R-Square | 0.61 | 0.39 | 0.46 | 0.48 |
| N | 1022 | 1022 | 1022 | 1022 |

Robust standard errors in parenthesis are adjusted for clustering at the county-level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. All regressions include state fixed effects and topographic variables.

**Figure A.3.1:** Alternative datasets on race riots



## A.4 Quality of the Irregular Terrain Model

The Irregular Terrain Model (ITM) is usually used in the propagation of Frequency Modulation (FM) radio signals, rather than the Amplitude Modulation (AM) signals that are considered in this study.[7] As a first mean to evaluate the quality of the ITM, Section 1.4.3 presents the results from two-stage least squares regressions and, reassuringly, the IV results remain robust to the ones obtained when coverage is estimated through the ITM.[8]

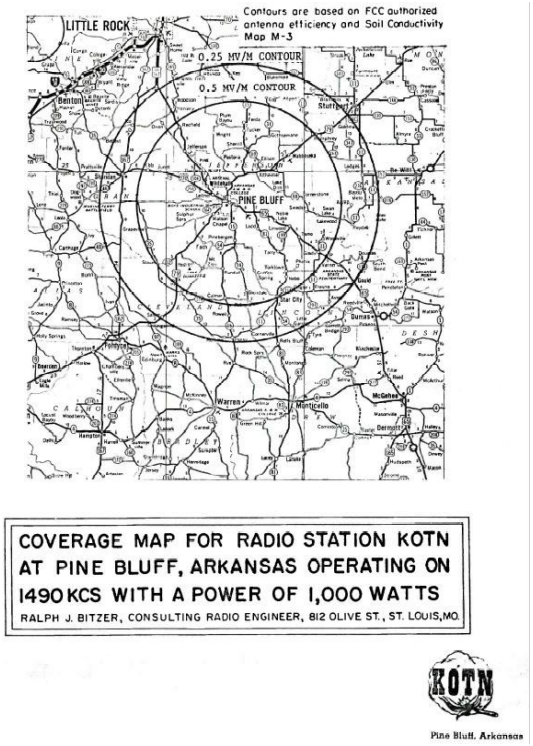
In this section, a second assessment of the quality of the ITM is presented, by considering the visual correspondence between the ITM and what Black-appeal radio stations were reporting to be their actual coverage. That is, the predicted coverage obtained from this physical model of electromagnetic signal propagation is directly compared to the historical records of coverage reported by eight Black-appeal radio stations. Comparing these maps shows that the ITM delivers a reasonable and reliable approximation of the true signal. The estimates present in these historical documents, often carried out by third-party radio consulting engineers, were then used by the radio stations to seek advertisements from private companies.[9] The coverage reported by these Black-appeal radio stations is often shown as two contours, one for the reception of a strong signal (ranging from $0.5MV/M$ to $2MV/M$), and one allowing for a weaker signal (from $0.1MV/M$ to $0.5MV/M$). From the ITM, I have constructed similar measures to allow for the reception of both strong and weak signals. And although there is substantial variation in the definition used by each station on what constitutes a strong and a weak signal, the areas obtained from the ITM closely overlap the contours obtained from these historical sources.

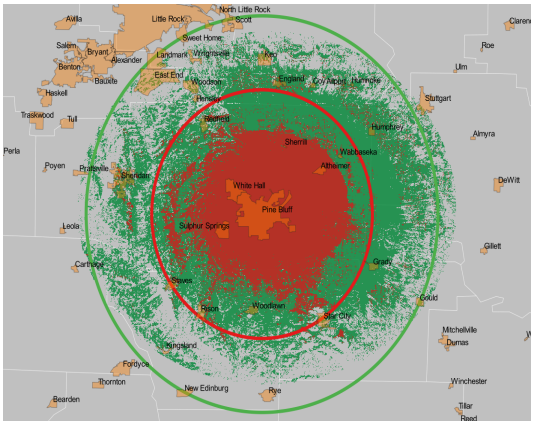
[7] Although the two types of signal function identically, they differ along one key dimension: the modulation of the carrier waves, with the former transmitting sounds through changes in frequency, rather than by varying the wave's amplitude.

[8] Among others, a measure of ground conductivity at the county-seat is exploited. This is the same indicator used by the U.S. Federal Communications Commission (FCC) in their assessment of AM propagation characteristics. I thank David Strömberg for sharing the data.

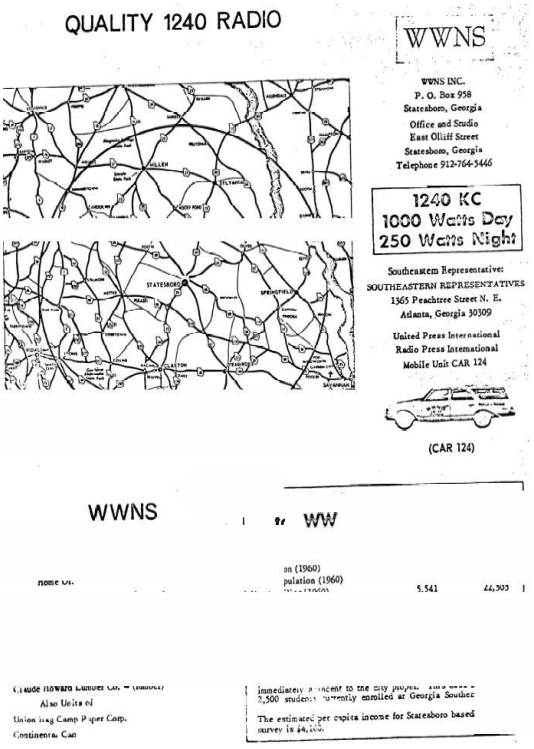
[9] During the 1960s, it was often hard for these (usually small) Black-appeal radio stations to generate a sustainable stream of revenue to sustain their operations, leading to a relatively low survival rate.

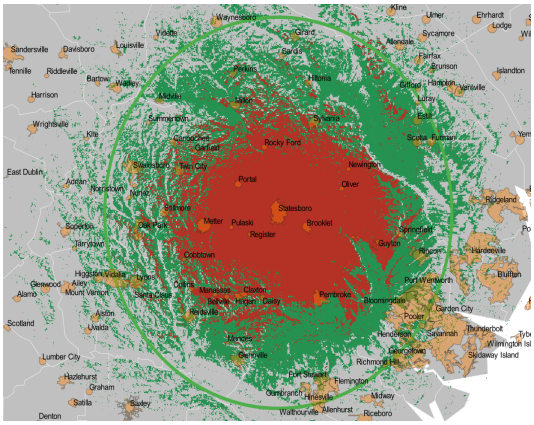
Arkansas: KOTN-AM Radio Station in Pinebluff



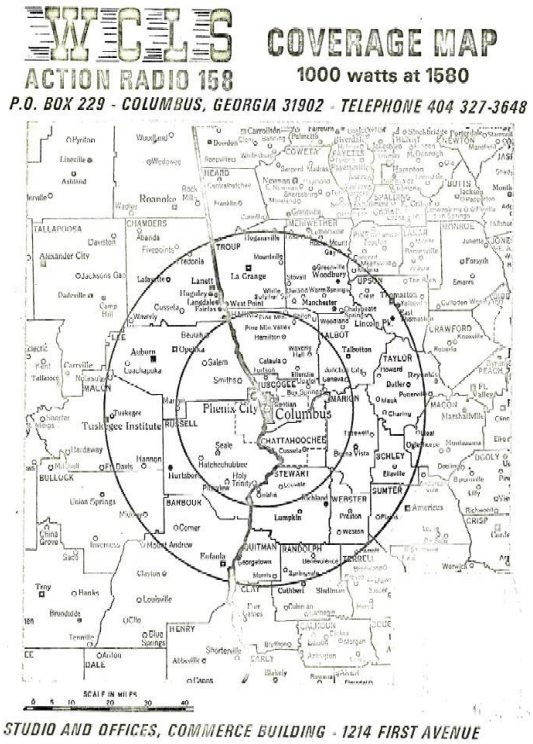

Georgia: WWNS-AM Radio Station in Statesboro

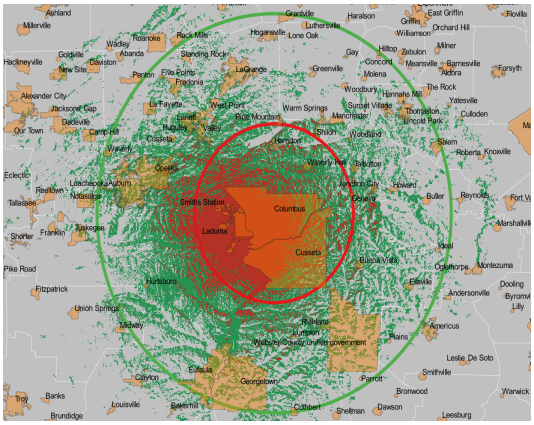


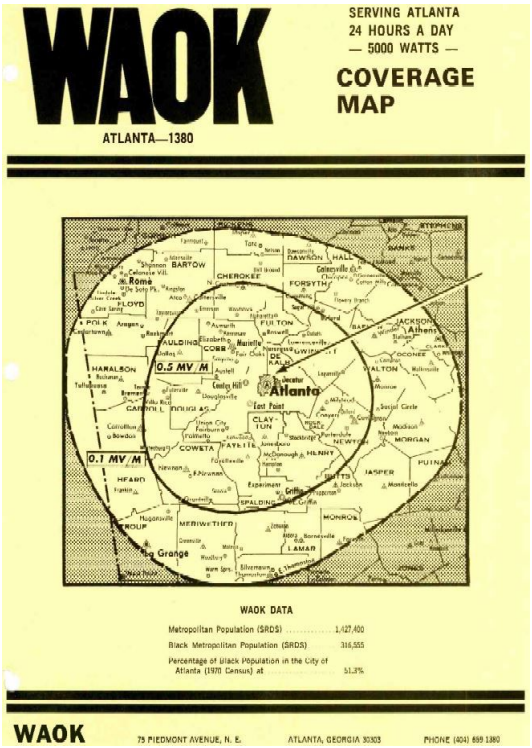

A.4. Quality of the Irregular Terrain Model

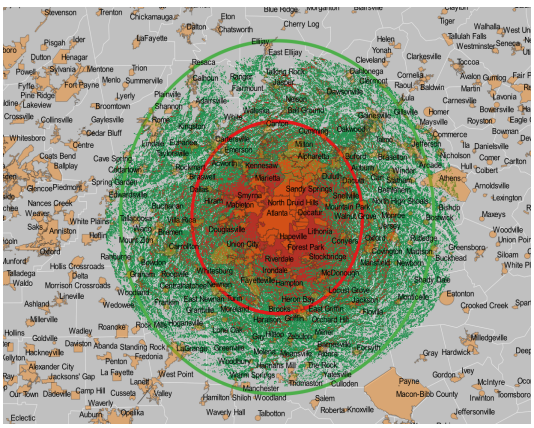
Georgia: WCLS-AM Radio Station in Columbus



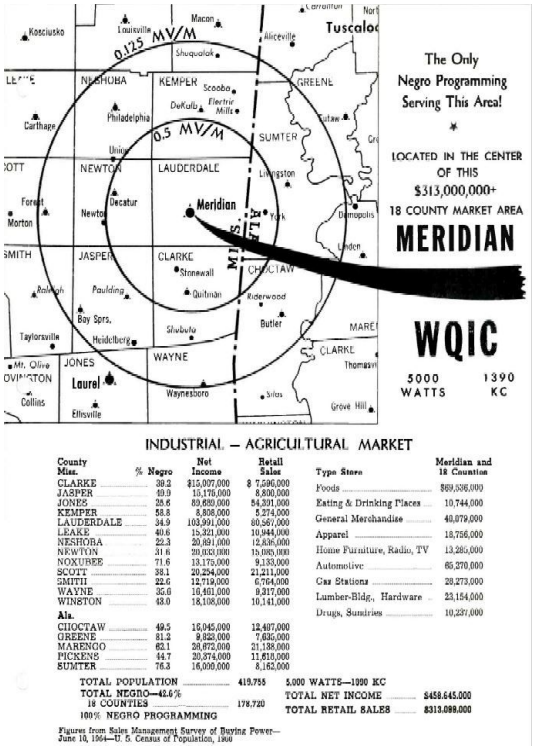



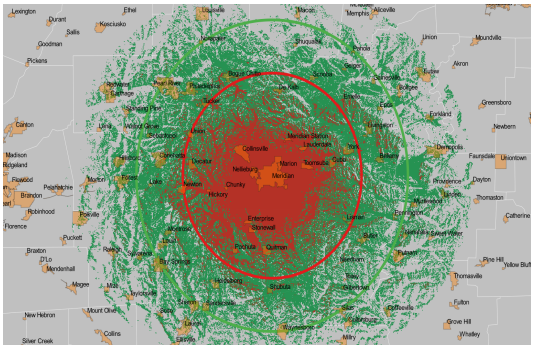
Georgia: WAOK-AM Radio Station in Atlanta



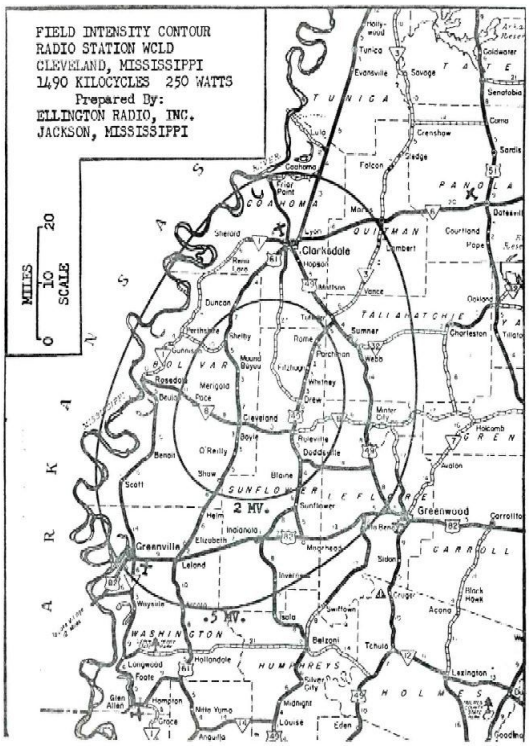



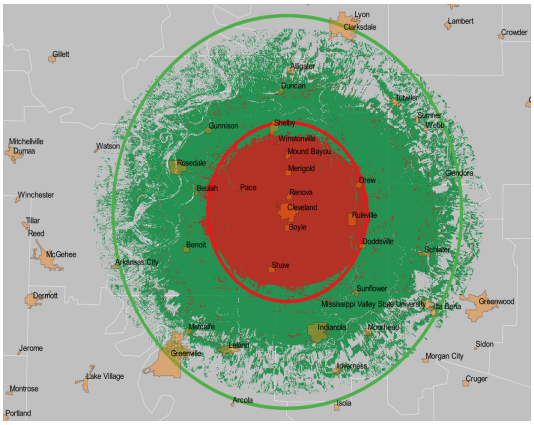
Mississippi: WQIC-AM Radio Station in Meridian



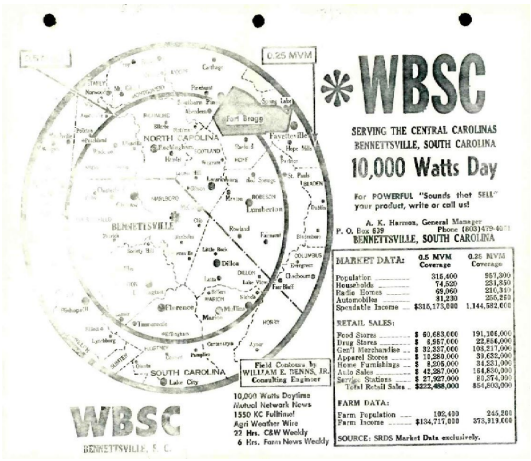
Mississippi: WCLD-AM Radio Station in Cleveland

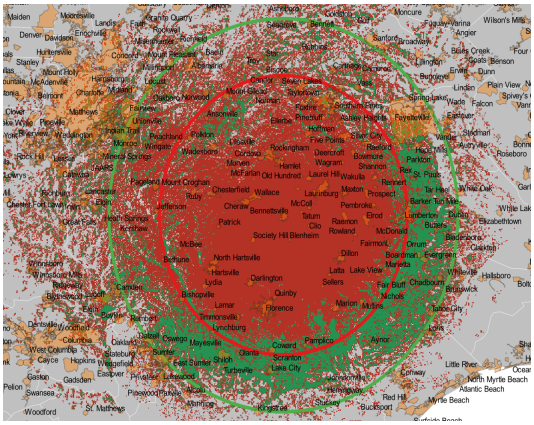


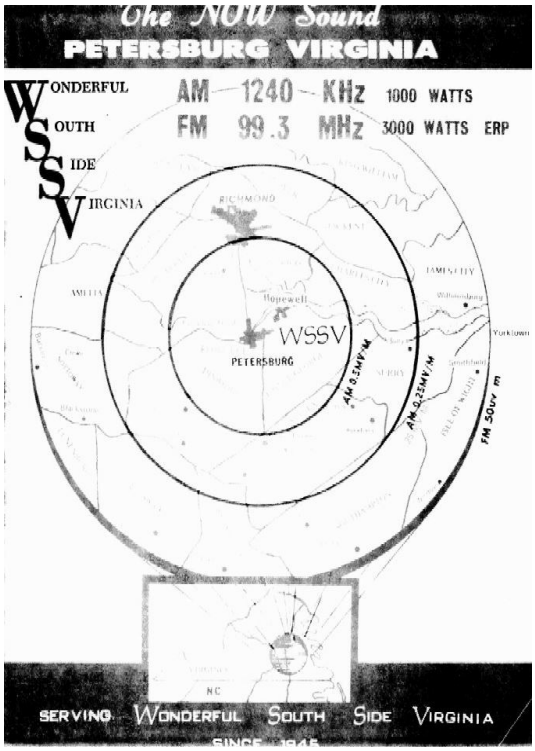
## A.4. Quality of the Irregular Terrain Model

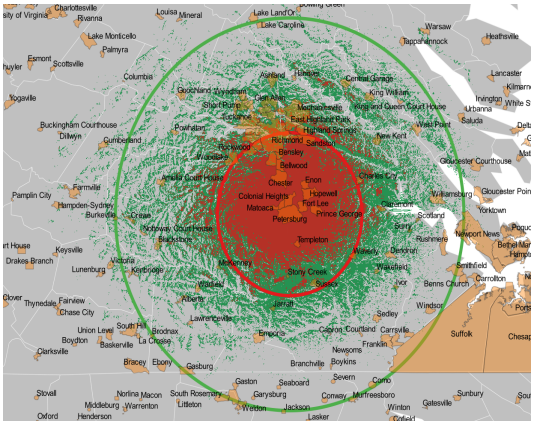
South Carolina: WBSC-AM Radio Station in Bennettsville



Virginia: WSSV-AM Radio Station in Petersburg



# B

# Appendix to Chapter 2

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# B.1 Summary statistics

**Table B.1.1:** Summary statistics

| | Covered | | Not covered | |
|---|---|---|---|---|
| | Mean | St. Dev. | Mean | St. Dev. |
| *African American Elected Officials* | | | | |
| All local officials, 1964 | 0.11 | 1.02 | 0.05 | 0.47 |
| All local officials, 1980 | 7.03 | 11.41 | 2.16 | 4.93 |
| Municipality, 1964 | 0.18 | 1.62 | 0.04 | 0.43 |
| Municipality, 1980 | 5.35 | 9.39 | 1.91 | 4.83 |
| School district, 1964 | 0.00 | 0.00 | 0.07 | 0.99 |
| School district, 1980 | 5.81 | 11.60 | 1.40 | 5.30 |
| County government, 1964 | 0.00 | 0.00 | 0.00 | 0.00 |
| County government, 1980 | 4.09 | 11.56 | 0.61 | 2.03 |
| County governing body, 1964 | 0.00 | 0.00 | 0.00 | 0.00 |
| County governing body, 1980 | 2.26 | 5.51 | 0.47 | 1.81 |
| Judiciary and Enforcement, 1964 | 0.00 | 0.00 | 0.00 | 0.00 |
| Judiciary and Enforcement, 1980 | 1.26 | 4.97 | 0.14 | 0.92 |
| Other administrative body, 1964 | 0.00 | 0.00 | 0.00 | 0.00 |
| Other administrative body, 1980 | 0.42 | 2.84 | 0.00 | 0.00 |
| *County Characteristics* | | | | |
| African American Share, 1960 | 32.42 | 20.00 | 13.18 | 14.07 |
| Unemployment, 1960 | 4.96 | 1.94 | 4.87 | 2.16 |
| Poverty, 1960 | 46.15 | 16.18 | 43.67 | 14.94 |
| Unskilled, 1960 | 73.87 | 8.68 | 70.74 | 9.60 |
| Agricultural productivity | 0.26 | 0.23 | 0.59 | 0.44 |
| Population, 1960 | 34.74 | 57.77 | 40.88 | 101.86 |
| Urban, 1960 | 28.18 | 29.01 | 32.95 | 28.15 |
| Pro-Afr. Am. protest, 1960-64 | 1.09 | 5.67 | 0.54 | 3.19 |
| Anti-Afr. Am. protest, 1960-64 | 0.30 | 2.04 | 0.05 | 0.49 |
| Counties | 512 | | 538 | |

**Figure B.1.1:** Share of African Americans in the population in 1960

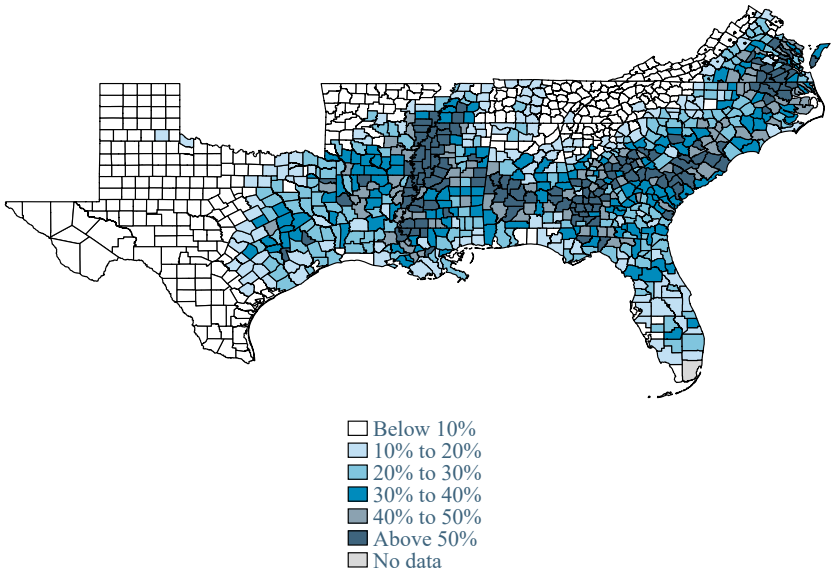


**Table B.1.2:** Summary statistics: Local expenditure

| | Covered | | Not covered | |
|---|---|---|---|---|
| | Mean | St. Dev. | Mean | St. Dev. |
| *Local Expenditure, per capita (real 2000 USD)* | | | | |
| Total expenditure, 1957 | 243.79 | 158.86 | 272.62 | 195.97 |
| Total expenditure, 1982 | 582.53 | 422.08 | 581.58 | 611.90 |
| Current expenditure, 1957 | 196.42 | 123.09 | 226.95 | 157.42 |
| Current expenditure, 1982 | 528.55 | 392.09 | 528.25 | 568.20 |
| Capital expenditure, 1957 | 49.70 | 59.76 | 50.26 | 62.38 |
| Capital expenditure, 1982 | 58.27 | 79.37 | 54.61 | 68.96 |
| *Log Local Expenditure, per capita (real 2000 USD)* | | | | |
| Total expenditure, 1957 | 5.31 | 0.62 | 5.39 | 0.66 |
| Total expenditure, 1982 | 6.12 | 0.71 | 6.04 | 0.82 |
| Current expenditure, 1957 | 5.09 | 0.63 | 5.21 | 0.65 |
| Current expenditure, 1982 | 6.01 | 0.73 | 5.93 | 0.84 |
| Capital expenditure, 1957 | 3.25 | 1.31 | 3.31 | 1.22 |
| Capital expenditure, 1982 | 3.39 | 1.25 | 3.40 | 1.20 |

## B.2 Additional results

**Table B.2.1:** Pre-VRA trends

| | NACCP | Turnout | KKK | Percent Republican |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Afr. Am. Share, 1960 X VRA | –0.0022 | 0.0002 | –0.0039 | –0.0001 |
| | (0.002) | (0.001) | (0.006) | (0.001) |
| Afr. Am. Share, 1960 | 0.0056*** | 0.0042*** | 0.0107** | 0.0072*** |
| | (0.002) | (0.001) | (0.004) | (0.001) |
| Urban, 1960 | 0.0009 | 0.0005** | 0.0051** | 0.0007 |
| | (0.001) | (0.000) | (0.002) | (0.001) |
| Unemployment, 1960 | –0.0204** | –0.0013 | –0.0136 | –0.0175*** |
| | (0.008) | (0.003) | (0.021) | (0.005) |
| Poverty, 1960 | –0.0038* | 0.0016*** | –0.0004 | –0.0023* |
| | (0.002) | (0.001) | (0.004) | (0.001) |
| Population, 1960 | –0.0002 | 0.0001 | 0.0017 | –0.0002 |
| | (0.000) | (0.000) | (0.001) | (0.000) |
| Unskilled, 1960 | 0.0001 | 0.0014*** | 0.0006 | 0.0099*** |
| | (0.002) | (0.000) | (0.004) | (0.001) |
| Agricultural productivity | 0.0670 | –0.0236 | –0.2216* | 0.0056 |
| | (0.057) | (0.017) | (0.116) | (0.036) |
| State Trends | Yes | Yes | Yes | Yes |
| Adj. R-Square | 0.30 | 0.79 | 0.30 | 0.87 |
| N | 1022 | 1022 | 1022 | 1022 |

Robust standard errors in parenthesis are adjusted for clustering at the county-level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

**Table B.2.2:** Common support

| | County Governments | | | Other Governments | |
|---|---|---|---|---|---|
| | Commission | Judiciary & Enforcement | Other | Municipality | School Districts |
| | (1) | (2) | (3) | (4) | (5) |
| Afr. Am. Share, 1960 X VRA X SMD | 0.139*** | | | | |
| | (0.041) | | | | |
| Afr. Am. Share, 1960 X VRA | –0.016 | 0.031*** | 0.012** | –0.024 | 0.006 |
| | (0.030) | (0.011) | (0.005) | (0.038) | (0.060) |
| Afr. Am. Share, 1960 X SMD | –0.016 | | | | |
| | (0.028) | | | | |
| Afr. Am. Share, 1960 | 0.067*** | 0.009 | –0.000 | 0.160*** | 0.177*** |
| | (0.025) | (0.006) | (0.000) | (0.027) | (0.040) |
| State Trends | Yes | Yes | Yes | Yes | Yes |
| Controls X VRA | Yes | Yes | Yes | Yes | Yes |
| Adj. R-Square | 0.43 | 0.26 | 0.06 | 0.41 | 0.40 |
| N | 942 | 940 | 942 | 899 | 759 |

The sample consists of counties with the 1960 share of African Americans above the $10^{th}$ percentile of the distribution of non-covered counties (2%) and below the maximum value in the non-covered sample (68%), corresponding to the $96^{th}$ percentile of the covered sample distribution. Robust standard errors in parenthesis are adjusted for clustering at the county-level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

**Table B.2.3:** Change in African American elected officials by electoral rules

| | Commission | Municipality & School Districts |
|---|---|---|
| | (1) | (2) |
| *Panel A: Overall Sample* | | |
| Afr. Am. Share, 1960 X VRA X SMD | 0.103** | 0.182 |
| | (0.045) | (0.151) |
| Afr. Am. Share, 1960 X VRA | 0.042 | 0.025 |
| | (0.038) | (0.139) |
| State Trends | Yes | Yes |
| Controls | Yes | Yes |
| Controls X VRA | Yes | Yes |
| Adj. R-Square | 0.40 | 0.50 |
| N | 1019 | 691 |
| *Panel B: Border Sample* | | |
| Afr. Am. Share, 1960 X VRA X SMD | 0.094* | 0.166 |
| | (0.048) | (0.119) |
| Afr. Am. Share, 1960 X VRA | 0.020 | –0.060 |
| | (0.017) | (0.082) |
| County-Pair Trends | Yes | Yes |
| Adj. R-Square | 0.53 | 0.66 |
| N | 295 | 241 |

Robust standard errors in parenthesis are adjusted for clustering at the county-level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

# C

# Appendix to Chapter 3

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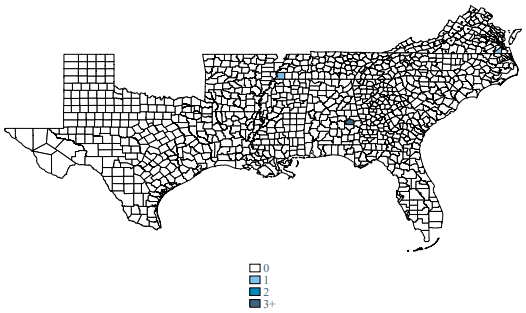
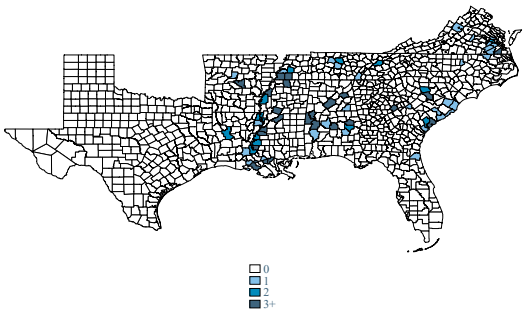
# C.1 Summary statistics

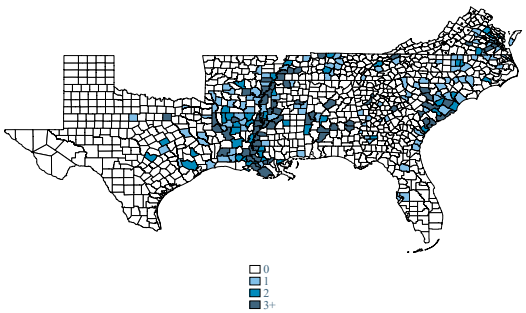
**Figure C.1.1:** African American elected officials in county governments



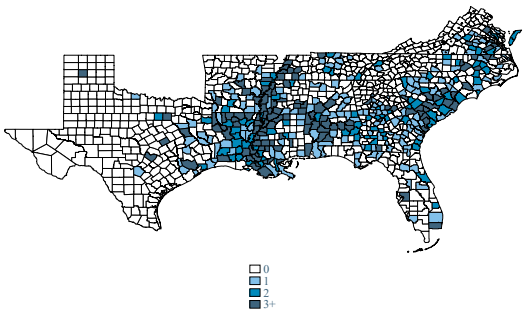

**(a)** Number of African American Elected Officials, 1964




**(b)** Number of African American Elected Officials, 1970




**(c)** Number of African American Elected Officials, 1980



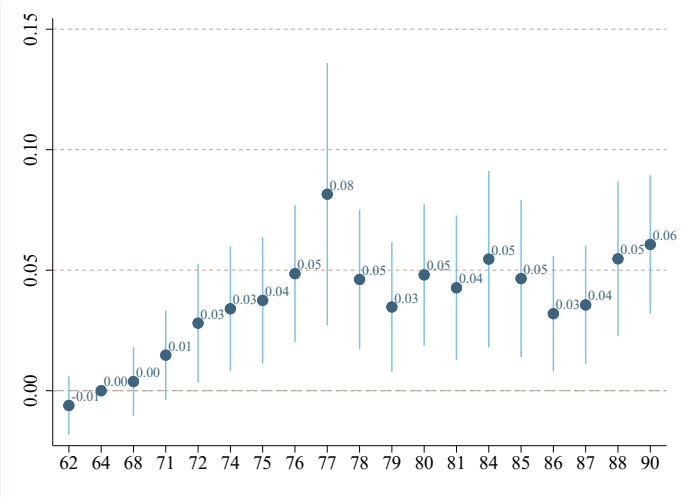

**(d)** Number of African American Elected Officials, 1990

**Table C.1.1:** Summary statistics

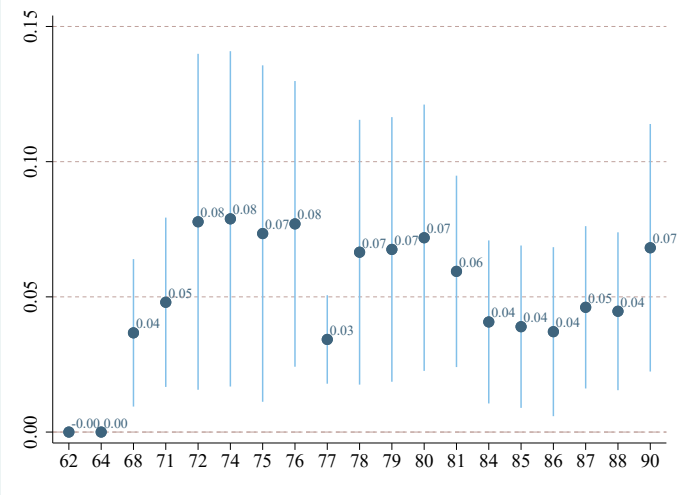
| | Covered | | Not covered | |
|---|---|---|---|---|
| | Mean | St. Dev. | Mean | St. Dev. |
| *Local black elected officials* | | | | |
| All county-level officials (%), 1964 | 0.04 | 1.02 | 0.00 | 0.12 |
| All county-level officials (%), 1970 | 1.47 | 6.84 | 0.08 | 0.62 |
| All county-level officials (%), 1980 | 4.22 | 12.05 | 0.62 | 2.09 |
| All county-level officials (%), 1990 | 8.51 | 14.53 | 1.83 | 5.57 |
| County governing bodies (%), 1964 | 0.00 | 0.00 | 0.00 | 0.00 |
| County governing bodies (%), 1970 | 0.43 | 3.02 | 0.06 | 0.56 |
| County governing bodies (%), 1980 | 2.26 | 5.51 | 0.47 | 1.81 |
| County governing bodies (%), 1990 | 5.82 | 8.65 | 1.26 | 4.08 |
| All judiciary and law enforcement (%), 1964 | 0.03 | 0.68 | 0.00 | 0.12 |
| All judiciary and law enforcement (%), 1970 | 0.78 | 3.75 | 0.02 | 0.26 |
| All judiciary and law enforcement (%), 1980 | 1.52 | 6.84 | 0.15 | 0.95 |
| All judiciary and law enforcement (%), 1990 | 2.08 | 7.88 | 0.54 | 2.65 |
| Judiciary (%), 1964 | 0.03 | 0.68 | 0.00 | 0.12 |
| Judiciary (%), 1970 | 0.34 | 1.89 | 0.00 | 0.00 |
| Judiciary (%), 1980 | 0.74 | 3.82 | 0.07 | 0.56 |
| Judiciary (%), 1990 | 0.90 | 4.37 | 0.37 | 2.33 |
| Law enforcement (%), 1964 | 0.00 | 0.00 | 0.00 | 0.00 |
| Law enforcement (%), 1970 | 0.44 | 3.03 | 0.02 | 0.26 |
| Law enforcement (%), 1980 | 0.78 | 4.93 | 0.08 | 0.69 |
| Law enforcement (%), 1990 | 1.18 | 5.19 | 0.17 | 1.13 |
| Other administrative bodies (%), 1964 | 0.01 | 0.34 | 0.00 | 0.00 |
| Other administrative bodies (%), 1970 | 0.26 | 2.02 | 0.00 | 0.09 |
| Other administrative bodies (%), 1980 | 0.45 | 2.81 | 0.00 | 0.00 |
| Other administrative bodies (%), 1990 | 0.80 | 3.62 | 0.03 | 0.53 |
| *County characteristics* | | | | |
| African American Share, 1960 | 32.47 | 19.98 | 13.18 | 14.07 |
| Unemployment, 1960 | 4.96 | 1.94 | 4.87 | 2.16 |
| Poverty, 1960 | 46.14 | 16.19 | 43.67 | 14.94 |
| Unskilled, 1960 | 73.86 | 8.69 | 70.74 | 9.60 |
| Agricultural productivity | 0.26 | 0.23 | 0.59 | 0.44 |
| Population, 1960 | 34.75 | 57.82 | 40.88 | 101.86 |
| Urban, 1960 | 28.22 | 29.01 | 32.95 | 28.15 |
| Afr. Am. lynching, 1902-64 | 0.03 | 0.05 | 0.04 | 0.17 |
| Pro-Afr. Am. protest, 1960-64 | 1.09 | 5.67 | 0.54 | 3.19 |
| Anti-Afr. Am. protest, 1960-64 | 0.30 | 2.04 | 0.05 | 0.49 |
| Counties | 512 | | 538 | |

## C.2 Additional results

**Figure C.2.1:** Judicial and law enforcement bodies




**(a)** Judiciary




**(b)** Law Enforcement

**Table C.2.1:** Pre-VRA trends

| | Turnout (1) | NACCP (2) | KKK (3) |
|---|---|---|---|
| Share of Afr. Am. in 1960 X VRA | 0.0002 | –0.0022 | –0.0039 |
| | (0.001) | (0.002) | (0.006) |
| Share of Afr. Am. in 1960 | 0.0042*** | 0.0056*** | 0.0107** |
| | (0.001) | (0.002) | (0.004) |
| Share of urban population, 1960 | 0.0005** | 0.0009 | 0.0051** |
| | (0.000) | (0.001) | (0.002) |
| Unemployment rate, 1960 | –0.0013 | –0.0203** | –0.0141 |
| | (0.003) | (0.008) | (0.021) |
| Share of families below poverty line, 1960 | 0.0016*** | –0.0038* | –0.0003 |
| | (0.001) | (0.002) | (0.004) |
| Total population, 1960 | 0.0001 | –0.0002 | 0.0017 |
| | (0.000) | (0.000) | (0.001) |
| Share of unskilled workers, 1960 | 0.0014*** | 0.0001 | 0.0006 |
| | (0.000) | (0.002) | (0.004) |
| Agricultural productivity | –0.0237 | 0.0669 | –0.2207* |
| | (0.017) | (0.057) | (0.116) |
| State Trends | Yes | Yes | Yes |
| Adj. R-Square | 0.79 | 0.30 | 0.30 |
| N | 1021 | 1021 | 1021 |

Robust standard errors in parenthesis are adjusted for clustering at the county-level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

**Table C.2.2:** Human capital on the change in the share of elected officials

| | Commissioners (1) | Judges (2) | Sheriffs (3) |
|---|---|---|---|
| Share of Afr. Am. in 1960 X College Educ 1960 X VRA | 0.020** | 0.011*** | 0.094*** |
| | (0.008) | (0.004) | (0.019) |
| Share of Afr. Am. in 1960 X College Educ 1960 | –0.001 | –0.001 | –0.012* |
| | (0.003) | (0.002) | (0.007) |
| Share of Afr. Am. in 1960 X VRA | 0.024 | –0.023 | –0.141 |
| | (0.052) | (0.019) | (0.162) |
| College Educ 1960 X VRA | –0.326 | –0.443* | –2.551*** |
| | (0.289) | (0.232) | (0.768) |
| Share of Afr. Am. in 1960 | 0.201*** | –0.002 | 0.203* |
| | (0.038) | (0.014) | (0.119) |
| African American College Education in 1960 | 0.132 | 0.050 | 0.427** |
| | (0.106) | (0.092) | (0.209) |
| Controls X VRA | Yes | Yes | Yes |
| State Trends | Yes | Yes | Yes |
| Adj. R-Square | 0.57 | 0.24 | 0.18 |
| N | 740 | 740 | 740 |

Robust standard errors in parenthesis are adjusted for clustering at the county-level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

**Table C.2.3:** Political organizations on the change in the share of elected officials

| | Commissioners (1) | Judges (2) | Sheriffs (3) |
|---|---|---|---|
| Share of Afr. Am. in 1960 X Church 1936 X VRA | 0.001 | 0.002** | 0.015* |
| | (0.002) | (0.001) | (0.008) |
| Share of Afr. Am. in 1960 X Church 1936 | 0.003** | –0.000 | 0.005 |
| | (0.001) | (0.000) | (0.007) |
| Share of Afr. Am. in 1960 X VRA | 0.143*** | 0.006 | 0.065 |
| | (0.042) | (0.014) | (0.107) |
| Church 1936 X VRA | –0.172** | –0.083** | –0.909*** |
| | (0.086) | (0.039) | (0.296) |
| Share of Afr. Am. in 1960 | 0.077*** | 0.003 | 0.023 |
| | (0.023) | (0.005) | (0.039) |
| African American Church in 1936 | –0.045 | 0.001 | –0.061 |
| | (0.054) | (0.018) | (0.183) |
| Controls X VRA | Yes | Yes | Yes |
| State Trends | Yes | Yes | Yes |
| Adj. R-Square | 0.56 | 0.23 | 0.21 |
| N | 1021 | 1021 | 1021 |

Robust standard errors in parenthesis are adjusted for clustering at the county-level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

**Table C.2.4:** Racial attitudes on the change in the share of elected officials

| | Commissioners (1) | Judges (2) | Sheriffs (3) |
|---|---|---|---|
| Share of Afr. Am. in 1960 X Lynching X VRA | –1.047*** | 0.088 | –1.281* |
| | (0.314) | (0.131) | (0.654) |
| Share of Afr. Am. in 1960 x Lynching | –0.285* | –0.220* | –0.367 |
| | (0.163) | (0.118) | (0.240) |
| Share of Afr. Am. in 1960 X VRA | 0.152*** | 0.014** | 0.203* |
| | (0.031) | (0.007) | (0.108) |
| Lynching X VRA | 18.874*** | 2.598* | 35.094*** |
| | (6.982) | (1.452) | (13.486) |
| Share of Afr. Am. in 1960 | 0.132*** | 0.001 | 0.103 |
| | (0.019) | (0.003) | (0.065) |
| Lynching of African Americans, 1902-1964 | 0.858** | 0.271 | 1.088* |
| | (0.433) | (0.180) | (0.559) |
| Controls X VRA | Yes | Yes | Yes |
| State Trends | Yes | Yes | Yes |
| Adj. R-Square | 0.56 | 0.22 | 0.15 |
| N | 1021 | 1021 | 1021 |

Robust standard errors in parenthesis are adjusted for clustering at the county-level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

**Table C.2.5:** Electoral rules on the change in the share of elected officials

| | 1980-1964 (1) | 1990-1964 (2) |
|---|---|---|
| *Panel A: Without switch* | | |
| Share of Afr. Am. in 1960 X VRA X SMD | 0.105** | 0.134* |
| | (0.044) | (0.071) |
| Share of Afr. Am. in 1960 X VRA | 0.035 | 0.012 |
| | (0.038) | (0.066) |
| Share of Afr. Am. in 1960 X SMD | –0.031 | –0.166*** |
| | (0.027) | (0.060) |
| Share of Afr. Am. in 1960 | 0.077*** | 0.256*** |
| | (0.026) | (0.059) |
| Controls | Yes | Yes |
| Controls X VRA | Yes | Yes |
| State Trends | Yes | Yes |
| Adj. R-Square | 0.40 | 0.56 |
| N | 1021 | 1021 |
| *Panel B: With switch* | | |
| Share of Afr. Am. in 1960 X VRA X SMD | 0.133*** | 0.108*** |
| | (0.029) | (0.033) |
| Share of Afr. Am. in 1960 X VRA X Early Switch | 0.066* | 0.139*** |
| | (0.034) | (0.035) |
| Share of Afr. Am. in 1960 X VRA X Late Switch | –0.018 | 0.139 |
| | (0.032) | (0.116) |
| Share of Afr. Am. in 1960 | 0.053*** | 0.128*** |
| | (0.011) | (0.018) |
| Controls | Yes | Yes |
| Controls X VRA | Yes | Yes |
| State Trends | Yes | Yes |
| Adj. R-Square | 0.40 | 0.55 |
| N | 1021 | 1021 |

Robust standard errors in parenthesis are adjusted for clustering at the county-level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

# D

# Appendix to Chapter 4

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# D.1 Summary statistics

**Table D.1.1:** Summary statistics: Long-run specification

| | Covered | | | | Not covered | | | |
|---|---|---|---|---|---|---|---|---|
| | Mean | St. Dev. | Min | Max | Mean | St. Dev. | Min | Max |
| *Political participation: Full sample* | | | | | | | | |
| Afr. Am. registration rates | 45.7 | 27.8 | 0.0 | 100.0 | 52.0 | 24.1 | 0.0 | 100.0 |
| White registration rates | 75.2 | 18.9 | 5.5 | 100.0 | 76.0 | 14.6 | 46.3 | 100.0 |
| Total registration rates | 65.7 | 19.0 | 4.6 | 100.0 | 73.3 | 14.7 | 30.6 | 100.0 |
| Gap Afr. Am.-white registration rates | -29.5 | 30.7 | -100.0 | 55.0 | -24.1 | 25.5 | -100.0 | 44.7 |
| Total turnout rates | 42.8 | 12.4 | 3.5 | 92.9 | 52.3 | 11.2 | 15.5 | 92.8 |
| Democratic vote shares | 49.7 | 18.9 | 0.0 | 95.4 | 47.4 | 17.4 | 16.5 | 100.0 |
| *Political participation: Pre-VRA sample* | | | | | | | | |
| Afr. Am. registration rates | 28.4 | 24.3 | 0.0 | 100.0 | 43.0 | 24.8 | 0.0 | 100.0 |
| White registration rates | 77.9 | 21.3 | 5.5 | 100.0 | 78.7 | 17.2 | 46.3 | 100.0 |
| Total registration rates | 60.7 | 21.1 | 4.6 | 100.0 | 73.3 | 17.8 | 30.6 | 100.0 |
| Gap Afr. Am.-white registration rates | -49.5 | 29.6 | -100.0 | 30.1 | -35.7 | 24.6 | -100.0 | 44.7 |
| Total turnout rates | 35.9 | 10.1 | 3.5 | 92.9 | 51.9 | 14.4 | 15.5 | 92.8 |
| Democratic vote shares | 58.3 | 21.7 | 0.0 | 95.4 | 55.2 | 15.6 | 19.6 | 100.0 |
| *County characteristics* | | | | | | | | |
| African American Share, 1960 | 37.6 | 17.5 | 0.1 | 83.4 | 19.5 | 16.2 | 0.0 | 68.9 |
| Unemployment, 1960 | 5.3 | 5.1 | 1.3 | 100.0 | 5.4 | 1.9 | 0.4 | 11.4 |
| Poverty, 1960 | 47.9 | 14.3 | 7.8 | 100.0 | 46.6 | 13.9 | 17.6 | 78.0 |
| Population density, 1960 | 10.5 | 20.3 | 0.4 | 100.0 | 7.1 | 12.3 | 0.4 | 100.0 |
| Urban population, 1960 | 28.4 | 27.1 | 0.0 | 100.0 | 28.8 | 25.5 | 0.0 | 96.6 |
| Rural farms, 1960 | 20.9 | 15.0 | 0.0 | 100.0 | 19.0 | 15.3 | 0.1 | 66.8 |
| Manufacturing sector, 1960 | 25.1 | 12.4 | 3.4 | 100.0 | 22.0 | 12.6 | 4.4 | 57.3 |
| Pro-Afr. Am. activism, 1960-64 | 1.5 | 7.7 | 0.0 | 100.0 | 0.9 | 3.6 | 0.0 | 34.0 |
| Anti-Afr. Am. activism, 1960-64 | 0.6 | 5.5 | 0.0 | 100.0 | 0.1 | 0.7 | 0.0 | 9.0 |
| County-year pairs: Full sample | | 796 | | | | 410 | | |
| County-year pairs: Pre-VRA sample | | 398 | | | | 205 | | |

**Table D.1.2:** The dataset on voter registration by race

| | AL | AK | FL | GA | LA | MS | NC | SC | TN | VA |
|---|---|---|---|---|---|---|---|---|---|---|
| *Afr. Am. voter registration* | | | | | | | | | | |
| 1956 | VEP | VEP(55) | VEP | VEP | CoCR59 | | VEP(58) | VEP | | VEP |
| 1960 | VEP | VEP(59) | CoCR61 | VEP | CoCR61 | | VEP | VEP(62) | CoCR61 | CoCR61 |
| 1964 | VEP | VEP(63) | VEP | VEP | VEP | VEP | | VEP | VEP(62) | VEP |
| 1968 | VEP | | VEP | VEP | VEP | VEP | VEP | VEP | VEP(66) | |
| 1972 | VEP | | VEP | VEP(73) | VEP | VEP | VEP | ICPSR | VEP(70) | |
| 1976 | VEP(74) | | VEP | | VEP | | VEP | VEP | | |
| 1980 | VEP(82) | | VEP | VEP | VEP | | VEP | VEP | | |
| 1984 | VEP | VEP(83) | VEP | VEP | VEP | VEP | VEP | VEP | VEP | VEP |
| 1988 | ICPSR | | VEP | ICPSR | VEP | | ICPSR | VEP | | |
| *White voter registration* | | | | | | | | | | |
| 1956 | CoCR59(58) | CoCR59(58) | CoCR59(58) | VEP | VEP | | VEP(58) | VEP(58) | | CoCR59(58) |
| 1960 | VEP | VEP(59) | CoCR61 | VEP | CoCR61 | | VEP | VEP(62) | CoCR61 | CoCR61 |
| 1964 | VEP | VEP(63) | VEP | VEP | VEP | VEP | | VEP | VEP(62) | VEP |
| 1968 | VEP | | VEP | VEP | VEP | VEP | VEP | VEP | VEP(66) | |
| 1972 | VEP | | VEP | VEP(73) | VEP | VEP | VEP | ICPSR | VEP(70) | |
| 1976 | VEP(74) | | VEP | | VEP | | VEP | VEP | | |
| 1980 | VEP(82) | | VEP | VEP | VEP | | VEP | VEP | | |
| 1984 | VEP | VEP(83) | VEP | VEP | VEP | VEP | VEP | VEP | VEP | VEP |
| 1988 | ICPSR | | VEP | ICPSR | VEP | | ICPSR | VEP | | |

The following abbreviations are used. AL: Alabama; AK: Arkansas; FL: Florida; GA: Georgia; LA: Louisiana; MS: Mississippi; NC: North Carolina; SC: South Carolina; TN: Tennessee; VA: Virginia. Moreover, VEP stands for the Southern Regional Council's Voter Education Project; ICPSR stands for the Inter-university Consortium for Political and Social Research (1992); CoCR (59 and 61) stands for the United States Commission on Civil Rights (1959 and 1961). If the information for a given year was not available, a neighboring year was considered, and this is shown in parenthesis.

**Table D.1.3:** The dataset on total voter registration and party support by race

| | AL | AK | FL | GA | LA | MS | NC | SC | TN | VA |
|---|---|---|---|---|---|---|---|---|---|---|
| *Total voter registration* | | | | | | | | | | |
| 1956 | CoCR59(58) | CoCR59(58) | CoCR59(58) | VEP | CoCR59 | | VEP(58) | VEP(58) | | CoCR59(58) |
| 1960 | VEP | VEP | CoCR61 | VEP | CoCR61 | | VEP | VEP | CoCR61 | CoCR61 |
| 1964 | VEP | VEP | VEP | VEP | VEP | VEP | | VEP | VEP | VEP |
| 1968 | VEP | | VEP | VEP | VEP | VEP | VEP | VEP | VEP | |
| 1972 | VEP | | VEP | VEP | VEP | VEP | VEP | ICPSR | VEP | |
| 1976 | ICPSR | | VEP | | VEP | | VEP | VEP | | |
| 1980 | ICPSR | VEP | VEP | VEP | VEP | | VEP | VEP | | |
| 1984 | VEP | VEP | VEP | VEP | VEP | VEP | VEP | VEP | VEP | VEP |
| 1988 | ICPSR | | VEP | ICPSR | VEP | | ICPSR | VEP | | |
| *Party support by race* | | | | | | | | | | |
| 1956 | | | | | | | | | | |
| 1960 | | | VEP | | TTP | | | | | |
| 1964 | | | VEP | | VEP(63) | | | | | |
| 1968 | | | VEP | | VEP | | | | | |
| 1972 | | | VEP | | VEP | | | | | |
| 1976 | | | VEP | | VEP | | | | | |
| 1980 | | | VEP | | VEP | | | | | |
| 1984 | | | VEP | | VEP | | | | | |
| 1988 | | | VEP | | VEP | | | | | |

The following abbreviations are used. AL: Alabama; AK: Arkansas; FL: Florida; GA: Georgia; LA: Louisiana; MS: Mississippi; NC: North Carolina; SC: South Carolina; TN: Tennessee; VA: Virginia. Moreover, VEP stands for the Southern Regional Council's Voter Education Project; ICPSR stands for the Inter-university Consortium for Political and Social Research (1992); CoCR (59 and 61) stands for the United States Commission on Civil Rights (1959 and 1961); TTP stands for the newspaper "The Times-Picayune" of New Orleans, Louisiana, published on October 30, 1960. If the information for a given year was not available, a neighboring year was considered, and this is shown in parenthesis.

## D.2 Pre-VRA parallel trends

The identifying assumption of Chapter 4 is that, absent the VRA, both treatment and control groups would have experienced the same trends in the outcomes of political participation (voter registration rates, turnout rates, and party support) in all the post-VRA years. If this assumption holds, the behavior of the control group could be considered as the appropriate counterfactual of what would have happened to the treatment group had it not received coverage from the VRA. Any remaining statistically significant difference that is observed following the enactment of the VRA, could then be attributed to the VRA alone.

Statistically, the Parallel Trend Assumption is tested by estimating the following model for all the available pre-VRA years:

$$\begin{aligned} PoliticalParticipation_{c,s,t} &= \sum_{n>1956} \gamma_n D_n^t TimeTrend_t + \\ &+ \sum_{n>1956} \theta_n D_n^t \, TimeTrend_t \times VRA_{c,s} + \\ &+ \mathbf{X'}_{c,s}\beta + I_{s,t} + I_{c,t} + \epsilon_{c,s} \end{aligned} \tag{D.1}$$

where $PoliticalParticipation_{c,s,t}$ is one of the political outcomes considered in Chapter 4, $TimeTrend_t$ is a linear time trend equal to 1 for 1956, 2 for 1960, and 3 for 1964, $VRA_{c,s}$ is an indicator taking a value of one if the county is covered from 1965 onward, $D_n^t$ is an indicator taking a value of one if $n = t$, $\mathbf{X'}_{c,s}$ is the vector of controls, $I_{s,t}$ and $I_{c,t}$ are state-year and county-year interactions, respectively, with $\epsilon_{c,s}$ being the idiosyncratic error term.[1]

In Table D.2.1, the estimated coefficients $\theta_n$ from Eq. D.1 are presented. The requirements stated in the Parallel Trend Assumption are satisfied for all the dependent variables. That is, the estimated coefficients are statistically indistinguishable from zero, meaning that the treatment and the control groups were indeed on parallel trends before the VRA. This more formally confirms what

[1] As everywhere else in the chapter, standard errors are clustered by county and all observations are weighted by the 1960 county population. In the last two columns of Table D.2.1, where only Louisiana and Florida are considered, with information on only two time periods pre-VRA, the time trend gives 1 to 1960 and 2 to 1964.

was already stated in Chapter 4. In fact, the choice of control counties was already shown to be a suitable match across all the specifications, as the difference in the gradients between covered and non-covered counties was indeed indistinguishable from zero in all the pre-VRA years.

In order to move toward causality, it is reassuring to observe that the Parallel Trend Assumption holds. This is especially important as many of the dependent variables could be seen as slow-moving processes that might not have deviated much following the VRA and that could instead be driven by more secular factors, rather than a one-off episode. For instance, the support for the Democratic Party, or the voter registration rates of whites could all have been changing before the VRA. Then, the results of this section provide a strong justification in support of the adequacy of the empirical analysis.

**Table D.2.1:** Pre-VRA trends: The parallel trend assumption

| | Black voter reg. (1) | White voter reg. (2) | Total voter reg. (3) | Gap bla-whi vot. reg. (4) | Turnout rates (5) | Dem vote share (6) |
|---|---|---|---|---|---|---|
| Time Trends X VRA | –3.549 | 2.501 | 1.010 | –6.050 | –1.198 | –3.900 |
| | (5.865) | (5.092) | (4.796) | (7.685) | (1.562) | (3.264) |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Time Trends | Yes | Yes | Yes | Yes | Yes | Yes |
| County Trends X Year Trends | Yes | Yes | Yes | Yes | Yes | Yes |
| State Trends X Year Trends | Yes | Yes | Yes | Yes | Yes | Yes |
| Adj. R-Square | 0.83 | 0.89 | 0.90 | 0.82 | 0.93 | 0.87 |
| N | 1311 | 1311 | 1311 | 1311 | 1311 | 1311 |

Robust standard errors in parenthesis are adjusted for clustering at the county-level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

# E

# Data sources and variables construction

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## E.1 Race riots of the 1960s

*Likelihood of riots*: the share of all the riots that occurred in a county, standardized by the total number of riots in all southern counties. When considering the period 1964-1971, the denominator refers to the total number of riots that occurred in the U.S. South during 1964-1971. In this case, the dataset is based on the information collected by Carter (1986). When considering the longer period 1960-1971, the denominator refers to the total number of riots that occurred in the U.S. South during 1960-1971. In this longer period, the dataset is based on the information collected by Carter (1986) for 1964-1971 and by Olzak (2015) for 1960-1963.
*Intensity of riots*: it is constructed as the Severity Index proposed by Collins and Margo (2004) and Collins and Margo (2007). When considering the period 1964-1971, the five indicators of severity that are used to construct this index are: the length of the riot, the number of deaths, injuries, arrests, and the episodes of arson. When considering the longer period 1960-1971, the indicator measuring the episodes of arson is omitted and the remaining four categories are the ones included in the index. This is to ensure consistency between the datasets collected by Carter (1986) and Olzak (2015).

## E.2 Black-appeal radio stations

*Black-appeal radio stations in 1964*: it includes the list of 200 radio stations that aired a minimum of 12 hours of Black-appeal programming a week. The dataset is based on the information collected by Sponsor (1964). In case of missing information, other sources used are: i) previous publications of Sponsor; ii) the 1964 edition of the Broadcasting Yearbook, a yearly directory published by the magazine Broadcasting; and iii) the 1964 edition of the North American Radio-TV Station Guide.
*Black-appeal radio stations in 1968*: it includes the list of 157 radio stations that aired a minimum of 12 hours of Black-appeal programming a week. It has been obtained from Spot Radio Rates and Data (1968).

## E.3 African American elected officials

*Share of African American elected officials by type of office*: number of African American elected officials in county governing bodies, judiciary and law enforcement, other administrative bodies, municipalities, and school boards, as reported by the National Roster of Black Elected Officials (NRBEO), divided by the total number of elected officials for the corresponding offices at the county-level, as reported by the Census of Governments. The number of African American elected officials by type of office is the count of African Americans in office as reported in the NRBEO published in 1969, 1971, 1973-1981, 1984-1988, and 1990. Each elected official has been matched to the corresponding county using the address provided by the NRBEO. Furthermore, using information from the documents published by the Southern Regional Council and from local newspapers, information has been collected on the number of African American elected officials by type of office at the county-level also for the years 1962 and 1964. When the numerator is zero and the denominator is missing, the share is zero.

The information on the total number of elected officials by type of office that is used to construct the share of African American elected officials is available from the Census of Governments of 1957, 1967, 1977, and 1987. More specifically, the total number of elected officials that is used to construct the shares for the years 1962 and 1964 has been obtained from the 1957 Census of Governments, Volume 2, Governmental Employment, Number 2, Compendium of Public Employment. For the period 1969-1973, the total number of elected officials has been obtained from the 1967 Census of Governments, Volume 6, Topical Studies, Number 1, Popularly Elected Officials of State and Local Governments. For the period 1974-1981, the total number of elected officials has been obtained from the 1977 Census of Governments, Volume 1, Governmental Organization, Number 2, Popularly Elected Officials. Lastly, for the period 1984-1990, the total number of elected officials has been obtained from the 1987 Census of Governments, Volume 1, Government Organization, Number 2, Popularly Elected Officials.

The offices considered in Chapter 3 are those included in the: i) county governing bodies, ii) judiciary, iii) law enforcement, and iv) other administrative bodies. Further details on the offices included in these categories is presented below.

*County governing bodies*: the elected officials included in this group are those responsible for administering local county governments. The precise title that these officers take varies among states. In Louisiana, they are called police jurors; in Mississippi and Virginia, they are known as supervisors. In Alabama, Florida, Georgia, North Carolina, South Carolina, Tennessee, and Texas, they are called commissioners. In Arkansas, the governing body in each county is instead formed by the quorum court, which includes the county judge and all the justices of the peace elected in that county.

*Judicial bodies*: in this category, the following officials have been included.

Judges: the focus is only on those judges that belong to a court whose jurisdiction does not exceed the boundaries of a county, excluding all those judges working for a court that decides on legal matters spanning more than one county. This makes this category comparable to all the other county-level offices included in the chapter. Although some variation in jurisdictions exists between different states for the same type of court (with this variation taken into consideration in the counts of elected officials), the following courts have been considered in the analysis: city and municipal courts, traffic courts, family courts, juvenile courts, probate courts, general sessions courts, and county courts. On the other hand, the elected judges working for the following courts have been excluded from the counts (again, with some variation between states): criminal courts, chancery courts, circuit courts, district courts, courts of appeal, superior courts, supreme courts, state courts, and federal courts.

Magistrates and justices of the peace: this category includes all the other locally-elected officials holding judicial powers. Although the exact classification of these powers varies between states and sometimes also between counties in a given state, these offices tend to have reduced jurisdictions compared to county judges, and often involve administering minor civil and criminal justice.

Other judicial officials: this category includes all remaining officials working for or belonging to the judicial system. These elected offices generally include the clerk of courts and lower-level attorneys.

*Law enforcement bodies*: in this category, the following officials have been included. Constables: although much variation exists between different states in terms of jurisdiction and duties, all constables included in this study are elected peace officers with a varying degree of police powers. Their duties range from serving the judicial courts to more police-like tasks such as conducting patrols, arrests, and stops and searches.

Police chiefs, sheriffs, and marshals: this category includes all other locally-elected law enforcement officials.

*Other administrative bodies*: this category includes all other locally-elected officials that perform any remaining county-level administrative task: tax assessor, tax collector, coroner, prosecutor, clerk, register of deeds, treasurer, and commissioner of revenue.

## E.4 Voter registration rates

In order to construct the voter registration rates, the total number of county-level registered voters, separated by race, is divided by the appropriate county-level voting age population, separated by race. The county-level voting age population has been obtained from Manson et al. (2019). In following the applicable laws, age 21+ is considered for 1970 and prior years and age 18+ for 1980 and later years, obtaining voting age statistics for every decennial census year from 1950 until 1990. Then, to obtain information for the intercensal years, linear interpolations are considered.[1]

For the census years of 1950 and 1960, the available county-level voting age population is split into three categories: total, whites, and nonwhites. It is only from 1970 and later years that the census provides a further disaggregation of nonwhites into African Americans and other categories. The main concern is that, if the

[1]For the 9 years in between each census, the weights used are: (0.9/0.1); (0.8/0.2); (0.7/0.3); ... (0.2/0.8); (0.1/0.9).

numerator did indeed distinguish between African Americans and other categories, the correct voter registration rates of African Americans before 1960 would be under-estimated due to a larger denominator. However, it was often the case that the numerator also did not provide such a detailed breakdown, presenting figures on total, white, and nonwhite voter registrations. A second concern is that, even if the numerator and denominator were identifying similar statistics, nonwhite voter registration rates pre-1960 would still be compared with African American voter registration rates post-1970. However, during the 1950s and 1960s, other races were a small part of the population, and this error should therefore be limited.[2] For instance, during 1969, there were 200,820 African Americans registered to vote in South Carolina, with only 137 other races, or 0.1% of the total nonwhite voter registration statistics. In Florida, a state not subject to the VRA, by the end of the sample in 1988, there were 513,326 African Americans registered to vote and only 9,296 other nonwhites were registered, or 1.8% of the total nonwhite population.[3]

Lastly, whenever the voter registration rates exceeded 100% (due to, for instance, an inexact denominator – which can only be derived from a linear interpolation of the intercensal years), these rates are capped to 100%. All the results of Chapter 4 continue to hold, although with a slightly modified magnitude, if these ratios are left uncapped. Finally, whenever information on voter registration was found at a higher frequency than yearly (e.g., every quarter or every semester), the mean for the year was considered before constructing the rates.

[2]That this error ought to be small in magnitude could also be justified by the fact that the share of the nonwhite voting age population in 1950 or 1960 is relatively close to the share of the African American population in 1960 (the main variable used in Chapter 4). The average deviation of the 1950 and 1960 nonwhite voting age population share to the 1960 African American population share is small in magnitude, around -0.94% and -2.78%, respectively.

[3]The documents used to construct the voter registration in Florida had information on total registration, and on the affiliation with the Democratic Party and the Republican Party, separated by race. However, besides the affiliation of whites and African Americans, a single category for any other party affiliation was also given, although without a distinction by race. Then, to ensure that the sum of white and African American voter registration equaled total voter registration, this additional category was split and assigned to whites and African Americans, by looking at the proportion of whites and African Americans for which county-level information existed on the affiliation with the Democratic Party and the Republican Party. This adjustments has been applied to the years: 1960, 1965-1972, 1974, 1976, 1978, 1980, 1982, 1984, 1986, and 1988.

## E.5 Political participation

*Total turnout rates*: information on total turnout statistics and on the total vote share for the Democratic Party has been obtained from the Inter-university Consortium for Political and Social Research (2013). Similarly to the construction of voter registration rates, turnout statistics are considered as rates, dividing them by the appropriate voting age population. As information on the votes cast could only be obtained for the total population (without a disaggregation by race), the total voting age population statistics has been considered (with a linear interpolation for the intercensal years).

*Share of votes cast for the Democratic Party*: to construct the variable for the total Democratic Party support, the total votes cast for the Democratic candidate in each presidential election are divided by the total votes cast in the corresponding presidential election. This gives the share of the votes in favor of the Democratic Party.

*Support for the Democratic Party*: consistent information on party affiliation with the Democratic Party and the Republican Party, by race and at the county-level, has been collected for two states: Florida and Louisiana. As detailed in Table D.1.3, these statistics are based on the information found in the VEP's archives, from documents originally computed by the Secretary of State of each state.[4] Besides showing voter registration, these documents also provided information on the affiliation of voters with the Democratic Party and the Republican Party. In the analysis, the outcome variable of Democratic Party support by African American voters has been constructed by dividing the number of African American voters affiliated with the Democratic Party by the total African American voters that were registered to vote. The white voters' support was similarly computed.

[4]For Louisiana in 1960, the information used was found in the newspaper "The Times-Picayune", New Orleans, Louisiana, published on October 30, 1960, and reported in the "Louisiana Almanac and Fact Book, 1962." It had the same structure as any other official publication obtained from the VEP.

## E.6 County characteristics

*VRA coverage, 1965*: an indicator equal to one for the counties covered by the VRA, and equal to zero otherwise. The counties of six states (Alabama, Georgia, Louisiana, Mississippi, South Carolina, and Virginia) were fully covered. Arkansas, Florida, Tennessee, and Texas were not covered. Of the 100 counties in North Carolina, 39 counties were subject to the VRA.[5]

*Share of the African American population, 1960*: the share of the African American population in the total population in 1960, at the county-level, has been obtained from the Inter-university Consortium for Political and Social Research (2012).

*Total population, 1960*: the total population in 1960, at the county-level, has been obtained from the Inter-university Consortium for Political and Social Research (2012).

*Population density, 1960*: the population density in 1960, at the county-level, has been obtained from the Inter-university Consortium for Political and Social Research (2012).

*Share of the urban population, 1960*: the share of the urban population in 1960, at the county-level, has been obtained from the Inter-university Consortium for Political and Social Research (2012).

*Size of manufacturing, 1960*: the share of the manufacturing sector in the total production system in 1960, at the county-level, has been obtained from the Inter-university Consortium for Political and Social Research (2012).

*Size of agriculture, 1960*: the share of the agricultural sector in the total production system in 1960, at the county-level, has been obtained from the Inter-university Consortium for Political and Social Research (2012).

*Size of mining, 1960*: the share of the mining sector in the total production system in 1960, at the county-level, has been obtained from the Inter-university Consortium

[5]Covered counties: Anson, Beaufort, Bertie, Bladen, Camden, Caswell, Chowan, Cleveland, Craven, Cumberland, Edgecombe, Franklin, Gaston, Gates, Granville, Greene, Guilford, Halifax, Harnett, Hertford, Hoke, Jackson, Lee, Martin, Nash, Northampton, Onslow, Pasquotank, Perquimans, Person, Pitt, Robeson, Rockingham, Scotland, Union, Vance, Washington, Wayne, and Wilson.

for Political and Social Research (2012).

*Unemployment rate, 1960*: the unemployment rate in 1960, at the county-level, has been obtained from the Inter-university Consortium for Political and Social Research (2012).

*Unskilled workers, 1960*: the share of workers with at least 25 years without a high school diploma in 1960, at the county-level, has been obtained from the Inter-university Consortium for Political and Social Research (2012).

*Households below the poverty line, 1960*: the share of households with an income less than 3,000 USD in 1960, at the county-level, has been obtained from the Inter-university Consortium for Political and Social Research (2012).

*Median family income, 1960*: the median income per household in 1960, at the county-level, using 1960 USD, has been obtained from the Inter-university Consortium for Political and Social Research (2012).

*Birth rate, 1960*: the birth rate in 1960, at the county-level, has been obtained from the Inter-university Consortium for Political and Social Research (2012).

*Members of the households, 1960*: the average number of members in each household in 1960, at the county-level, has been obtained from the Inter-university Consortium for Political and Social Research (2012).

*Size of farms, 1959*: the share of the land used for farming in 1959, at the county-level, has been obtained from the Inter-university Consortium for Political and Social Research (2012).

*Share of rural farms, 1960*: the share of rural farms in 1960, at the county-level, has been obtained from the Inter-university Consortium for Political and Social Research (2012).

*Nonwhite households, 1950*: the share of households that are nonwhite in 1950, at the county-level, has been obtained from the Inter-university Consortium for Political and Social Research (2012).

*Farms with electricity, 1954*: the share of farms with electricity in 1954, at the county-level, has been obtained from the Inter-university Consortium for Political and Social Research (2012).

E.6. County characteristics

*Households with televisions, 1960*: the share of households with at least one television set in 1960, at the county-level, has been obtained from the Inter-university Consortium for Political and Social Research (2012).

*Households with two or more cars, 1960*: the share of households with at least two cars in 1960, at the county-level, has been obtained from the Inter-university Consortium for Political and Social Research (2012).

*Population above 65 years, 1960*: the share of the population with at least 65 years in 1960, at the county-level, has been obtained from the Inter-university Consortium for Political and Social Research (2012).

*Expenditure on highways, 1962*: the expenditure per capita on highways by the local government in 1962, at the county-level, has been obtained from the Inter-university Consortium for Political and Social Research (2012).

*Agricultural productivity*: the maximum potential cotton yield, expressed as a cotton suitability index, at the county-level, has been obtained from Hornbeck and Naidu (2014).

*Vote for the Democratic president, 1960*: the share of votes cast for the Democratic president in the 1960 presidential election, at the county-level, has been obtained from the Inter-university Consortium for Political and Social Research (2012).

*Median school years, 1960*: the median years of schooling of the population with at least 25 years in 1960, at the county-level, has been obtained from the Inter-university Consortium for Political and Social Research (2012).

*School enrollment, 1950*: the share of the population between 14 and 17 years of age that is enrolled in schooling in 1950, at the county-level, has been obtained from the Inter-university Consortium for Political and Social Research (2012).

*College-level educational attainment*: the share of African Americans with at least one year of college-level education in each county in 1960, divided by the total count of African Americans in the corresponding county in 1960, has been obtained from Table 87, General Social and Economic Characteristics, Census of Population 1960. From the Census, information is reported only for those "counties with 1,000 or more nonwhite persons."

*African American church, 1936*: the membership with African American churches in 1936 has been obtained from the Census of Religious Bodies (1906-1936).
*County area*: the total area of the county, in squared miles, has been obtained from the Inter-university Consortium for Political and Social Research (2012).
*Length of rivers*: the total length of the county that is covered by rivers, in miles. The raster file has been obtained from Natural Earth, and ArcGIS has been used to obtain this statistics.
*Area of lakes*: the total area of the county that is covered by lakes, in squared miles. The raster file has been obtained from Natural Earth, and ArcGIS has been used to obtain this statistics.
*National Parks*: an indicator equal to one if there is at least one National Park in the county. The raster file has been obtained from Natural Earth, and ArcGIS has been used to obtain this statistics.
*Mountains above 800m*: the height of all the mountains above 800m, in meters. The raster file has been obtained from Natural Earth, and ArcGIS has been used to obtain this statistics.
*Coastal county*: an indicator equal to one for the set of counties along the coast has been obtained from the U.S. Census.

## E.7 Pre-VRA measures of conflict

*Lynching of African Americans, 1902-1964*: episodes of African American lynching that occurred in each county from 1902 until 1964, standardized by the African American population in the county in 1960. Information on each episode of lynching has been obtained from the project Monroe Work Today. It provides information on the name, race, sex, location, alleged crime, and original source. The choice to start the counts from 1902 is driven by two considerations: i) in 1901, anti-lynching measures were presented to Congress for the first time (since then, about 240 anti-lynching bills have been proposed); ii) in May 1902, during a speech at Arlington, Virginia, President Theodore Roosevelt openly condemned the act of lynching: this was the first time that a President ever made a statement against it.

*Pro-African American activism, 1960-1964*: the counts of pro-African American events that occurred between 1960 and 1964, as reported by the Dynamics of Collective Action Dataset by States and Cities.

*Anti-African American activism, 1960-1964*: the counts of anti-African American events that occurred between 1960 and 1964, as reported by the Dynamics of Collective Action Dataset by States and Cities.

*NAACP branches, 1957-1964*: the average number of local branches of the National Association for the Advancement of Colored People (NAACP) active between 1957 and 1964. Information on the location of local branches of the NAACP has been obtained from the University of Washington's project Mapping American Social Movements Through the $20^{th}$ Century.

*KKK Klaverns, 1925-1940*: the average number of Ku Klux Klan groups, known as Klaverns, active between 1925 and 1940. Information on the location of each Klavern has been obtained from the Virginia Commonwealth University's project Mapping the Second Ku Klux Klan.

## E.8 Electoral rules

Information on the system of elections of county governing bodies has been obtained from the 1957 Census of Governments (Elective Offices of State and Local Governments) and from the 1980 NRBEO. Indicators for the system of elections have been constructed as detailed below.

*Single-member districts (SMD)*: an indicator equal to one for covered states where members of county governing bodies are elected by SMD (Louisiana, Mississippi, and Virginia) and zero otherwise.

*Mixed*: an indicator equal to one for covered states where members of county governing bodies are elected with a combination of SMD and at-large systems (Alabama, North Carolina, and South Carolina) and zero otherwise.

*At-large*: an indicator equal to one for covered states where members of county governing bodies are elected at-large (Georgia) and zero otherwise.

*Switch*: an indicator equal to one for covered states that, by 1980, had transitioned

toward SMD systems. To code a state as a switcher, information on the legal challenges to the elections at-large of county governing bodies, as reported by Davidson and Grofman (1994), has also been used.

## E.9 Local public finances

### E.9.1 Expenditure

*Real 2000 USD county government expenditure per capita*: expenditure figures (total, current, and capital) have been digitized from the Census of Government, Finances of County Governments, for 1957, 1962, 1967, 1972, 1977, and 1982. Data on county government expenditure relate only to county governments and their dependent agencies, and do not include other local governments within or between county areas. Real per capita figures have been obtained dividing the nominal figures by the county population and converting them in 2000 USD using the CPI index. *Capital expenditure* consists of direct expenditure for contracts in the construction of buildings, roads, other infrastructure, for the purchase of equipment, land, existing structures, and for payments on capital leases. It includes amounts for additions, replacements, and major alterations to fixed works and structures. *Current expenditure* consists of all expenditure with the exception of capital outlays. It includes assistance and subsidies, interest on debt, insurance benefits and repayments, and current operations. The latter consists of direct expenditure for compensation of own officers and employees, and for supplies, materials, operating leases, and contractual services except amounts for capital outlays. Expenditure on public welfare belongs to current expenditure and consists of payments for support of and assistance to people contingent upon their needs.
*Real 2000 USD expenditure per capita for local governments other than county governments*: expenditure figures (total, current, and capital) for local governments other than county governments have been obtained by subtracting county government expenditure from county areas expenditure. The latter consists of all county-level expenditure by local governments (counties, municipalities, townships, special districts, and independent school districts) and their dependent agencies,

and does not include amounts spent directly by the state and federal governments. County areas expenditure has been obtained from the Census of Government Historical Database available at five-year intervals starting in 1957. Real per capita figures have been obtained dividing the nominal figures by the county population and converting them in 2000 USD using the CPI index.

### E.9.2 Revenue

*Real 2000 USD county government revenue per capita*: own revenue and intergovernmental revenue have been obtained from the Census of Government Historical Database available at five-year intervals starting in 1957. For own revenue, the variable on total revenue own sources is reported in the County Government Finances section, Expenditures, Revenues. Intergovernmental revenue is the sum of total state intergovernmental revenue and total federal revenue reported in the County Government Finances section, Expenditures, Revenues. Real per capita figures have been obtained dividing the nominal figures by the county population and converting them in 2000 USD using the CPI index.

## E.10 Instrumental variables

*HBCUs enrollment, 1976*: an indicator for low, medium, or high enrollment in historically Black colleges and universities (HBCUs). The list of HBCUs active in 1960 has been obtained from HBCU Lifestyle. The enrollment statistics refers to 1976 and it is available from the U.S. Department of Education, National Center for Education Statistics (2004).

*Ground conductivity*: a measure of ground conductivity at the county-seat, available from the Federal Communications Commission, Media Bureau, Audio Division, M3 Map of Effective Ground Conductivity in the U.S.A. I thank David Strömberg for sharing the data.

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