Political Voice and (Mortgage) Market Participation: Evidence from Minority Disenfranchisement*

Seongjin Park[†] Arkodipta Sarkar[‡] Nishant Vats[§]

August 4, 2021

Abstract

This paper documents the link between political voice and economic decision-making. Combining the repeal of Section 5 of the Voting Rights Act as a shock to the enfranchisement of black Americans with granular data on the US mortgage market, we document a 14% decline in mortgage origination for black Americans. This is driven by their self-selection out of the mortgage market rather than a change in denial rate. Additionally, we observe a flight of black demand to black lenders, indicating an increase in racial homophily. Our results indicate that disenfranchisement reduces demand directly by increasing uncertainty, violence, and borrowing constraints and indirectly by increasing the threat of rejection.

JEL Codes: D72, G21, G28, G51, J15

*We thank Sumit Agarwal, Utpal Bhattacharya, Emilio Bisetti, Kristle Romero Cortés, Theresa Kuchler, Alan Kwan, Filippo Mezzanotti, Abhiroop Mukherjee, Deniz Okat, José-Luis Peydró, Matthew Ringgenberg, Rik Sen, Alp Simseck, Amir Sufi, Qifei Zhu and seminar participants at the Chicago Booth PhD Brownbag, 2021 Trans-Atlantic Doctoral Conference and Asia-Pacific Corporate Finance Online Workshop for helpful comments.

[†]Seongjin Park is at the Booth School of Business, University of Chicago. e-Mail: spark160@chicagobooth.edu

[‡]Arkodipta Sarkar is at the Hong Kong University of Science and Technology. e-Mail: asarkar@ust.hk

[§]Nishant Vats (corresponding author) is at the Booth School of Business, University of Chicago. Send correspondence to 5807 S Woodlwan Ave, Chicago, IL, USA. e-Mail: nvats@chicagobooth.edu

1 Introduction

"*So long as I do not firmly and irrevocably possess the right to vote I do not possess myself. I cannot make up my mind — it is made up for me. I cannot live as a democratic citizen, observing the laws I have helped to enact — I can only submit to the edict of others.*"

-Dr. Martin Luther King Jr.

The power of individuals to affect election outcomes is the gateway to advancement in all aspects of life (Button (2014)). By exercising their options, the electorate can vote out of office any politician that provides barriers to the provision of basic necessities like housing, safety and jobs. Also, the right to vote empowers individuals with political voice that allows them to draw greater public good toward themselves. Consequently, any change in the degree of enfranchisement can impact an agent's decision making by altering their physical and economic environment. While existing studies have highlighted the role of enfranchisement on public good provision, microeconomic evidences on the relationship between political voice and individual decision making is limited – Do economic agents respond to changes in voting right through their economic and financial decisions? Can exclusion in the voting process result in exclusion from markets? To what extent can difference in voting rights across groups lead to inequality? What are the mechanisms through which exclusion from the voting process can increase inequality?

In this paper we study the link between electoral disenfranchisement and economic decision making, in a setting that allows us to examine the the channels through which disenfranchisement can widen existing economic cleavages. Specifically we study the impact of the dilution of Voting Rights Act (VRA), that eroded the political voice of black Americans, on their participation in the mortgage market and the consequent racial disparity in home ownership in the United States. Home purchases is a natural set up for the empirical investigation as houses are the largest asset owned by most households and thereby one of the most important economic choice made by a household over its lifetime (Chetty and Szeidl (2007), Chetty, Sándor and Szeidl (2017)). Home mortgages are an integral part of home purchases.[1] Therefore, we focus on mortgage market outcomes as a

[1]A large number of homes are purchased through mortgage borrowing. The 2021 Statistics Research Department report - *"Number of new home sales in the U.S. 2000-2020, by financing type"* states that two in three house purchases between 2000 and 2020 were financed through a conventional mortgage <LINK>. Redfin analysis of home purchases indicates that an average of 25% of homes were purchased using all cash between 2001 and 2021 <LINK>. The 2014 survey of potential home-buyers by loanDepot finds that 71% of all Americans who want to buy a home will need financing <LINK>.

setup to identify the effect of disenfranchisement on exclusion from markets. This follows Charles and Hurst (2002) who suggest that the racial gap in home ownership can potentially be explained by differences in mortgage application propensities.

Theoretically, disenfranchisement can affect both the participation and access to mortgage markets. Disenfranchisement shifts the median voter in the economy and alters the electoral incentives of politicians. This can result in lower state protection from discrimination and hate crimes, greater migration, lower income, and higher future uncertainty for the disenfranchised groups. Such instances can tighten the borrowing constraints and lead to lower demand for mortgages and illiquid assets such as homes (Campbell and Cocco (2003)). Alternatively, these forces can result in contraction of supply by banks. Disentangling these forces is crucial to understanding the underlying mechanisms through which exclusion from the voting process can increase inequality in mortgage markets. The granularity of the data in mortgage markets allows us to examine these underlying channels and disentangle the demand and the supply side forces through which disenfranchisement can reduce mortgage market participation.

We begin our analysis by emphasizing three stylized facts. First, there exists a racial homeownership gap of 30% between black and white Americans. Second, we document a positive relationship between cost of voting index (COVI) and the racial homeownership gap across states. We find that COVI, on average, can explain 20% of variation in the racial homeownership gap. The within-state variation in racial homeownership gap explained by COVI increases with the racial tension in the state. Third, black homeowners are relatively more reliant on debt compared to white homeowners. Black Americans use debt to finance 94% of their home value while white Americans finance 87% of their home value through debt. The key takeaways from these facts are that the racial homeownership gap is large, there is positive association between racial homeownership gap and cost of voting, and black Americans are more likely to finance a higher value of their home with debt.

These facts motivate economic relevance of the question and the direction of inquiry in this paper. In particular, the second fact establishes a positive relationship between cost of voting and racial homeownership gap. While this fact is informative, a direct test to identify the effect of changes in enfranchisement requires an exogenous variation in the cost of voting. We use the repeal of the Section 5 of VRA as a natural experiment to study the effect of erosion in voting rights

of black Americans on the racial homeownership gap. Since mortgage lending is imperative in determining homeownership, we exploit the richness of information available in mortgage markets to empirically investigate the role of voting rights in causing racial economic disparity. The mortgage market data allows us to track an application from origination until the final decision allowing us to disentangle supply and demand side factors of borrowers and lenders through which changes in cost of voting can affect the participation and access to mortgage markets.

VRA was signed into law on the 6th of August 1965 and outlawed many discriminatory practices in the United States that prevented minorities in general and black Americans in particular from participating in the electoral process. Section 2 of VRA eliminated all voting restrictions that denied the right to vote on account of race. Section 5 of the VRA empowered federal authorities with oversight powers to protect minorities' right to vote by requiring compulsory preclearance of all changes in voting laws in selected jurisdictions. Section 5 is widely considered to be the heart of the VRA as it shifted the burden of proof from voters to the election officials. However, the US Supreme Court struck down the Section 5 of VRA in 2013 in the case of *Shelby County v Holder* with a 5-4 majority.[2]

This judgement stripped VRA of its most powerful provision and struck a blow to the political voice of black Americans. This is evident by Justice Ginsberg's dissenting judgement highlighting that "[t]hrowing out preclearance when it has worked and is continuing to work to stop discriminatory changes is like throwing away your umbrella in a rainstorm because you are not getting wet." Removal of preclearance requirement was immediately met with enactment of controversial voter laws in jurisdictions previously covered by Section 5 of VRA (Ang (2019)). For example within 24 hours of the ruling, Texas announced and passed strict photo identification law that had previously failed the preclearance under Section 5. Other states such as Georgia and North Carolina also implemented several discriminatory voter laws and purged several black Americans from its voter list. We establish the quantitative relevance of the dilution VRA as a potential shock to political voice of black Americans. We show that voter turnout during presidential elections declined sharply in counties with prior protection under the VRA and a greater share of black population, while there was no change in voter turnout for protected counties with low share of black population. This result, following the seminal work Tingsten (1937), reflects a decline in

[2]We use the repeal of the Section 5 of VRA and the Shelby ruling interchangeably.

political voice of black Americans after the repeal of the Section 5 of VRA.

We combine the spatial information on jurisdictions originally covered under Section 5 of VRA with detailed Home Mortgage Disclosure Act (HMDA) dataset to identify the relationship between changes in cost of voting and mortgage market outcomes. Two features of our data allow for credible identification. First, the data provides information on borrowers' race which is imperative in examining racial differences in mortgage origination. Second, detailed information on property location allows us to focus on contiguous counties separated only by jurisdictional borders in cases where one county was covered by Section 5 of VRA and the other was not. The is important because a direct comparison of all covered and uncovered counties is likely to contaminate the estimation either due to selection bias or the presence of unobserved confounding variables. The identifying assumption of using this sub-sample of covered and uncovered contiguous counties is that these counties are immediately adjacent neighbors and are expected to be similar in both observable, and more importantly, unobservables and likely to follow similar paths in the absence of policy changes.

The empirical strategy of the paper estimates a triple differences specification which exploits three dimensions of heterogeneity - spatial differences in the coverage of Section 5 across adjacent counties, the difference in the impact of the repeal of Section 5 across races within a county, and time series variation before and after the repeal of Section 5. Finally the information on loan application along with the acceptance and denial tags, aids in our attempt to disentangle supply and demand side forces.

Using the triple differences specification in a sample of bordering counties, we find that the amount (number) of total mortgage origination fell by 14.3% (13.2%) for black borrowers in treated counties following the Shelby ruling. The inclusion of county $\times$ year, race $\times$ year and county $\times$ race fixed effects in the empirical specification indicates that the results are not driven by time-varying shocks to the supply and demand in mortgage market within a county, aggregate time-varying shocks to a race, or time invariant status of a race within a county. We address issues of selection bias using a regression discontinuity (RD) design. All counties with voter turnout of 50% or less in the 1964 Presidential elections were subject to Section 5 of VRA. We find similar results comparing mortgage outcomes in counties within a small interval around the 1964 Presidential election voter turnout of 50%. The RD estimates indicates that the results are unlikely to be driven by the specific

sample employed in baseline estimation or driven by selection bias. Furthermore, we note that these results are not driven by pre-trends, are robust to inclusion of county-pair × race × year fixed effects, and unlikely to be driven by spillovers between treated and control counties. Moreover, these results cannot be generated in a placebo exercise indicating the results are unlikely to be spurious.

We further investigate if the decline in mortgage origination for the black Americans post the repeal of VRA is driven by reduction in demand or bank loan supply. The demand channel would imply that black Americans self-select out of the mortgage applicant pool while the supply channel would manifest through increase in mortgage loan denial for black Americans. Our results indicate that the amount (number) of total mortgage applications by black Americans fell by 16% (15%) in treated counties post the Shelby ruling. However, we do not observe any changes in the denial rate across races. These results indicate that the dilution of political voice led to a reduction in mortgage applications by black Americans and consequently their lower participation in the mortgage market. Whereas, the mortgage denial rate was unaffected due to the Shelby ruling. Broadly, this analysis indicates that disenfranchisement results in exclusion from the mortgage markets by reducing the participation of the disenfranchised group.

Next, we probe the underlying mechanism that drives the decline in mortgage applications. First, lower mortgage demand among black Americans can be driven by their migration to other areas in search of better political patronage. However, we do not find any evidence of racial differences in migration post the repeal of VRA. Second, we document an increase in the public-sector wage gap between black and white Americans following the Shelby ruling. This suggests that disenfranchisement results in a negative income shock to black Americans increasing their borrowing constraints. Third, we show that black Americans reduce their holdings in risky assets and increase their holdings of safe government securities following the Shelby ruling. Following Guiso, Jappelli and Terlizzese (1996) we interpret this shift in portfolio holding among black Americans as an indication of increased anticipation of future uncertainty and borrowing constraints. Fourth, we document an increase in hate crimes and an increase in animosity against black Americans following the Shelby ruling. This increased risk of violence, resulting in loss of life or destruction of property, can reduce the time-horizon for financial planning thereby reducing the housing demand, and consequently mortgage demand among black Americans. Fifth, we document a decline in total

expenditure by local government in treated counties with a high share of black population following the Shelby ruling. This reduction in public goods provision in predominantly black areas can also explain lower demand for mortgages.

Finally, we document a flight in demand for mortgages by black Americans to black lenders, and a consequent increase in credit supply by black lenders. This result stands in contrast to the non-black lenders where we observe a decrease in mortgage applications and originations for black Americans. There are two key takeaways from this result. First, this result highlights the role of the racial affiliation of a bank in mitigating the reduction in mortgage market participation following the disenfranchisement of black Americans. This indicates that the removal of political voice can make racial identity salient and increase homophily turning black Americans to community based institutions for insurance against socio-political shocks (Ambrus, Mobius and Szeidl (2014)). Second, this result indicates that the aggregate contraction in mortgage applications among black Americans is not purely driven by direct demand effects, but can also be a consequence of increased anticipation of mortgage application rejection by non-black lenders.

Contribution: The primary contribution of our work is to investigate the economic impact of disenfranchisement. The extant literature has studied the relationship between expansion of enfranchisement and provision of public goods and government spending.[3] More recently, Aneja and Avenancio-Leon (2019*a*) and Aneja and Avenancio-León (2019*b*) provide evidence showing that improvement in voting rights of black Americans increased government incentives to improve blacks' relative economic position through increased public employment. Broadly, these works examine the change in government relationship with the (dis)enfranchised group. In contrast, this paper documents that individuals alter their economic decision making as a response to changes in their political voice. Specifically, using mortgage markets as a setup, our results show that mortgage origination declines for black Americans post their *de facto* disenfranchisement, primarily driven by a reduction in their demand. Our work highlights that disenfranchisement reduces demand directly by increasing uncertainty, violence, and borrowing constraints and indirectly by increasing the threat of rejection. Overall, this paper shows that discrimination in the voting process can result in exclusion from the markets and the underlying channels.

[3]See Husted and Kenny (1997), Lott and Kenny (1999), Miller (2008), Moehling and Thomasson (2012), Naidu (2012), Cascio and Washington (2014), Carruthers and Wanamaker (2015), Fujiwara (2015), Debnath, Kapoor and Ravi (2017), Aidt and Jensen (2009), and, Aidt and Jensen (2013) among others.

Our work joins the literature that attempts to understand racial differences in mortgage lending. Munnell et al. (1996) document the role of discrimination in explaining the racial disparity in mortgage lending. Several works since then have highlighted the role of supply side discrimination in explaining the racial differences in mortgage originations.[4] Our results adds to this set of papers by showing that existing socio-political environment may cause black Americans to self select out of the mortgage market, thereby exacerbating the existing racial-divide. We document that reduction in political voice through disenfranchisement can reduce demand for home mortgages. Specifically, disenfranchisement reduces demand directly by increasing uncertainty, violence, and borrowing constraints and indirectly by increasing the threat of rejection. Hence, our result is closest in spirit to the hypothesized explanation for racial differences in mortgage origination presented in Charles and Hurst (2002) - *"We speculate that the portion of the gap that remains unexplained after controlling for income, demographics, and wealth may be the result of blacks anticipating a greater chance of rejection when they apply for mortgages."*

Our paper also contributes to the broad literature studying the impact of political influence on expansion in consumer credit supply and delaying foreclosure on delinquent mortgages.[5] Akey et al. (2018) find a reduction in credit supply in less-competitive political races where politicians' have lower incentives to cater to their constituents' preferences. We differ from this literature in two ways. First, we show that the primary instrument of political voice - electoral enfranchisement - can have a role in mortgage outcomes in general and participation of disenfranchised groups in particular. Second, we argue that reduction in politicians incentives to cater to the disenfranchised groups can manifest as a direct reduction in demand by increasing uncertainty, violence, and borrowing constraints and indirectly by increasing the threat of application rejection. Hence, our results highlight the role of changes in political incentives in changing the individual choice set.

We document flight of black borrowers to black banks following the attenuation of their political voice and a subsequent increase in credit supply by blank banks. This indicates that the removal of political voice can make racial identity salient and increase homophily, turning black Americans to community based institutions for insurance against such shocks. This result supports the theoretical work of Ambrus, Mobius and Szeidl (2014) showing that social networks

[4]See, Holmes and Horvitz (1994), Tootell (1996), Ross et al. (2008), Ghent, Hernandez-Murillo and Owyang (2014), Cheng, Lin and Liu (2015), Hanson et al. (2016), Bartlett et al. (2021), and Bhutta and Hizmo (2021) among others.
[5]See, Mian, Sufi and Trebbi (2010), Mian, Sufi and Trebbi (2013), Agarwal et al. (2018), Chavaz and Rose (2019), Antoniades and Calomiris (2020), and Akey, Heimer and Lewellen (2020) among others.

can provide insurance against such shocks. Hence, we contribute to the literature examining the role of of cultural, racial and social proximity in determining economic outcomes in general and bank lending in particular.[6]

This paper proceeds as follows. Section 2 discusses background information on the VRA. Section 3 describes the data. Section 4 delineates the empirical strategy. Section 5 presents the baseline effect of the repeal of Section 5 of VRA on mortgage origination. Section 6 identifies the relevance of supply and demand side factors in driving the baseline results. Section 7 documents the underlying mechanisms. Section 8 highlights the role of perceived racial affiliation of a bank in mitigating the negative effects of the repeal of VRA. Section 9 concludes.

2 Institutional Details

This section discusses the Voting Rights Act of 1965 and its significance for black American. Following which we highlight the 2013 US Supreme Court Judgement in *Shelby v. Holder* that led to the repeal of section 5 of VRA - that required preclearance for any change in voting rules by the areas that were subjected to greater discrimination.

2.1 The Voting Rights Act

The growing racial disparity in the US led to the emergence of the American Civil Rights movement during the mid 1950s.[7] The "Jim Crow" laws and the subsequent decline in the economic and the social status of black Americans put the right to vote at the heart of the American Civil Rights movement.[8] The enactment of the Voting Rights Act (VRA) in 1965 is regarded as the biggest legislative achievements of the Civil Rights movement. The law was enacted following the aftermath of the Selma's "Bloody Sunday" and provided life to the Fifteenth amendment. President Lyndon B. Johnson described VRA as, "the goddamndest, toughest voting rights act [possible]".

2.1.1 What did VRA Do?

The VRA prohibited the denial or abridgement of the right to vote on account of race or color, forbidding all electoral structures that deny racial minorities the "opportunity...to participate [equally]

[6]See, Karlan (2007), Hjort (2014) Fisman, Paravisini and Vig (2017), Haselmann, Schoenherr and Vig (2018), Agarwal et al. (2019), and Fisman et al. (2020) among others. We direct the readers to Jackson, Rogers and Zenou (2017) and Shayo (2020) for an in-depth review of literature highlighting the economic consequences of social network structure.
[7]We expound on this background in appendix A.
[8]The southern state legislatures enacted several laws between the late 19th and the early 20th century, referred to as the "Jim Crow" laws, to impose *de-facto* suffrage restrictions on black Americans.

in the political process and to elect representatives of their choice." VRA achieved the equal opportunity to vote through two principal mechanisms enrishned in its Section 2 and 5.

Section 2 eliminated all voting restrictions, in the spirit of Jim Crow laws, that denied the right to vote on account of race. Section 2 is seen as the reinforcement of the voting rights act guaranteed in the Fourteenth and the Fifteenth Amendments. This section was implemented nationwide and increased citizens ability to sue as means of enforcing equal voting opportunity and challenging vote-denying practices.

Section 5 of the VRA empowered federal authorities with oversight powers to protect minorities' right to vote. Section 2 made it easier to strike down discriminatory voting laws. However, as noted by Pitts (2003), suspension of discriminatory laws in the past often resulted in an immediate enactment of new discriminatory rules hampering the ability of such ex-post checks. Section 5 of the VRA addresses this issue by requiring compulsory pre-clearance of all changes in voting laws from either the US Attorney General or the US District Court for DC. The jurisdictions, proposing changes to voting laws, were required to demonstrate that the proposed change neither had a discriminatory purpose or effect on black American voters. Hence, Section 5 shifted the burden of proof from voters to the election officials and is widely considered to be the heart of the VRA.

2.1.2 Implementation and Impact of VRA

While the Section 2 was implemented nationwide, the Section 5 of the VRA was primarily active in the South where the voting rights of the black Americans had been suppressed the most. The counties or states where the the Section 5 of VRA was active were referred as "covered" jurisdictions. States or counties were covered if they had less than 50% turnout in the 1964 presidential election. Section 5 was initially applied to all counties in Alabama, Georgia, Louisiana, Mississippi, South Carolina, and Virginia, 41 counties in North Carolina, and 1 county in Arizona. Amendments to the VRA , in 1970 and 1975, extended coverage to all counties in Texas, and several counties in Florida, Oklahoma, Arizona, New Mexico, Michigan, California, New York and New Hampshire. We refer to the counties covered by the Section 5 of the VRA by 1975 as the covered counties or the treated counties. Figure 1 shows the counties covered under the Section 5 of VRA by 1975.

VRA was instrumental in reducing the widespread political and economic disparity across

races. The impact of VRA on enfranchisement of black Americans was immediate. Valelly (2009) shows that between 1964 and 1968 presidential elections, black voter registration increased 67% among southern states. Using data for 40 years since 1975, Ang (2019) shows that the preclearance oversight of the Section 5 of VRA increased the long-run voter turnout by 4-8 percentage points, due to lasting gains in minority participation in the electoral process. Aneja and Avenancio-León (2019*b*) shows that the enactment of the Section 5 of the VRA helped reduce the black-white labor market inequality over the second half of the twentieth century. They argue that the black-white convergence in labor market inequality is driven by changes in the incentives faced by all politicians rather than just the increased presence of black elected officials.

2.2 Repeal of Section 5 of VRA - *Shelby County v Holder*

The US Supreme Court ruling of 2013 in the case of *Shelby County v Holder* came as a huge blow to the Section 5 of VRA. US Supreme Court ruled by 5 to 4 that the continued enforcement of preclearance of selected areas based on historical coverage formulas was unconstitutional. In the majority opinion Chief Justice John Roberts claimed that the things in the South have changed and using 40-year-old facts to define preclearance today was not logical. Justice Roberts further proposed that political discrimination was no longer a problem and the law was no longer needed. Others on the bench expressed doubt to the majority opinion. Justice Ruth Bader Ginsberg in her dissent with the majority opinion argued that the increased voting equality is because of VRA and warned against the dangers of overthrowing the act - "throwing away your umbrella in a rainstorm because you are not getting wet". The 2013 US Supreme Court ruling freed all states and counties covered by Section 5 of the VRA from federal oversight. While, the Congress has attempted to enact several new preclearance formulas since the 2013 ruling, none have passed the Congress.

2.3 Voting Laws after *Shelby County v Holder*

The effect of removal of protections provided under Section 5 on electoral process was rather immediate. Since the Shelby v Holder ruling of 2013, several covered jurisdictions have implemented controversial voting changes (Ang (2019)). Within 24 hours of the ruling, Texas announced and passed strict photo identification law that had previously been rejected by the US Attorney General under preclearance. Mississippi and Alabama, also began to enforce photo identification laws that

had previously been barred because of federal preclearance. Less than two months after Shelby ruling, North Carolina enacted a voting bill that instituted a strict photo identification requirement; curtailed early voting; eliminated same day registration; restricted pre-registration; ended annual voter registration drives; and eliminated the authority of county boards of elections to keep polls open for an additional hour. This law was later stuck down by the U.S. Court of Appeals for the Fourth Circuit in July 2016. However, the discriminatory law remained active in North Carolina for three years highlighting the challenges of ex-post litigation as opposed to the preventive machinery under Section 5 of VRA.

The Shelby ruling also had quantitative effect on voter turnout. Ang (2019) shows that following the Shelby decision the voter turnout in the covered counties declined by 1.5 percentage points, the largest drop in voter turnout since 1975. The 2018 state of voting study by the Brennan Center for Justice found that voters in 23 states were likely to face tougher voting restrictions than they did in 2010 (Weiser and Feldman (2018)). These restrictions include tougher voting identification laws, additional burden for registration among voters and cutbacks to early voting and absentee voting. The study notes that after the Shelby decision voters experienced a seesaw effect as new voting rules were imposed, blocked by courts, and then reinstated in modified form, only to be challenged again preventing thousands of voters to cast their ballots across multiple elections. Another 2018 Brenan Center report found that previously covered states have purged voters off their rolls at a significantly higher rate than non-covered jurisdictions (Brater et al. (2018)). The study calculates that 2 million fewer voters would have been purged over those four years if jurisdictions previously subject to federal preclearance had purged at the same rate as those jurisdictions not subject to that provision in 2013. For instance, after the Shelby decision Georgia purged twice as many voters as it did before the ruling.

2.3.1 Disproportionate effect on black Americans

The voting restrictions implemented post Shelby ruling disproportionately affects minorities. For example, the North Carolina law HB 589, passed within a month of the Shelby ruling, had an outsized impact on the state's growing African-American population. Three years after the implementation of HB 589 the Fourth Circuit Court of Appeals found that the North Carolina voter identification law was an unconstitutional effort to "target African-Americans with almost surgical

precision" (*NAACP v McCrory*). Voting purges and strict voting identification laws are likely to adversely affect the voting power of black Americans more than whites. Several preclearance requests regarding voter identification laws, before the Shelby ruling, were rejected by the federal government on the grounds that such requirements impose an undue burden among minorities such as Hispanics and black Americans. Purging of voters also disproportionately affects minorities. Crosscheck program, used for purging voters, eliminates voters based on common names. The 2010 US Census states that 16.3 percent of Hispanic people and 13 percent of black people have one of the 10 most common surnames, compared to 4.5 percent of white people. Therefore, purging programs based on common names are likely to purge minority voters more than white. Moreover, the undue burden of voting restrictions on black American voters was brought to national attention during the 2018 race for Georgia Governor involving Stacey Abrams and Brian Kemp. The US Commission on Civil Rights released a report on September 12, 2018, documenting the adverse effects of voter identification laws, voter roll purges, reduction in early voting, and polling place closures on minority voter participation.

3 Data & Summary Statistics

This section presents an overview of the data used in this paper and reports summary statistics.

3.1 Data

The empirical analysis of this paper hinges on different datasets. Below we provide a brief description of the data sources.

Home Mortgage Disclosure Act (HMDA). Our primary analysis uses mortgage application data collected and provided under the Home Mortgage Disclosure Act (HMDA). The HMDA dataset provides application level information on requested loan amount, purpose (home purchases/home improvement/refinancing), final status of application (approved/denied), census-tract level location of property for which loan is applied, along with information on the race and ethnicity of the borrower. Our sample period spans from 2010 to 2019 to include four (five) years before (after) the repeal of VRA. Finally, we restrict the sample to adjacent county pairs that straddle states covered by Section 5 in accordance with our identification strategy (see figure 2). Our final dataset is a county-race-year level aggregated data covering 426 counties in 30 states.

The Current Population Survey (CPS). CPS is used to examine the labor markets effects of the repeal of VRA. Data from the CPS are at the individual level and record working status and earnings of respondents. CPS allows us to identify public-sector workers and trace working status changes. We focus our analysis on people between ages 25 and 65 who work more than 32 hours per week, for at least 40 weeks a year. The data spans from 2008 to 2017.

Survey of Income and Program Participation Data (SIPP). SIPP is a household-level panel survey conducted by the US Census Bureau and each survey features sampled households interviewed over two to four years. This survey provides comprehensive information on households' investment, including the type of assets they invest in, and their state. We use data from the two most recent vintages spanning from 2010 to 2016.

American Community Survey Summary File (ACSSF). ACS collects housing and demographic information from over 3.5 million households each year. We construct national and state-level home-ownership rate by race from 2009 through 2019 using the 1-year ACSSF. We construct home-ownership rate and proportion of black Americans at the Zip Code Tabulation Area (ZCTA) level using two waves of 5-year ACSSF – 2008-2012 and 2013-2017.

The Internal Revenue Service (IRS). IRS maintains the address reported by individuals in their tax report filing. Consequently through a year-on-year change in the address, the IRS maintains data on migration.

Hate crime data collected by the Federal Bureau of Investigation (FBI). The Hate Crime Statistics Program of the FBI's Uniform Crime Reporting (UCR) Program collects data regarding geographically tagged criminal offenses that were motivated, in whole or in part, by the offender's bias against the victim's race/ethnicity/ancestry, gender, gender identity, religion, disability, or sexual orientation, and were committed against persons, property, or society. We focus on hate crimes against black Americans from 2010 to 2019 for our analysis.

American National Election Series (ANES). ANES is an in-person survey conducted on a stratified random sample of individuals around each presidential election. The data provides information on respondent's race, gender, and state along with their stated political preferences. We use the survey waves of 2008, 2012 and 2016 focusing on the *feeling thermometer* recording responses of white males towards black Americans. The feeling thermometer records the level of warmth or coldness that the respondent feels towards an issue or a group, in this case black

Americans, on a scale ranging from 0 to 97 with higher value indicating a higher degree of warmth.

US Census Bureau's Annual Survey of State and Local Government Finances. This dataset provides information on local government expenditure and revenue. We direct readers to Pierson, Hand and Thompson (2015) for the discussion on this dataset. Our data spans from 2010 until 2018 and include local governments (counties, cities, special districts, and school districts) that are present in all years of the sample period as in Adelino, Cunha and Ferreira (2017). We aggregate all sources of expenditure and revenues at county-year level.

3.2 Summary Statistics

Table 1 presents the summary statistics for the key outcome variables explored in the analysis. Panel A reports the summary statistics for county-race-year level mortgage markets variables and the state-race-year level homeownership rate. The mortgage market variables are constructed from the HMDA database and include mortgage applications and originations for home purchases and refinancing. The mortgage origination and applications are measured in number and total dollar amount. The mortgage market variables also include the denial rate, defined as the ratio of the number of denied applications to the total number of applications. The homeownership rate is collected from the ACSSF and is defined as the owner-occupied housing units divided by the sum of the owner-occupied housing units and the renter-occupied housing units. Panel B presents the summary statistics for county-year level migration variables collected from the database on income tax returns. Migration is measured in four different ways: outflow and inflow of individuals, outflow relative to inflow, and the difference between outflow and inflow scaled by the population in 2010. Panel C summarizes the statistics for the hourly wages of public-sector workers and the number of hate crimes against black Americans at the county level. The wage variable is collected from the CPS, and the hate crime variable is constructed from the FBI's hate crime database. Finally, panel D reports the summary statistics of expenditure and revenue of local government at the county level.

4 Empirical Strategy

The goal of this paper is to evaluate the effect of disenfranchisement on mortgage market outcomes. The empirical strategy involves comparing the counties covered under Section 5 of VRA with the

uncovered counties. The covered counties were affected by the Shelby ruling as the US Supreme Court decision took away the protections, instated half a century ago, provided to the covered counties with a history of racial discrimination. However, the ruling did not affect the status-quo in uncovered counties.

There are two keys empirical challenges in directly comparing all covered counties with the all uncovered counties. First, the counties covered under the Section 5 of VRA were not randomly assigned. The geographic coverage of Section 5 was deliberately designed to target counties with high degree of racial discrimination. Moreover the formula used in 1965, based on voter turnout in the 1964 Presidential election, is unlikely to be random. Voter turnout and racial discrimination are likely to be correlated with economic outcomes, a direct comparison of covered and uncovered counties is likely to suffer from a selection bias. Second, comparing covered and uncovered counties tantamount to comparing the American Deep South with the rest of America – two regions that exhibit systematic differences along economic, social and cultural dimensions. Therefore, a direct comparison of all covered and uncovered counties is likely to contaminate the estimation either due to selection bias or the presence of unobserved confounding variables, rendering the exercise futile.

We address these issues by focusing on contiguous counties separated only by jurisdictional borders in cases where one county was covered by Section 5 of VRA and the other was not. The identifying assumption of using this sub-sample of covered and uncovered contiguous counties is that the cultural, political, and economic conditions are expected to be similar in both observable , and more importantly, unobservables and consequently likely to follow similar paths in the absence of policy changes. Figure 2 presents the sub-sample of contiguous covered and uncovered counties used in the analysis. The empirical methodology of comparing bordering counties to evaluate the effect of a policy change has earlier been used in prior works evaluating the effects of banking deregulation (Huang (2008)), minimum wage (Dube, Lester and Reich (2010)), credit supply (Favara and Imbs (2015)), and enfranchisement (Ang (2019), Aneja and Avenancio-Leon (2019*a*), Aneja and Avenancio-León (2019*b*)) among others.

4.1 Baseline Specification

We combine the sample of contiguous covered and uncovered counties with data on mortgage market applications, denial rate, and originations to investigate the impact of the 2013 Shelby

decision. We refer to the counties covered by Section 5 of VRA prior to the Shelby ruling as treated counties and the uncovered counties as control counties. We estimate the following difference-in-difference-in-differences (DDD) specification:

$$y_{rct} = \beta \cdot Black_r \cdot Treat_c \cdot Post\text{-}Shelby_t + \alpha_{rc} + \alpha_{rt} + \alpha_{ct} + \varepsilon_{rct} \tag{1}$$

where, y_{rct} denotes the variable of interest aggregated at the county (c), race (r) and time (t) level. The different key dependent variables employed in this paper include - natural logarithm of number and amount of mortgage originations, natural logarithm of number and amount of mortgage applications, and denial rate. For each dependent variable we separately evaluate the effect on new mortgages, refinancing and the sum of the two. The coefficient of interest in equation 1 is the interaction term of $Black_r$, $Treat_c$ and $Post\text{-}Shelby_t$. $Black_r$ is a binary variable taking a value of one for black Americans and zero for white Americans. $Treat_c$ takes a value of one if the county was covered by Section 5 of VRA and zero otherwise. All counties included in the sample are identified in figure 2. $Post\text{-}Shelby_t$ is a binary variable taking a value of one for years after the 2013 Shelby ruling and zero otherwise. The specification includes time-varying race fixed effects (α_{rt}) which account for all time-varying race-specific shocks and allows the estimation in β to come from variation in treated and control counties for the same race. (α_{rc}) controls for and non-time varying characteristics that are specific to a race living in a county and allows the estimation to exploit time series variation in the repeal of the the section 5 of VRA. Importantly, county-race fixed effects (α_{rc}) non-parametrically account for the 1965 county-race specific characteristics that can explain selection into the treatment group. Finally, (α_{ct}) controls for any time-varying characteristics that might impact a county and allows the identification from variation in race. The standard errors are estimated by clustering at the county level.

4.1.1 Pre-Trends

The underlying assumption for identification of β in equation 1 is that the mortgage market outcomes for black Americans relative to white Americans in treated counties would have evolved similarly to the mortgage market outcomes for black Americans relative to white Americans in control counties in the absence of Shelby ruling. Potential threats to the identification of β include omitted factors correlated with the coverage of treated counties. So far we have addressed this issue through two

claims. First, the usage of the sub-sample of contiguous covered and uncovered counties allows us to compare similar jurisdictions. Second, the county-race fixed effects allows us to non-parametrically control for unobservable confounding variables that led to a particular county being covered by the Section 5 of VRA. While it is difficult to completely rule out all possible concerns, as the theoretical counterfactual outcome in the absence of Shelby ruling is unobservable, we explore the issue relating to the validity of the control counties being a suitable counterfactual for the treated counties.

We can evaluate the extent to which the mortgage market outcomes for black Americans relative to white Americans are parallel among treated and control counties before the Shelby ruling. Under the parallel trends assumption, any divergence between the control and the treated counties after the Shelby ruling can be attributed to the change in law. Hence, the path of the control counties after the law change can be viewed as a valid counterfactual to the trajectory of the treated counties had they not been exposed to the Shelby ruling. We test for pre-trends between the control and the treated counties by estimating a dynamic specification that interacts the $Black_r$ and $Treat_c$ binary variables with time dummies, as shown in equation 2. All variables have the same meaning as in equation 1 and 2013 is the omitted year. We use the different estimates of β_k, from equation 2, to asses the validity of the pre-trends assumption, and the impact on the treated counties relative to the control counties post Shelby ruling.

$$y_{rct} = \sum_{k=2010, k\neq 2013}^{2019} \beta_k \cdot Black_r \cdot Treat_c \cdot 1(t = k) + \alpha_{rc} + \alpha_{rt} + \alpha_{ct} + \varepsilon_{rct} \tag{2}$$

5 Results

This section begins by establishing the relevance of the dilution VRA as a potential shock to enfranchisement of black Americans. This is followed by presenting three facts describing the intersection of race, home-ownership, cost of voting and financing home-ownership through mortgages. Lastly, this section empirically investigates the causal link between disenfranchisement of black Americans and racial difference in mortgage outcome.

5.1 Voter Turnout and the Repeal of VRA

This section establishes the relevance of the dilution of VRA as a potential shock to political voice of black Americans. This test builds on the narrative analysis in section 2.3.1 and is important to verify the underlying assumption that the repeal of the Section 5 of VRA led to *de-facto* disenfranchisement of black Americans by reducing their electoral participation.

We compare the voter turnout in our sample of treated and control counties (see, figure 2) with high and low share of black population during presidential elections. A county is defined as high black county if its 2010 share of black population is greater than the median value of share of black population across all sample counties in 2010. The intuition for examining counties by their share of black population is that the counties with greater black population shares were counties where most people were adversely hit by the Shelby ruling. Figure 3 reports these results. The figure shows that the voter turnout in treated high black counties sharply declined after dilution of VRA. We further show in table 2 that the reduction is driven by treated counties with high share of black population. However, there was no significant change in voter turnout for treated counties with low share of black population relative to control counties. Specifically, high black treated counties experienced a decline of 2.5 percentage-points relative to high black control counties. This result following the seminal work of Tingsten (1937) suggests increase in political inequality between white and black Americans and consequently erosion of political voice for the latter.

We supplement this analysis using data on google searches for the term “Voting Rights Act" around the Shelby ruling. We find the searches are significantly higher by 11 percentage points in the treated counties compared to the control counties. We report the results in appendix figure A.1. This provides an additional evidence on the relevance of dilution of VRA as a shock to the treated counties.

5.2 Three Aggregate Facts

Fact #1: There exists a racial home-ownership gap of 30%. There exists a home-ownership gap of 30% between white and black Americans (see, appendix figure B.1a). The aggregate home-ownership gap between black and white Americans is large and has been stable for the last one decade. Furthermore, the proportion of owner occupied homes decreases exponentially with the increase in the proportion of black population in the ZIP Code Tabulation Area (ZCTA), showing

that a 10 percentage point (pp) increase in black population is associated with a 30 pp decline in the proportion of owner occupied homes (see, appendix figure B.1b).

Fact #2: Racial home-ownership gap increases with the cost of voting. Racial home-ownership gap between white and black Americans increases with the cost of voting. Figure 4a presents the relationship between the state-level racial home-ownership gap, the gap between white and black Americans, and the state-level cost of voting index (COVI).[9] The racial home-ownership gap increases with the increase in the COVI, and COVI can explain 20% of total variation in the racial home-ownership gap. There is, however, significant cross sectional heterogeneity across states in the amount of total variation in the racial home-ownership gap that COVI can explain. Figure 4b presents the variation in the model R^2, for each state, obtained from state-wise time-series regression of racial home-ownership gap on COVI. Figure 4c shows that this heterogeneity in the R^2 can be explained by the extent of racial harmony in the state.[10] The total variation in racial home-ownership gap that COVI can explain increases with decline in racial harmony.

Fact #3: Black home-owners are more reliant on debt for home purchases. Black home-owners, relative to white home-owners, are more reliant on debt for home purchases. On average 87% of the home value tends to be financed using home mortgages (see, appendix figure B.2a). However, black home-owners, relative to white home-owners, tend to rely more on external finance for their home purchase (see, appendix figure B.2b). The average combined loan-to-value (CLTV) ratio for black borrowers is 94% and the average CLTV ratio for white borrowers is 87%.

5.3 Baseline Results

This section explores the impact of increasing the cost of voting on mortgage market outcomes. This is motivated by the three aggregate facts presented in section 5.2. We use the natural experiment of 2013 Shelby ruling as a negative shock to the voting ability of black Americans, relative to white Americans, to identify the effects on mortgage market outcomes.

[9]The data on state-level racial home-ownership gap comes from the one-year summary files of the American Community Survey between 2009 and 2019 and the annual state-level data on COVI during the same period comes from Schraufnagel, Pomante and Li (2020). The estimate for the regression of state-level racial home-ownership gap on state-level COVI is 0.012 and significant at 5% level.

[10]The measure of state-level racial harmony comes from Dougal et al. (2019). The estimate of the cross-sectional regression of state-level model R^2 on racial harmony is -0.145 and significant at 5% level.

5.3.1 Univariate Analysis

We begin our analysis by examining the differential effect of Shelby ruling on black and white Americans in treated counties relative to the control counties. Figure 5 plots the weighted average of the county-level aggregate amount (figure 5a) and number (figure 5b) of mortgage originations for black and white Americans in treated counties relative to the control counties for each year from 2010 through 2019.[11] The mortgage origination index (Treat−Control) is computed by estimating the weighted average of the natural logarithm of amount and number of mortgage originations for black and white Americans for treated and control counties, and taking the difference between the two. The mortgage origination index for both black and white Americans is standardized to a value of 0 in 2013. The solid red line reports the mortgage origination index (Treat−Control) for the black borrowers, and the dash blue line reports the mortgage origination index (Treat−Control) for the white borrowers.

The results presented in figure 5 provide prima-facie evidence indicating that the mortgage origination declined for black Americans in treated counties relative to control counties post 2013 Shelby ruling. However, the mortgage origination for white Americans remained largely same in both the treated and control counties. Meanwhile, we do not find any difference in the mortgage originations across race in the pre-Shelby period. The pattern thereby suggest a structural change for black borrowers, while leaving the white borrowers largely unaffected.

5.3.2 Multivariate Analysis

In this section, we extend the univariate analysis presented in section 5.3.1 using a more empirically rigorous specification as in equation 1 and 2. Table 3 reports the results from the estimation of equation 1. Columns (1), (3), and (5) use the natural logarithm of the amount of total mortgage origination, mortgage origination for home purchases, and mortgage origination for refinancing respectively, for each race-county-year, as the dependent variable.[12] Columns (2), (4), and (6) use the natural logarithm of the number of total mortgage origination, mortgage origination for home purchases, and mortgage origination for refinancing respectively, for each race-county-year, as the dependent variable. Across all columns, the coefficient of the triple interaction term is consistently

[11] The sample of treated and control counties is shown in figure 2. Each observation is weighted by the 2010 county population and the county-race-year level aggregate data on number and amount for mortgage originations comes from the HMDA dataset from 2010 through 2019.

[12] Total mortgage is defined as the sum of home purchase and refinancing.

negative and statistically significant. The results indicate that the amount (number) of total mortgage origination fell by 14.3% (13.2%) for black borrowers relative to white borrowers in treated counties post 2013 Shelby ruling, relative to the black-white borrower mortgage origination gap in the control counties. We find quantitatively similar results when we examine home purchases (column (3) and (4)) and refinancing (column (5) and (6)) separately. Given the quantitative similarity in results and the importance of mortgages for home-purchases in real economic activity we will focus on home-purchases, henceforth. Moreover, we observe similar results for other minorities (see appendix table C.1).[13]

Figure 6 reports the results from the dynamic specification, equation 2. Figure 6a (6b) reports the results using the amount (number) of total home purchases as the dependent variable.[14] The dynamic estimate of β_k shows that the estimate in the pre-Shelby era are economically and statistically similar to the 2013 estimate, whereas the estimates post 2013 show that the mortgage origination for black borrowers fell relative to white borrowers in the treated counties relative to the black-white borrower mortgage origination gap in the control counties. There are three key takeaways from figure 6. First, the result shown in table 3 is unlikely to be driven by pre-trends indicating that the control counties act as a valid counterfactual for the treated counties had the Shelby ruling not happened. Second, the effect develops only after the Shelby ruling indicating that the effect is driven by the Shelby ruling. Third, the effect develops slowly over time, increasing over the three years following the Shelby ruling and does not revert thereafter.

5.4 Robustness Tests

This section presents the robustness of baseline results using an array of robustness exercises including placebo analysis, falsification test, a regression discontinuity design, and using the non-treated hinterland counties of control counties as control counties.

5.4.1 Placebo Analysis

We conduct a placebo test wherein we randomize the treatment variable keeping the timing of Shelby ruling fixed. This test addresses two concerns. First, it addresses whether the treatment variable is meaningful by checking if the results disappear if the treatment is randomly selected in a

[13] Our baseline results are economically similar across gender groups. See appendix table C.2.
[14] See appendix figure C.1 for specification without the fixed effects.

non-meaningful way. Second, it validates the non-spuriousness of the results. A placebo treatment variable is generated for each county from a binomial distribution with the probability of treatment assignment being equal to the empirical probability of treatment in the sample. The baseline specification, equation 1, is estimated using the new placebo treatment. We repeat this process of random treatment assignment 3,000 times and estimate the baseline specification for each randomly assigned treatment status. Appendix figure C.2 plots the kernel density of the estimated coefficient on $Black_r \cdot Placebo\text{-}Treat_c \cdot Post\text{-}Shelby_t$ obtained from 3,000 Monte-Carlo simulations where we randomize the treatment variable for counties. The distribution of the coefficient of the triple interaction term in the placebo analysis is centred around zero and the placebo exercise generates an effect greater than the baseline effect, documented in table 3, in less than 5% of cases.

5.4.2 Falsification Test

Next, we conduct a falsification test on an alternative sample that includes the sample of control counties shown in figure 2 and the counties that share a border with these control counties but are never covered by the Section 5 of VRA. This falsification sample is shown in appendix figure C.3. As neither of these counties were treated, we do not expect our baseline results to replicate in this sample. The original control counties in figure 2 are likely to be similar to the treated counties in most aspects except the applicability of the Section 5 of VRA. Hence, comparing these original control counties with a new set of control counties bordering these original control counties can serve as a reasonable falsification benchmark. If we can generate the baseline result in this falsification sample then the identified baseline relationship is unlikely to be causal and probably confounded by unobserved characteristics.

Appendix figure C.4 reports the results of the falsification exercise. We assign the original control counties to be the treated counties and the new bordering control counties to be the control counties for this falsification exercise. Appendix figure C.4a (C.4b) shows the estimates of β_k using mortgage origination amount (number) as the key dependent variable. The estimate of β_k from 2010 through 2019 are economically and statistically indistinguishable from each other in either panels. This indicates that the baseline results presented in this paper are unique to the treatment and cannot be generated in an otherwise similar sample but devoid of the actual treatment. The lack of results in the falsification sample supports the main claim of this paper.

5.4.3 Regression Discontinuity

This section supplements our baseline empirical strategy of comparing bordering counties with an alternate specification using a regression discontinuity (RD) design. The 1965 applicability of the Section 5 of VRA was decided based on the 1964 Presidential election voter turnout. All counties with voter turnout of 50% or less were subject to Section 5 of VRA. We use the counties within a narrow margin of 5% around this treatment threshold to conduct an RD estimation. Our RD design includes counties treated in 1965 with the 1964 voter turnout between 45% and 50% as a sample of treated counties, and the sample of counties with 1964 voter turnout between 51% and 55% on which Section 5 of VRA was never applied as a sample of control counties.[15] Appendix figure C.5 shows the treated and the control counties used in the RD design. The identifying assumption of this test is that the counties within a small interval around the threshold are randomly distributed around the threshold. However, counties on one side of the threshold were covered under Section 5 of VRA and others were not. This allows us to estimate the local treatment effect devoid of selection bias.

Table 4 reports the formal results using a regression specification. Columns (1) and (2) conduct an RD analysis while controlling for a function of the running variable of voter turnout and its interaction with the treatment. The dependent variable in columns (1) and (2) are county-level mortgage origination growth in amount and number, respectively, for black Americans relative to white Americans from 2013 to 2016. Columns (3) and (4) present results from a DDD estimation, similar to the baseline specification (equation 1), using the RD sample. The dependent variables in column (3) and (4) are the natural logarithm of mortgage origination amount and number, respectively. Across all columns the coefficient of interest is negative and statistically significant. Moreover, the magnitude of the estimate across all columns is quantitatively similar to the baseline estimate reported in table 3. Appendix figure C.6 graphically compares the county-level mortgage origination growth, amount and number, for black Americans relative to white Americans from 2013 to 2016 around the threshold of 50% voter turnout. The figure resonates the results in table 4 showing a clear discontinuity in mortgage origination around the threshold. Hence, this section lends credence to the baseline results and indicates that the results are unlikely to be an artifact of the specific sample employed in baseline estimation or driven by selection bias.

[15] Data on county-level 1964 Presidential election voter turnout comes from Ang (2019).

5.4.4 Hinterland Counties as Control Counties

Disenfrachised groups could potentially circumvent the adverse effects of disenfranchisement by seeking economic opportunities in bordering counties where the Shelby ruling did not have an effect. This can result in a negative effect on these bordering control counties by increasing the supply of opportunities seeker in these counties. The negative spillovers from the treated to the control counties could result in our baseline estimation potentially understating the true effect. This section addresses this issue by using the hinterland non-treated counties bordering the control counties in figure 2 as control counties.[16]

We address the issue of negative spillovers from the treated counties to the original control counties by using a second control group. We refer to these counties as the hinterland counties. The hinterland counties are non-treated counties that border the original control counties. The hinterland counties are farther away from the control counties but contiguous with the original set of control counties. So, these hinterland counties are co-contiguous with the treated counties, with the original control counties located in-between them. Appendix figure C.7 shows the treated counties and the hinterland counties used in the analysis in this section.

Table 5 presents the results from the estimation of equation 1 using the sample of treated and hinterland counties in figure C.7. Across all columns the coefficient of interest is negative and statistically significant. Moreover, as hypothesized the estimate of β reported in table 5, across all columns, is larger in magnitude relative to the baseline estimate reported in table 3. The magnitude of the effect obtained from using hinterland counties as control counties is -22%, which is larger than the baseline effect of -14%.

5.4.5 Local Identification Approach

We supplement the baseline analysis presented in table 3 with a local identification approach which includes race $\times$ county-pair $\times$ year fixed effects in addition to the fixed effects discussed in equation 1. County-pair refers to the adjoining pair of treated and control counties in our sample. The inclusion of race $\times$ county-pair $\times$ year fixed effects allows us to infer the estimate of the triple interaction term of Black $\times$ Treat $\times$ Post-Shelby through within county-pair variation of the same

[16]The falsification test presented in section 5.4.2 presents the relative response of the original control counties and the hinterland non-treated counties. While we do not find any economically or statistically significant results, Figure C.4 shows a small negative response of the original control counties relative to the hinterland counties, following the Shelby ruling. This could indicate the presence of some negative spillovers from the treated counties to the original control counties.

race, i.e., we effectively compare the mortgage origination of the black Americans relative to white Americans in the treated counties with mortgage origination of the black Americans relative to white Americans in the directly adjacent control counties. However, the usage of county-pairs can result in the presence of a single county in multiple pairs that could bias the estimate and induce a mechanical correlation across county-pairs. We account for this source of correlation in the error term by clustering the standard errors at the county-pair level. Appendix table C.3 reports the estimation results from this exercise. The results from the local identification approach echo the baseline findings and are qualitatively similar to the results reported in table 3.

6 What Drives the baseline results? - Supply or Demand

This section probes if the exclusion of black Americans from mortgage markets is through reduced lending to black Americans by banks or muted demand by the borrowers following dilution of their political voice. We use mortgage applications by potential borrowers to capture demand and denial rate of mortgage applications by lenders to capture supply.

Disenfranchisement can alter the supply and demand curves by increasing uncertainty, borrowing constraints and fear of rejection through multiple channels.[17] First, the disenfranchised group could migrate to other areas in search of better political patronage. This could mechanically lower their housing demand. Second, disenfranchisement can result in lower income, as noted by Aneja and Avenancio-León (2019*b*), which could financially marginalize the disenfranchised group consequently reducing their demand for mortgages. Alternatively, a negative income shock could disqualify aspiring borrowers pushing them below the bank risk-appetite or regulatory threshold resulting in the rejection of their application. Possible anticipation of application rejection can also result in self-censorship by the borrower. Third, disenfranchisement can reduce the incentives of the politicians to direct bank lending towards the disenfranchised group resulting in the shrinkage of credit supply. Fourth, disenfranchisement can reduce the incentives of the politicians to improve or maintain the current quality and quantity of public good provisions that are primarily used by the disenfranchised groups (Cascio and Washington (2014)). Fifth, disenfranchisement can reduce

[17]Fear of rejection matters because application rejection is not costless for potential applicants. Rejection of an application can result in decline in credit score, adversely affecting the rejected individual in their access and cost of other forms of credit such as credit cards, personal loans etc. Moreover, fear of rejection is not an innocuous concern. The Survey of Consumer Finance data between 2001 and 2019 indicates that 16.8% of all Americans are afraid of being rejected when applying for mortgages. This fear is higher among black Americans. 33.3% of black Americans fear rejection as opposed to 12.3% of white Americans who fear rejection. See figure D.1.

the de-facto protection – both personal and property protection– provided by the state to these communities which may also manifest as low housing demand. Sixth, discrimination in lending markets can increase as the incentives of the state to enforce anti-discriminatory laws decrease following disenfranchisement. This can decrease the supply as well as the demand by increasing the fear of rejection.

We examine the importance of demand side factors in explaining the baseline results by evaluating possible differences in loan application across races in the treated vis-á-vis control counties. Table 6 reports these results. Column (1), (3), and (5) use the natural logarithm of the amount of total mortgage applications, applications for home purchases, and applications for refinancing respectively, for each race-county-year, as the dependent variable.[18] Column (2), (4), and (6) use the natural logarithm of the number of total mortgage applications, applications for home purchases, and applications for refinancing respectively, for each race-county-year, as the dependent variable. Across all columns, the coefficient of the triple interaction term is consistently negative and statistically significant. The results indicate that the amount (number) of total mortgage applications fell by 16% (15%) for black borrowers relative to white borrowers in treated counties post 2013 Shelby ruling, relative to the black-white borrower mortgage application gap in the control counties. This effect is quantitatively large given the average year-on-year amount (number) mortgage application growth rate of 2.6% (-0.3%) from 2010 to 2019. We find quantitatively similar results when we examine applications for home purchases and refinancing. Appendix figure D.2 lends further credence to the results indicating that the results are unlikely to be driven by pre-trends and appear only after the 2013 Shelby ruling. Moreover, these results are qualitatively similar across sub-samples of different income quartiles (see appendix table D.1).

Next, we examine the relevance of supply side factors in explaining the baseline results by evaluating the effect of the Shelby ruling on denial rate of applications. Table 7 reports these results. Column (1), (2), and (3) use the denial rate for total mortgage applications, denial rate for applications for home purchases, and denial rate for applications for refinancing respectively, for each race-county-year, as the dependent variable. Across all columns, the coefficient of the triple interaction term is statistically insignificant. While the coefficient of interest is negative the estimate itself is small in magnitude and economically insignificant given the average black-white

[18]Total mortgage is defined as the sum of home purchase and refinancing.

denial rate difference of 10% (16%) for applications for home purchase (refinancing).

Hence, the results presented in this section lend support to the dominance of demand side factors rather than the supply side factors in explaining the baseline factors. It is to be noted that we cannot completely rule out absence of supply side factors as anticipation of rejection of application, either due to discrimination, negative income shock or shrinkage in politically motivated lending, can have pre-effects that may manifest as contraction in demand. While we do not claim absence of changes in supply following Shelby ruling, our results indicate that such changes in credit supply result in pre-effects manifesting as reduction in demand. Broadly, this analysis indicates that disenfranchisement results in exclusion from the mortgage markets by reducing the participation of the disenfranchised group.

7 Mechanism

This section explores the underlying mechanism operating through changes in demand for home mortgages from black borrowers following the Shelby ruling. Specifically, this section explores the importance of migration of the disenfranchised black Americans, income shocks, increased future uncertainty, racial violence and provision of public goods as potential channels that may explain the reduced participation of black Americans in mortgage markets documented in section 6.

7.1 Migration of Black Americans

Black Americans could circumvent the adverse effects of disenfranchisement by moving to other areas within the US where their voting rights are relatively better protected.[19] The emmigration of disenfranchised groups would lower the housing demand among the disenfranchised group. This section examines the importance of the migration channel in explaining the demand effect.

We use county level data on aggregate inflow and outflow of people to examine the effect of Shelby ruling on migration of people in treated counties relative to people in the control counties. A county is defined as high black county if their 2010 share of black population was greater than the median value of share of black population in all sample counties in 2010. The intuition for using black share to classify counties is that the counties with greater black population shares were

[19]The movement of 6 million African Americans out of the rural Southern United States to the urban Northeast, Midwest and West between 1916 and 1970, also known as the Great Migration, in search for better economic opportunities and freedom from oppression is a case in point.

counties where most people were adversely hit by the Shelby ruling.[20] Table 8 reports these results using different measures of migration. Column (1), (2), (3) and (4) use natural logarithm of county outflows, natural logarithm of county inflows, natural logarithm of county outflows minus the natural logarithm of county outflows, and the difference of county outflows and inflows divided by 2010 county population respectively. Across all measures of migration the coefficient of the triple interaction term is statistically and economically insignificant. In column (1) the coefficient of the interaction term of Treat and Post is statistically significant for outflows. However, the estimate is negative indicating that the aggregate outflow from treated counties declined post Shelby.

Overall, the results indicate that the Shelby ruling did not effect net migration in treated counties relative to the control counties in statistically or economically significant ways. Hence, we can rule out migration as a plausible explanation for the decline in mortgage application. The lack of changes in migration patterns after Shelby ruling is likely to be the case when migration is financially or psychologically costly especially for the financially marginalized minorities living in those areas for generations.

7.2 Changes in Income post Shelby

This section evaluates the effect of Shelby ruling on labor market inequality. Aneja and Avenancio-Leon (2019*a*) document improvement in black workers position within the public sector after implementation of Section 5 of the VRA. Aneja and Avenancio-León (2019*b*) provides suggestive evidence showing that the black Americans working in the public sector were adversely affected following the Shelby ruling. We supplement the analysis presented in Aneja and Avenancio-León (2019*b*) by estimating the following regression specification, with the natural logarithm of wages as the dependent variable:

$$ln(Wage)_{ict} = \beta \cdot Black_i \cdot Treat_c \cdot Post\text{-}Shelby_t + \alpha_{rc} + \alpha_{rt} + \alpha_{ct} + \varepsilon_{ict} \tag{3}$$

Table 9 presents the results from the estimation of equation 3 using individual level wage data for the sample shown in figure 2 from CPS.[21] In our analysis we focus on public sector employees as the effect of losing enfranchisement is likely to be more direct and stronger for public sector

[20] Ideally we would want to look at migration of individuals by race, in the absence of which we use the share of race in a county to proxy for the racial characteristics for the individuals who migrate

[21] The sample includes individuals between ages 25 and 65 who work more than 32 hours per week for at least 40 weeks a year.

employees (Aneja and Avenancio-León (2019*b*), Aneja and Avenancio-Leon (2019*a*)). Each row of table 9 reports the point estimate of β using different samples. The first row uses the sample of all employees, the second row uses the sample of all existing employees, the third row uses the sample of new hires, and the fourth, the fifth, and the sixth row uses sample for all employees in counties with varying levels of 2010 black population. The point estimate of β across all samples is negative. The results from the first row indicates that the wage of black Americans fell by 10% relative to white Americans in treated counties relative to the white-black wage gap in the control counties. This effect, while not statistically significant, is economically significant given the unconditional white-black wage gap of 20%. We split the sample into existing employees and new hires. The intuition of this test is that the wages of existing employees are downward sticky and therefore the effect may be more pronounced for new hires. Rows two and three indicate that the effect shown in row 1 is primarily driven by new hires. Existing black American employees experience a relative decline of 5% in wages, whereas newly hired black American employees experience a relative decline of 11% in wages. Unlike results shown in previous rows the decline in relative wages for newly hired black Americans is statistically significant at 10% level. Rows four, five and six estimate equation 3 for different counties based on varying levels of percentage of 2010 black population in the county. The intuition for this split is that the counties with greater minority population shares were counties where minorities could exercise more influence pre Shelby, and lost most of their political voice post Shelby. As hypothesized, we find that the racial wage gap increase following Shelby ruling in treated counties relative to the control counties increases with increase in 2010 black population. The estimate of β is economically small when comparing counties with low levels of black population. The estimate of β in the fifth (sixth) row shows that the black American employees experienced a wage decline of 13% (48%) relative to whites Americans in the treated counties relative to the black-white wage gap in control counties following Shelby ruling.

Our results show that the wages for black Americans in the public sector, relative to wages of white Americans in the public sector, declined following the Shelby ruling in treated counties relative to the control counties. Moreover, this effect is pronounced for new hires and for counties with high pre Shelby black population.

7.3 Uncertainty and Disenfranchisement

Disenfranchisement can increase future uncertainty and borrowing constraints by increasing the likelihood of future negative uninsurable shocks for the disenfranchised communities. The increase in future uncertainty can reduce the ability of minorities to service debt, thereby reducing the demand for mortgages and illiquid assets such as homes (Kimball (1993)). A direct test of this channel is difficult as the data on geographically tagged individual-level future expectations of income is not readily available. We rather focus on an indirect test. Constantinides and Ghosh (2017) argue that higher future uncertainty with incomplete insurance can manifest as reduced stock market participation. Guiso, Jappelli and Terlizzese (1996) show that households with future anticipated constraints keep a lower proportion of their wealth in illiquid and risky assets. Giannetti and Wang (2016) show that households reduce their stock market participation following their experience with fraud. On a similar note, Agarwal et al. (2021) show reduced stock market participation by households during periods of high political uncertainty. Therefore this section provides an indirect test of the channel of future uncertainty by examining changes in financial market participation.

We examine the changes in the stock and bond market participation of black Americans relative to white Americans in treated states relative to control states using state-race-year level from SIPP spanning from 2010 to 2016. Table 10 reports these results. Column (1) and (2) compare all treated states with all control states, whereas column (3) and (4) compare all treated states with only neighbouring control states. Column (1) and (3) use the share of people investing in mutual funds, column (2) and (4) use the share of people investing in government securities as the key dependent variable. State-year fixed effects allows the identification to come from variation in investment of black Americans vis-a-vis white Americans. The coefficient of the interaction term of interest is negative, and similar in magnitude, for the share of people investing in mutual funds in both column (1) and (3). Meanwhile, the same coefficient is positive, and similar in magnitude, for the share of people investing in government securities in both column (2) and (4). The coefficients indicate that that the share of black Americans relative to the share of white Americans investing in mutual funds (government securities) decreases (increases) by 1.9% (1.3%) in treated states following the Shelby ruling relative to control states. This effect is economically significant as the

average share of people investing in mutual funds (government securities) is 6.9% (2.5%). This test indicates that black Americans anticipation of future uncertainty and constraints increased following the Shelby ruling.

7.4 Violence Against Black Americans

This section examines the change in violence and attitudes towards Black Americans following the 2013 Shelby ruling. The state provides protection against all forms of violence. The 2013 Shelby ruling decreases the political voice of black Americans and reduces the incentives of the state to provide such protections to black Americans. Hence, attenuating the barriers to explicit animosity and violence against black Americans by racist individuals and groups. This increased risk of violence, resulting in loss of life or destruction of property, can reduce the investment horizon thereby reducing housing demand among black Americans.[22]

Table 11 documents the change in the extent of hate crimes against Black Americans following the Shelby ruling using FBIs hate crime data from 2010 till 2019. The estimates in table 11 shows that the number of incidents of violent hate crimes against African Americans increased by 16-29% in treated areas relative to control areas following the Shelby ruling. We provide external validity to these results using ANES data on reported warmth for black Americans among white males in America around Presidential elections. Appendix table E.1 reports these results showing that the warmth towards black Americans declined by 4.5 pp in treated areas relative to control areas following the Shelby ruling. The two results together indicate that the usage of violence and animosity against black Americans by non-state actors increased following the Shelby ruling.

7.5 Public Good Provision

This section examines the affect of electoral disenfranchisement on the provision of public good. Political voice is imperative in drawing public good towards a region or a group. Doepke and Tertilt

[22]Historically, the black community has faced destruction of their property by white supremacist groups and individuals with little ex-ante protection or ex-post justice by the state. The 1912 Tulsa race riots is a case in point where white rioters rampaged through a Black neighborhood in the Greenwood district of Tulsa in Oklahoma, also referred to as the *Black Wall Street*, killing men and destroying property on the ground and from private aircraft. There was little to no state support or protection provided to the Black community and no charges were made against white rioters (Cook (2014)). The Rosewood massacre of 1923 in rural Levy County, Florida is yet another example. Violence against black Americans and destruction of their property is not simply an ancient phenomenon. The 2017 Hate Crime Victimization Report by the US Department of Justice documents that 35.4% of all violent hate crime victims between 2011 and 2015 were black Americans as opposed to black Americans accounting for 22% of all victims to violent non-hate crime incidents. The FBI's annual Hate Crime Statistics Act (HCSA) report of 2019 documents that 26% of hate crime incidents were racially motivated and targeted black Americans.

(2009) among others have highlighted the role of women suffrage and the public good being directed towards women. Fujiwara (2015) shows that an increase in enfranchisement of the poor, through the introduction of electoral voting machine, led to an increase in public spending on healthcare. With respect to minority voting rights in the US, Cascio and Washington (2014) highlight that the passage of VRA increased the share of public spending directed towards black Americans. Consequently, dilution of VRA would lead to reduction in public good provided towards black Americans.

This section uses the county level data on public expenditures in education, health and utilities to examine the effect of Shelby ruling on public goods provisions in treated counties relative to people in the control counties. A county is defined as high black county if their 2010 share of black population was greater than the median value of share of black population in all sample counties in 2010. The intuition for using black share to classify counties is that the counties with greater black population were most adversely hit by the Shelby ruling. Panel A of table 12 reports these results. Results show that following the repeal of VRA, total government expenditure reduced by 9% in treated counties with high share of black population. While education and utilities declined by an economically significant amount of around 8% and 24% respectively, they are not statistically significant. Meanwhile expenditure on health reduced by 64% that is both statistically significant and economically large. Panel B of table 12 verifies that the reduction in government expenditure is not driven by lower revenues of the local government.

8 Importance of Black Banks post Shelby

This section attempts to highlight the role of the perceived racial affiliation of a bank in mitigating the reduction in mortgage market participation. The intuition being that the direct and indirect demand effects, discussed in section 6, may vary depending on the relationship of the black community with the bank.

Black individuals may anticipate a lower shrinkage of credit supply from black banks for three reasons. First, individuals may fear an increase in discrimination from non-black banks post Shelby.[23] Second, black banks may have access to community based soft information on individuals that could compensate for the decline in income post Shelby. Third, black banks could attenuate the

[23]Using the ANES survey we verify that the trust among black Americans declines by 20% post Shelby. See table E.2.

magnitude of this shock for the community by increasing credit supply as a retaliation for increased discrimination against members of its community.[24] Alternatively, shocks to individuals based on their race can make racial identity salient. The salience of racial identity can have consequences for economic decisions (Akerlof and Kranton (2000), Akerlof and Kranton (2005)). This salience of race-specific shocks following Shelby ruling could result in racial homophily where they may turn to community based institutions for insurance against such shocks (Shayo (2020)). This may result in flight of home mortgage demand by black Americans to black lenders.

8.1 Definition of Black Lenders

We define black lenders using lending data of lenders operating in the sample counties shown in figure 2. This is done because black lenders defined at the national level could differ from lenders to which black borrowers in the sample counties feel comfortable and close. We then aggregate HMDA data for the pre-Shelby period, i.e. 2010 to 2013, at the lender level and sort the lenders by the share of black borrowers in their mortgage lending portfolio. We define lenders above the 90th percentile as black lenders.[25] We are able to identify 569 black lenders with this methodology. These banks are usually small banks and primarily cater to a small geographic area. Appendix table E.3 provides a description of five representative banks that are defined as black banks using our methodology.

8.2 Results

We identify the effect of change in demand by examining the change in application by black Americans to black and non-black banks following the Shelby ruling. Table 13 reports these results. Columns (1)-(4) use mortgage applications to non-black lenders and columns (5)-(8) use mortgage applications to black lenders. The coefficient on the triple interaction term of interest is consistently negative, statistically significant and economically meaningful across columns (1) to

[24] Baradaran (2017) describes black banks as quasi-crusaders filling the void created by Jim Crow and segregation to offer services to black individuals amidst exclusion. Black banks are often founded by black Americans in response to economic segregation with the aim to provide financial inclusion to black communities. Baradaran (2017) notes that some of the earliest black banks were started by former slaves, e.g. the True Reformers Savings Bank founded in 1888 in Richmond Virginia, as a direct response to white-owned banks' discriminatory practices. Black banks are often headed and run by black entrepreneurs and supported by black community leaders. The recent founding of the Greenwood Bank, a black bank, by rapper and activist Killer Mike is a case in point. The importance of black community banks, and the counterfactual in their absence, is reminiscent of the community banker George Bailey, a character in the fictional story - "It's A Wonderful Life." The importance of community affiliated banks is not specific to black Americans. The modern day Bank of America was founded as the Bank of Italy (United States) in 1904 as retaliation to the exclusion of Italians by the banking system of that time.

[25] Ross et al. (2008) show that lenders with substantial numbers of applications from African-Americans treat African-Americans more favorably than lenders with predominantly white application pools.

(4) for non-black lenders. This finding is consistent with the baseline results reported in table 3. Columns (5) to (8) estimate the same specification but for a sample of black lenders identified as in section 8.1. In contrast to our previous results the triple interaction term of interest is positive. The estimate is statistically significant in columns (5) and (6), but looses significance in columns (7) and (8). Regardless of the statistical significance the magnitude of the estimate is quantitatively large and positive consistently across columns (5) to (8). This indicates that the amount and number of black mortgage applications, relative to whites, increased to black lenders after Shelby ruling, relative to the black-white mortgage application change difference in control counties. This indicates that there was an increase in demand for home mortgages from disenfranchised black Americans from black lenders following Shelby ruling.

We further investigate the credit supply side changes by black banks to disenfranchised black borrowers following the Shelby ruling. Table 14 reports these results using mortgage origination and denial rates for black lenders. The coefficient of the triple interaction term across columns (1) to (4) is positive. While the estimate is mostly statistically insignificant, the estimate is economically meaningful. On a conservative note the estimate suggests that black mortgage origination relative to white mortgage origination increases by $\approx 5\%$, for both amount and number, by black lenders in treated counties relative to control counties following the Shelby ruling. This estimate is large relative to the average year-on-year amount (number) mortgage origination growth rate of 4.1% (0.9%). This indicates that the black banks increased mortgage origination to black borrowers relative to white borrowers in treated counties, post Shelby, relative to the control counties. Columns (5) and (6) use denial rate as the dependent variable. The estimate is statistically insignificant and economically small given the average black-white denial rate difference of 10%.

The two results above taken together indicate that there is flight of black demand for home mortgages to black lenders and they respond to this increased demand by an increase in supply of home mortgages. Lack of change in denial rate suggests that the black lenders are able to absorb a part of this increase in demand from black borrowers. Overall, our results point towards the importance of black lenders in black communities as providers of insurance to such social shocks.

9 Conclusion

In this paper, we identify the effect of electoral disenfranchisement of black Americans on their mortgage borrowing decisions. We combine the spatial information on jurisdictions previously covered under Section 5 of VRA with the race and location of mortgage applicants and use a triple difference estimation strategy. We document that the amount (number) of total mortgage originations fell by 14.3% (13.2%) for black borrowers relative to white borrowers in treated counties post 2013 Shelby ruling, relative to the black-white borrower mortgage origination gap in the control counties. We also find that the reduction in mortgage origination is driven by decline in mortgage applications by black Americans, whereas the denial rate of applications remains unchanged. The results taken together suggest that political disenfranchisement can push black Americans to self select out of the mortgage market. The real impact is manifested through a reduction in homeownership among black Americans post the Shelby ruling (see figure 7).

Using data on changes in public-sector wages, financial market participation, race-related hate crimes and flight of black demand to black lenders we argue that the reduction in demand is driven by increased borrowing constraints, uncertainity and greater fear of application rejections among black Americans following their disenfranchisement. Finally, we note that black lenders respond to the demand from black Americans by increasing their credit supply. Hence, our results highlight the importance of black lenders in black communities as providers of insurance to socio-political shocks.

Broadly, the results expand our understanding on the social and economic impact of changes in voting power. This paper documents that individuals alter their economic decision making as a response to changes in their political voice. Hence, our paper proposes a new channel through which discrimination in the voting process can result in exclusion from markets. Our work is also relevant to policy makers working on issues of voting rights, racial disparity, and community banks. Our results highlight that fifty years after the passage of voting rights act, there might still be a need to protect the ballot, especially for the historically marginalized.

References

Adelino, Manuel, Igor Cunha, and Miguel A Ferreira. 2017. "The economic effects of public financing: Evidence from municipal bond ratings recalibration." *The Review of Financial Studies*, 30(9): 3223–3268.

Agarwal, Sumit, Gene Amromin, Itzhak Ben-David, and Serdar Dinc. 2018. "The politics of foreclosures." *The Journal of Finance*, 73(6): 2677–2717.

Agarwal, Sumit, Hyun-Soo Choi, Jia He, and Tien Foo Sing. 2019. "Matching in housing markets: The role of ethnic social networks." *The Review of Financial Studies*, 32(10): 3958–4004.

Agarwal, Vikas, Hadiye Aslan, Lixin Huang, and Honglin Ren. 2021. "Political uncertainty and household stock market participation." University of Cologne, Centre for Financial Research (CFR) CFR Working Papers 21-06.

Aidt, Toke S, and Peter S Jensen. 2009. "Tax structure, size of government, and the extension of the voting franchise in Western Europe, 1860–1938." *International Tax and Public Finance*, 16(3): 362–394.

Aidt, Toke S, and Peter S Jensen. 2013. "Democratization and the size of government: evidence from the long 19th century." *Public Choice*, 157(3-4): 511–542.

Akerlof, George A, and Rachel E Kranton. 2000. "Economics and identity." *The Quarterly Journal of Economics*, 115(3): 715–753.

Akerlof, George A, and Rachel E Kranton. 2005. "Identity and the Economics of Organizations." *Journal of Economic Perspectives*, 19(1): 9–32.

Akey, Pat, Christine Dobridge, Rawley Heimer, and Stefan Lewellen. 2018. "Pushing boundaries: Political redistricting and consumer credit." *Available at SSRN 3031604*.

Akey, Pat, Rawley Z Heimer, and Stefan Lewellen. 2020. "Politicizing consumer credit." *Journal of Financial Economics*.

Ambrus, Attila, Markus Mobius, and Adam Szeidl. 2014. "Consumption risk-sharing in social networks." *The American Economic Review*, 104(1): 149–82.

Aneja, Abhay, and Carlos F Avenancio-Leon. 2019*a*. "The effect of political power on labor market inequality: Evidence from the 1965 Voting Rights Act."

Aneja, Abhay P, and Carlos F Avenancio-León. 2019*b*. "Disenfranchisement and Economic Inequality: Downstream Effects of Shelby County v. Holder." *AEA Papers and Proceedings*, 109: 161–65.

Ang, Desmond. 2019. "Do 40-year-old facts still matter? Long-run effects of federal oversight under the Voting Rights Act." *American Economic Journal: Applied Economics*, 11(3): 1–53.

Antoniades, Alexis, and Charles W Calomiris. 2020. "Mortgage market credit conditions and US Presidential elections." *European Journal of Political Economy*, 64: 101909.

Ayers, Edward L. 2007. *The promise of the new South: Life after reconstruction.* Oxford University Press.

Baradaran, Mehrsa. 2017. *The color of money: Black banks and the racial wealth gap.* Harvard University Press.

Bartlett, Robert, Adair Morse, Richard Stanton, and Nancy Wallace. 2021. "Consumer-lending discrimination in the FinTech era." *Journal of Financial Economics*.

Bhutta, Neil, and Aurel Hizmo. 2021. "Do minorities pay more for mortgages?" *The Review of Financial Studies*, 34(2): 763–789.

Brater, Jonathan, Christopher Deluzio, Kevin Morris, and Myrna Pérez. 2018. *Purges: A Growing Threat to the Right to Vote.* Brennan Center for Justice at New York University School of Law.

Button, James W. 2014. *Blacks and social change: Impact of the civil rights movement in southern communities.* Princeton University Press.

Campbell, John Y, and Joao F Cocco. 2003. "Household risk management and optimal mortgage choice." *The Quarterly Journal of Economics*, 118(4): 1449–1494.

Carruthers, Celeste K, and Marianne H Wanamaker. 2015. "Municipal Housekeeping The Impact of Women's Suffrage on Public Education." *Journal of Human Resources*, 50(4): 837–872.

Cascio, Elizabeth U, and Ebonya Washington. 2014. "Valuing the vote: The redistribution of voting rights and state funds following the voting rights act of 1965." *The Quarterly Journal of Economics*, 129(1): 379–433.

Charles, Kerwin Kofi, and Erik Hurst. 2002. "The transition to home ownership and the black-white wealth gap." *The Review of Economics and Statistics*, 84(2): 281–297.

Chavaz, Matthieu, and Andrew K Rose. 2019. "Political borders and bank lending in post-crisis America." *Review of Finance*, 23(5): 935–959.

Cheng, Ping, Zhenguo Lin, and Yingchun Liu. 2015. "Racial discrepancy in mortgage interest rates." *The Journal of Real Estate Finance and Economics*, 51(1): 101–120.

Chetty, Raj, and Adam Szeidl. 2007. "Consumption commitments and risk preferences." *The Quarterly Journal of Economics*, 122(2): 831–877.

Chetty, Raj, László Sándor, and Adam Szeidl. 2017. "The effect of housing on portfolio choice." *The Journal of Finance*, 72(3): 1171–1212.

Constantinides, George M, and Anisha Ghosh. 2017. "Asset pricing with countercyclical household consumption risk." *The Journal of Finance*, 72(1): 415–460.

Cook, Lisa D. 2014. "Violence and economic activity: evidence from African American patents, 1870–1940." *Journal of Economic Growth*, 19(2): 221–257.

Debnath, Sisir, Mudit Kapoor, and Shamika Ravi. 2017. "The impact of Electronic Voting Machines on electoral frauds, democracy, and development." *Democracy, and Development (March 16, 2017)*.

Doepke, Matthias, and Michele Tertilt. 2009. "Women's Liberation: What's in it for Men?" *The Quarterly Journal of Economics*, 124(4): 1541–1591.

Dougal, Casey, Pengjie Gao, William J Mayew, and Christopher A Parsons. 2019. "What's in a (school) name? Racial discrimination in higher education bond markets." *Journal of Financial Economics*, 134(3): 570–590.

Dube, Arindrajit, T William Lester, and Michael Reich. 2010. "Minimum wage effects across state borders: Estimates using contiguous counties." *The Review of Economics and Statistics*, 92(4): 945–964.

Favara, Giovanni, and Jean Imbs. 2015. "Credit supply and the price of housing." *The American Economic Review*, 105(3): 958–92.

Fisman, Raymond, Arkodipta Sarkar, Janis Skrastins, and Vikrant Vig. 2020. "Experience of communal conflicts and intergroup lending." *Journal of Political Economy*, 128(9): 3346–3375.

Fisman, Raymond, Daniel Paravisini, and Vikrant Vig. 2017. "Cultural proximity and loan outcomes." *The American Economic Review*, 107(2): 457–92.

Fujiwara, Thomas. 2015. "Voting technology, political responsiveness, and infant health: Evidence from Brazil." *Econometrica*, 83(2): 423–464.

Ghent, Andra C, Ruben Hernandez-Murillo, and Michael T Owyang. 2014. "Differences in subprime loan pricing across races and neighborhoods." *Regional Science and Urban Economics*, 48: 199–215.

Giannetti, Mariassunta, and Tracy Yue Wang. 2016. "Corporate scandals and household stock market participation." *The Journal of Finance*, 71(6): 2591–2636.

Guiso, Luigi, Tullio Jappelli, and Daniele Terlizzese. 1996. "Income risk, borrowing constraints, and portfolio choice." *The American Economic Review*, 158–172.

Hanson, Andrew, Zackary Hawley, Hal Martin, and Bo Liu. 2016. "Discrimination in mortgage lending: Evidence from a correspondence experiment." *Journal of Urban Economics*, 92: 48–65.

Haselmann, Rainer, David Schoenherr, and Vikrant Vig. 2018. "Rent seeking in elite networks." *Journal of Political Economy*, 126(4): 1638–1690.

Hjort, Jonas. 2014. "Ethnic divisions and production in firms." *The Quarterly Journal of Economics*, 129(4): 1899–1946.

Holmes, Andrew, and Paul Horvitz. 1994. "Mortgage redlining: Race, risk, and demand." *The Journal of Finance*, 49(1): 81–99.

Huang, Rocco R. 2008. "Evaluating the real effect of bank branching deregulation: Comparing contiguous counties across US state borders." *Journal of Financial Economics*, 87(3): 678–705.

Husted, Thomas A, and Lawrence W Kenny. 1997. "The Effect of the Expansion of the Voting Franchise on the Size of Government." *Journal of Political Economy*, 105(1): 54–82.

Jackson, Matthew O, Brian W Rogers, and Yves Zenou. 2017. "The economic consequences of social-network structure." *Journal of Economic Literature*, 55(1): 49–95.

Karlan, Dean S. 2007. "Social connections and group banking." *The Economic Journal*, 117(517): F52–F84.

Kimball, Miles S. 1993. "Standard risk aversion." *Econometrica*, 589–611.

Lemann, Nicholas. 2007. *Redemption: The last battle of the Civil War.* Macmillan.

Logan, Trevon D. 2020. "Do Black Politicians Matter? Evidence from Reconstruction." *The Journal of Economic History*, 80(1): 1–37.

Lott, Jr, John R, and Lawrence W Kenny. 1999. "Did women's suffrage change the size and scope of government?" *Journal of Political Economy*, 107(6): 1163–1198.

Mian, Atif, Amir Sufi, and Francesco Trebbi. 2010. "The political economy of the US mortgage default crisis." *The American Economic Review*, 100(5): 1967–98.

Mian, Atif, Amir Sufi, and Francesco Trebbi. 2013. "The Political Economy of the Subprime Mortgage Credit Expansion." *Quarterly Journal of Political Science*, 8(4): 373–408.

Miller, Grant. 2008. "Women's suffrage, political responsiveness, and child survival in American history." *The Quarterly Journal of Economics*, 123(3): 1287–1327.

Moehling, Carolyn M, and Melissa A Thomasson. 2012. "The political economy of saving mothers and babies: The politics of state participation in the Sheppard-Towner program." *The Journal of Economic History*, 72(1): 75–103.

Munnell, Alicia H, Geoffrey MB Tootell, Lynn E Browne, and James McEneaney. 1996. "Mortgage lending in Boston: Interpreting HMDA data." *The American Economic Review*, 25–53.

Naidu, Suresh. 2012. "Suffrage, schooling, and sorting in the post-bellum US South." National Bureau of Economic Research.

Perman, Michael. 2003. *Struggle for mastery: Disfranchisement in the South, 1888-1908.* Univ of North Carolina Press.

Pierson, Kawika, Michael L Hand, and Fred Thompson. 2015. "The government finance database: A common resource for quantitative research in public financial analysis." *PloS one*, 10(6): e0130119.

Pitts, Michael J. 2003. "Section 5 of the voting rights act: A once and future remedy." *Denv. UL Rev.*, 81: 225.

Rable, George C. 2007. *But there was no peace: The role of violence in the politics of Reconstruction.* University of Georgia Press.

Ross, Stephen L, Margery Austin Turner, Erin Godfrey, and Robin R Smith. 2008. "Mortgage lending in Chicago and Los Angeles: A paired testing study of the pre-application process." *Journal of Urban Economics*, 63(3): 902–919.

Schraufnagel, Scot, Michael J Pomante, and Quan Li. 2020. "Cost of Voting in the American States: 2020." *Election Law Journal: Rules, Politics, and Policy*, 19(4): 503–509.

Shayo, Moses. 2020. "Social identity and economic policy." *Annual Review of Economics*.

Sundstrom, William A. 2007. "The geography of wage discrimination in the pre-civil rights South." *Journal of Economic History*, 410–444.

Tingsten, Herbert. 1937. *Political behavior: Studies in election statistics.* PS King.

Tootell, Geoffrey MB. 1996. "Redlining in Boston: Do mortgage lenders discriminate against neighborhoods?" *The Quarterly Journal of Economics*, 111(4): 1049–1079.

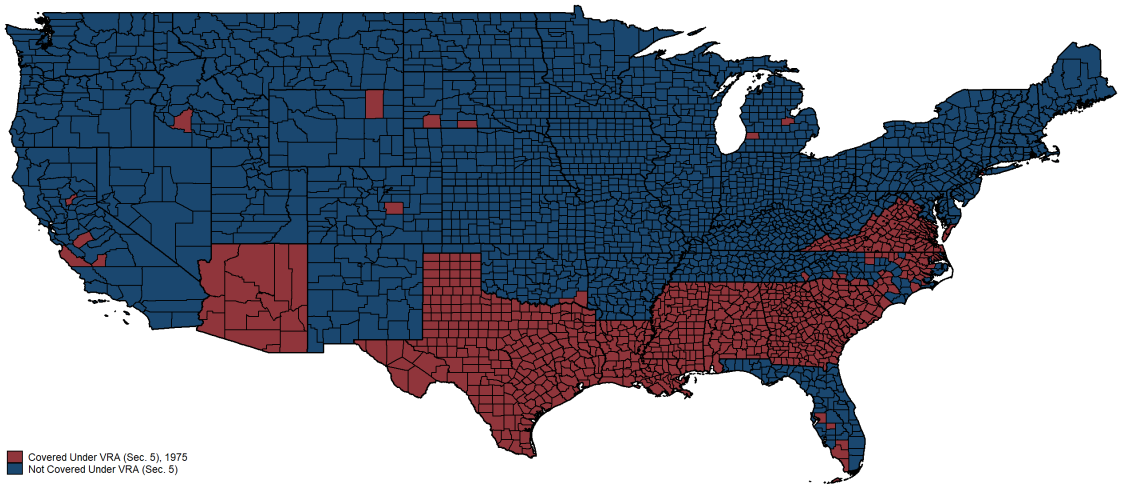
Valelly, Richard M. 2009. *The two reconstructions: The struggle for black enfranchisement.* University of Chicago Press.

Wanamaker, Marianne H. 2017. "150 Years of economic progress for African American men: measuring outcomes and sizing up roadblocks." *Economic History of Developing Regions*, 32(3): 211–220.

Weiser, Wendy, and Max Feldman. 2018. *The state of voting 2018.* Brennan Center for Justice at New York University School of Law.

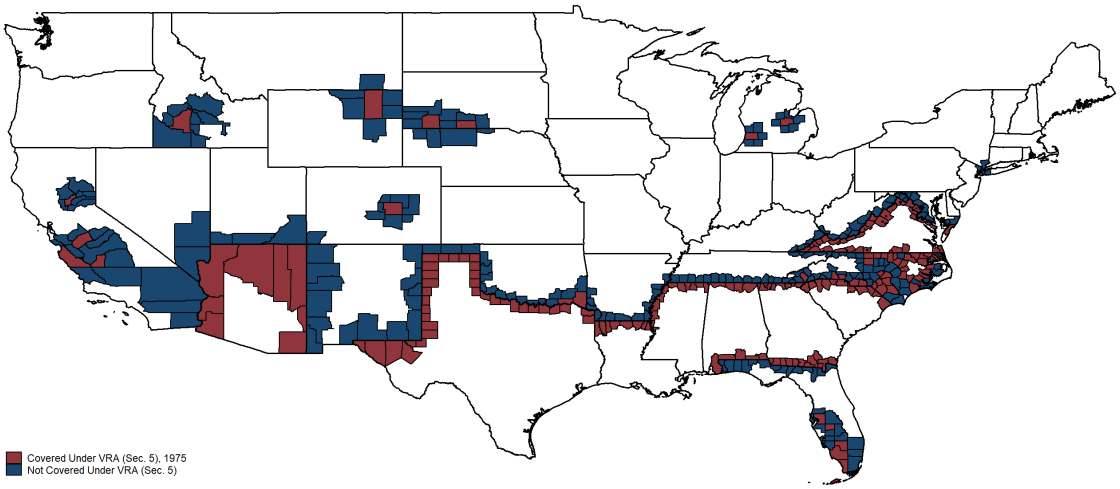
Woodward, C Vann. 1981. *Origins of the New South, 1877–1913: A history of the South.* Lsu Press.

Figure 1: Jurisdictions Under Preclearance Coverage



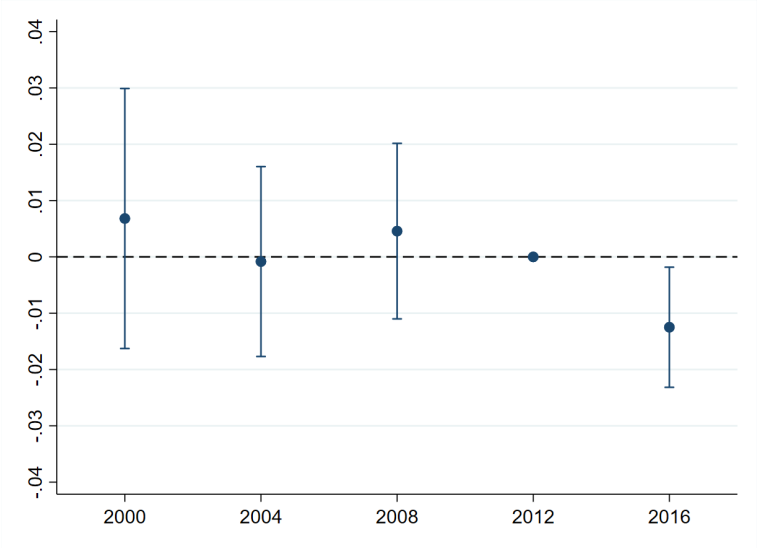
The figure shows all counties subject to preclearance under Section 5 of the Voting Rights Act by 1975. The counties covered under Section 5 of the VRA requires preclearance from either the US Attorney Gereral or the US District Court of DC. This list of counties covered under Section 5 of the VRA is obtained from the US Department of Justice. <LINK>

Figure 2: Sample of Bordering Counties Used in the Analysis



The figure shows the sample bordering counties used in the analysis. The covered counties were subject to preclearance under Section 5 of the Voting Rights Act by 1975. The counties covered under Section 5 of the VRA requires preclearance of all changes in voting laws from either the US Attorney Gereral or the US District Court of DC. This list of counties covered under Section 5 of the VRA is obtained from the US Department of Justice. <LINK> The uncovered counties in the immediate border of the covered counties are shown marked in navy blue color.

Figure 3: Black Voter Turnout and the Repeal of VRA

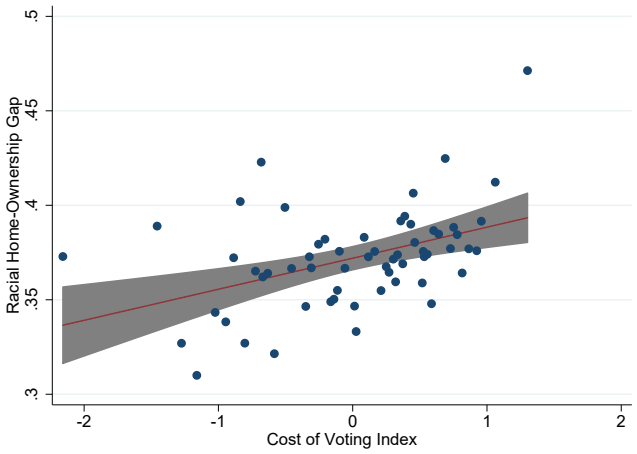


This figure uses county-level voter turnout data and plots coefficients $\{\beta_k\}$ from the specification

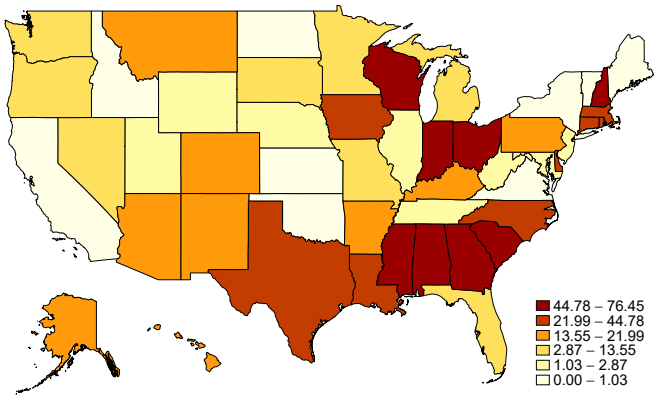
$$\text{Voter Turnout}_{ct} = \sum_{k=2000, k\neq 2012}^{2016} \beta_k \cdot Treat_c \cdot \text{High Black}_c \cdot 1(t = k) + \alpha_c + \alpha_{st} + \alpha_{hb,t} + \varepsilon_{ct}$$

and compares the presidential election voter turnouts in the VRA-treated counties to those in the control counties from 2000 to 2016. Subscript c, s, hb and t indicate county, state, high black, and year, respectively. The dependent variable Voter Turnout$_{ct}$ is the voter turnout in the presidential election in county c and year t, and $Treat_c$ is an indicator variable that takes one for VRA-treated counties. High Black is an indicator variable that takes one for all counties with more than median share of black population in 2010. The sample of treated and control counties is shown in Figure 2. α_c, α_{st} and $\alpha_{hb,t}$ represent county, state × year and High Black × year fixed effects, respectively. Regressions are weighted by the total county population in 2010. The unit of observation is county-year. Capped spikes drawn with the estimated coefficients $\{\beta_k\}$ indicate 95 percent confidence intervals obtained from standard errors clustered at the county level.

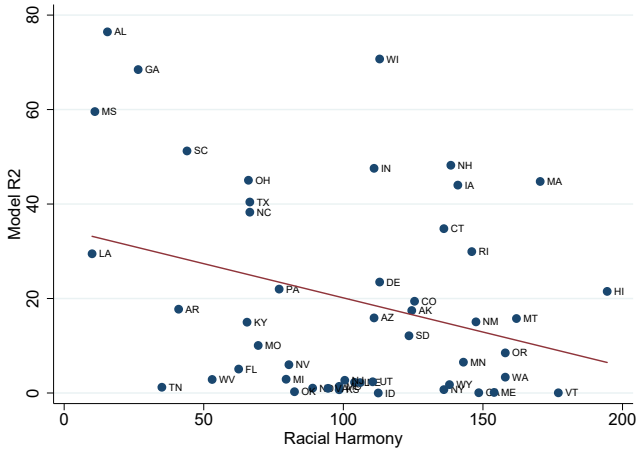
Figure 4: Racial Home-ownership Gap and the Cost of Voting Index



(a) Average Relationship



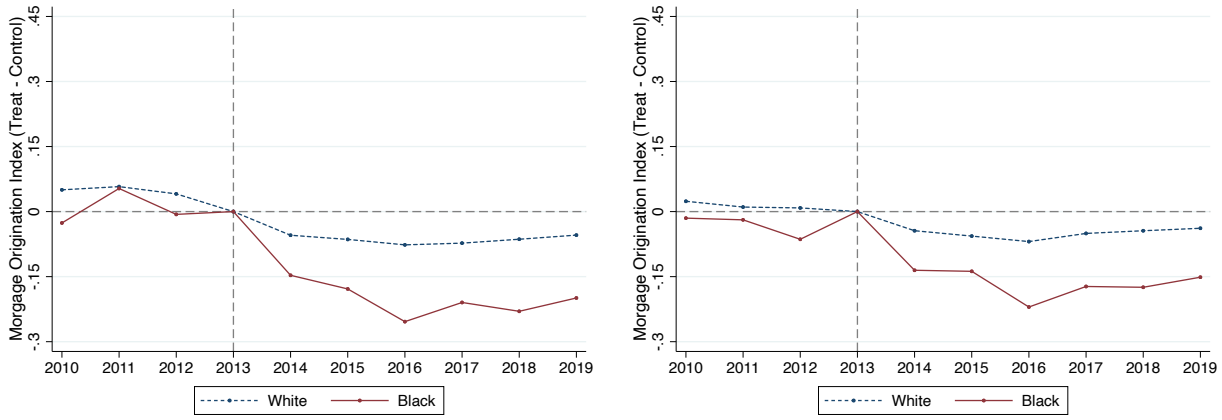
(b) R^2 for States



(c) R^2 and Measure of Racial Harmony

Figure 4a presents the relationship between the racial home-ownership gap between black and white Americans, and the cost of voting index (COVI) at the state level from 2009 to 2019. The data on the state-level racial home-ownership gap comes from the one-year summary files of the American Community Survey between 2009 and 2019. The annual state-level data on COVI between 2009 and 2019 comes from Schraufnagel, Pomante and Li (2020). Figure 4b presents heat-map of the model R^2, for each state, obtained from state-wise time-series regression of racial home-ownership gap on COVI. Figure 4c plots the the model R^2 against the racial harmony measure for each state. The model R^2 for each state comes from state-wise time-series regression of racial home-ownership gap on COVI. The measure of state-level racial harmony comes from Dougal et al. (2019).

Figure 5: Mortgage Origination and the Repeal of VRA

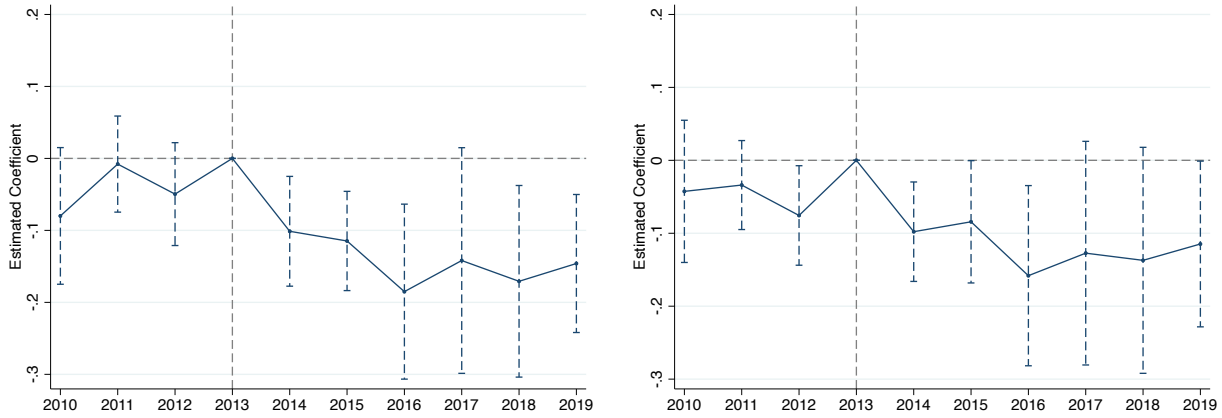


(a) Amount

(b) Number

This figure uses the HMDA data aggregated at the county-race-year level for the period 2010 to 2019 and plots the mortgage origination index for black and white Americans in treated counties relative to the control counties. Figure 5a uses the amount of mortgage origination, and figure 5b uses the number of mortgage origination. The mortgage origination index (Treat−Control) is computed by estimating the weighted average of the mortgage origination amount (Figure 5a) and number (Figure 5b) for black and white Americans in treated and control counties, and taking the difference between the two groups of counties for each race. The county population in 2010 is used as a weight. The sample of treated and control counties is shown in figure 2. The mortgage origination index is standardized to a value of 0 in 2013. The blue dashed line reports the mortgage origination index (Treat−Control) for the white borrowers, and the red solid line reports the mortgage origination index (Treat−Control) for the black borrowers.

Figure 6: Mortgage Origination and the Repeal of VRA



(a) Amount

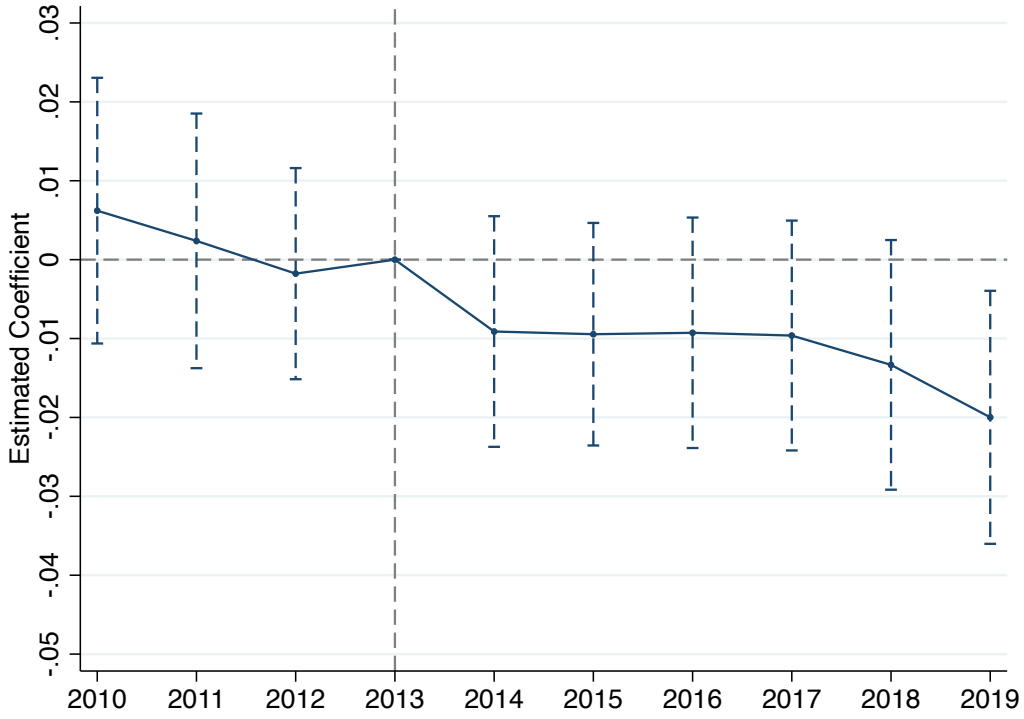
(b) Number

This figure uses the HMDA data aggregated at the county-race-year level for the period 2010 to 2019 and plots coefficients $\{\beta_k\}$ from the following specification:

$$ln(MortgageOrigination)_{rct} = \sum_{k=2010, k\neq 2013}^{2019} \beta_k \cdot Black_r \cdot Treat_c \cdot 1(t = k) + \alpha_{rc} + \alpha_{rt} + \alpha_{ct} + \varepsilon_{rct}$$

where subscript r, c and t indicate race, county, and year, respectively. $Black_r$ is an indicator variable that takes one for black borrowers, and $Treat_c$ is an indicator variable that takes one for VRA-treated counties. The sample of treated and control counties is shown in Figure 2. α_{rc}, α_{rt}, and α_{ct} represent race-county, race-year, and county-year fixed effects, respectively. As dependent variables, Figure 6a uses the natural logarithm of total mortgage origination amount for home purchases, and Figure 6b uses the natural logarithm of total mortgage origination number for home purchases. Regressions are weighted by the total county population in 2010. Capped spikes drawn with the estimated coefficients $\{\beta_k\}$ indicate 95 percent confidence intervals obtained from standard errors clustered at the county level.

Figure 7: Home Ownership Rate and The Repeal of VRA



This figure uses the state-level homeownership rate collected from the American Community Survey Summary File for the period 2010 to 2019 and plots coefficients $\{\beta_k\}$ from the following specification:

$$Homeownership\ Rate_{rst} = \sum_{k=2010, k\neq 2013}^{2019} \beta_k \cdot Black_r \cdot Treat_s \cdot 1(t = k) + \alpha_{rs} + \alpha_{rt} + \alpha_{st} + \varepsilon_{rst}$$

where subscript r, s and t indicate race, state, and year, respectively. $Black_r$ is an indicator variable that takes one for black borrowers, and $Treat_s$ is an indicator variable that takes one for VRA-treated states. Sample of states are restricted to bordering states. α_{rs}, α_{rt}, and α_{st} represent race-state, race-year, and state-year fixed effects, respectively. Regressions are weighted by the total state population in 2010. Capped spikes drawn with the estimated coefficients $\{\beta_k\}$ indicate 95 percent confidence intervals obtained from heteroskedasticity-robust standard errors.

Table 1: Summary Statistics

	# Obs	Mean	SD	P25	P50	P75
Panel A: Mortgage Market and Homeownership Rate						
Log Amount of Originations: Total	9,199	10.0084	2.5706	7.9725	9.9530	11.9035
Log Amount of Originations: Home Purchase	9,199	9.3157	2.6369	7.2971	9.2749	11.2639
Log Amount of Originations: Refinancing	9,199	9.1465	2.6268	7.1507	9.1129	11.0430
Log Number of Originations: Total	9,199	4.9123	2.3428	2.9957	4.8752	6.6529
Log Number of Originations: Home Purchase	9,199	4.2012	2.3826	2.3026	4.1744	6.0014
Log Number of Originations: Refinancing	9,199	4.1088	2.3839	2.1972	4.0775	5.8289
Log Amount of Application: Total	9,893	10.1560	2.5851	8.1438	10.0844	12.0326
Log Amount of Application: Home Purchase	9,893	9.3310	2.6680	7.2951	9.2749	11.2551
Log Amount of Application: Refinancing	9,893	9.4435	2.6285	7.4748	9.4307	11.3218
Log Number of Applications: Total	9,893	5.0953	2.3581	3.1781	5.0814	6.7991
Log Number of Applications: Home Purchase	9,893	4.2600	2.4028	2.3979	4.2195	6.0403
Log Number of Applications: Refinancing	9,893	4.4267	2.3948	2.5649	4.4543	6.1442
Denial Rate: Total	9,893	0.2400	0.1525	0.1628	0.2278	0.3109
Denial Rate: Home Purchase	9,893	0.1873	0.1685	0.0943	0.1476	0.2308
Denial Rate: Refinancing	9,893	0.3515	0.1878	0.2432	0.3229	0.4358
Homeownership Rate	900	0.5372	0.1441	0.4406	0.5300	0.6834
Panel B: Migration						
Log Outflow	3,526	-0.0010	0.9931	-0.7362	-0.1454	0.6417
Log Inflow	3,526	-0.0008	0.9936	-0.7194	-0.1394	0.6538
Log of Outflow/Inflow	3,526	-0.0002	0.1621	-0.0811	-0.0027	0.0807
(Outflow-Inflow)/Population in 2010	3,526	-0.0017	1.0004	-0.4122	0.0818	0.5489
Panel C: Labor Market and Hate Crime						
Log Hourly Wage	4,247	0.1284	0.4836	-0.1598	0.1660	0.4565
Log Hate Crime	2,090	0.4897	0.7157	0.0000	0.0000	0.6931
Panel D: Local Government Expenditure and Revenue						
Log Total Expenditure	2,542	12.1664	1.8331	10.7639	12.2002	13.3824
Log Education Expenditure	2,542	11.5450	1.6338	10.4201	11.5690	12.6031
Log Health Expenditure	2,542	6.1872	4.3003	0.0000	7.4289	9.6956
Log Utilities Expenditure	2,542	6.4369	5.0710	0.0000	8.0774	10.7548
Log Others Expenditure	2,542	10.5256	3.5343	9.1532	11.3355	12.8810
Log Total Revenue	2,542	11.4400	2.0888	9.8478	11.4849	12.8351
Log Intergovernmental Revenue	2,542	11.3571	1.6959	10.1760	11.3435	12.4782
Log Tax Revenue	2,542	10.8053	2.0026	9.3234	10.8348	12.2167
Log Property Tax Revenue	2,542	10.5658	1.9606	9.1546	10.5287	11.9153
Log Other Revenue	2,542	10.3623	2.4441	8.5560	10.5400	12.1632

This table presents summary statistics for the key outcome variables explored in this paper. Panel A reports the summary statistics for the mortgage market variables and homeownership rate. The mortgage market variables are collected from the HMDA data and are aggregated at the county-race-year level. The HMDA data are restricted to mortgage applications or origination for one-to-four family houses in the adjacent county pairs that straddle Section 5 state and county borders shown in Figure 2. The denial rate is defined as the ratio of the number of denied applications to the total number of applications. Homeownership rate is estimated from American Community Survey Summary File and is constructed at the state-race-year level for Section 5 states and their bordering states. Panel B reports the summary statistics for the variables in the IRS migration data aggregated at the county-year level. Panel C uses the individual-level Current Population Survey (CPS) data and reports summary statistics for the log hourly wages of the public-sector workers at the bordering counties. The sample is restricted to workers aged between 25 and 65 who work more than 32 hours per week for at least 40 weeks a year. Panel C also reports the summary statistics for the log number of hate crimes against black Americans in the bordering counties. The hate crime data is collected by the Federal Bureau of Investigation (FBI) under the Uniform Crime Reporting (UCR) Program. Finally, Panel D reports the summary statistics for local governments' expenditure and revenue at the county level. The data is collected from the US Census Bureau's Annual Survey of State and Local Government Finances. The sample of counties are restricted to the bordering counties.

Table 2: Voter Turnout and the Repeal of VRA

Dep Var: Voter Turnout	(1)	(2)	(3)
	High Black County	Low Black County	All Counties
Treat x Post	-0.0245**	-0.0048	-0.0048
	(0.0093)	(0.0066)	(0.0066)
High Black x Treat x Post			-0.0197*
			(0.0112)
County FE	Yes	Yes	Yes
State × Year FE	Yes	Yes	
High Black × State × Year FE			Yes
# Obs	316	1372	1688
Within R^2	0.0126	0.0019	0.0188

In this table, columns (1) and (2) report the coefficient β from the following specification separately for high black and low black counties:

$$\text{Voter Turnout}_{ct} = \beta \cdot Treat_c \times Post_t + \alpha_c + \alpha_{st} + \varepsilon_{ct}$$

and columns (3) report coefficients β from the following specification:

$$\text{Voter Turnout}_{ct} = \beta \cdot \text{High Black}_c \cdot Treat_c \times Post\text{-}Shelby_t + \alpha_c + \alpha_{hb,st} + \varepsilon_{ct}.$$

Subscript c, s, hb and t indicate county, state, high black, and year, respectively. The dependent variable Voter Turnout$_{ct}$ is the voter turnout in the presidential election in county c and year t between 2000 and 2016. High Black$_c$ is an indicator variable that takes one for all counties with more than median share of black population in 2010. $Treat_c$ is an indicator variable that takes one for VRA-treated counties. The sample of treated and control counties is shown in Figure 2. $Post\text{-}Shelby_t$ is an indicator variable that takes one for the 2016 (i.e. the first presidential election year after the repeal of VRA in 2013). α_c, α_{st}, and $\alpha_{hb,st}$ represent county, state × year, and high black × state × year fixed effects, respectively. All regressions are weighted by the total county population in 2010. Standard errors clustered at the state level are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Table 3: Mortgage Origination and the Repeal of VRA

	Total		Home Purchase		Refinancing	
	(1) Amount	(2) Number	(3) Amount	(4) Number	(5) Amount	(6) Number
Black x Treat x Post	-0.1431***	-0.1324***	-0.1286**	-0.1109*	-0.1289***	-0.1248***
	(0.0543)	(0.0479)	(0.0624)	(0.0616)	(0.0370)	(0.0353)
County x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
County x Race FE	Yes	Yes	Yes	Yes	Yes	Yes
Race x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Within R^2	0.0202	0.0219	0.0098	0.0092	0.0088	0.0120
# Obs	9,199	9,199	9,199	9,199	9,199	9,199

This table uses the HMDA data aggregated at the county-race-year level for the period 2010 to 2019 and reports coefficients β from the following specification:

$$ln(MortgageOrigination)_{rct} = \beta \cdot Black_r \cdot Treat_c \cdot Post\text{-}Shelby_t + \alpha_{rc} + \alpha_{rt} + \alpha_{ct} + \varepsilon_{rct}$$

where subscript r, c and t indicate race, county, and year, respectively. $Black_r$ is an indicator variable that takes one for black borrowers. $Treat_c$ is an indicator variable that takes one for VRA-treated counties. The sample of treated and control counties is shown in Figure 2. $Post\text{-}Shelby_t$ is an indicator variable that takes one for years from 2014. α_{rc}, α_{rt}, and α_{ct} represent race-county, race-year, and county-year fixed effects, respectively. As dependent variables, columns (1) and (2) use the natural logarithm of total mortgage originations that combines originations for home purchases and refinancing. Columns (3) and (4) use the natural logarithm of mortgage originations for home purchases, and columns (5) and (6) use the natural logarithm of mortgage originations for refinancing. Columns (1), (3), and (5) measure the mortgage originations in dollar amounts, and columns (2), (4), and (6) measure the mortgage originations in numbers. Standard errors clustered at the county level are reported in parentheses. All regressions are weighted by the total county population in 2010. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Table 4: Regression Discontinuity and Difference in Differences using Voter Turnout

	Regression Discontinuity		DDD Estimation	
	(1) Amount	(2) Number	(3) Amount	(4) Number
Treat	-0.2374**	-0.2049**		
	(0.1148)	(0.0896)		
Black x Treat x Post			-0.1446***	-0.1120***
			(0.0447)	(0.0420)
County x Year FE			Yes	Yes
County x Race FE			Yes	Yes
Race x Year FE			Yes	Yes
Within R^2	–	–	0.0074	0.0060
# Obs	164	164	6,046	6,046

In this table, columns (1) and (2) use the HMDA data aggregated at the county level and report the coefficient β from the specification:

$$Growth_{c,B,1316} - Growth_{c,W,1316} = \alpha + \beta \cdot Treat_c + \gamma_1 \cdot Turnout_c + \gamma_2 \cdot Treat_c \cdot Turnout_c + \varepsilon_c,$$

and columns (3) and (4) use the HMDA data aggregated at the county-race-year level for the period 2010 to 2019 and report coefficients β from the following specification:

$$ln(MortgageOrigination)_{rct} = \beta \cdot Black_r \cdot Treat_c \cdot Post\text{-}Shelby_t + \alpha_{rc} + \alpha_{rt} + \alpha_{ct} + \varepsilon_{rct}$$

where subscript B, W, r, c and t indicate black Americans, white Americans, race, county, and year, respectively. $Treat_c$ is an indicator variable that takes one for counties whose voter turnout in the 1964 Presidential election is greater than 45% but less than 50% and zero for counties whose voter turnout in the 1964 Presidential election is greater than 50% but less than 55%. The sample of treated and control counties is shown in Figure C.5. $Turnout_c$ is voter turnout in the 1964 Presidential election. $Black_r$ is an indicator variable that takes one for black borrowers, and $Post\text{-}Shelby_t$ is an indicator variable that takes one for years from 2014. α_{rc}, α_{rt}, and α_{ct} represent race-county, race-year, and county-year fixed effects, respectively. As dependent variables, columns (1) and (2) use the growth rate of home-purchase mortgage originations for black Americans from 2013 to 2016 minus the growth rate of home-purchase mortgage originations for white Americans from 2013 to 2016, and columns (3) and (4) use the natural logarithm of total mortgage originations for home purchases. Column (1) and (3) measure the mortgage origination in dollar amounts, and column (2) and (4) measure the mortgage origination in numbers. In parentheses, columns (1) and (2) report heteroskedasticity-robust standard errors, and columns (3) and (4) report standard errors clustered at the county level. All regressions are weighted by the total county population in 2010. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Table 5: Robustness: Baseline Estimation using Hinterland and Treated Counties

	Total		Home Purchase		Refinancing	
	(1) Amount	(2) Number	(3) Amount	(4) Number	(5) Amount	(6) Number
Black x Treat x Post	-0.2272***	-0.2245***	-0.1723***	-0.1516**	-0.1553***	-0.1624***
	(0.0536)	(0.0478)	(0.0593)	(0.0587)	(0.0489)	(0.0442)
County x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
County x Race FE	Yes	Yes	Yes	Yes	Yes	Yes
Race x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Within R^2	0.0525	0.0633	0.0181	0.0182	0.0123	0.0189
# Obs	9,365	9,365	9,365	9,365	9,365	9,365

This table uses the HMDA data aggregated at the county-race-year level for the period 2010 to 2019 and reports coefficients β from the following specification:

$$ln(MortgageOrigination)_{rct} = \beta \cdot Black_r \cdot Hinter\text{-}Treat_c \cdot Post\text{-}Shelby_t + \alpha_{rc} + \alpha_{rt} + \alpha_{ct} + \varepsilon_{rct}$$

where subscript r, c and t indicate race, county, and year, respectively. $Black_r$ is an indicator variable that takes one for black borrowers. $Hinter\text{-}Treat_c$ is an indicator variable that takes one for VRA-treated counties and zero for the uncovered hinterland counties immediately bordering the original control counties. The sample of treated and control counties is shown in Figure C.7. $Post\text{-}Shelby_t$ is an indicator variable that takes one for years from 2014. α_{rc}, α_{rt}, and α_{ct} represent race-county, race-year, and county-year fixed effects, respectively. As dependent variables, columns (1) and (2) use the natural logarithm of total mortgage originations that combines originations for home purchases and refinancing. Columns (3) and (4) use the natural logarithm of mortgage originations for home purchases, and columns (5) and (6) use the natural logarithm of mortgage originations for refinancing. Columns (1), (3), and (5) measure the mortgage originations in dollar amounts, and columns (2), (4), and (6) measure the mortgage originations in numbers. Standard errors clustered at the county level are reported in parentheses. All regressions are based on the HMDA data from 2010 to 2019 and are weighted by the total county population in 2010. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Table 6: Mortgage Application and the Repeal of VRA

	Total		Home Purchase		Refinancing	
	(1) Amount	(2) Number	(3) Amount	(4) Number	(5) Amount	(6) Number
Black x Treat x Post	-0.1594***	-0.1498***	-0.1318**	-0.1153*	-0.1528***	-0.1431***
	(0.0566)	(0.0467)	(0.0619)	(0.0592)	(0.0419)	(0.0343)
County x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
County x Race FE	Yes	Yes	Yes	Yes	Yes	Yes
Race x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Within R^2	0.0261	0.0300	0.0097	0.0097	0.0138	0.0178
# Obs	9,893	9,893	9,893	9,893	9,893	9,893

This table uses the HMDA data aggregated at the county-race-year level for the period 2010 to 2019 and reports coefficients β from the following specification:

$$ln(MortgageApplication)_{rct} = \beta \cdot Black_r \cdot Treat_c \cdot Post\text{-}Shelby_t + \alpha_{rc} + \alpha_{rt} + \alpha_{ct} + \varepsilon_{rct}$$

where subscript r, c and t indicate race, county, and year, respectively. $Black_r$ is an indicator variable that takes one for black borrowers. $Treat_c$ is an indicator variable that takes one for VRA-treated counties. The sample of treated and control counties is shown in Figure 2. $Post\text{-}Shelby_t$ is an indicator variable that takes one for years from 2014. α_{rc}, α_{rt}, and α_{ct} represent race-county, race-year, and county-year fixed effects, respectively. As dependent variables, columns (1) and (2) use the natural logarithm of total mortgage applications that combines applications for home purchases and refinancing. Columns (3) and (4) use the natural logarithm of mortgage applications for home purchases, and columns (5) and (6) use the natural logarithm of mortgage applications for refinancing. Columns (1), (3), and (5) measure the mortgage applications in dollar amounts, and columns (2), (4), and (6) measure the mortgage applications in numbers. Standard errors clustered at the county level are reported in parentheses. All regressions are weighted by the total county population in 2010. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Table 7: Denial Rate and the Repeal of VRA

Dep Var: Denial Rate	(1)	(2)	(3)
	Total	Home Purchase	Refinancing
Black x Treat x Post	-0.0119	-0.0013	-0.0145
	(0.0078)	(0.0063)	(0.0091)
County x Year FE	Yes	Yes	Yes
County x Race FE	Yes	Yes	Yes
Race x Year FE	Yes	Yes	Yes
Within R^2	0.0011	0.0000	0.0010
# Obs	9,893	9,893	9,893

This table uses the HMDA data aggregated at the county-race-year level for the period 2010 to 2019 and reports coefficients β from the following specification:

$$DenialRate_{rct} = \beta \cdot Black_r \cdot Treat_c \cdot Post\text{-}Shelby_t + \alpha_{rc} + \alpha_{rt} + \alpha_{ct} + \varepsilon_{rct}$$

where subscript r, c and t indicate race, county, and year, respectively. $Black_r$ is an indicator variable that takes one for black borrowers. $Treat_c$ is an indicator variable that takes one for VRA-treated counties. The sample of treated and control counties is shown in Figure 2. $Post\text{-}Shelby_t$ is an indicator variable that takes one for years from 2014. α_{rc}, α_{rt}, and α_{ct} represent race-county, race-year, and county-year fixed effects, respectively. As dependent variables, column (1) uses the mortgage application denial rate for either home purchases or refinancing. Columns (2) and (3) use the denial rate for home purchases and refinancing, respectively. The mortgage application denial rate is defined as the ratio of the number of denied applications to the total number of applications. Standard errors clustered at the county level are reported in parentheses. All regressions are weighted by the total county population in 2010. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Table 8: Migration and the Repeal of VRA

	(1)	(2)	(3)	(4)
	Ln(Outflow)	Ln(Inflow)	Ln($\frac{Outflow}{Inflow}$)	$\frac{Outflow-Inflow}{Pop_{2010}}$
High Black × Treat × Post	-0.0209	-0.0232	0.0023	0.0219
	(0.0308)	(0.0322)	(0.0246)	(0.2422)
Treat × Post	-0.0402***	-0.0229	-0.0173	-0.0632
	(0.0125)	(0.0156)	(0.0163)	(0.1839)
County FE	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes
High Black × Year FE	Yes	Yes	Yes	Yes
Within R^2	0.0128	0.0124	0.0000	0.0057
# Obs	3,526	3,526	3,526	3,526

This table uses IRS's county-level migration data and reports coefficients β from the following specification:

$$y_{ct} = \beta_1 \cdot High\text{-}Black_c \cdot Treat_c \cdot Post\text{-}Shelby_t + \beta_2 \cdot Treat_c \cdot Post\text{-}Shelby_t + \alpha_c + \alpha_{st} + \alpha_{hb,t} + \varepsilon_{ct}$$

where the subscripts c, hb and t indicate county, high black, and year, respectively. $High\text{-}Black_c$ is an indicator variable that takes one for counties with more than median share of black population in 2010. $Treat_s$ is an indicator variable that takes one for VRA-treated states. The sample of treated and control counties is shown in Figure 2. $Post\text{-}Shelby_t$ is an indicator variable that takes one for years from 2014. α_c, α_{st} and $\alpha_{hb,t}$ represent county. state × year, and high black × year fixed effects, respectively. Column (1) and (2) use the natural logarithms of inflow and outflow as the dependent variable, respectively. Column (3) uses the natural logarithm of the ratio of outflow to inflow, and the column (4) uses migration (i.e., outflow minus inflow) as a share of population in 2010 as the dependent variable. Inflow refers to the number of new individuals who filed the income tax returns in a particular county and year. Outflow refers to number of individuals who had filed the the income tax return in a county in the previous year, but filed in a different county in a given year. Standard errors clustered at the county level are reported in the parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Table 9: Changes in Racial Wage Gap and the Repeal of VRA

Sample	Estimate of β	# Obs	Within R^2
All Employees	-0.1012	4,247	0.0003
	(0.1051)		
Existing Employees	-0.0514	3,956	0.0001
	(0.1038)		
New Hires	-0.1129*	224	0.0001
	(0.0541)		
% Black Population ∈ [0-20%)	0.0106	1,532	0.0000
	(0.2080)		
% Black Population ∈ [20-39%)	-0.1280**	535	0.0007
	(0.0548)		
% Black Population ∈ [40% +)	-0.4839***	2,180	0.0045
	(0.0015)		

This table uses individual level wage data from the Current Population Survey (CPS) between 2008 and 2017 and reports the coefficient β from the following specification:

$$ln(Wage)_{ict} = \beta \cdot Black_i \cdot Treat_c \cdot Post\text{-}Shelby_t + \alpha_{rc} + \alpha_{rt} + \alpha_{ct} + \varepsilon_{ict}$$

where i, c, and t indicate individual, county, and year, respectively. The dependent variable $ln(Wage)_{ict}$ is the natural logarithm of wages for individual i in county c and year t. $Black_i$ in an indicator variable that takes one for a black American. $Treat_c$ is an indicator variable that takes one for VRA-treated counties, and $Post\text{-}Shelby_t$ is an indicator variable that takes one for years from 2014. α_{rc}, α_{rt}, and α_{ct} represent race-county, race-year, and county-year fixed effects, respectively. The sample includes individuals between ages 25 and 65 who work more than 32 hours per week for at least 40 weeks a year. The sample includes only state and federal public sector employees. The sample counties used in this table are shown in Figure 2. Each row in table reports the point estimate of β obtained from a different sample. The first row uses the sample of all employees in the sample, the second row uses the sample of all existing employees, the third row uses the sample of new hires, and the fourth, the fifth, and the sixth row uses sample for all employees in counties with varying levels of black population pre Shelby ruling. Row four includes counties if their pre Shelby percent black population in 2012 was between 0% and 19%. Row five includes counties if their pre Shelby percent black population in 2012 was between 20% and 39%. Row five includes counties if their pre Shelby percent black population in 2012 was greater than equal to 40%. Columns (3) and (4) reports the number of observations used in the regression and the model R^2, respectively. All regressions are weighted by the survey weights in the CPS, which is the inverse probability of being selected into the survey. Standard errors clustered at the county level are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Table 10: Investment by Asset Type and the Repeal of VRA

	All States		Bordering States	
	Risky	Risk Free	Risky	Risk Free
	(1)	(2)	(3)	(4)
Black x Treat x Post	-0.0208**	0.0127**	-0.0188*	0.0131*
	(0.00902)	(0.00586)	(0.00937)	(0.00730)
State x Year FE	Yes	Yes	Yes	Yes
State x Race FE	Yes	Yes	Yes	Yes
Race x Year FE	Yes	Yes	Yes	Yes
Mean	.0691	.0254	.0677	.0231
Std.Dev	.1025	.0611	.1059	.0617
Within R^2	0.0380	0.0429	0.0393	0.0517
# Obs	306	210	220	170

This table uses the Survey of Income and Program Participation (SIPP) data summarized at the state-race-year level for the period 2010 to 2016 and reports coefficients β from the following specification:

$$\textit{Share of households who invest}_{rst} = \beta \cdot Black_r \cdot Treat_s \cdot \textit{Post-Shelby}_t + \alpha_{st} + \alpha_{sr} + \alpha_{rt} + \varepsilon_{rst}$$

where subscript r, s and t indicate race, state, and year, respectively. $Black_r$ is an indicator variable that takes one for black borrowers, and $Treat_s$ is an indicator variable that takes one for VRA-treated states (i.e., AL, AZ, GA, LA, MS, SC, TX, and VA). $\textit{Post-Shelby}_t$ is an indicator variable that takes one for years from 2014. α_{rs}, α_{rt}, and α_{st} represent race-state, race-year, and state-year fixed effects, respectively. As dependent variables, columns (1) and (3) use the share of households in the SIPP who invests in long-term risky assets (e.g., mutual funds), columns (2) and (4) use the share of households in the SIPP who invests in risk-free securities (e.g., Treasuries). Sample states used in columns (1) and (2) include all states except Alaska, and sample states used in columns (3) and (4) are VRA-treated states and their immediate bordering states. Standard errors clustered at the state level are reported in parentheses. All regressions are weighted by the total state population represented by the SIPP respondents in 2010. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Table 11: Hate Crime and the Repeal of VRA

	(1) OLS	(2) OLS	(3) Poisson	(4) Poisson	(5) OLS
Treat x Post	0.2244**	0.2914***	0.2173***	0.2601***	0.1611*
	(0.1002)	(0.1049)	(0.0690)	(0.0665)	(0.0966)
Sample	All States	Border States	All States	Border States	Border Counties
State/County FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Within R^2	0.0192	0.0398	-	-	0.0056
# Obs	490	290	490	290	2,090

This table uses the FBI's hate crime statistics summarized at the state (column (1) through (4)) and county (column (5)) level for the period 2010 to 2019 and reports coefficients β from the following specification:

$$y_{c(s)t} = \beta \cdot Treat_{c(s)} \cdot Post\text{-}Shelby_t + \alpha_{c(s)} + \alpha_t + \varepsilon_{c(s)t}$$

where subscript c, s, and t indicate county, state, and year, respectively. $Treat_{c(s)}$ is an indicator variable that takes one for VRA-treated counties (states). The sample of treated and control counties (states) is shown in Figure 2 (Figure 1). $Post\text{-}Shelby_t$ is an indicator variable that takes one for years from 2014 . As dependent variables, columns (1), (2) and (5) use the natural logarithm of one plus the number of hate crime against black Americans, and columns (3) and (4) use the raw number of hate crime against black Americans. Columns (1), (2), and (5) report OLS estimates, and columns (3) and (4) report Poisson estimates. Regressions in columns (1) through (4) are weighted by the total state population in 2010, and regression in column (5) is weighted by the total county population in 2010. Standard errors clustered at the state in columns (1)-(4) and county level in column (5), are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Table 12: Expenditure and Revenue of Local Government

Panel A: Expenditure					
	(1)	(2)	(3)	(4)	(5)
	Total	Education	Health	Utilities	Others
High Black x Treat x Post	-0.0937**	-0.0789	-0.6338**	-0.2568	-0.1009
	(0.0421)	(0.0714)	(0.2466)	(0.3664)	(0.0803)
Treat x Post	0.0417**	0.0577***	-0.1312	0.0438	-0.0038
	(0.0194)	(0.0175)	(0.1238)	(0.0878)	(0.0358)
County FE	Yes	Yes	Yes	Yes	Yes
High Black x Year FE	Yes	Yes	Yes	Yes	Yes
# Obs	2,542	2,542	2,542	2,542	2,542
Within R^2	0.0163	0.0119	0.0326	0.0012	0.0017

Panel B: Revenue					
	(1)	(2)	(3)	(4)	(5)
	Total Revenue	Intergovernmental Revenue	Tax Revenue	Property Tax Revenue	Other Revenue
High Black x Treat x Post	-0.0054	-0.0735	-0.0097	0.0278	-0.0053
	(0.0585)	(0.0812)	(0.0733)	(0.0593)	(0.1578)
Treat x Post	0.0186	0.0757***	0.0228	0.0346	-0.0074
	(0.0284)	(0.0185)	(0.0245)	(0.0240)	(0.0527)
County FE	Yes	Yes	Yes	Yes	Yes
High Black x Year FE	Yes	Yes	Yes	Yes	Yes
# Obs	2,542	2,542	2,542	2,542	2,542
Within R^2	0.0013	0.0089	0.0322	0.0089	0.0001

This table uses the county-level revenue and expenditure data from 2010 to 2018 and reports β_1 and β_2 in the following specification:

$$y_{ct} = \beta_1 \cdot High\text{-}Black_c \cdot Treat_c \cdot Post\text{-}Shelby_t + \beta_2 \cdot Treat_c \cdot Post\text{-}Shelby_t + \alpha_c + \alpha_t \cdot High\text{-}Black_c + \varepsilon_{ct}$$

where c and t indicate county and year, respectively. $Treat_c$ is an indicator variable that takes one for VRA-treated counties. The sample of treated and control counties is shown in Figure 2. $Post\text{-}Shelby_t$ is an indicator variable that takes one for years from 2014. $High\text{-}Black_c$ is an indicator variable that takes one for counties with more than median share of black population in 2010. α_c and $\alpha_t \cdot High\text{-}Black_c$ represent county and high black county-year fixed effects, respectively. Panel A reports the results for local government expenditure. Column (1), (2), (3), (4), and (5) of panel A uses the natural logarithm of local government's total direct expenditure, expenditure on education, health, utilities and others, respectively, as the dependent variable. Panel B reports the results for revenues of local government. Column (1), (2), (3), (4), and (5) of panel B uses uses the natural logarithm of local government's total revenues, revenues through intergovernmental transfers, taxes, property taxes, and other sources, respectively by the local government, as the dependent variable. The unit of observation is county-year spanning from 2010 until 2018. All regressions are weighted by 2010 county population. Standard errors in the regressions are clustered at the county level and reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Table 13: Mortgage Applications by Lender Type after the Repeal of VRA

	Non-Black Lenders				Black Lenders			
	(1) Amount	(2) Number	(3) Amount	(4) Number	(5) Amount	(6) Number	(7) Amount	(8) Number
Black x Treat x Post	-0.1233**	-0.0749*	-0.1827**	-0.1272**	0.1463**	0.1415**	0.0789	0.0753
	(0.0549)	(0.0442)	(0.0765)	(0.0589)	(0.0726)	(0.0638)	(0.0781)	(0.0624)
County x Year FE	Yes	Yes			Yes	Yes		
Year x Race FE	Yes	Yes			Yes	Yes		
County x Race FE	Yes	Yes			Yes	Yes		
Lender x County x Year FE			Yes	Yes			Yes	Yes
Lender x County x Race FE			Yes	Yes			Yes	Yes
Lender x Race x Year FE			Yes	Yes			Yes	Yes
Within R^2	0.0001	0.0000	0.0016	0.0011	0.0001	0.0002	0.0004	0.0005
# Obs	333,169	333,169	128,306	128,306	49,015	49,015	24,467	24,467

This table uses the HMDA data aggregated at the county-lender-race-year level for the period 2010 to 2019 and reports coefficients β from the following specification:

$$ln(MortgageApplication)_{lrct} = \beta \cdot Black_r \cdot Treat_c \cdot Post\text{-}Shelby_t + \alpha_{lrc} + \alpha_{lrt} + \alpha_{lct} + \varepsilon_{lrct}$$

where subscript l, r, c and t indicate mortgage lender, race, county, and year, respectively. $Black_r$ is an indicator variable that takes one for black borrowers. $Treat_c$ is an indicator variable that takes one for VRA-treated counties. The sample of treated and control counties is shown in Figure 2. $Post\text{-}Shelby_t$ is an indicator variable that takes one for years from 2014. α_{lrc}, α_{lrt}, and α_{lct} represent lender-race-county, lender-race-year, and lender-county-year fixed effects, respectively. As dependent variables, columns (1), (3), (5), and (7) use the natural logarithm of the total dollar amount of mortgage applications for home purchases, and columns (2), (4), (6), and (8) use the natural logarithm of the total number of mortgage applications for home purchases. Columns through (1) through (4) restrict samples to non-black lenders, and columns through (5) through (8) restrict samples to black lenders. Lenders are defined as black lenders if they are above the 90th percentile when sorted by the share of black borrowers in their mortgage lending portfolio in 2010 to 2013. Non-black lenders are lenders below the 90th percentile of the share of black borrowers in their mortgage lending portfolio. Non-black lenders who received less than 50 applications in 2010 to 2013 or whose total assets are above the 95th percentile are excluded. All regressions are based on the HMDA data from 2010 to 2019 and are weighted by the total application numbers that each lender received in 2010. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Table 14: Mortgage Originations and Denial by Black Lenders

	Origination Amount		Origination Number		Denial Rate	
	(1)	(2)	(3)	(4)	(5)	(6)
Black x Treat x Post	0.0669	0.0474	0.1157*	0.0579	0.0276	0.0243
	(0.0715)	(0.0781)	(0.0648)	(0.0650)	(0.0263)	(0.0294)
County x Year FE	Yes		Yes		Yes	
County x Race FE	Yes		Yes		Yes	
Race x Year FE	Yes		Yes		Yes	
Lender x County x Year FE		Yes		Yes		Yes
Lender x County x Race FE		Yes		Yes		Yes
Lender x Race x Year FE		Yes		Yes		Yes
Within R^2	0.0000	0.0002	0.0001	0.0003	0.0001	0.0003
# Obs	42,150	20,739	42,150	20,739	49,015	24,467

This table uses the HMDA data aggregated at the county-lender-race-year level for the period 2010 to 2019 and reports coefficients β from the following specification:

$$y_{lrct} = \beta \cdot Black_r \cdot Treat_c \cdot Post\text{-}Shelby_t + \alpha_{lrc} + \alpha_{lrt} + \alpha_{lct} + \varepsilon_{lrct}$$

where subscript l, r, c and t indicate lender, race, county, and year, respectively. $Black_r$ is an indicator variable that takes one for black borrowers. $Treat_c$ is an indicator variable that takes one for VRA-treated counties. The sample of treated and control counties is shown in Figure 2. $Post\text{-}Shelby_t$ is an indicator variable that takes one for years from 2014. α_{lrc}, α_{lrt}, and α_{lct} represent lender-race-county, lender-race-year, and lender-county-year fixed effects, respectively. As dependent variables, columns (1) and (2) use the natural logarithm of the total dollar amount of mortgage originations for home purchases, columns (3) and (4) use the natural logarithm of the total number of mortgage originations for home purchases, columns (5) and (6) use the denial rate, defined as the ratio of the number of denied applications to the total number of applications. Sample of lenders includes only black lenders, defined as lenders above the 90th percentile when sorted by the share of black borrowers in their mortgage lending portfolio in 2010 to 2013. All regressions are based on the HMDA data from 2010 to 2019 and are weighted by the total application numbers that each lender received in 2010. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Online Appendix for:

"Financial Implications of Disenfranchisement: Evidence from the Repeal of Voting Rights Act"

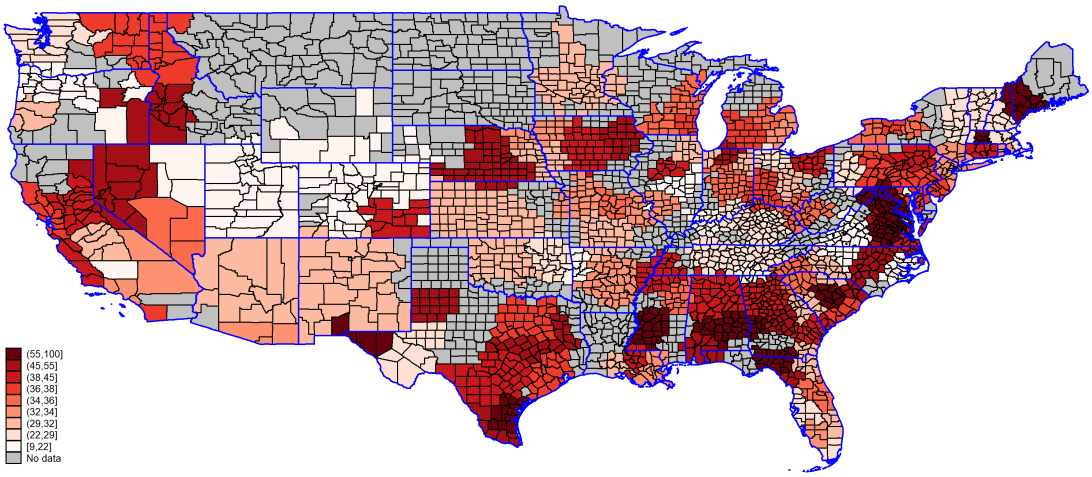
Appendix A Background and Enactment of the VRA

The years following the enactment of the three reconstruction amendments – the thirteenth, the fourteenth, and the fifteenth amendments – were marked by active involvement of the black American population in politics, including holding of public offices, and their economic prosperity (Logan (2020)). The increasing political and economic involvement of the black Americans led to a wide-spread campaign among southern whites to overturn the Reconstruction era policies. This movement of re-establishing the antebellum racial hierarchy is referred to as the Southern Redemption. Several works including Woodward (1981), Ayers (2007), Lemann (2007), and Rable (2007) among others have noted that the Southern Redemption was concentrated on reducing black political involvement both through laws and intimidation. As a result southern state legislatures enacted several laws between the late 19th and the early 20th century, referred to as the "Jim Crow" laws, to impose *de-facto* suffrage restrictions on black Americans.

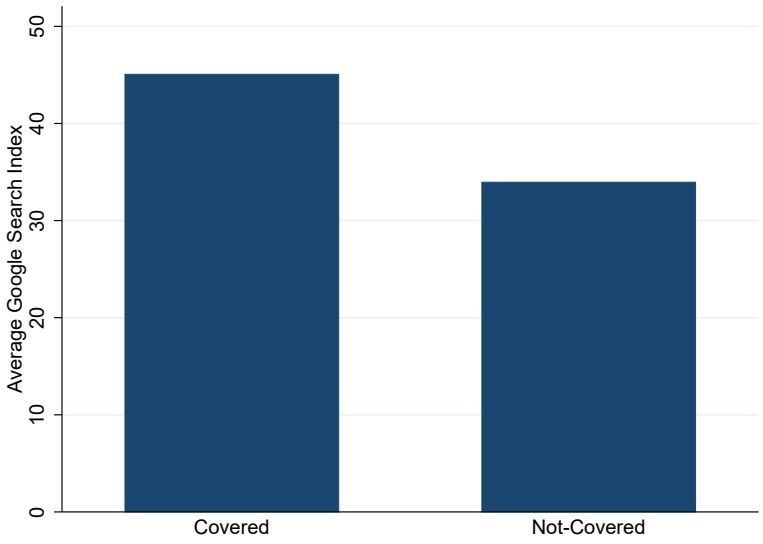
The goals of these laws were achieved through imposition of poll taxes, literacy tests administered in a discriminatory manner by county officials, whites-only party primaries etc. which were unduly burdensome to the black Americans.[26] Valelly (2009) notes that these restrictions disenfranchised most eligible black Americans before the civil rights era. Furthermore, these restrictions contributed to the decline in the social and economic status of black Americans (Sundstrom (2007), Wanamaker (2017), Logan (2020)).

[26] We direct the readers to Perman (2003) for an extensive discussion on the disenfranchisement of black Americans in the South during this period.

Figure A.1: Google Search for Voting Rights Act



(a) Across Counties

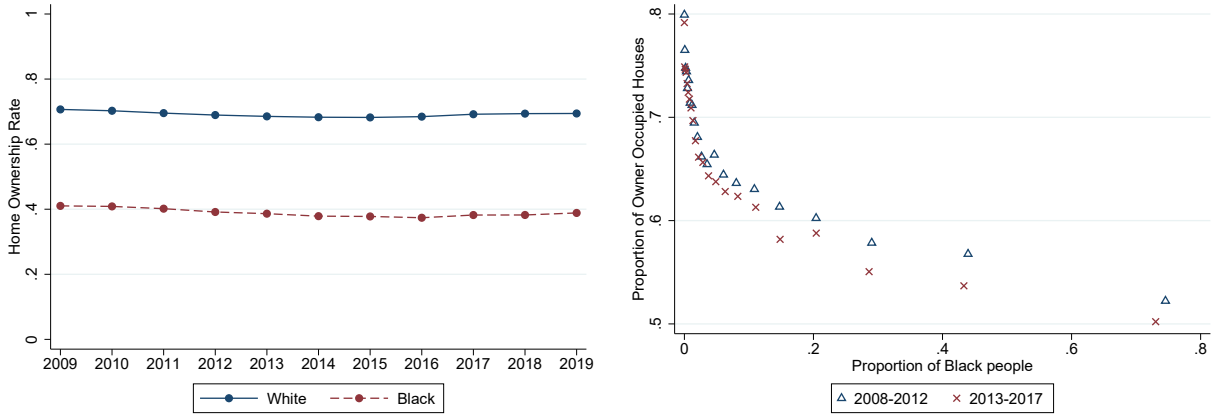


(b) Covered vs Uncovered Counties

This figure plots the geographic dispersion in the google search index for the term “Voting Rights Act" from January 1, 2012 until December 30, 2014. Figure A.1a plots the heat map for google search index across different counties. Counties with no data have very low search traffic for the term “Voting Rights Act". Figure A.1b plots the average search index for the term “Voting Rights Act" for counties covered and not-covered by the Section 5 of VRA. The t-statistic for the equality of the average search index across covered and uncovered counties is 19 and significant at 1% level.

Appendix B Aggregate Facts

Figure B.1: Racial Homeownership Gap

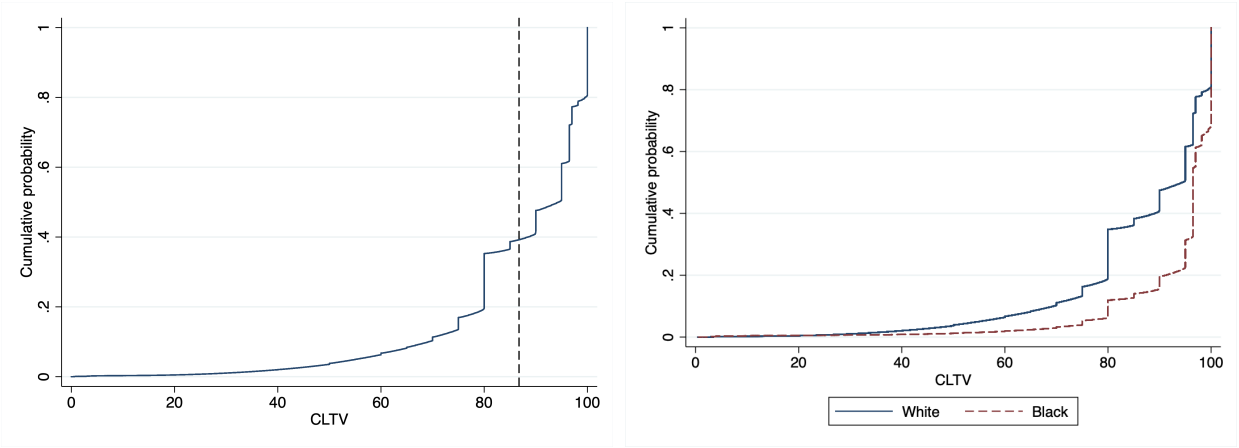


(a) Time Series

(b) Cross Section

The figure presents the racial gap in home-ownership rates between black and white Americans. Figure B.1a plots the time-series of aggregate home-ownership rates for black and white Americans between 2009 and 2019. The panel B.1a is based on the one-year summary files of the American Community Survey between 2009 and 2019. The blue solid line reports the home-ownership rate for white Americans and the red dashed line reports the home-ownership rates for black Americans. Figure B.1b reports the correlation between the proportion of owner-occupied households and the proportion of black households at the ZIP Code Tabulation Area (ZCTA) level. The panel B.1b is based on the five-year summary files of the American Community Survey for two waves 2008-2012 and 2013-2017. The blue triangle and the red cross indicate the bivariate relationship between proportion of owner occupying households and proportion of black households at the ZCTA level for the 2008-2012 wave and the 2013-2017 wave, respectively.

Figure B.2: Distribution of combined loan-to-value (CLTV) ratios by Race



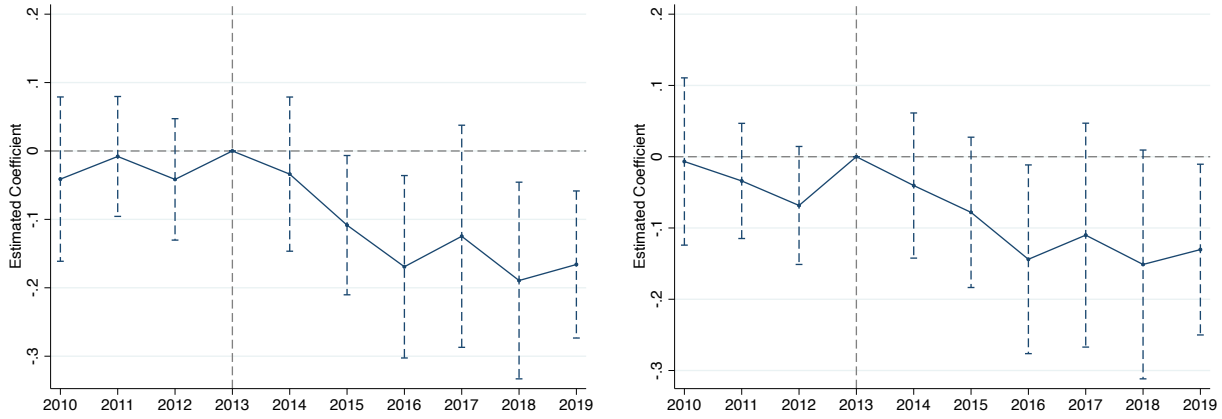
(a) CLTV

(b) CLTV by Race

This figure plots the cumulative distribution of combined loan-to-value (CLTV) ratios for originated home-purchase mortgage loans in the 2019 HMDA data. Figure B.2a plots the cumulative CLTV distribution for all borrowers, and figure B.2b plots the cumulative CLTV distribution for white and black borrowers separately. The dashed vertical line in Figure B.2a indicates the average CLTV ratio. The blue solid line and red dash line in Figure B.2b indicate the cumulative CLTV distribution for white and black Americans, respectively. The CLTV ratio is defined as the ratio of the sum of all loans secured by the property to the value of that property. The CLTV ratio is top coded at 100 percent.

Appendix C Robustness

Figure C.1: Mortgage Application and the Repeal of VRA (An Alternative Specification)



(a) Amount

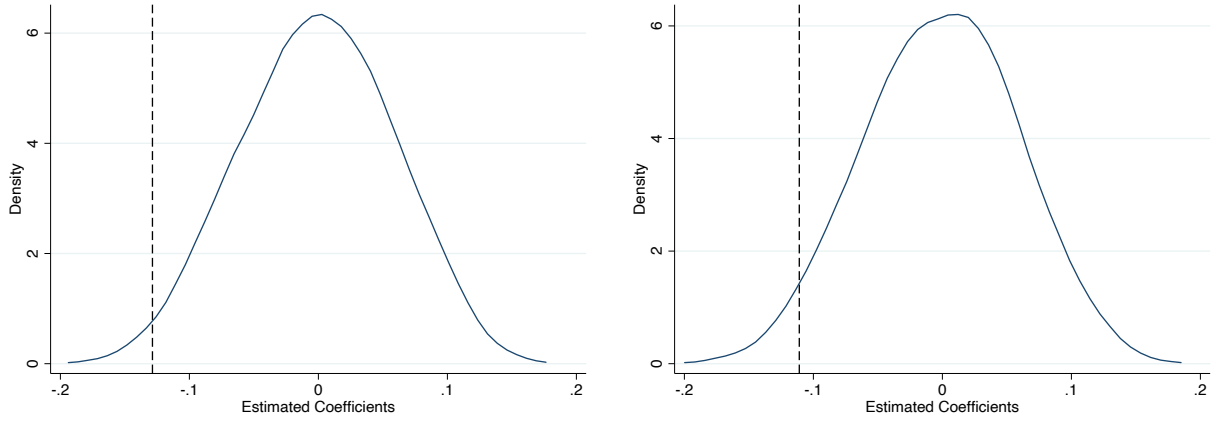
(b) Number

This figure uses the HMDA data aggregated at the county-race-year level for the period 2010 to 2019 and plots coefficients $\{\beta_k\}$ from the following specification:

$$ln(MortgageOrigination)_{rct} = \sum_{k=2010, k\neq 2013}^{2019} \beta_k \cdot Black_r \cdot Treat_c \cdot 1(t = k) + \alpha_{rt} + \alpha_{rTreat} + \alpha_{tTreat} + \varepsilon_{rct}$$

where subscript r, c, $Treat$, and t indicate race, county, treatment status, and year, respectively. $Black_r$ is an indicator variable that takes one for black borrowers. $Treat_c$ is an indicator variable that takes one for the VRA-treated counties. The sample of treated and control counties is shown in Figure 2. α_{rt}, α_{rTreat}, and α_{tTreat} represent race-year, race-treatment status, and year-treatment status fixed effects, respectively. As dependent variables, Figure C.1a uses the natural logarithm of total mortgage origination amount for home purchases as the dependent variable. Figure C.1b uses the natural logarithm of total mortgage origination number for home purchases as the dependent variable. Regressions are weighted by the total county population in 2010. Capped spikes drawn with the estimated coefficients $\{\beta_k\}$ indicate 95 percent confidence intervals obtained from standard errors clustered at the county level.

Figure C.2: Placebo Test



(a) Amount

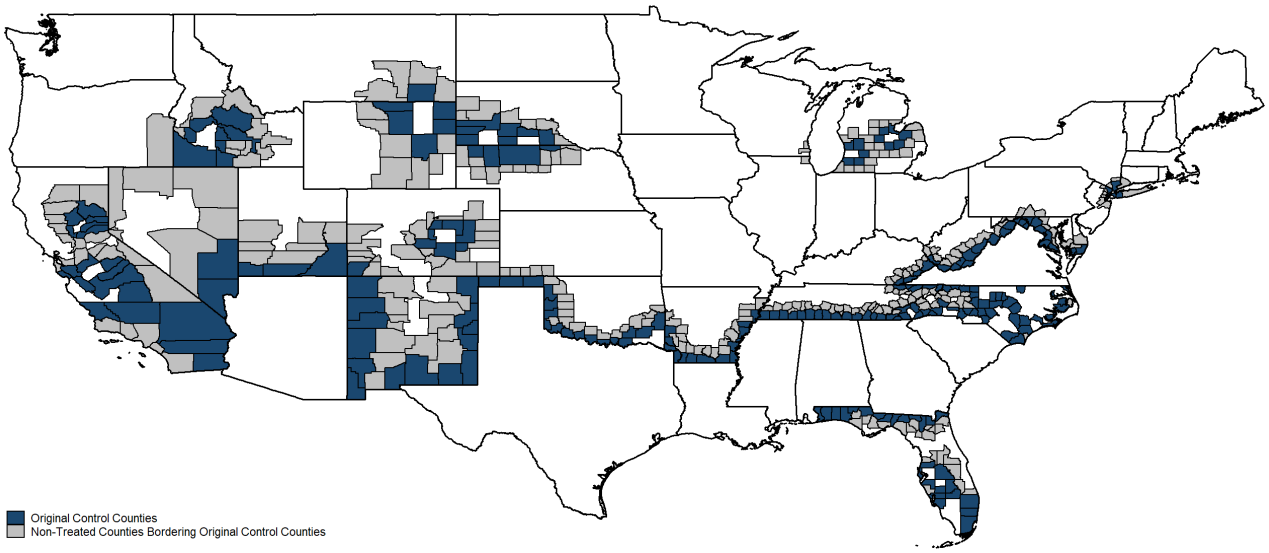
(b) Number

This figure uses the HMDA data aggregated at the county-race-year level for the period 2010 to 2019 and plots the kernel density of the point estimates β obtained from 3,000 Monte-Carlo simulations of the treatment status $Placebo\text{-}Treat_c$ in the following specification:

$$ln(MortgageOrigination)_{rct} = \beta \cdot Black_r \cdot Placebo\text{-}Treat_c \cdot Post\text{-}Shelby_t + \alpha_{rc} + \alpha_{rt} + \alpha_{ct} + \varepsilon_{rct}$$

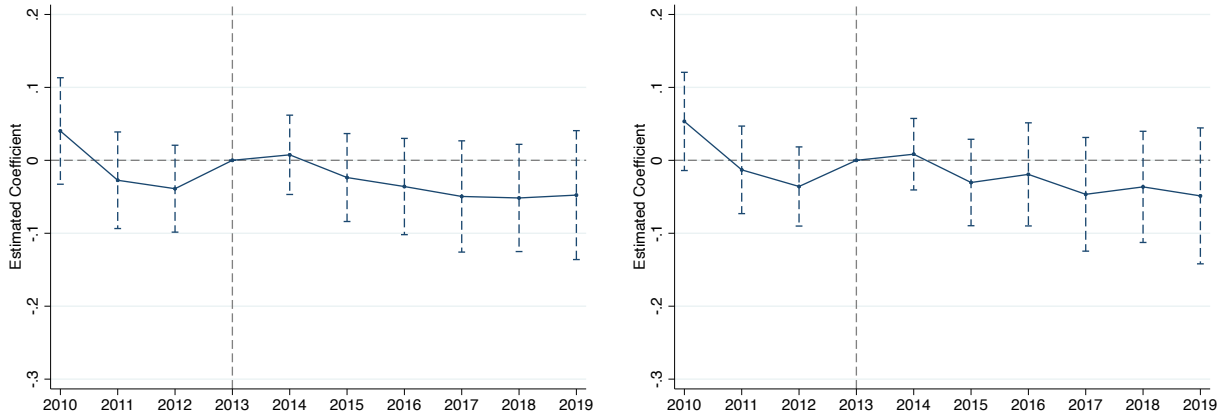
where subscript r, c and t indicate race, county, and year, respectively. $Black_r$ is an indicator variable that takes one. $Placebo\text{-}Treat_c$ is generated from a binomial distribution for each county with the probability of treatment being equal to the empirical probability of treatment. $Post\text{-}Shelby_t$ is an indicator variable that takes one for years from 2014. α_{rc}, α_{rt}, and α_{ct} represent race-county, race-year, and county-year fixed effects, respectively. Figure C.2a uses the natural logarithm of total mortgage origination amount as the dependent variable. Figure C.2b uses the natural logarithm of total mortgage origination number as the dependent variable. Regressions are weighted by the total county population in 2010.

Figure C.3: Sample of Bordering Counties Used in Falsification Test



The figure shows the sample bordering counties used in the falsification analysis. The uncovered counties, marked in navy blue, are counties in the immediate border of the covered or treated counties shown in figure 2. These counties are originally used as the control counties in baseline analysis. The other set of control counties, marked in gray, is a set of non-treated counties bordering the original control counties shown in figure 2. In the falsification test the original control counties are assigned to be treated counties and the non-treated counties bordering the original control counties are assigned to be the control counties. The list of counties covered under Section 5 of the VRA is obtained from the US Department of Justice. <LINK>

Figure C.4: Falsification Test



(a) Amount

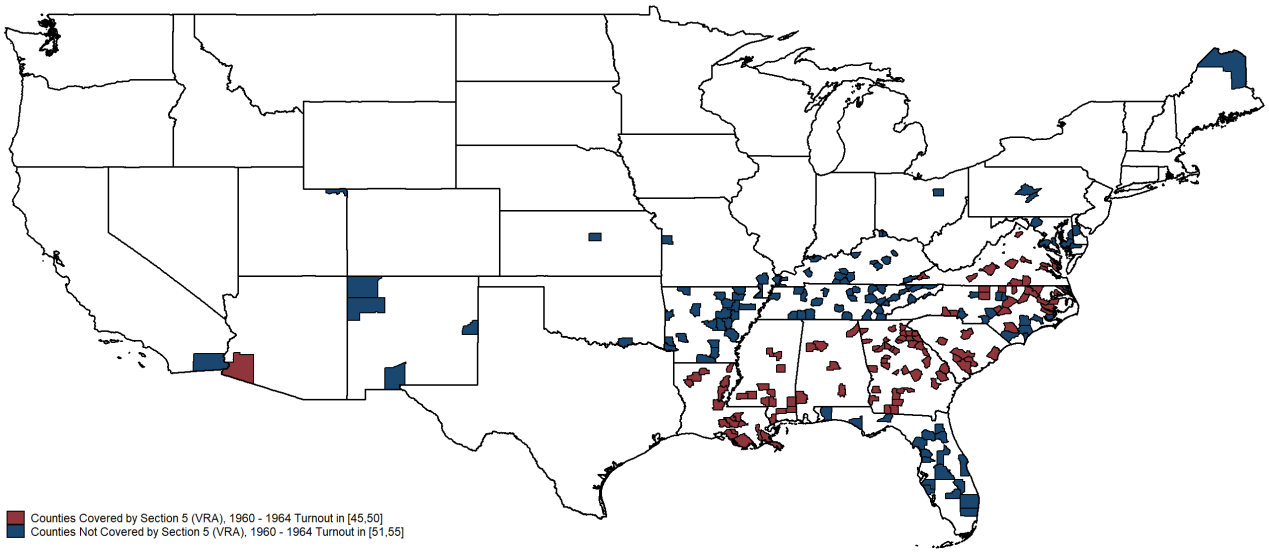
(b) Number

This figure uses the HMDA data aggregated at the county-race-year level for the period 2010 to 2019 and plots coefficients $\{\beta_k\}$ from the following specification:

$$ln(MortgageOrigination)_{rct} = \sum_{k=2010, k\neq 2013}^{2019} \beta_k \cdot Black_r \cdot False\text{-}Treat_c \cdot 1(t=k) + \alpha_{rc} + \alpha_{rt} + \alpha_{ct} + \varepsilon_{rct}$$

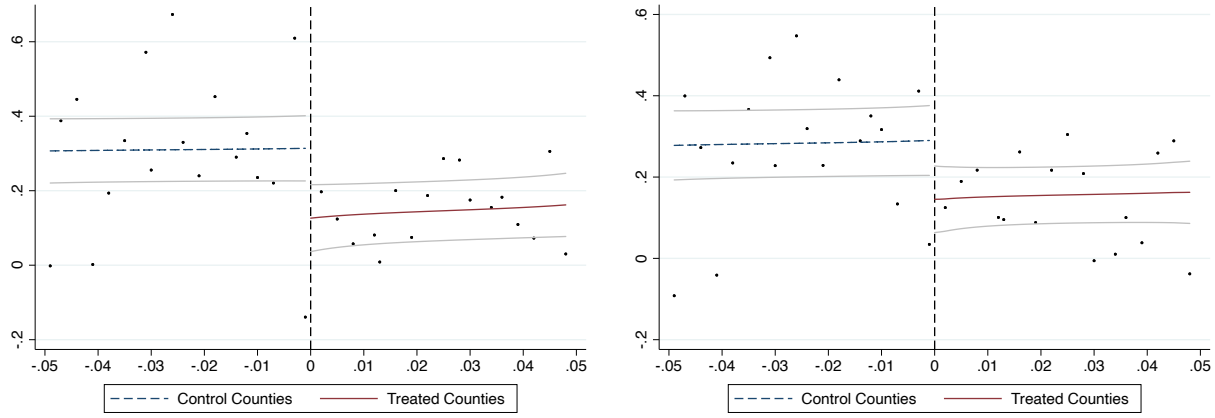
where subscript r, c and t indicate race, county, and year, respectively. $Black_r$ is an indicator variable that takes one for black borrowers. $False\text{-}Treat_c$ is an indicator variable that takes one for the VRA-uncovered counties bordering to original control counties in the baseline analysis and zero for original control counties. The sample of treated and control counties is shown in Figure C.3. α_{rc}, α_{rt}, and α_{ct} represent race-county, race-year, and county-year fixed effects, respectively. Figure C.4a uses the natural logarithm of total mortgage origination amount for home purchases as the dependent variable. Figure C.4b uses the natural logarithm of total mortgage origination number for home purchases as the dependent variable. Regressions are weighted by the total county population in 2010. Capped spikes drawn with the estimated coefficients $\{\beta_k\}$ indicate 95 percent confidence intervals obtained from standard errors clustered at the county level.

Figure C.5: Sample of Treated Counties and Control Counties used in Regression Discontinuity



The figure shows the sample bordering treated and control counties used in the regression discontinuity analysis. The covered counties were subject to preclearance under Section 5 of the Voting Rights Act by 1965. The counties covered under Section 5 of the VRA requires preclearance from either the US Attorney Gereral or the US District Court of DC. This list of counties covered under Section 5 of the VRA is obtained from the US Department of Justice. <LINK> The covered counties with the 1964 Presidential voter turnout from 45% till 50% are included in the treated sample. The uncovered counties are counties that were never covered by Section 5 of VRA and have the 1964 Presidential voter turnout from 51% till 55%.

Figure C.6: Regression Discontinuity

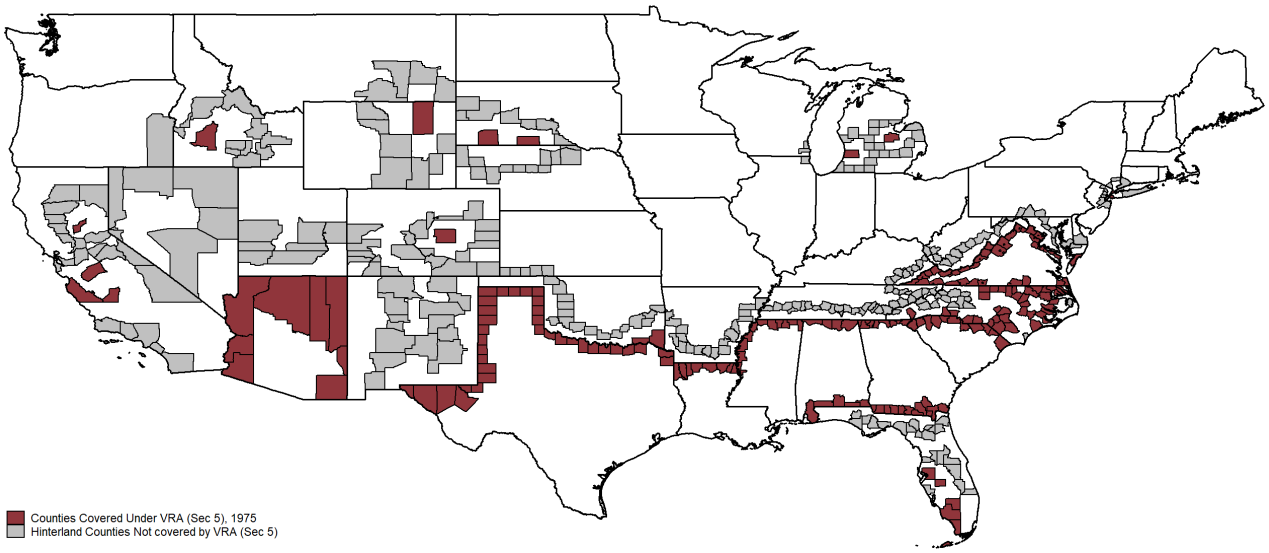


(a) Amount

(b) Number

This figure plots binned scatter-plot of the county-level mortgage origination growth for black Americans relative to white Americans from 2013 to 2016 (Y-axis) against the running variable, i.e., 0.5 minus the voter turnout in the 1964 Presidential election (X-axis). Figure C.6a measures the total mortgage origination growth in amount, and figure C.6b measures the total mortgage origination in number. The voter turnout is measured in fraction. Counties with the relative mortgage origination growth less than -1 or greater than 1 are excluded in the analyses. The red solid line illustrates the local linear mean smoothing for the treated counties whose 1964 Presidential voter turnout was between 46% and 50%. The navy dashed line illustrates the local linear mean smoothing for the control counties whose 1964 Presidential voter turnout was between 40% and 45%. The black vertical dashed line separates the treated and control groups, and the gray solid line indicates the 95 percent confidence interval of the local linear mean smoothing. The mortgage origination data for each county and race in 2013 and 2016 come from the HMDA datasets.

Figure C.7: Sample of Treated Counties and Hinterland Control Counties



The figure shows the sample bordering counties used in the hinterland county analysis. The covered counties, marked in red, were subject to preclearance under Section 5 of the Voting Rights Act by 1975. The counties covered under Section 5 of the VRA requires preclearance from either the US Attorney Gereral or the US District Court of DC. This list of counties covered under Section 5 of the VRA is obtained from the US Department of Justice <LINK>. The hinterland counties, marked in gray, are uncovered counties immediately bordering the original control counties shown in figure 2. In the hinterland county analysis, the original treated counties are still assigned to be treated counties, and the hinterland counties bordering the original control counties are assigned to be the control counties.

Table C.1: Mortgage Origination for Hispanic Americans and the Repeal of VRA

	Total		Home Purchase		Refinancing	
	(1) Amount	(2) Number	(3) Amount	(4) Number	(5) Amount	(6) Number
Black x Treat x Post	-0.1424***	-0.1319***	-0.1278**	-0.1105*	-0.1280***	-0.1243***
	(0.0541)	(0.0477)	(0.0622)	(0.0614)	(0.0369)	(0.0352)
Hispanic x Treat x Post	-0.1104**	-0.0964*	-0.1122*	-0.0962*	-0.0916**	-0.0841**
	(0.0560)	(0.0503)	(0.0630)	(0.0555)	(0.0409)	(0.0376)
County x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
County x Race FE	Yes	Yes	Yes	Yes	Yes	Yes
Race x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Within R^2	0.0161	0.0172	0.0087	0.0082	0.0063	0.0086
# Obs	12,702	12,702	12,702	12,702	12,702	12,702

This table uses the HMDA data aggregated at the county-race-year level for the period 2010 to 2019 and reports coefficients β_1 and β_2 from the following specification:

$$ln(MortgageOrigination)_{rhct} = \beta_1 \cdot Black_r \cdot Treat_c \cdot Post\text{-}Shelby_t + \beta_2 \cdot Hispanic_h \cdot Treat_c \cdot Post\text{-}Shelby_t + \alpha_{rc} + \alpha_{rt} + \alpha_{ct} + \alpha_{hc} + \alpha_{ht} + \varepsilon_{rhct}$$

where subscript r, c and t indicate race, county, and year, respectively. $Black_r$ is an indicator variable that takes one for black borrowers, and $Hispanic_h$ is an indicator variable that takes one for Hispanic borrowers. $Treat_c$ is an indicator variable that takes one for VRA-treated counties. The sample of treated and control counties is shown in Figure 2. $Post\text{-}Shelby_t$ is an indicator variable that takes one for years from 2014. α_{rc}, α_{rt}, α_{ct}, α_{hc}, and α_{ht} represent race-county, race-year, county-year, Hispanic-county, and Hispanic-year fixed effects, respectively. As dependent variables, columns (1) and (2) use the natural logarithm of total mortgage originations that combines originations for home purchases and refinancing. Columns (3) and (4) use the natural logarithm of mortgage originations for home purchases, and columns (5) and (6) use the natural logarithm of mortgage originations for refinancing. Columns (1), (3), and (5) measure the mortgage originations in dollar amounts, and columns (2), (4), and (6) measure the mortgage originations in numbers. Standard errors clustered at the county level are reported in parentheses. All regressions are weighted by the total county population in 2010. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Table C.2: Mortgage Origination by Gender and the Repeal of VRA

	Male		Female	
	(1) Amount	(2) Number	(3) Amount	(4) Number
Black x Treat x Post	-0.1454***	-0.1400***	-0.1289**	-0.1183**
	(0.0548)	(0.0490)	(0.0523)	(0.0461)
County x Year FE	Yes	Yes	Yes	Yes
County x Race FE	Yes	Yes	Yes	Yes
Race x Year FE	Yes	Yes	Yes	Yes
Within R^2	0.0244	0.0276	0.0131	0.0138
# Obs	6,986	6,986	6,986	6,986

This table uses the HMDA data aggregated at the county-race-year level for the period 2010 to 2019 and reports coefficients β from the following specification by the gender of mortgage borrowers:

$$ln(MortgageOrigination)_{rct} = \beta \cdot Black_r \cdot Treat_c \cdot Post\text{-}Shelby_t + \alpha_{rc} + \alpha_{rt} + \alpha_{ct} + \varepsilon_{rct}$$

where subscript r, c and t indicate race, county, and year, respectively. $Black_r$ is an indicator variable that takes one for black borrowers. $Treat_c$ is an indicator variable that takes one for VRA-treated counties. The sample of treated and control counties is shown in Figure 2. $Post\text{-}Shelby_t$ is an indicator variable that takes one for years from 2014. α_{rc}, α_{rt}, and α_{ct} represent race-county, race-year, and county-year fixed effects, respectively. As dependent variables, all columns use the natural logarithm of total mortgage originations that combines originations for home purchases and refinancing. Columns (1) and (3) measure the mortgage originations in dollar amounts, and columns (2) and (4) measure the mortgage originations in numbers. Standard errors clustered at the county level are reported in parentheses. All regressions are weighted by the total county population in 2010. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Table C.3: Local Identification Approach: Mortgage Origination and the Repeal of VRA

	Total		Home Purchase		Refinancing	
	(1) Amount	(2) Number	(3) Amount	(4) Number	(5) Amount	(6) Number
Black x Treat x Post	-0.1183***	-0.1071***	-0.0989**	-0.1011**	-0.0756**	-0.0585*
	(0.0309)	(0.0290)	(0.0416)	(0.0410)	(0.0325)	(0.0312)
County x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
County x Race FE	Yes	Yes	Yes	Yes	Yes	Yes
Race x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Race x County Pair x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Within R^2	0.0181	0.0199	0.0070	0.0097	0.0036	0.0032
# Obs	10,032	10,032	10,032	10,032	10,032	10,032

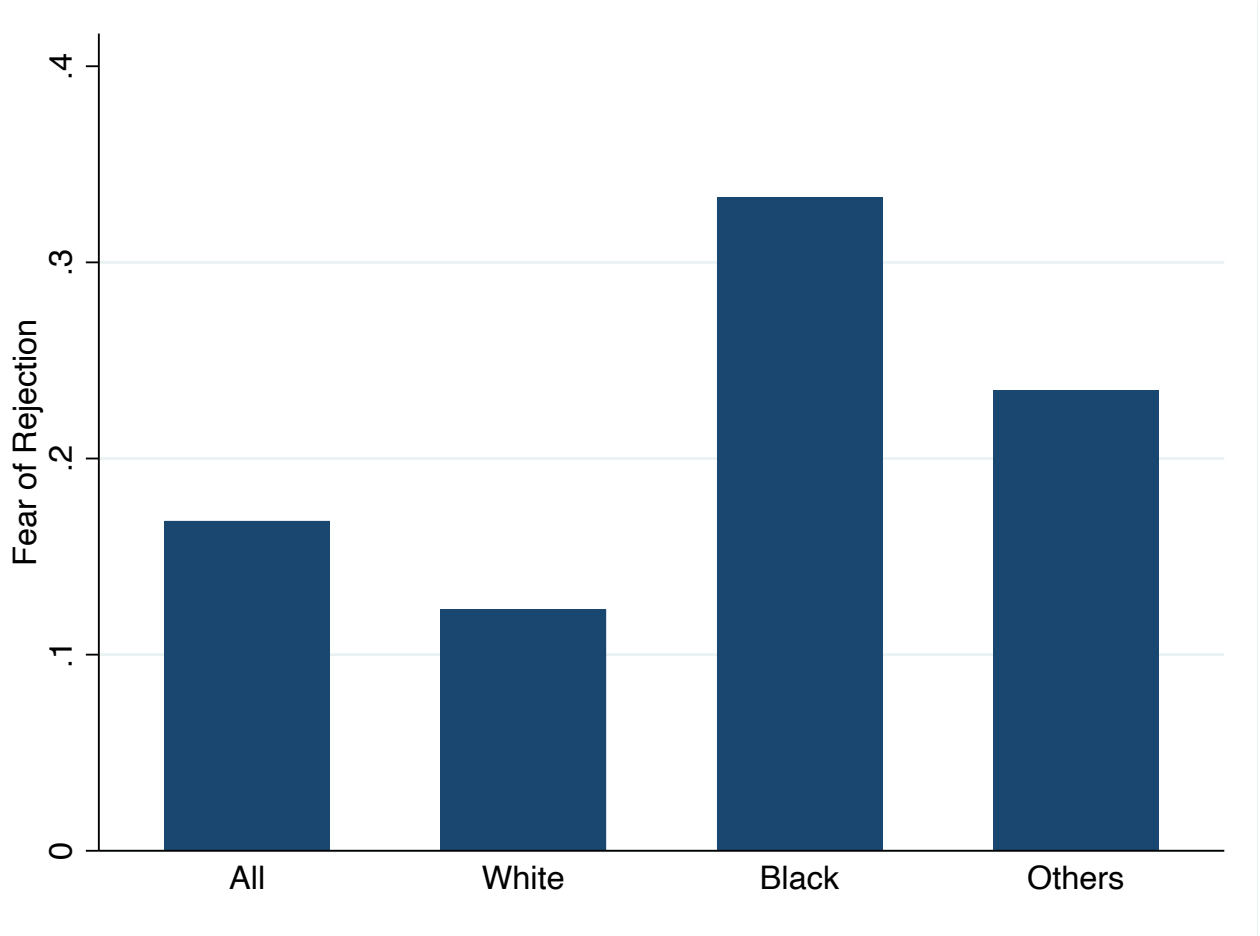
This table uses the HMDA data aggregated at the county-county pair-race-year level for the period 2010 to 2019 and reports coefficients β from the following specification:

$$ln(MortgageOrigination)_{rcpt} = \beta \cdot Black_r \cdot Treat_c \cdot Post\text{-}Shelby_t + \alpha_{rc} + \alpha_{rt} + \alpha_{ct} + \alpha_{rpt} + \varepsilon_{rcpt}$$

where subscript r, c, p, and t indicate race, county, county pair, and year, respectively. $Black_r$ is an indicator variable that takes one for black borrowers. $Treat_c$ is an indicator variable that takes one for VRA-treated counties. The sample of treated and control counties is shown in Figure 2. $Post\text{-}Shelby_t$ is an indicator variable that takes one for years from 2014. α_{rc}, α_{rt}, α_{ct}, and α_{rpt} represent race-county, race-year, county-year, and race-county pair-year fixed effects, respectively. As dependent variables, columns (1) and (2) use the natural logarithm of total mortgage originations that combines originations for home purchases and refinancing. Columns (3) and (4) use the natural logarithm of mortgage originations for home purchases, and columns (5) and (6) use the natural logarithm of mortgage originations for refinancing. Columns (1), (3), and (5) measure the mortgage originations in dollar amounts, and columns (2), (4), and (6) measure the mortgage originations in numbers. Standard errors clustered at the county pair-year level are reported in parentheses. All regressions are weighted by the total county population in 2010. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

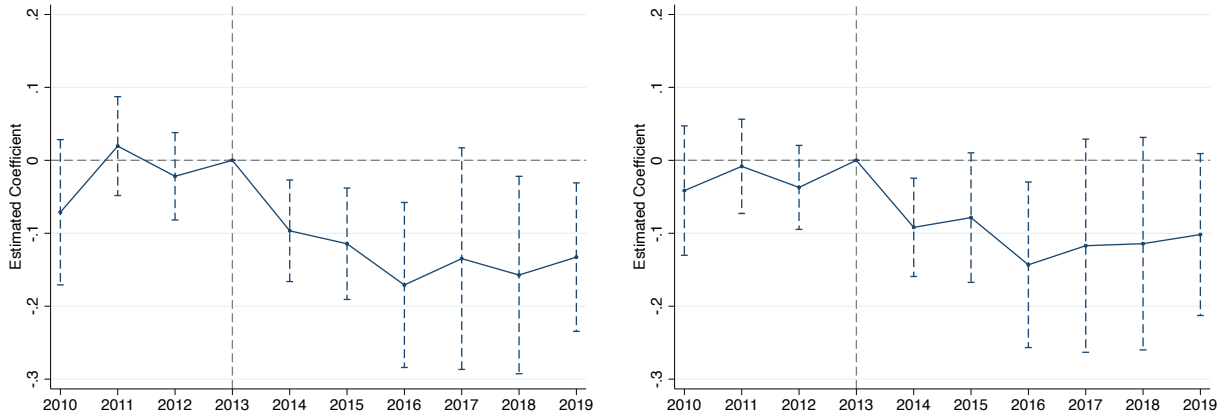
Appendix D What Drives the baseline results?

Figure D.1: Fear of Rejection of Mortgage Application



The figure shows fraction of respondents who fear their mortgage application will be rejected. The data on fear of rejection comes from the Survey of Consumer Finances from 2001 until 2019.

Figure D.2: Mortgage Application and the Repeal of VRA



(a) Amount

(b) Number

This figure uses the HMDA data aggregated at the county-race-year level for the period 2010 to 2019 and plots coefficients $\{\beta_k\}$ from the following specification:

$$ln(MortgageApplication)_{rct} = \sum_{k=2010, k\neq 2013}^{2019} \beta_k \cdot Black_r \cdot Treat_c \cdot 1(t = k) + \alpha_{rc} + \alpha_{rt} + \alpha_{ct} + \varepsilon_{rct}$$

where subscript r, c and t indicate race, county, and year, respectively. $Black_r$ is an indicator variable that takes one for black borrowers, and $Treat_c$ is an indicator variable that takes one for VRA-treated counties. The sample of treated and control counties is shown in Figure 2. α_{rc}, α_{rt}, and α_{ct} represent race-county, race-year, and county-year fixed effects, respectively. As dependent variables, Figure D.2a uses the natural logarithm of total mortgage application amount for home purchases, and Figure D.2b uses the natural logarithm of total mortgage application number for home purchases. Capped spikes drawn with the estimated coefficients $\{\beta_k\}$ indicate 95 percent confidence intervals obtained from standard errors clustered at the county level. Regressions are weighted by the total county population in 2010.

Table D.1: Mortgage Application by Income Quartile

	1st Quartile		2nd Quartile		3rd Quartile		4th Quartile	
	(1) Amount	(2) Number	(3) Amount	(4) Number	(5) Amount	(6) Number	(7) Amount	(8) Number
Black x Treat x Post	-0.1307**	-0.1461***	-0.1600**	-0.1444**	-0.1624**	-0.1528**	-0.1765***	-0.1656***
	(0.0624)	(0.0556)	(0.0662)	(0.0581)	(0.0763)	(0.0649)	(0.0584)	(0.0491)
County x Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County x Race FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Race x Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Within R^2	0.0082	0.0130	0.0134	0.0133	0.0161	0.0178	0.0186	0.0227
# Obs	7,567	7,567	7,567	7,567	7,567	7,567	7,567	7,567

This table uses the HMDA data aggregated at the county-race-income quartile-year level for the period 2010 to 2019 and reports coefficients β from the following specification for each income quartile:

$$ln(MortgageApplication)_{rct} = \beta \cdot Black_r \cdot Treat_c \cdot Post\text{-}Shelby_t + \alpha_{rc} + \alpha_{rt} + \alpha_{ct} + \varepsilon_{rct}$$

where subscript r, c and t indicate race, county, and year, respectively. $Black_r$ is an indicator variable that takes one for black borrowers. $Treat_c$ is an indicator variable that takes one for VRA-treated counties. The sample of treated and control counties is shown in Figure 2. $Post\text{-}Shelby_t$ is an indicator variable that takes one for years from 2014. α_{rc}, α_{rt}, and α_{ct} represent race-county, race-year, and county-year fixed effects, respectively. The income quartile is computed by county and race based on the income reported in 2010 HMDA data. The dependent variables $ln(MortgageApplication)_{rct}$ indicates the natural logarithm of total mortgage applications that combines applications for home purchases and refinancing. Columns (1), (3), (5), and (7) measure the mortgage applications in dollar amounts, and columns (2), (4), (6), and (8) measure the mortgage applications in numbers. Column (1) and (2) report the estimated coefficients for the bottom income quartile, Column (3) and (4) report the estimated coefficients for the second income quartile, Column (5) and (6) report the estimated coefficients for the third income quartile, and Column (7) and (8) report the estimated coefficients for the top income quartile. Standard errors clustered at the county level are reported in parentheses. All regressions are weighted by the total county population in 2010. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Appendix E Additional Results

Table E.1: Warmth towards Black Americans and the Repeal of VRA

	(1)	(2)	(3)
Treat × Post	-4.6808***	-4.6335***	-4.3129**
	(1.6654)	(1.7080)	(1.7855)
State FE	Yes	Yes	Yes
Year FE	Yes		Yes
Age Group-Year FE		Yes	
# Obs	3250	3250	133
Within R^2	0.0017	0.0017	0.0436
Sample	Respondent	Respondent	State

This table uses the American National Election Series (ANES) data and reports coefficients β from the following specification:

$$y_{i(s)t} = \beta \cdot Treat_s \cdot Post\text{-}Shelby_t + \alpha_s + \alpha_{t(age,t)} + \varepsilon_{i(s)t}$$

where subscript i, s, age, and t indicate individual, state, individual's age, and year, respectively. $Treat_s$ is an indicator variable that takes one for VRA-treated states. $Post\text{-}Shelby_t$ is an indicator variable that takes one for 2016 (i.e., the survey year after the repeal of VRA). α_s and $\alpha_{t(age,t)}$ represent state and year (age group-year) fixed effects. The dependent variable is feeling thermometer measuring the level of warmth towards Black Americans on a scale ranging from 0 to 97 with higher value indicating a higher degree of warmth. The sample comprises of white male American survey respondents in the ANES survey waves of 2008, 2012 and 2016. Columns (1) and (2) use respondent level data, and column (3) uses data averaged at the state level. All observations are weighted by survey weights. Standard errors clustered at the state level are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Table E.2: Difference in Trust among black and white Americans and the Repeal of VRA

	(1)	(2)
Black × Treat × Post	-0.1992***	-0.2019***
	(0.0616)	(0.0517)
State × Year FE	Yes	Yes
Race × Year FE	Yes	Yes
State × Race FE	Yes	Yes
# Obs	4,103	2,710
Within R^2	0.0056	0.0084
Sample	All States	Bordering States

This table uses the American National Election Series (ANES) data and reports coefficients β from the following specification:

$$y_{rst} = \beta \cdot Black_r \cdot \cdot Treat_s \cdot Post\text{-}Shelby_t + \alpha_{rs} + \alpha_{rt} + \alpha_{st} + \varepsilon_{rst}$$

where subscript r, s, and t indicate race, state, and year, respectively. $Treat_s$ is an indicator variable that takes one for VRA-treated states. $Post\text{-}Shelby_t$ is an indicator variable that takes one for 2016 (i.e., the survey year after the repeal of VRA). α_{rs}, α_{rt} and α_{st} represent race × state, race × year, and state × year fixed effects, respectively. The dependent variable is the level of trust that a respondent has on others and takes a value of 0 when the respondent reports no trust on others. The sample comprises of white and black male American survey respondents in the ANES survey waves of 2008, 2012 and 2016. Columns (1) uses data from all states and column (2) uses data from bordering states. All observations are weighted by survey weights. Standard errors clustered at the state level are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Table E.3: Example of Black Lenders

FDIC Certificate No.	Name	City	State	Est. Date	2013 Total Assets ($ thou.)
20856	LIBERTY BANK & TRUST CO	NEW ORLEANS	LA	11/16/1972	547,984
8033	CITIZENS TRUST BANK	ATLANTA	GA	6/18/1921	387,410
33938	CAPITOL CITY BANK & TRUST CO	ATLANTA	GA	10/3/1994	286,761
35241	SOUTH CAROLINA CMTY BANK	COLUMBIA	SC	3/26/1999	67,203
22229	COMMONWEALTH NATIONAL BANK	MOBILE	AL	2/19/1976	59,613

This table presents examples of black lenders in southern states. Lenders are defined as black lenders if they operate in border counties and are above the 90th percentile when sorted by the share of black borrowers in their mortgage lending portfolio in 2010 to 2013.