Do 40-Year-Old Facts Still Matter? Long-Run Effects of Federal Oversight under the Voting Rights Act

By Desmond Ang*

Abstract

In 2013, the Supreme Court struck down parts of the Voting Rights Act that mandated federal oversight of election laws in discriminatory jurisdictions, prompting a spate of controversial new voting rules. Utilizing difference-in-differences to examine the Act's 1975 revision, I provide the first estimates of the effects of "preclearance" oversight. I find that preclearance increased long-run voter turnout by 4-8 percentage points, due to lasting gains in minority participation. Surprisingly, Democratic support *dropped* sharply in areas subject to oversight. Using historical survey and newspaper data, I provide evidence that this was the result of political backlash among racially conservative whites.

"But a more fundamental problem remains: Congress did not use that record to fashion a coverage formula grounded in current conditions. It instead re-enacted a formula based on 40-year-old facts having no logical relation to the present day."

— *Chief Justice John Roberts (Shelby v. Holder, 2013)*

The Voting Rights Act (VRA) of 1965 has been hailed as one of the "greatest legislative achievements of the Civil Rights Movement" (Menand, 2013). Passed months after the Selma to Montgomery marches, the Act prohibited

*Mailing address: 79 John F. Kennedy St., Cambridge, MA 02138. Email: desmond_ang@hks.harvard.edu. Telephone: (617) 495-4505. I thank Jim Andreoni, Kate Antonovics, Julian Betts, Prashant Bharadwaj, Gordon Dahl, Dave Donaldson, Mitch Downey, James Fowler, Seth Hill, Ian Larkin, Paul Niehaus, Ebonya Washington, four anonymous referees, and seminar participants at UCSD for valuable feedback. I gratefully acknowledge financial support from the National Science Foundation. Any errors are my own.

the denial or abridgement of "the right to vote on account of race or color." The effects of the VRA on minority enfranchisement were immediate. Between the 1964 and 1968 presidential elections, black voter registration rates increased 67 percent among Southern states (Vallely, 2009).

The Act achieved this through two principal mechanisms. The first was the prohibition of literacy tests, which were used throughout the Jim Crow era to disenfranchise Southern blacks. The VRA's second and more controversial mechanism was a federal oversight process commonly known as preclearance. Jurisdictions subject to preclearance (henceforth called *covered* jurisdictions) were prohibited from implementing any new electoral rule without first obtaining federal approval. While preclearance's geographic purview was limited only to areas that met certain historical criteria, the scope of its protections was expansive and encompassed *all* future changes affecting voting in those areas. Thus, preclearance restrictions, which have been called "the most effective means of preventing racial bias in voting" (Bennett, 2013), were designed as a broad prophylaxis against voter discrimination, shifting onto covered jurisdictions the burden of proving *ex ante* that new voting rules did not have a "discriminatory purpose" and would not have a "discriminatory effect."

Since its inception, preclearance oversight has been alternately praised and criticized as "extraordinary legislation otherwise unfamiliar to our federal system" (Northwest Austin Municipal Utility District No. 1 v. Holder, 2009). These arguments came to a head in Shelby County v. Holder (2013), in which the Supreme Court ruled that continued coverage based on historical — rather than current — measures of discrimination is unconstitutional. As a result, until and unless Congress enacts a new coverage formula, previously covered jurisdictions are no longer subject to federal oversight.

Immediately following the Shelby ruling, lawmakers in several previously covered areas enacted controversial new voting changes, many of which have been challenged in federal courts. Alabama, Mississippi, North Carolina and Texas introduced restrictive voter ID requirements, while Florida, Georgia and Virginia sought to purge their voter rolls of thousands of eligible minorities. Though Republicans have justified these measures as necessary to combat widespread voter fraud, Senate Democrat Chuck Schumer denounced them as "clear front[s] for constricting the access to vote to poor Americans...and - above all - African-Americans and Latinos." Underpinning this partisan divide is the common belief that the minorities most affected by restrictive voting rules lean heavily Democratic. Indeed, President Donald Trump, a Republican, claimed that, of the "millions" of allegedly illegal ballots cast in 2016, "none of 'em come to me. They would all be for the other side." Given America's growing minority electorate, the legal fate of these voting laws could have lasting implications for future elections.

Despite their relevance to ongoing policy debates, the specific effects of preclearance have never been estimated. While researchers have examined the VRA's impact on turnout (Filer et al., 1991), representation (Besley et al., 2010; Washington et al., 2012; Schuit and Rogowski, 2016), and minority aid (Cascio and Washington, 2014), these studies focus on the 1965 implementation of the Act and are thus unable to disentangle the effects of preclearance from the simultaneous abolition of literacy tests, which were among the most discriminatory tools ever employed in the U.S. election system and are unlikely to ever be reinstated (Springer, 2014). Furthermore, all of these papers, as well as the broader literature exploring the enfranchisement of minorities (Husted and Kenny, 1997), women (Miller, 2008), and the poor (Fujiwara, 2015), examine policies designed to alleviate specific, existing barriers and prohibitions to voting – such as the elimination of literacy tests, the introduction of electronic voting, and the extension of suffrage rights.

Preclearance restrictions differ fundamentally from these interventions. Rather than targeting specific voting barriers already in use, federal oversight was designed to restrict the implementation of any *new* discriminatory measures. Understanding the implications of these blanket protections is especially relevant in light of evidence from Trebbi et al. (2008) and Alesina et al. (2004) of the strategic manipulations that local election officials engage in to maintain power. Indeed, broad preventative oversight encompassing the universe of potential voting changes may be the most effective means of curbing discrimination in settings like the U.S., where electoral rule-making is highly decentralized and opaque.

This paper seeks to better understand the effects of such oversight. Using a flexible difference-in-differences model, I examine the geographic expansion of coverage under the 1975 revision of the Voting Rights Act to estimate the causal impact of preclearance on county-level voter turnout and Democratic vote share from 1960 to 2016. Unlike the 1965 VRA which was "reverse-engineered" by Congress to capture Southern states that employed literacy tests, the 1975 coverage formula relied on noisy measures of voter turnout and minority population share to determine which counties were subject to preclearance (Holder, 2013). Thus, application of the 1975 formula resulted in heterogeneity of coverage within states throughout the country, subjecting 283 counties across nine states to federal oversight. I am able to exploit this heterogeneity to precisely estimate the policy's effects and to demonstrate its plausible exogeneity.

I find that preclearance restrictions led to gradual and significant increases in voter participation and that these gains persisted for over 40 years, bolstering turnout by 4-8 percentage points in recent elections. Examining state-level turnout by race, I demonstrate that these effects were due entirely to increased participation among minorities, who were 17 p.p. more likely to vote in the 2012 election as a result of preclearance coverage. Analyzing electoral rules data, I show that municipalities subject to voter protections were significantly less likely to employ "winner-take-all" election systems, which are commonly believed to dilute minority voting power. Combined with heterogeneity analysis demonstrating larger effects among areas with more historical discrimination, these results suggest that gains in turnout were the result of reduced voter discrimination as opposed to other demographic or political factors.

Surprisingly, I find that preclearance coverage led to significant and immediate *decreases* in the share of Democratic votes cast. These estimates are large – averaging 3.2 p.p. across post-treatment elections – and exceed the 1992 and 1996 presidential margins of victory in the covered states of Texas and Arizona. Using historical survey data, I show that this rightward shift was driven by increased Republican support among whites opposed to government aid for minorities. As demonstration of the political controversy surrounding preclearance, I find a sharp spike in newspaper mentions of the VRA in covered areas beginning in 1975, particularly among those papers that had endorsed President Richard Nixon, whose Republican administration sought to abolish election oversight restrictions. Taken together, these results provide strong evidence that the implementation of minority voter protections triggered political re-alignment among whites.

This paper makes several important contributions. First, it complements the existing literature on voter enfranchisement efforts, which concentrates on targeted, remedial interventions, by demonstrating the efficacy of broader preventative measures. Viewed from a different angle, my findings suggest that strategic manipulations of electoral rules affecting ballot access and voting power can have deleterious effects on voter participation, and demonstrate that these effects are disproportionately received by minorities.

Second, this paper provides new evidence in support of race-based theories of Southern dealignment, which argue that the collapse of the New Deal coalition in the South was due primarily to the Democratic Party's embrace of the Civil Rights Movement in the 1960's (Kuziemko and Washington, 2015). In examining the expansion of preclearance coverage to counties across the nation more than a decade later, I validate the salience of "white backlash" against minority political threats in other settings (Key and Heard, 1950; Tesler and Sears, 2010; Enos, 2016). By demonstrating geographic and partisan differences in local media coverage of preclearance, this study also adds to a growing body of literature exploring the role of media in politics (Besley and Prat, 2006; Snyder Jr and Strömberg, 2010; Gentzkow and Shapiro, 2010; Chiang and Knight, 2011; Gentzkow et al., 2015).

Perhaps most importantly, this paper contributes to current policy debate by deriving the first estimates of preclearance's impact. The Shelby ruling was predicated on the Court's opinion that "a [coverage] formula based on 40-year-old facts" has "no logical relation to the present day." I show not only that preclearance coverage led to historical increases in minority participation but also that the application of these restrictions in 1975 continued to bolster enfranchisement over four decades later. To the extent that the future of the Voting Rights Act hinges on the formulation of new coverage criteria relevant to the "present day," understanding these effects and the role they played in shaping the current political landscape is critical to Congress' ability to craft meaningful legislation capable of protecting voting rights today and into the future.

This paper is organized as follows: Section I provides details surrounding preclearance's history, enforcement and coverage, Section II describes my empirical strategy and data, Section III presents estimation results for voter turnout and Democratic vote share, Section IV includes various robustness analyses, Section V explores mechanisms, Section VI discusses implications of the Shelby ruling and Section VII concludes.

I. Background

Passed at the height of the American Civil Rights movement under a Democratic-controlled government, the Voting Rights Act of 1965 was designed to be, as President Lyndon B. Johnson described, "the goddamndest, toughest voting rights act [possible]" (May, 2013).

Section 2 of the Act broadly reinforced the voting rights guaranteed in the 14th and 15th Amendments and allowed private citizens to sue as means of enforcing prohibitions on discrimination. The Act further banned the use of literacy tests - first in the South, then nationwide in 1970 - and beginning in 1975, mandated the provision of translated election materials and language assistance in minority-heavy areas.

In drafting the Act, members of Congress feared a never-ending cat-and-mouse game would ensue without more expansive protections. Previous efforts by the Department of Justice to strike down discriminatory laws often resulted in the enactment of new discriminatory rules, even within 24 hours (Pitts, 2003). The difficulty of pursuing piecemeal remedies was perhaps best summarized by one Mississippi election official, who explained "what those smart fellows [at the Justice Department] don't realize is that we can still get to these darkies in a whole lot of subtle ways" (Stanley, 1987).

Thus, Congress designed Section 5 of the Act as a counter these "subtle" methods of discrimination. Section 5 essentially froze in place the voting processes of covered jurisdictions, requiring that any and all future changes to those processes be "precleared" by the federal government. Only if and when a proposed change received preclearance could that change be enforced by the jurisdiction. These blanket protections — widely considered the "heart" of the VRA — sought to shift the burden of proof of discrimination off of aggrieved voters *ex post* and onto discriminatory election officials *ex ante* (Tucker, 2006).

A. Enforcement

The scope of preclearance restrictions was extensive. All changes, no matter how minor, pertaining to voting "laws, practices or procedures" and affecting a covered jurisdiction or any of its sub-jurisdictions required federal approval before they could be implemented.

Proposed changes had to be submitted to the Attorney General, who had 60 days to interpose an objection.[1] Proposals were then assessed on a case-by-case basis according to a "retrogression test." This test considered the effects of the proposal on minority enfranchisement in relation to the *status quo*. Any change that was deemed to leave minorities *worse* off was denied preclearance. Regardless of its likely effect, any change that the Attorney General believed was *intended* to harm minorities was also denied preclearance. Importantly, this process was only designed to prevent the implementation of *new* changes with discriminatory intent or effect. It did not reverse existing discriminatory practices.

As Panel A of Figure 1 shows, preclearance oversight was actively enforced throughout its existence. From 1965 to 2013, over half a million Section 5 submissions were made to the Attorney General. Of these, 1,134 were objected to, representing over 3,000 blocked voting changes. Notably, more than 85

[1] Changes could alternatively be submitted to the U.S. District Court for D.C. However, due to large relative material and time costs of pursuing "judicial" preclearance, over 99 percent of submissions were directed to the Attorney General.

percent of objections pertained to local or municipal, as opposed to state-level, proposals.[2]

[Figure 1 about here.]

As Panel B shows, objections were lodged against a wide range of discriminatory voting changes. The majority related to either election system changes or redistricting and annexation. The former primarily refer to attempts by local governments to transition from district-based to election systems, which dilute minority votes in areas of high racial segregation.[3] The latter encompass not only boundaries for national and state legislatures but also those for school districts, city councils and other local governing bodies.[4] Changes pertaining to voter registration, including identification, residency and re-registration requirements, comprised fewer than 20 percent of objections in any decade, less than those regarding the timing and placement of elections and polling locations.

While these rule changes may not appear particularly onerous in the cross-section, the protections afforded under Section 5 become more obvious when considering a historical case study. Table 1 lists the entire history of objections lodged against voting changes in Harris, Texas, the state's most populous county. Had they been enacted, these 18 state-level and 15 local-level submissions would have resulted in, among other changes: the purge of all voters that did not re-register in 60 days, the elimination of state-funded primaries for the state's leading Mexican-American party, the creation of a white-controlled school district and criminal court, the elimination of hundreds of polling stations, the institution of gerrymandered districts and at-large elections for school boards, city councils and the state legislature, the allowance of

[2] Another several thousand proposals were withdrawn or amended following the issuance of a "more information request" by the Attorney General (Adler and Kousser, 2011)

[3] While one commonly cited benefit of at-large systems is that they produce less pork-barrel spending, relative to district-based systems (Persson et al., 2000), Baqir (2002) finds that at-large election systems have little effect on size of government.

[4] Though perhaps less salient, annexations are an important strategic margin employed by local jurisdictions to manipulate demographic heterogeneity. See, for example, Alesina et al. (2004)

administrative challenges of voter citizenship and the implementation of strict voter identification requirements.

[Table 1 about here.]

As both Figure 1 and Table 1 demonstrate, the number and type of objections interposed by the Attorney General evolved over time. While further exploration of these dynamics is outside the scope of this paper, they nonetheless serve to highlight the expansiveness of preclearance oversight, which protected against ever-shifting efforts to discriminate across a wide range of different electoral rules.[5]

B. Coverage

Figure **??** maps the jurisdictions brought under preclearance coverage by each revision of the VRA. As shown, preclearance coverage was initially limited to areas in the Deep South. The 1965 VRA imposed federal oversight restrictions and banned literacy tests only among those Southern states that employed such tests. This led to preclearance coverage of the entire states of Alabama, Georgia, Louisiana, Mississippi, South Carolina and Virginia, as well as parts of North Carolina. The 1970 VRA then brought under preclearance coverage a handful of jurisdictions in California, New York and New Hampshire that had continued to administer literacy tests.

[Figure **??** about here.]

The 1975 revision of the Act expanded preclearance coverage to include those areas where discrimination may have been less overt. Specifically, any jurisdiction where a single language minority group (i.e., Hispanic, Asian, Alaskan or Native Americans) comprised greater than 5 percent of the voting age population in 1970 *and* where voter turnout was less than 50 percent in

[5] It is certainly possible that these enforcement patterns were also influenced by the partisan leanings of the Executive Branch. However, Posner (2006) notes that the staff attorneys at the Department of Justice responsible for day-to-day enforcement were largely insulated from political pressure since the "legal bases on which the Department could invalidate non-retrogressive change" were "well-set" by the mid-1980's.

1972 was brought under federal oversight.[6] This resulted in the coverage of 283 counties across nine states, only three of which (Texas, Arizona and Alaska) were covered in their entirety.[7]

For all versions of the VRA, preclearance coverage lasted indefinitely (until the 2013 Shelby decision) and included all of a covered jurisdiction's political sub-jurisdictions.[8] Thus, for example, in covering the state of Texas, the 1975 VRA also brought under federal supervision all of its counties and cities, even those with high turnout rates or low minority population shares.

C. Response

Given the sovereignty and material costs associated with preclearance coverage, the implementation of Section 5 sparked considerable outrage among local and state officials.[9] After Texas fell under coverage in 1975, the state's governor called preclearance "a fraud", "an insult", and "an administrative nightmare" (Seguiin Gazette, 1975). Other critics claimed that preclearance's selective geographic application represented an "unfair" and "unprecedented breach of federalism" (South Carolina v. Katzenbach, 1966). Preclearance remained a political flashpoint years after its initial implementation. While campaigning in 1980, President Ronald Reagan called the Act "humiliating to

[6]Technically, the trigger only applied if a jurisdiction's language minorities experienced illiteracy rates greater than the national average. However, this was true for all cases, except for a few Native American reservations.

[7]Newly covered counties as well as any county with greater than 5 percent language minorities, regardless of 1972 turnout, were also subject to bilingual election requirements. Except as applied to counties subject to preclearance, these requirements were temporary and determined on a rolling basis following each Census.

[8]The VRA included"bail out" provisions, allowing areas to escape preclearance coverage after demonstrating non-discrimination. However, these criteria were designed such that "bail out" was virtually impossible prior to 1985 and remained extremely onerous afterwards. In over 40 years, only a handful of cities in Virginia successfully bailed out of coverage after proving non-discrimination. The VRA also included a "pocket trigger" to "bail in" uncovered jurisdictions. However, this was seldom used and generally imposed only temporary coverage of certain types of voting changes (for example, New Mexico was subject to preclearance only for redistricting plans and only from 1984 to 1994).

[9]Expenses related to obtaining preclearance for even minor voting changes were estimated to range from $500-$1000. Over a period of several decades, these costs could become quite burdensome, especially for local governments. Officials in Merced County, California estimated spending over $1 million from 2000 to 2010 alone (Nidever, 2012).

the South" and promised to "restore to state and local governments the power that properly belongs to them" (Wolters, 1996).[10]

In Shelby County v. Holder (2013), the Supreme Court agreed with these perspectives and ruled that the continued enforcement of preclearance based on historical coverage formulas was unconstitutional. This freed from federal oversight all states and counties that had been captured under the 1965, 1970 and 1975 coverage formulas. While the Court upheld the constitutionality of preclearance itself, it placed on Congress the onus of drafting a new coverage formula. Though several new formulas have been proposed, none have been enacted.

Since the Shelby decision, previously covered jurisdictions have implemented numerous controversial voting changes. Within hours of the ruling, Texas passed a voter identification law that had previously been rejected under preclearance, while North Carolina enacted registration requirements that federal courts have since found to "target African Americans with almost surgical precision."[11] Though local voting changes have attracted less media attention, many are no less controversial. In Maricopa County, Arizona, home to over one million Hispanic voters, election officials opted to reduce the number of polling stations for the 2016 presidential primary by over 70 percent, leading to lines up to five hours long. This is part of a larger trend among previously covered counties, 43% of which have closed polling locations since Shelby, resulting in nearly 900 fewer places to vote in the 2016 election (Leadership Conference Education Fund, 2016).

[10] The Reagan administration then attempted to weaken Section 5 by proposing regulatory guidelines that required affirmative evidence of discrimination for changes to be invalidated. However, this proposal was ultimately abandoned under Congressional pressure (Kousser, 2007).

[11] Though the Texas law was overturned in 2014 by federal courts under Section 2 of the VRA, many of its controversial provisions remained in effect during the 2016 presidential election, thus highlighting the challenges of *ex-post* litigation as compared to preclearance's preventative mechanisms

II. Empirical Strategy

Though the majority of covered counties fell under coverage beginning in 1965, this paper focuses only on those covered by the 1975 revision of the VRA. The reasons for this are many.

First, jurisdictions that fell under coverage beginning in 1965 were simultaneously banned from using literacy tests, which were not eliminated nationwide until 1970.[12] Thus, identifying the specific effects of preclearance from the 1965 VRA, which essentially introduced two concurrent interventions to an identical treatment group, would require strict and possibly unrealistic assumptions.[13]

Second, the 1975 VRA's reliance on an objective coverage formula made precise geographic targeting of problematic areas nearly impossible. As former Attorney General Eric Holder noted, the use of noisy estimates for determining coverage meant that "the scope of coverage had the potential to be over- and under-inclusive" (Holder, 2013). Indeed, officials in Kings County have long contended that the county's coverage was the result of the Census Bureau's failure to properly account for a large military population that was ineligible to vote in Kings (Nidever, 2012). Furthermore, the estimates used to determine coverage were not known to Congress — nor had they even been calculated by the Census Bureau — at the time of the Act's passage.

Last, the nature of the 1975 VRA's coverage formula, which took into account county-level demographic measures, resulted in substantial within-state treatment heterogeneity as well as a diverse regional representation. Indeed, 283 counties from nine states were brought under coverage in 1975, representing three of the nation's four Census regions. This geographic heterogeneity bolsters the study's internal and external validity.

[12] Though the 1975 VRA introduced bilingual election requirements in addition to expanding preclearance coverage, hundreds of counties *not* covered under preclearance were also subject to these language restrictions.

[13] Furthermore, much of this paper's heterogeneity analysis would not possible through examination of the 1965 VRA due to the lack of historical data prior to its implementation. For example, the CPS voter supplement data on state-level turnout by race is only available from 1968 onwards.

A. Data

The purpose of this paper is to assess the impact of preclearance restrictions on voter enfranchisement and representation. While I would ideally estimate a first-stage effect on direct measures of voter discrimination, comprehensive data on discriminatory incidents and policies does not exist for uncovered counties and is fraught with issues of selection for covered areas. Furthermore, preclearance's very existence was predicated on the notion that it is nearly impossible to enumerate every policy channel by which local officials are able to discriminate against voters. Thus, I instead estimate effects on voter turnout and share of Democratic votes cast in presidential elections, perhaps the most common and direct expressions of political participation and preferences.

County-level voting data for presidential elections come from the Interuniversity Consortium for Political and Social Research (ICPSR) and Dave Leip's Atlas of U.S. Presidential Elections. Estimates of voting age citizens are interpolated from Census and American Community Survey demographic data.[14] These data are used to construct county-level estimates of voter turnout (the share of votes cast to eligible voting population) and Democratic vote share (the share of Democratic votes cast to major party votes cast) in all presidential elections from 1960-2016.[15] To examine changes in political preferences, I also obtain district-level measures of political ideology and party affiliation for the 87th to 113th Congresses from Poole and Rosenthal's DW-NOMINATE data (Poole and Rosenthal, 1985, 2011).

As county-level estimates of historical turnout by ethnicity do not exist, I rely on the Current Population Survey Voting and Registration supplement to examine effects on minority turnout at the state-level.[16] The CPS data contains individual-level self-reports of race and voting participation which I

[14]Interpolated estimates were obtained from (Gentzkow et al., 2011) for 1960-2004 and were calculated by the author using the same methodology from 2004 onwards.

[15]Democratic vote share is measured against major party votes cast due to the presence of significant third-party presidential candidates in 1968, 1980, 1992 and 1996. However, as shown in the Appendix, results are virtually identical if Democratic vote share is instead calculated using all presidential votes cast.

[16]The voter supplement is carried out after each federal election using a sample of roughly 100,000-150,000 individuals. Historical micro-data are from CPS Utilities.

aggregate to construct representative estimates of state-level turnout for whites and non-whites from 1968-2016.[17] Unfortunately, the voting supplement only contains information on whether a respondent voted and not on her ballot choice.

Thus, to examine racial differences in political affiliation, I use data from the American National Election Series (ANES), an in-person survey conducted on a stratified random sample of roughly 2,000 individuals around each presidential and midterm election. Prior to 2000, the survey contains unrestricted access to each respondent's race and county as well as consistently asked questions regarding political preferences.[18] As validation of the ANES data, I also analyze historical Gallup survey data from 1961 to 2003. This data is identified by respondent race and state and has been used by researchers such as Kuziemko and Washington (2015).

To explore mechanisms, I make use of two other sources of data. First, I employ municipality-level data on election rules from the International City/County Municipal Association (ICMA).[19] The ICMA conducts regular surveys of U.S. municipal governments regarding the number and type of council seats in each city. These surveys — which have been employed by Baqir (2002) and Trebbi et al. (2008), among others — were merged to form muncipality-level panel data from 1970 to 2010.

Second, I obtain data on media coverage of the Voting Rights Act. Specifically, I search newspapers.com for articles containing the phrase "Voting

[17]I am unable to calculate estimates for 1976, for which state-level identifiers are not available, and for those cases where the voting age population of a group is less than 75,000, the minimum threshold used by the Census Bureau for calculating summary statistics. As microdata from the 1968 supplement is not available, voting estimates for that year are derived from the 1972 supplement, which also asked respondents if they voted in the 1968 election. The Census Bureau only began surveying Hispanic or Latino ethnicity in 1974. Thus, for consistency across pre- and post-treatment samples, non-whites include any individual that did not identify as "White."

[18]Whites are those who self-report as "White non-Hispanic." All others are defined as "non-white."

[19]ICMA data from 1980 onwards is available in electronic format, while data for 1970 and 1975 was hand-coded from hard copies. Further discussion of this data is included in the Appendix.

Rights Act."[20] To account for non-random attrition in the database, I limit my search from 1965 to 1980, returning 85,471 mentions from 502 papers.[21] This information is collapsed to form paper-level panel data containing the number of VRA mentions and the total number of digitized pages in each paper-year. Finally, papers are mapped to counties based on the location of their headquarters and merged with information about historical presidential endorsements using data from Gentzkow et al. (2011).

B. Estimating Equation

This paper employs a flexible difference-in-differences (DD) design to estimate the effects of preclearance requirements on county-level voter turnout and Democratic vote share. This model allows me to estimate average treatment effects across all affected areas and to compare those effects across elections, shedding light on the unique time dynamics of preventative anti-discrimination measures. DD estimation is also readily extended to secondary data sources, even in cases with limited sample sizes (as with historical surveys) or where coverage was determined at other geographic levels (as with state-level turnout).

My sample consists of 2,515 counties in 43 states and the District of Columbia and includes all U.S. counties except those that were subject to Section 5 coverage prior to the enactment of the 1975 VRA and those in Alaska, where county-level turnout is not available.[22] The treatment group is comprised of the 283 counties in nine states captured by the 1975 VRA

[20]The search was conducted on "Voting Rights Act" instead of more detailed phrases like "Section 5" or "preclearance" for two main reasons. First, the latter are technical terms largely unfamiliar to the public. Thus, many articles that discuss the preclearance do not include those terms. Second, searching on those phrases returns many false matches wholly unrelated to the VRA (i.e., "Section 5" often refers to the section of a newspaper, while "preclearance" primarily references customs and travel requirements). In light of the labor-intensive nature of the data collection (newspapers.com does not allow automated scraping), the search was kept as broad as possible to limit the amount of false positives and negatives.

[21]While newspapers.com contains nearly 300 million digitized pages from over 5,000 newspapers, it does not include the universe of all U.S. papers nor the full set of pages in each sample paper.

[22]Analysis including previously covered areas is discussed in the Appendix and produces consistent results.

coverage formula, while the control group is composed of the remaining 2,232 counties in 41 states and the District of Columbia that remained uncovered under all versions of the VRA. A descriptive comparison of the treatment and control groups is presented in Table A1.

With this sample, I estimate the following base model comparing changes over time between treatment and control counties:

$$y_{c,t} = \delta_c + \delta_{s,t} + \sum_{\tau \neq 1972} \beta_\tau I_{\tau,t} \times PC_c + \gamma_1 bilingual_{c,t} + \epsilon_{c,t}, \tag{1}$$

where $y_{c,t}$ is the outcome of interest in county c during the presidential election in year t. δ_c and $\delta_{s,t}$ are county and state-year fixed effects, respectively. The coefficients of interest β_τ correspond to the interaction between a set of year indicators, $I_{\tau,t}$, and a treatment group dummy, PC_c, equal to 1 for those counties covered under the 1975 VRA. I control for the VRA's concurrent introduction of language minority protections by including $bilingual_{c,t}$, a dummy variable set to unity if county c is subject to bilingual election requirements in year t. Observations are weighted by eligible voters for turnout and by ballots cast for Democratic vote share. Standard errors are clustered at the state-level, allowing for correlation of errors between observations within the same state.

The inclusion of county and state-year fixed effects controls for static differences in outcome between counties as well as any time-varying, state-level shocks. The latter are important for accounting for the timing of state elections (such as for governor), for the passage of relevant policies by states (which wield signficant electoral influence in the American federalist system) and for any other threats due to underlying state-level trends (such as in demographics, political advertisement, etc.). In the Section IV, I test alternative specifications replacing state-year fixed effects with year and Census division-year controls and find similar results.

While DD models do not require random assignment or that treatment and control groups "look similar," identification relies crucially on a parallel trends assumption. Though there is no way to prove the existence of parallel trends in the counterfactual, the estimates of β_τ for $\tau < 1972$ allow me to test for common pre-treatment trends between treatment and control groups. As demonstrated below, these estimates are close to zero and insignificant in the majority of the models presented. In Section IV, I further validate my findings using a simple regression discontinuity design, which does not require parallel trends for identification.

III. Results

A. Voter Turnout

Figure 3 depicts the coefficients of interest and confidence intervals from estimation of Equation 1 on voter turnout. These results are also shown in Table 2, Column 1. Each point estimate represents the weighted-average difference in turnout between treatment and control counties in that year relative to the same difference in 1972, the last presidential election prior to treatment. In support of parallel trends, the pre-treatment estimates (1960-1972) are close to zero in magnitude. These estimates are also statistically insignificant at the 5 percent level, except for 1968, which — though similar in magnitude — has appreciably smaller standard errors than the other pre-treatment estimates. As shown in Table B2 of the Appendix, this coefficient is *not* statistically different from zero when estimated with state and year multi-way clustered, county-clustered, heteroskedasticity-robust or wild-t bootstrapped standard errors.[23] Furthermore, all of the pre-1972 coefficients are insignificantly different from each other (Wald test: $1960 = 1968$, $p = .927$; $1960 = 1964$, $p = .585$; $1964 = 1968$, $p = .259$). Taken together, these estimates indicate the similarity of time trends between groups prior to treatment.

[Figure 3 about here.]

Following the implementation of oversight restrictions in 1975, average turnout among covered jurisdictions, relative to uncovered counties, gradually

[23]State-clustered standard errors are smaller for DD coefficients near the treatment date, as compared to other specifications, but larger in later years and are thus the most conservative method of estimating post-treatment effects.

increased. These effects are modest at first (2.1 percentage points in 1976), peaking more than twenty years after implementation (8.3 p.p. in 2000) before leveling off. Notably, all post-treatment coefficients are positive and statistically significant at the 5 percent level, demonstrating the persistence of preclearance's impact over several decades. To highlight these broader time dynamics, Table A2 displays the treatment effects averaged across the short- (1976-1988), medium- (1992-2004), and long-runs (2008 onwards).[24] Again, analysis suggests that preclearance led to stable gains in turnout of nearly 8 p.p. over the past two decades.

The gradual and lasting nature of these effects makes sense in context of the policy itself. Because preclearance oversight did not expand the franchise *per se*, there is little reason to expect large increases in voter turnout immediately following its implementation. Instead, the restrictions were designed to limit *future* constrictions of minority voting. Thus, as evidenced here and by the Harris County case study, the benefits of preclearance accumulate in relation to a counterfactual in which new discriminatory changes are enacted over time. Furthermore, because proposed voting changes were only granted preclearance if they were determined not to harm minority representation relative to the *status quo*, any incremental gains in minority turnout that acrued over time served to raise the standard of comparison for all proposed voting changes in the future, essentially locking in the effects from one year to the next.[25]

The long-run effects are large. The 2012 point estimate of 8.1 p.p. represents 15 percent of average turnout in that election (54.9 percent). Though confidence intervals also include more modest gains ranging from less than 1 p.p. to roughly 4 p.p., the estimated treatment effects are in range of those identified by Filer et al. (1991) and Highton (2004) for banning literacy tests (2 to 9 p.p.) and poll taxes (13 p.p. to 15 p.p.). As I will demonstrate later,

[24]Specifically, I estimate Equation 1 replacing the full set of treatment-year interactions with interactions between treatment and a set of period indicators ($I_{\tau_1-\tau_2,t}$), where $I_{\tau_1-\tau_2,t}$ is set to 1 for years between τ_1 and τ_2 and 0 otherwise.

[25]These effects are also consistent with habit-formation among those voters newly enfranchised by preclearance protections. See Gerber et al. (2003); Madestam et al. (2013); Fujiwara et al. (2016).

I find larger effects among minority populations, demonstrating the sizable impact that preventative anti-discrimination measures can have.

As shown in Table 2, these estimates are robust to the inclusion of flexible controls for historical demographic predictors of civic engagement (Smets and Van Ham, 2013). Column 2 displays estimates after controlling for pre-treatment differences in income, eduction and minority share (i.e. by including interactions between a full set of year indicators and county-level measures of non-white population share, college-educated population share and average income in 1970) and voter eligibility (i.e, by interacting year with historical shares of 18- to 21-year-olds, who were not enfranchised until the passage of the 26th Amendment in 1971, and of military personnel, who may not have been able to vote in their county of residence due to deployment during the Vietnam War). Including these controls decreases the magnitude and significance of all pre-treatment coefficients. Though the post-treatment coefficients also decrease in magnitude by about one-third, they remain highly significant, suggesting large treatment effects even when accounting for unobserved trends in, for example, youth activism or minority mobilization.

Column 3 of Table 2 shows similar results after limiting the sample to counties near the coverage cutoffs — specifically, those with 1972 turnout between 40-60% and 1970 language minority share between 0-10%. These estimates are also plotted in Figure A1. Due to noise in the determination measures and non-linearities in the coverage formula, treatment was plausibly exogeneous among this restricted sample. In support of this, all of the pre-treatment coefficients are precise zeros. However, following treatment, I again find gradual increases in voter turnout that persist for several decades. These coefficients are similar in magnitude to those in Column 2 and the majority are statistically significant at the 5 percent level, suggesting that preclearance had lasting effects on covered counties, even when comparing politically and demographically similar areas.

Table A3 corroborates these findings by extending the analysis to include non-presidential elections. Prior to treatment, I find little evidence of differential pre-trends, despite shifting the omitted year to 1974, just one year before the Act's passage. Following treatment, I find larger gains in turnout for midterm elections than for presidential elections. Indeed, the average treatment estimate for the former (6.1 p.p.) is roughly 20% larger than the latter (5.2 p.p.).[26] This is consistent with the fact that most Section 5 objections were lodged against local voting changes and suggests that preclearance's effects on enfranchisement may have extended far beyond presidential turnout.

As I discuss in Section IV, the estimated treatment effects are robust to a host of different specifications and controls and are unlikely to be caused by selection bias or unobserved demographic or political trends. The validity of my model and results are also supported by alternative estimation using regression discontinuity. In sum, these analyses reinforce a causal interpretation of the coefficients of interest, which demonstrate that preclearance protections contributed to long-run gains in voter turnout of 4 to 8 p.p.

Turnout by Race

Using state-level data from the CPS voter supplement, I assess the policy's effects on minority turnout. Here, I exclude all states that were wholly-covered prior to 1975. This drops those Southern states that fell under coverage in 1965 due to their use of literacy tests.

I then use a state-level DD model to compare changes in race-specific turnout over time between covered and uncovered states. In particular, I estimate the following state-level analogue of Equation 1 for white and non-white turnout, separately:

$$y_{s,t} = \delta_s + \delta_{d,t} + \sum_{\tau \neq 1972} \beta_\tau I_{\tau,t} \times PC_s + \epsilon_{s,t}, \tag{2}$$

where, $y_{s,d,t}$ represents turnout among whites (non-whites) in state s at time t. PC_s is a treatment state dummy set to 1 if the entire state was subject to preclearance starting in 1975 (i.e. Texas or Arizona). As observations are at the state-level, I am no longer able to include state-year fixed effects.

[26] Turnout is defined as the maximum voter participation rate among Presidential, Senate, House and gubernatorial elections taking place in a given county on a given Election Day.

Instead, I include state and Census division-year indicators, which account for level differences in turnout between states as well as time-varying regional differences due to, for example, trends in migration. Observations are weighted by each state's voting age population of whites (non-whites) and standard errors are clustered at the state-level.

To help mitigate any state-level confounds, I also estimate Equation 2 controlling for the presence of other state-wide elections (by including indicators for gubernatorial and Senate elections in a given state-year) as well as for county-level bilingual restrictions (by including a dummy set to 1 if a state contains any county that is subject to bilingual elections in that year).

[Figure 4 about here.]

Estimates of β_τ from Equation 2 are plotted in Figure 4 and displayed in Table 3, separately for whites and non-whites.[27] As shown in the right panel of Figure 4, I find little evidence of differential pre-trends between minorities in treatment and control states, as the 1968 DD estimate is insignificantly different from zero. However, following implementation, I find that minority turnout increased steadily over time in covered states, relative to uncovered states, before regressing modestly in recent elections. These results are striking both for their size — the 2012 estimate of 17 p.p. represents 30 percent of average minority turnout (48.5 percent) and is bounded below at 7 p.p. — and for their consistency — all post-treatment coefficients are positive and the majority are highly significant (F-test of joint significance yields $F = 3.7 \times 10^5, p < 0.001$).

These estimates complement the county-level results presented earlier. As before, the coefficients increase with time before leveling off. Furthermore, as minorities comprised roughly 30% of the population in treatment states, the average treatment estimate of 14.2 p.p. (averaging all post-treatment coefficients from Column 3 in Table 3) translates to a net gain in turnout of 3.7 p.p., roughly similar to the corresponding 4.8 p.p. increase recovered from

[27] Treatment estimates averaged across period are also shown in Table A4.

the county-level analysis (Column 1 of Table 2).[28]

As the left panel of Figure 4 shows, I find no effect on white turnout. None of the post-treatment coefficients are statistically significant and most are near zero in magnitude. That I find no significant effects on white turnout is consistent with preclearance's objective to bolster minority participation. As whites historically controlled over 98% of city councils, it is also consistent with findings by Trebbi et al. (2008) and Hajnal et al. (2017) that strategic manipulations of electoral rules by local governments are intended to disenfranchise minorities, specifically.[29] Finally, the null effects for whites imply that the minority turnout estimates are not driven by unobserved threats, as such factors would have had to differentially affect not only treatment states relative to control states in the same Census division-year, but also minorities relative to whites within those states.

In Table A5, I validate the above results using a treatment intensity variable equal to the proportion of each state's population residing in covered counties. Again, I find no effects on white turnout, as the post-treatment estimates are inconsistent in sign and never rise above 0.7 p.p. in magnitude. However, following treatment, I find gradual gains in minority turnout as large as 14 p.p. While these estimates are not individually significant due to the bimodal distribution of the treatment intensity variable, they remain jointly so ($F = 52.7, p < 0.001$).[30] Taken together with the county-level analysis presented earlier, these results suggest that the increased turnout observed under preclearance coverage was driven by large and lasting gains in minority participation.

[28] While the county-level results presented in Table 2 are essentially identified off of counties in partially-covered states, robustness analysis excluding state-year fixed effects shown in Table B6 demonstrate similar effects among the entire treatment group (i.e. including Texas and Arizona).

[29] Information on the racial make-up city councils comes from 1980 ICMA data, the first year for which breakdowns of city council seats by race are available.

[30] $PCintensity_s$ is distributed with a mass point at 1 and all the remaining values clustered below .20, suggesting that the binary treatment variable may be more appropriate.

B. Democratic Vote Share

In line with theories of identity and distributive politics, enfranchisement has been shown to increase political and economic representation for members of marginalized groups (Pande, 2003; Olken, 2010; Cascio and Washington, 2014; Fujiwara, 2015).[31] To the extent that political preferences of whites and minorities differ, changes in minority enfranchisement may alter the net balance of support between political parties. Since to the passage of the Civil Rights Act of 1964, minorities have overwhelmingly voted Democratic (Bositis, 2012).[32] In the 1972 presidential election, Democratic nominee George McGovern received 87 percent of non-white votes, but only 32 percent of white votes. Thus, *ceteris paribus*, one might expect that preclearance coverage, in bolstering turnout among minorities, would also increase Democratic support.

To assess this hypothesis, I examine preclearance's effects on the share of votes cast for Democratic candidates. The coefficients and confidence intervals from estimation of the county-level DD model (Equation 1) including demographic controls are displayed in Figure 5 and Column 5 of Table 2. As with the voter turnout results, I find little evidence of differential trends between treatment and control groups prior to the passage of the 1975 VRA. Two of the three pre-treatment estimates are less than 1 p.p. in magnitude and all are insignificantly different from zero, even at the 35 percent level.

[Figure 5 about here.]

Following implementation, I find significant *decreases* in Democratic vote share among counties subject to Section 5 coverage. Roughly half of the post-treatment estimates are significant at the 5 percent level, and an F-test of their joint significance rejects the null at the 1 percent level ($F = 36.41, p < 0.001$). The point estimates indicate that, among covered counties, average

[31] For theoretical work, see Cox and McCubbins (1986), Lindbeck and Weibull (1987), Osborne and Slivinski (1996), Besley and Coate (1997) and Dixit and Londregan (1998).

[32] While this was part of a larger national trend dating back to the New Deal, data on regional minority voting prior to the 1960's is scarce (Black, 2004) and some historians have credited Southern blacks with helping Dwight Eisenhower win the presidency in 1952 and 1956 (Strong, 1971).

Democratic support dropped by 2.6 percentage points in 1976 and by 5.3 p.p. in the tightly contested 2000 election before recovering in recent years. Though confidence intervals include more modest effects ranging from 0.8 p.p. in 1976 to 1.5 p.p. in 2000, these magnitudes are nonetheless politically relevant. The mean treatment estimate of 3.2 p.p. is greater than half the average presidential margin of victory since 1975 (5.7 p.p.).

As shown in Table 2, I find similar (though larger) effects even without demographic controls. Under the base model (Column 4), all pre-treatment coefficients are statistically insignificant at the 5 percent level. Though the 1960 and 1964 coefficients are relatively large in size, average party support differed by less than one percentage point between treatment and control counties in each election prior to 1972. Rather than indicating differential trends, the pre-treatment coefficients likely reflect large group differences (of 7 p.p.) in Democratic support during the omitted election of 1972, a historical outlier and the largest presidential landslide in U.S. history.[33]

As shown in Column 5, after accounting for the influence of the Vietnam War and 26th Amendment on the 1972 election by controlling for historical military and youth population shares, I find little evidence of differential group pre-trends. These estimates also control for non-white population share in 1970, suggesting that trends in racial polarization are unlikely confounds. Even among a restricted sample of counties near the coverage cutoffs, I find large and significant decreases in Democratic vote share immediately after treatment (Column 6 and Figure A1). As I discuss Section IV, the observed effects are robust to numerous alternative specifications — including flexibly controlling for Democratic support in 1972 — and translated to significant

[33]Democratic nominee George McGovern received only 3 percent of the electoral vote and 18 million fewer popular votes than Republican incumbent Richard Nixon. As political historians have noted, the Vietnam War and the 1971 ratification of the 26th Amendment made "the 1972 election very different from the [previous] presidential elections" (Miller et al., 1976). Prior to America's withdrawal in 1973, over 2.7 million Americans had been deployed to Vietnam. As legislation guaranteeing absentee ballots for military personnel was not passed until 1986, many of these service members were unable to vote. On the other hand, the 26th Amendment allowed 10 million Americans between the ages of 18 to 21 to cast a presidential ballot for the first time in 1972.

changes in Congressional representation. Taken together, these results suggest that preclearance protections meaningfully influenced party support in covered areas. Across the three models, I find immediate effects on Democratic vote share ranging from 2.5 to 3 p.p. that persisted for multiple decades.

The direction and timing of these effects is surprising. Despite increased turnout among Democratic-leaning minorities, I find that oversight restrictions led to *decreased* net support for Democratic candidates. Furthermore, whereas voter protections produced gradual gains in turnout, vote shares immediately shifted in response to coverage. In light of the historical controversy surrounding preclearance's implementation, this partisan swing was not simply a second-order effect of changes in the voter base. Indeed, as I demonstrate in the following subsection, political backlash against Section 5 restrictions likely played a direct role in decreased Democratic support.

Party Affiliation by Race

To examine heterogeneous changes in political preferences by voter race, I rely on individual-level, time-series data from the American National Election Studies. In particular, I investigate responses to a series of questions regarding self-identified party affiliation and support for government aid to minorities.

Thus, I estimate the following DD model — separately for whites and non-whites — comparing political preferences before and after 1975 between individuals in treatment and control counties:

$$y_i = \delta_c + \delta_{d,t} + \beta PC_c \times Post_t + \epsilon_i. \tag{3}$$

Here, y_i is the response of individual i in county c of Census division d during year t, and $Post_t$ is an indicator equal to 1 for years after 1975. Though observations are at the individual-level, I am unable to estimate β if both county and state-year fixed effects are included, because of insufficient within-state treatment heterogeneity in the survey sample. Instead, I include county and Census division-year fixed effects. Standard errors are clustered at the state-level.

[Table 4 about here.]

Table 4 displays the coefficient and standard error for β from estimation of Equation 3 on a number of survey responses. As Column (1) of Panel A shows, following treatment, whites in covered counties were significantly more likely to identify as weakly (7.4 percentage points) or strongly Republican (5.2 p.p). Non-whites, on the other hand, were significantly less likely to identify as Republican. As shown in Column (2), these effects are robust to the inclusion of individual-level demographic controls (i.e., age, education, income and military service). In support of the model's validity, Figure 6 displays results using a flexible DD design and shows no evidence of differential pre-trends in party affiliation among either race group.

[Figure 6 about here.]

These ethnicity-level results align neatly with the county-level effects presented earlier. As whites comprised roughly 80 percent of voter base in the ANES sample, a 7.4 p.p. increase in Republican vote share among this group would more than offset any concurrent Democratic gains from increased minority turnout or support. Indeed, the partisan shift among whites translates to a roughly 6 p.p. decrease in county-wide Democratic vote share, while the combined mobilization and party preference effects among minorities correspond to a 4 p.p. increase in Democratic support.[34] Thus, on net, the race-level estimates for turnout and party affiliation predict an approximately 2 p.p. decrease in Democratic vote share, in range of the average treatment effect of 3 p.p. found in the county-level analysis (i.e., averaging the 1976-1996 DD coefficients for Democratic vote share from Table 2, Column 5).

In Panel B of Table 4, I examine questions concerning whether the government should help to "improve the social and economic positions" of minorities.

[34]Roughly, the change in net vote share can be decomposed as the net change due to party-switching among whites ($.80 \times -7.4 = -5.9$) and minorities ($.20 \times 12 = 2.4$), holding turnout constant, plus the differential change due to increased minority turnout of approximately 1.5 p.p. This last estimate uses the 8.8 p.p. (17 percent) average increase in minority turnout from 1976-1996 (Column 4 of Table 3) and average pre-treatment Democratic vote share of 35% (Table A1.)

Respondents were asked to state their own preferences — scaled from 1 (government should help minorities) to 6 (government should *not* help minorities) — as well as their beliefs about the preferences of the Republican and Democratic parties. Following treatment, I find that whites in covered counties were significantly more likely to *oppose* government aid to minorities, while non-whites were insignificantly more likely to *support* it. However, consistent with the VRA's contentious legislative history, both groups report a growing partisan divide over the government's position towards minorities.

These findings suggest that racial attitudes may have played an important role in explaining the partisan shifts found in covered areas. To test this, I examine changes in Republican identification by interacting preclearance coverage with opposition to minority aid. In particular, I estimate the following equation, separately for whites and non-whites:

$$\begin{aligned} Rep_i = &\delta_c + \delta_{d,t} + \beta_1 NoAid_i + \beta_2 Post_t \times NoAid_i + \beta_3 PC_c \times NoAid_i \\ &+ \beta_4 PC_c \times Post_t + \beta_5 PC_c \times Post_t \times NoAid_i + \epsilon_i, \end{aligned} \tag{4}$$

where Rep_i is an indicator for whether respondent i self-identifies as weakly or strongly Republican and $NoAid_i$ is an indicator equal to 1 if she opposes government aid to minorities.

The results are shown in Column 1 of Table 5. Examining whites, the DD coefficient (β_4) on $PC_c \times Post_t$ is small and statistically insignificant, suggesting that preclearance had little effect on the party affiliation of those who favored minority aid. However, both the triple-difference coefficient (β_5) on $PC_c \times Post_t \times NoAid_i$ and the sum of β_4 and β_5 are large, positive and highly significant, indicating that whites opposed to government support for minorities were significantly more likely (12 p.p.) to identify as Republican following coverage. On the other hand, I find that non-whites in covered areas were significantly *less* likely to identify as Republican following treatment and that this effect was largest among those opposed to minority aid. However, given that the pre-treatment sample includes only eight non-whites in the PC_c-$NoAid_i$ cell, this last result is likely spurious.

[Table 5 about here.]

Please consider these findings with caveats. As noted, the estimates, particularly for non-whites, are derived from small samples. For this reason, I cross-validate the race-level party affiliation analysis using historical Gallup survey data. These results are shown in Tables A7 and A8 and are consistent with the ANES analysis, demonstrating increased polarization between whites and minorities in covered areas that is largely explained by racial attitudes.[35] One may also be concerned that $NoAid_i$ is merely a proxy for conservatism and contains little information about underlying views on race. However, as shown in Column 2 of Table 5, controlling for respondents' self-reported conservatism actually increases the magnitude and significance of β_5, suggesting that racial preferences played an important role in party switching.

Taken together, the observed dynamics strongly complement those discussed by Kuziemko and Washington (2015), who find robust evidence that Southern dealignment, the period spanning the Civil Rights era during which the Deep South transitioned from Democratic to Republican, was caused by political backlash among racially conservative whites.[36] Despite examining a different geographic and temporal setting, I find that white racial attitudes similarly explain Democratic defections following the expansion of federal anti-discrimination oversight. These results suggest a causal link between the application of preclearance restrictions to covered counties and decreased Democratic support in those areas, particularly among whites.

IV. Robustness

A. Regression Discontinuity

This paper's primary DD design is able to recover relevant treatment effects across a range of individual-, county- and state-level data, the validity of

[35] Additional information regarding the Gallup data is included in the Appendix.

[36] That study builds on a large literature in political science demonstrating the salience of racial preferences on vote choice and turnout, such as Key and Heard (1950); Carmines and Stimson (1989); Kousser (2010); Tesler and Sears (2010); Enos (2016).

which are supported by consistent evidence of parallel pre-treatment trends. Nonetheless, the model's primary identifying assumption is untestable.

One alternative strategy for estimating county-level effects is a regression discontinuity (RD) design. While RD does not rely on parallel trends assumptions, it is only able to recover *local* average treatment effects around the policy triggers, a limitation that is exacerbated by the scarce historical variation around those triggers (i.e., only 16 counties in uncovered states were within 3 p.p. of both the minority and turnout cutoffs). That being said, RD estimates are consistent and allow me to corroborate my findings under alternative assumptions.

Thus, I employ a simple RD design, which relies only on exogeneity around the historical turnout trigger for identification. In particular, I restrict the sample to counties with greater than 5% language minority share in 1970 and estimate the following equation:

$$(5) \quad y_{c,t} = \beta_1 Less50_c + \beta_2 turnout1972_c + \beta_3 Less50_c \times turnout1972_c + \epsilon_{c,t}.$$

Here, $turnout1972_c$ represents turnout in county c in 1972 (normalized to 0 at the 50% cutoff), which is allowed to have different slopes on either side of the discontinuity. The coefficient of interest β_1 represents the effect of crossing the cutoff from the right, which activated preclearance coverage among high-minority counties. Observations are weighted by voting eligible population and votes cast. Bandwidths include all high-minority counties within 20 percentage points of the turnout trigger, except those in the wholly-covered states of Texas and Arizona, where preclearance was determined by state (rather than county) turnout. To account for limited power, I pool observations across all post-treatment years.

[Figure 7 about here.]

Panel A of Figure 7 displays the relationship between the running variable and average post-treatment turnout and Democratic vote share. Notably, crossing from just above the 50% cutoff to just below it is associated with a significant jump in post-treatment turnout. The point estimate for β_1 suggests a 6.9 p.p. increase in turnout at the cutoff (Table A9, Column 3). The coverage trigger is also associated with a significant decrease in Democratic vote share of 5.4 p.p. In both cases, the local effects strongly resemble the average effects recovered from the difference-in-differences estimation.

Panel B estimates the same RD model across all *pre*-treatment years. Prior to 1975, no significant discontinuity in turnout or Democratic vote share exists at the coverage trigger. Since preclearance coverage did not begin until 1975, the null effects corroborate the policy's exogeneity. That is, controlling for the assignment variable, there is little evidence that historical turnout or party support differed between treatment counties just below the threshold and control counties just above it. Table A9 displays results using other bandwidths and including controls for year, bilingual restrictions and historical minority share. Across the models, I find consistent support for post-treatment effects and little evidence of pre-treatment discontinuities.

To examine effects over time, Figure A2 and Table A10 display RD coefficients from estimation of Equation 5 separately for each election in the sample. Though none of the coefficients are statistically significant due to limited power, their direction, magnitude and dynamics strongly resemble the DD estimates presented in Figures 3 and 5 with immediate changes in party support and gradual gains in turnout. The long-run similarities are particularly noteworthy. Indeed, the RD coefficients for 2016 turnout (7.0 p.p.) and vote share (-2.3 p.p.) differ by just around one percentage point from their respective DD estimates (7.8 p.p. and -1.2 p.p.).

Taken as a whole, it seems highly unlikely that unobserved confounds could produce nearly identical treatment estimates across two distinct estimation strategies. Thus, as causal identification under regression discontinuity does *not* require parallel trends, these results help to validate the paper's difference-in-differences model and preclearance's effects on county turnout and vote share.

B. Noisy Determination Measures

Because the VRA's coverage formula primarily captured counties with low turnout and high minority shares, one may be concerned that the estimated effects are merely the result of mean reversion or other political trends affecting areas with large minority populations. However, as I displayed in Table 2 and Figure A1, I find similar estimates even when restricting the analysis to counties within 5 p.p. of the minority cutoff and 10 p.p. of the turnout cutoff and when controlling for historical demographic differences.

In Column 1 of Table A11, I show that my findings are robust to even further limitations of the sample. Restricting analysis to the 169 counties within 8 p.p. of the turnout cutoff and 4 p.p. of the minority share cutoff, the significance, sign and magnitude of the treatment estimates are largely preserved. These results suggest that preclearance had lasting effects even among counties with similar political and demographic characteristics. As I demonstrate in Column 2, I find consistent, though less significant, effects when analyzing neighboring counties (i.e., restricting the sample only to those treatment counties that are adjacent to at least one control county and to those control counties that are adjacent to at least one treatment county). Thus, the estimated effects are likely not driven by unobserved local trends or the non-random distribution of treatment counties throughout the country.

Another concern may be that Congress strategically crafted the coverage formula to capture specific areas and that selection bias, rather than the policy itself, accounts for the observed changes. Though precise manipulation was impossible since the Census Bureau did not calculate the determination measures until after the Act's passage, policymakers could have used turnout and population measures from prior years as proxies. Thus, in Column 3 of Table A11, I restrict the sample only to those counties with the highest predicted likelihood of coverage, based on county turnout and minority share from the 1960's. Again, I find similar estimates, indicating that the observed effects are the result of actual preclearance coverage as opposed to confounds related to the criteria determining that coverage.

As an alternative method of addressing potential selection bias, I also estimate models flexibly controlling for the coverage determination measures as well as for each county's probability of coverage and historical party support. These results are shown in Table A12. In Column 1 and Figure A3, I display estimation results after including interactions between year indicators and 1972 turnout and 1970 language minority share. In Column 2, I control for year and 1972 Democratic vote share interactions. In Column 3, I flexibly control for predicted likelihood of coverage.

In all cases, I find similar effects to those presented in the main results, though controlling for the coverage determination figures produces smaller turnout estimates. Prior to 1975, I find little evidence of differential trends. After 1975, I find significant increases (decreases) in turnout (Democratic vote share) among treatment counties that again peak roughly twenty years after implementation. These estimates bolster the validity of my main findings and support preclearance's exogeneity. Historical variation in turnout, minority population and Democratic support cannot fully explain the observed treatment effects. Nor do the estimates appear biased by Congressional targeting.

C. Election Outcomes

Given the large observed effects on party support, one would expect that preclearance coverage also impacted election outcomes. Thus, I examine changes in the political ideology of elected Congressional representatives using DW-NOMINATE data. DW-NOMINATE collapses a representative's legislative roll-call voting in a given Congressional session into a two-dimensional ideal point, the first dimension of which is commonly understood to be a measure of conservatism (scaled from -1 to 1) and has been employed by numerous political scientists and economists to examine changes in political ideology over time (Poole and Rosenthal, 2001; Erikson and Tedin, 2015; Gentzkow et al., 2016).[37]

[37]While the second dimension of DW-NOMINATE historically tracked to policy issues that cut across party lines, such as bimetallism and slavery, Congressional voting since the 1960's has been virtually unidimensional (McCarty et al., 1997). As Bateman and Lapinski (2016) note, "this measure bears little relationship to patterns of voting on civil rights" and,

In particular, I estimate a district-level analogue of Equation 1 on the Republican affiliation and first dimension DW-NOMINATE scores of House representatives. Treatment is defined as districts containing at least one covered county. Because district boundaries change over time, I include fixed effects for the counties that comprise each district as well as for Census division-Congress.[38]

These results are shown in Figure A4 and Table A13. Prior to treatment, I find no evidence of differential trends in conservatism or party affiliation between covered and uncovered districts. After preclearance's expansion, the probability of electing a Republican representative spiked among newly covered districts by as much as 35 p.p. Similar to the Democratic vote share results, this effect recedes over time. However, I observe lasting changes in representative ideology. Immediately following treatment, conservatism jumped significantly in covered districts and continued to increase until the most recent Congress. Taken together, these results corroborate the Democratic vote share estimates and suggest that preclearance coverage may have had large ramifications on policy-making.

D. Other Robustness

The Appendix includes other analyses demonstrating that the observed treatment effects are unlikely to be caused by statistical artifacts or unobserved confounds. As mentioned in Section III, Table B2 shows robustness to alternative methods of calculating standard errors. In Table B3 Columns 1 and 2, I use randomized treatment placebos to provide evidence that the effects were not caused by serial correlation. Column 3 and 4 of the same table further demonstrate that treatment and control areas did not experience

during the 1970's, second dimension scores of Southern Republicans were actually higher than those of Northern Democrats but lower than those of Southern Democrats. Given the difficulty of interpreting these scores, results from their analysis, which reveal no clear or consistent treatment effect, are left to the Appendix.

[38]Counties are mapped to districts using relationship files generously provided by James Snyder, as well as hand-coded information from the Congressional District Atlas. The Appendix includes additional discussion of the DW-NOMINATE data and its construction.

differential trends in population or minority share over time. In Table B4, I find that placebo tests based on "faux" treatments cutoffs around the actual trigger fail to produce similar post-treatment estimates. Table B5 shows that the inclusion of time-varying demographic controls and alternative controls for bilingual language restrictions does not alter the sign, magnitude or significance of my primary estimates. Table B6 shows that treatment effects are largely invariant to alternative area-time fixed effects. Finally, Table B7 demonstrates similar results when examining other measures of turnout and party vote share.

V. Mechanisms

A. Voter Turnout

Electoral Rules

As preclearance's enforcement depended on the subjective analysis of context-specific voting changes across a wide range of election rules, a comprehensive accounting of its effects on electoral policymaking is impractical and neglects the policy's *raison d'etre*. Nonetheless, better understanding the mechanisms behind minority turnout gains is vital to future policy efforts to safeguard enfranchisement.

Thus, utilizing data from the International City/County Municipal Association, I assess preclearance's effects on the prevalence of one particular type of electoral rule: at-large election systems. In prior research, Trebbi et al. (2008) found that white city councils respond to minority political threats by switching from district-based systems, in which council seats are reserved by neighborhood, to at-large or "winner-take-all" systems, which may dilute minority voting power by awarding council seats based on city-wide voting. As proposed changes to at-large systems comprised the plurality of Section 5 objections until the 1980's, examining their prevalence may provide insight into preclearance's broader effects on voter discrimination.

Using a municipality-level analogue of Equation 1, I estimate the effects of preclearance coverage on whether a city employed at-large elections and on the share of a city's council seats awarded by those elections. These results are shown in Figure A5 and Table A14.

From 1970 to 1975, I observe no differential change in the use of at-large elections between treatment and control cities. However, by 1980, covered areas were significantly less likely to employ at-large elections, relative to uncovered areas. After treatment, these cities also exhibited significant decreases in the share of seats elected at-large. Notably, these results mirror those observed in the voting analysis, with gradual effects that peak more than a decade after preclearance's implementation.

The same logic explaining the changes in turnout applies even more directly here. Because preclearance restrictions precluded many covered cities from ever implementing at-large systems, the gradual adoption of "winner-take-all" rules in uncovered cities would produce growing relative differences between the two groups. Though at-large systems are but one margin by which local officials may influence the election process, these findings are strongly suggestive of preclearance's prophylactic effect on voter discrimination and are consistent with the observed increases in minority turnout.

Historical Discrimination

To further investigate the role of voter discrimination, I examine whether preclearance's effects on turnout varied according to historical racial disparities in an area. Specifically, I modify Equation 1 to estimate the following linear triple-differences (DDD) model:

$$\begin{aligned} y_{c,t} =& \delta_c + \delta_{s,t} + \beta_1 PC_c \times Post_t + \beta_2 PC_c \times Post_t \times discriminate_{c,1970} \\ &+ \sum_{\tau \neq 1972} \lambda_\tau I_{\tau,t} \times discriminate_{c,1970} + \gamma_1 bilingual_{c,t} + \epsilon_{c,t}, \end{aligned} \tag{6}$$

where $discriminate_{c,1970}$ is a defined as one of three predictors of pre-treatment racial discrimination in county c: the ratio of average white to minority income, the difference between minority and white poverty rates and the difference between minority and white illiteracy rates (Loury, 1977; Williams, 1999; Farkas, 2003). Interactions between $discriminate_{c,1970}$ and year account for time-varying differences due to historical racial disparities, while the DDD coefficient (β_2) represents heterogeneous treatment effects based on discrimination, averaged over all post-treatment years. For ease of comparison, I employ a single pre-post DD coefficient (β_1) demonstrating average treatment effects among areas with no historical discrimination.

Table A15 displays the coefficients and standard errors for β_1 and β_2 from estimation of Equation 6 including demographic controls. Consistent with my main findings, preclearance significantly increased voter turnout in covered areas. However, I find larger treatment effects among areas with greater historical racial disparities in income, poverty and education, though this last differential is not statistically significant. These differences are large relative to the base treatment effects (β_1). Compared to treatment areas with no historical discrimination, predicted turnout gains for counties with mean historical income, poverty and education disparities are 22%, 83% and 20% greater, respectively.[39] In line with preclerance's objective, these results suggest that reduced voter discrimination contributed to the observed turnout effects.

B. Democratic Vote Share

Media Coverage

Though the role of civil rights legislation on party-switching is supported by the ANES analysis presented in Section III as well as by other research (Wattenberg, 1991; Valentino and Sears, 2005; Kuziemko and Washington, 2015), I provide further evidence of preclearance's political salience using historical newspaper data. Throughout the 20th century, newspapers were not only the primary source of information regarding local and state politics but also partisan agents themselves (Hamilton, 2004; Strömberg, 2004; Gentzkow et al., 2006, 2011). Thus, if preclearance was an important political issue for

[39] Based on average treatment income ratio of 2.1, poverty gap of 27.4 p.p. and illiteracy gap of 8.1 p.p.

voters in covered areas, one might expect to see differential changes in media coverage of the VRA around its enactment.

To this end, I estimate changes in media mentions of the VRA using a newspaper-level analogue of Equation 1 controlling for paper and region-time effects. To account for attrition within the newspapers.com data, the outcome of interest is defined as the ratio of VRA mentions to the total number of digitized pages in a paper-year. These results are displayed in Figure A6 and Table A16.

Notably, all pre-treatment coefficients after 1970 are precise zeros, suggesting common trends in press coverage immediately prior to the expansion of Section 5 restrictions. However, beginning in 1975, the number of VRA mentions increased sharply among newspapers located in newly covered areas. Relative to the control group, treatment newspapers referenced the Act roughly once more per dozen pages in 1975. This increased media attention persisted for several years, as evidenced by the positive coefficients from 1976 to 1979.

A few points are worth noting here. First, the VRA was a politically salient topic throughout the sample. Prior to 1975, papers mentioned the Act about once every 15 pages, indicating that the public was aware of the civil rights legislation and that it was a non-trivial issue. Second, the increased media coverage due to treatment was quite large, as the point estimate for 1975 (.08) exceeds the pre-treatment mean.[40] Last, the small, negative coefficients for 1965 and 1970 — when the VRA was first enacted and extended, respectively — suggest that the 1975 spike was driven specifically by discussion of preclearance and its application to covered areas, as opposed to more general aspects of the Act.

Though rigorous text analysis is outside the scope of this study, I exam-

[40] Though it is possible that control papers also served readers in treatment areas (and vice versa), the dataset is primarily comprised of local papers, which distribute a large majority of copies to readers in the same county (Gentzkow and Shapiro, 2010). If spillover effects do exist, they would likely bias my estimates towards zero, suggesting that the observed effects may represent a lower bound of actual disparities in local public and media focus on the VRA.

ine partisan responses to the VRA by exploiting heterogeneity in newspaper endorsements of President Nixon in 1972. During Congressional debate over the Act's 1970 extension, Nixon's administration proposed eliminating preclearance restrictions entirely. Though the proposal ultimately failed, House Republicans overwhelmingly voted in its favor.[41]

[Figure 8 about here.]

As shown in Figure 8, media coverage of the VRA spiked among all treatment papers, regardless of political affiliation. However, increases in VRA mentions were twice as large among papers that endorsed Nixon as those that did not (i.e., comparing the 1975 point estimates of .146 and .063, respectively). Furthermore, heightened media attention of the VRA persisted for many years among Nixon-endorsing papers, but quickly dissipated among others. Given that partisan media often reflects the preferences of its audience (Mullainathan and Shleifer, 2008; Gentzkow and Shapiro, 2010; Gentzkow et al., 2014), these findings suggest that preclearance remained a political flashpoint years after its implementation and corroborate the existence of conservative backlash against the legislation.

Political Contact

To further examine the role of political and media exposure on party-switching, I return to the ANES survey data. Specifically, I estimate the simple DD model presented in Equation 3 on whether respondents were contacted by a political party "to get them to vote for their candidate" and on the number of forms of media they reported reading, seeing or hearing about an election. These estimates are presented in Table 6.

[Table 6 about here.]

Examining whites (Column 1), I find no effect of Section 5 coverage on media exposure, suggesting that the observed spike in newspaper mentions of the

[41] For a comprehensive legislative history of Section 5, see Kousser (2007)

VRA was not driven by a broader growth in political media. However, whites report significant increases in political contact following treatment. Though outreach by both major parties increased, changes in Republican contact (13.2 p.p.) were more than twice as large as those in Democratic contact (6.1 p.p.). As Column 2 demonstrates, I find no evidence of increased exposure to political targeting or media among minorities. Following treatment, non-whites report significantly *reduced* election media consumption and Republican contact and no change in Democratic contact.

Taken together, these results help to explain both the turnout and vote share effects. That non-whites experienced less political and media exposure after treatment suggest that the observed gains in minority turnout were the result of actual changes in voter protections and ballot access, as opposed to increased political campaigning and outreach. Furthermore, the differential increases in Republican targeting of whites are consistent with the rightward shift observed among treatment areas and accord with a theory of white backlash against federal oversight restrictions.

VI. Discussion

In light of the 2013 Shelby decision, determining whether historical gains in turnout will persist without continued federal oversight of election laws is critical to the future of the Voting Rights Act. I provide preliminary insight into this issue by examining the 2016 presidential election.

In particular, I employ a flexible, county-level DD model similar to that used in the paper's primary analysis (Equation 1), except here the treatment group is expanded to capture *all* counties freed from federal oversight by the Shelby ruling — including those initially covered by the 1965 and 1970 VRAs — and the sample period is shifted to examine elections after 1965, when preclearance was first implemented.[42] These estimates are plotted in Figure 9 and displayed in Column 1 of Table 7. In interpreting the results, note that

[42] As the 1965 VRA brought under coverage many states in their entirety, I also modify Equation 1 to include Census division-year fixed effects in place of state-year fixed effects, which would otherwise absorb all the treatment variation from those areas.

the omitted year is 2012. Thus, negative DD coefficients for years before 2012 indicate differential *increases* in turnout among treatment counties from prior elections to 2012, while a negative coefficient for 2016 indicates a differential *decrease* in turnout from 2012 to 2016.

[Figure 9 about here.]

Prior to the Shelby decision, treatment areas experienced larger historical increases in turnout, relative to control counties. These results are reminiscent of the main estimates presented in Section III and suggest that preclearance had similar effects on areas covered by prior VRAs as on those covered by the 1975 revision, a claim I further support in Table B8 of the Appendix. Following the Shelby decision, treatment counties experienced a significant differential *decrease* in turnout of 1.5 percentage points, the single largest year-to-year drop in the sample. Replicating this analysis on state-level turnout by race (Columns 2 and 3 of Table 7), I find that, while white turnout remained unchanged, minority participation dropped by 2.1 p.p. after the Shelby ruling.

These effects are only suggestive and are presented without claims of causality. Nevertheless, the change in direction around the ruling — whereas turnout in covered counties was differentially increasing prior to 2012, it differentially decreased following 2012 — implies that recently enacted election laws may have negated many of the gains made under preclearance. Evidence that participation decreased only among minorities further bolsters federal claims regarding the targeted and discriminatory nature of these laws. While the true impact of the Supreme Court's decision may not be known for several years, these results provide early evidence that the Shelby ruling may jeopardize decades of voting rights progress.

VII. Conclusion

This study exploits the 1975 revision of the Voting Rights Act to identify the causal effects of preclearance restrictions. The estimated gains in voter turnout are large — ranging from 4 to 8 percentage points — and lasting —

having persisted for 40 years. Importantly, I find that increases in turnout were driven entirely by participation among minorities and provide evidence that these gains were facilitated by reduced voter discrimination. These results are the first estimates of preclearance's impact and demonstrate the effectiveness of broad, prophylactic anti-discrimination measures.

Surprisingly, I find that preclearance led to net *decreases* in Democratic support due to defections among whites, particularly those opposed to government support for minorities. This effect is corroborated by newspaper analysis demonstrating the political controversy surrounding oversight restrictions in newly covered areas. These findings complement the existing literature by illustrating the political importance of "white backlash" against minority threats in other contexts and settings.

This paper suggests several additional areas of research. Most obviously, updating the analysis following future elections would allow researchers to better understand the ramifications of the Shelby County decision. Alternatively, as elections of school boards, sheriffs, judges and mayors are often decided by small blocs of voters, examining local settings could shed more light on preclearance's economic and political implications. A closer investigation of the interplay between race, media and politics would also be an important area of study, especially considering the salience of race in public and political discourse during and after the 2016 presidential election.

The preclearance process itself poses interesting research questions. Descriptive statistics show a shift in the types of changes submitted over time, as well as a decreased propensity of objection. Though Chief Justice Roberts took the latter as "illuminating" evidence that covered counties had become less discriminatory, this interpretation elides the possibility of strategic interactions between local election officials and federal supervisors in the costly submissions and approvals process.

Perhaps most importantly, this paper provides valuable information for policymakers as Congress considers if and how to reinstate preclearance restrictions. In explaining the Court's decision to strike down the previous coverage formulas, Chief Justice Roberts noted that "voter turnout and registration rates now approach parity" between covered and uncovered jurisdictions. Yet, the relevant question in determining preclearance's fate may not be whether covered and uncovered jurisdictions *are* different today, but whether they *would have been* different without preclearance. This paper provides the first insight into just such a counterfactual.

References

Adler, A. and M. Kousser (2011). The Voting Rights Act in the 21st Century: Reducing Litigation and Shaping a Country of Tolerance. *Stanford Undergraduate Research Journal*.

Alesina, A., R. Baqir, and C. Hoxby (2004). Political jurisdictions in heterogeneous communities. *Journal of political economy 112*(2), 348–396.

Baqir, R. (2002). Districting and government overspending. *Journal of political Economy 110*(6), 1318–1354.

Bateman, D. A. and J. Lapinski (2016). Ideal points and american political development: Beyond dw-nominate. *Studies in American Political Development 30*(2), 147–171.

Bennett, J. (2013). A New Defense of Voting Rights. *New York Times*.

Bertrand, M., E. Duflo, and S. Mullainathan (2002). How Much Should We Trust Differences-in-Differences Estimates? *National Bureau of Economic Research*.

Besley, T. and S. Coate (1997). An Economic Model of Representative Democracy. *The Quarterly Journal of Economics*, 85–114.

Besley, T., T. Persson, and D. M. Sturm (2010). Political competition, policy and growth: theory and evidence from the us. *The Review of Economic Studies 77*(4), 1329–1352.

Besley, T. and A. Prat (2006). Handcuffs for the grabbing hand? media capture and government accountability. *The American Economic Review 96*(3), 720–736.

Black, M. (2004). The transformation of the southern democratic party. *The Journal of Politics 66*(4), 1001–1017.

Bonica, A. (2014). Mapping the ideological marketplace. *American Journal of Political Science 58*(2), 367–386.

Bositis, D. A. (2012). *Blacks and the 2012 Democratic National Convention.* Joint Center for Political and Economic Studies.

Cameron, A. C., J. B. Gelbach, and D. L. Miller (2008). Bootstrap-Based Improvements for Inference with Clustered Errors. *The Review of Economics and Statistics 90*(3), 414–427.

Carmines, E. G. and J. A. Stimson (1989). *Issue Evolution: Race and the Transformation of American Politics.* Princeton University Press.

Cascio, E. and E. Washington (2014). Valuing the Vote: The Redistribution of Voting Rights and State Funds following the Voting Rights Act of 1965.

Quarterly Journal of Economics 129(1), 379–433.

Chiang, C.-F. and B. Knight (2011). Media bias and influence: Evidence from newspaper endorsements. *The Review of Economic Studies 78*(3), 795–820.

Cox, G. W. and M. D. McCubbins (1986). Electoral Politics as a Redistributive Game. *The Journal of Politics 48*(02), 370–389.

Dixit, A. and J. Londregan (1998). Fiscal Federalism and Redistributive Politics. *Journal of Public Economics 68*(2), 153–180.

Enos, R. D. (2016). What the demolition of public housing teaches us about the impact of racial threat on political behavior. *American Journal of Political Science 60*(1), 123–142.

Erikson, R. S. and K. L. Tedin (2015). *American public opinion: Its origins, content and impact*. Routledge.

Farkas, G. (2003). Racial Disparities and Discrimination in Education: What Do We Know, How Do We Know It, and What Do We Need to Know? *Teachers College Record 105*(6), 1119–1146.

Filer, J. E., L. W. Kenny, and R. B. Morton (1991). Voting Laws, Educational Policies, and Minority Turnout. *Journal of Law and Economics*, 371–393.

Fujiwara, T. (2015). Voting Technology, Political Responsiveness, and Infant Health: Evidence from Brazil. *Econometrica 83*(2), 423–464.

Fujiwara, T., K. Meng, and T. Vogl (2016). Habit Formation in Voting: Evidence from Rainy Elections. *American Economic Journal: Applied Economics*.

Gentzkow, M., E. L. Glaeser, and C. Goldin (2006). The rise of the fourth estate. how newspapers became informative and why it mattered. In *Corruption and Reform: Lessons from America's Economic History*, pp. 187–230. University of Chicago Press.

Gentzkow, M., N. Petek, J. M. Shapiro, and M. Sinkinson (2015). Do newspapers serve the state? incumbent party influence on the us press, 1869–1928. *Journal of the European Economic Association 13*(1), 29–61.

Gentzkow, M. and J. M. Shapiro (2010). What drives media slant? evidence from us daily newspapers. *Econometrica 78*(1), 35–71.

Gentzkow, M., J. M. Shapiro, and M. Sinkinson (2011). The Effect of Newspaper Entry and Exit on Electoral Politics. *The American Economic Review 101*(7), 2980–3018.

Gentzkow, M., J. M. Shapiro, and M. Sinkinson (2014). Competition and ideological diversity: Historical evidence from us newspapers. *The American Economic Review 104*(10), 3073–3114.

Gentzkow, M., J. M. Shapiro, and M. Taddy (2016). Measuring polarization in high-dimensional data: Method and application to congressional speech. Technical report, National Bureau of Economic Research.

Gerber, A. S., D. P. Green, and R. Shachar (2003). Voting May Be Habit-Forming: Evidence from a Randomized Field Experiment. *American Journal of Political Science 47*(3), 540–550.

Hajnal, Z., N. Lajevardi, and L. Nielson (2017). Voter identification laws and the suppression of minority votes. *The Journal of Politics 79*(2), 000–000.

Hamilton, J. (2004). *All the news that's fit to sell: How the market transforms information into news.* Princeton University Press.

Highton, B. (2004). Voter Registration and Turnout in the United States. *Perspectives on Politics 2*(03), 507–515.

Holder, E. (2013). Shelby County v. Holder.

Husted, T. A. and L. W. Kenny (1997). The Effect of the Expansion of the Voting Franchise on the Size of Government. *Journal of Political Economy*, 54–82.

Key, V. O. and A. Heard (1950). Southern Politics in State and Nation.

Kousser, J. M. (2007). The strange, ironic career of section 5 of the voting rights act, 1965-2007. *Tex. L. Rev. 86*, 667.

Kousser, J. M. (2010). The Immutability of Categories and the Reshaping of Southern Politics. *Annual Review of Political Science 13*, 365–383.

Kuziemko, I. and E. Washington (2015). Why Did the Democrats Lose the South? Bringing New Data to an Old Debate. *National Bureau of Economic Research*.

Leadership Conference Education Fund (2016). The Great Poll Closure. Technical report.

Lindbeck, A. and J. W. Weibull (1987). Balanced-Budget Redistribution as the Outcome of Political Competition. *Public Choice 52*(3), 273–297.

Loury, G. (1977). A Dynamic Theory of Racial Income Differences. *Women, Minorities, and Employment Discrimination 153*, 86–153.

Madestam, A., D. Shoag, S. Veuger, and D. Yanagizawa-Drott (2013). Do Political Protests Matter? Evidence from the Tea Party Movement. *The Quarterly Journal of Economics*, 1633–1685.

May, G. (2013). *Bending toward Justice: The Voting Rights Act and the Transformation of American Democracy.* Basic Books.

McCarty, N. M., K. T. Poole, and H. Rosenthal (1997). Income redistribution and the realignment of american politics.

Menand, L. (2013). The Color of Law. *The New Yorker*.

Miller, A. H., W. E. Miller, A. S. Raine, and T. A. Brown (1976). A Majority Party in Disarray: Policy Polarization in the 1972 Election. *American Political Science Review 70*(03), 753–778.

Miller, G. (2008). Women's Suffrage, Political Responsiveness, and Child Survival in American history. *The Quarterly Journal of Economics 123*(3), 1287.

Mullainathan, S. and A. Shleifer (2008). The market for news. In *Economics, Law and Individual Rights*, pp. 90–122. Routledge.

Nidever, S. (2012). Kings Still Punished under Voting Rights Act. *Hanford Sentinel*.

Northwest Austin Municipal Utility District No. 1 v. Holder (2009).

Olken, B. A. (2010). Direct Democracy and Local Public Goods: Evidence from a Field Experiment in Indonesia. *American Political Science Review 104*(02), 243–267.

Osborne, M. J. and A. Slivinski (1996). A Model of Political Competition with Citizen-Candidates. *The Quarterly Journal of Economics*, 65–96.

Pande, R. (2003). Can Mandated Political Representation Increase Policy Influence for Disadvantaged Minorities? Theory and Evidence from India. *The American Economic Review 93*(4), 1132–1151.

Persson, T., G. Roland, and G. Tabellini (2000). Comparative politics and public finance. *Journal of political Economy 108*(6), 1121–1161.

Pitts, M. J. (2003). Section 5 of the Voting Rights Act: A Once and Future Remedy. *Denver University Law Review 81*, 225.

Poole, K. T. and H. Rosenthal (1985). A spatial model for legislative roll call analysis. *American Journal of Political Science*, 357–384.

Poole, K. T. and H. Rosenthal (2001). D-nominate after 10 years: A comparative update to congress: A political-economic history of roll-call voting. *Legislative Studies Quarterly*, 5–29.

Poole, K. T. and H. L. Rosenthal (2011). *Ideology and congress*, Volume 1. Transaction Publishers.

Posner, M. A. (2006). The real story behind the justice department's implementation of section 5 of the vra: Vigorous enforcement, as intended by congress. *Duke J. Const. L. & Pub. Pol'y 1*, 79.

Schuit, S. and J. C. Rogowski (2016). Race, representation, and the voting rights act. *American Journal of Political Science*.

Seguiin Gazette (1975). Application to Texas is Bitterly Resented Fraud.

Shelby County v. Holder (2013).

Smets, K. and C. Van Ham (2013). The Embarrassment of Riches? A Meta-Analysis of Individual-Level Research on Voter Turnout. *Electoral Studies 32*(2), 344–359.

Snyder Jr, J. M. and D. Strömberg (2010). Press coverage and political accountability. *Journal of political Economy 118*(2), 355–408.

South Carolina v. Katzenbach (1966).

Springer, M. J. (2014). *How the States Shaped the Nation: American Electoral Institutions and Voter Turnout, 1920-2000*. University of Chicago Press.

Stanley, H. (1987). *Voter Mobilization and the Politics of Race: The South and Universal Suffrage, 1952-1984*. Praeger Publishers.

Strömberg, D. (2004). Mass media competition, political competition, and public policy. *The Review of Economic Studies 71*(1), 265–284.

Strong, D. S. (1971). Further reflections on southern politics. *The Journal of Politics 33*(2), 239–256.

Tesler, M. and D. O. Sears (2010). *Obama's race: The 2008 election and the dream of a post-racial America*. University of Chicago Press.

Trebbi, F., P. Aghion, and A. Alesina (2008). Electoral Rules and Minority Representation in U.S. Cities. *The Quarterly Journal of Economics*, 325–357.

Tucker, J. T. (2006). Enfranchising Language Minority Citizens: The Bilingual Election Provisions of the Voting Rights Act. *N.Y.U. Journal of Legislation & Public Policy 10*, 195.

Valentino, N. A. and D. O. Sears (2005). Old times there are not forgotten: Race and partisan realignment in the contemporary south. *American Journal of Political Science 49*(3), 672–688.

Vallely, R. (2009). *The Two Reconstructions: The Struggle for Black Enfranchisement*. University of Chicago Press.

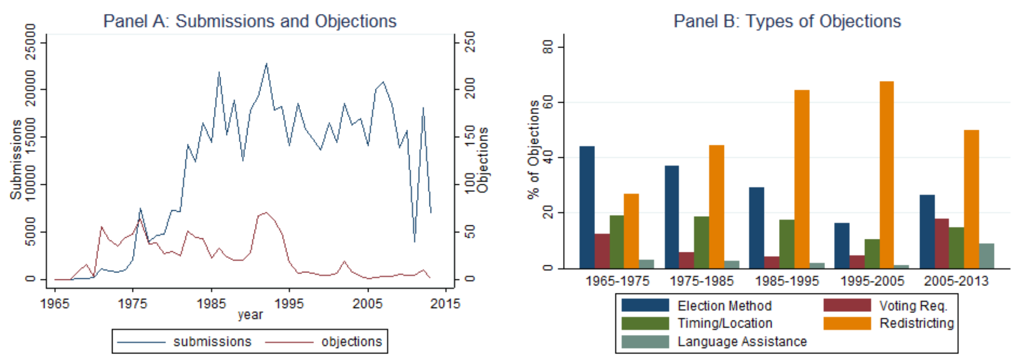
Washington, E. et al. (2012). Do Majority-Black Districts Limit Blacks' Representation? The Case of the 1990 Redistricting. *Journal of Law and Economics 55*(2), 251–274.

Wattenberg, M. P. (1991). The building of a republican regional base in the south the elephant crosses the mason-dixon line. *Public Opinion Quarterly 55*(3), 424–431.

Williams, D. R. (1999). Race, Socioeconomic Status, and Health: The Added Effects of Racism and Discrimination. *Annals of the New York Academy of Sciences 896*(1), 173–188.

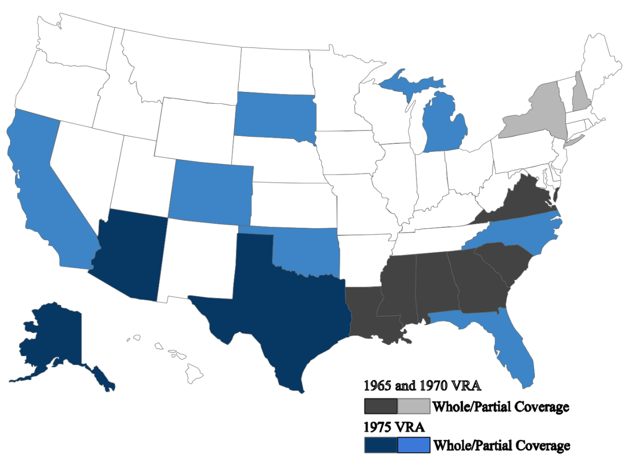
Wolters, R. (1996). *Right Turn: William Bradford Reynolds, the Reagan Administration and Black Civil Rights*. Transaction Publishers.

Figure 1: Preclearance Enforcement over Time



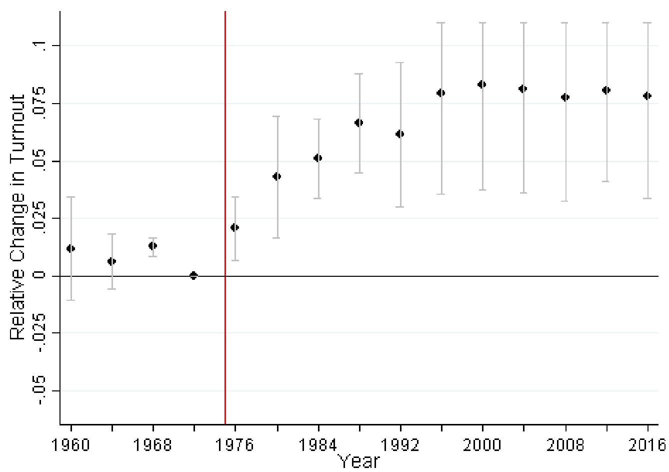
Notes: Data come from U.S. Department of Justice and author's calculations. Panel A depicts the number of preclearance submissions and objections over time. Panel B depicts the types of objections lodged by decade. For example, over 40 percent of objections from 1965-1975 pertained to changes in election methods.

Figure 2: Jurisdictions by Year of Preclearance Coverage



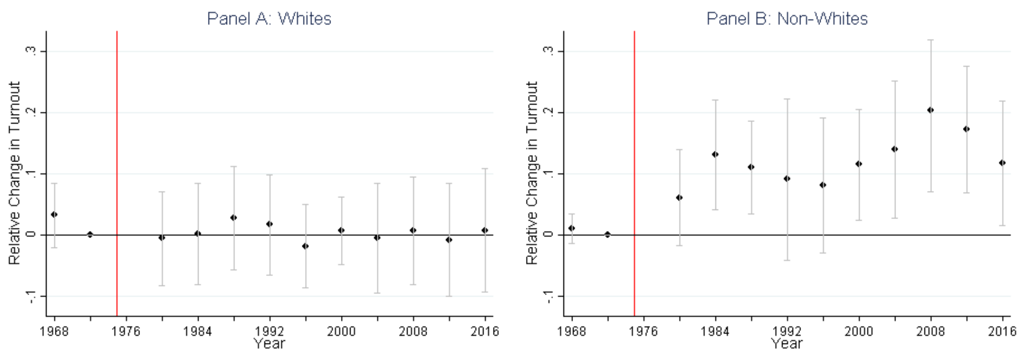
Notes: Whole coverage refers to states in which all counties are subject to preclearance, while partial coverage refers to states in which only some jurisdictions are subject to preclearance. Parts of North Carolina and California were also brought under coverage in 1965 and 1970.

Figure 3: Effect on Voter Turnout



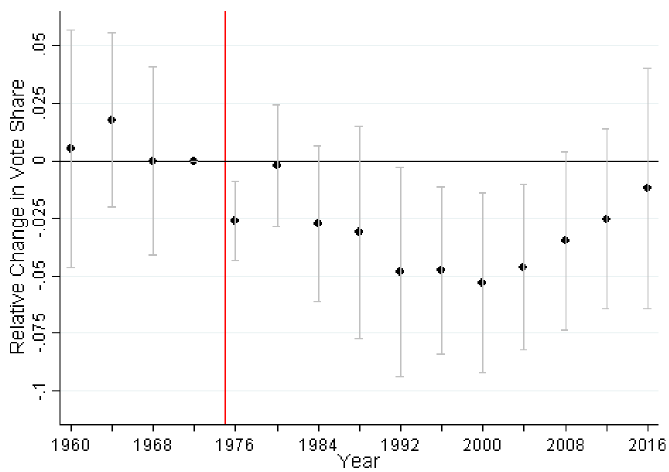
Notes: Data come from ICPSR and Dave Leip's Election Atlas. Graph shows DD coefficients and 95 percent confidence intervals from estimation of Equation 1. Observations are at the county-year level and weighted by voting eligibile population. Standard errors clustered at the state-level. Red vertical line represents passage of 1975 VRA. Full results displayed in Table 2, Column 1.

Figure 4: Effect on Turnout by Race



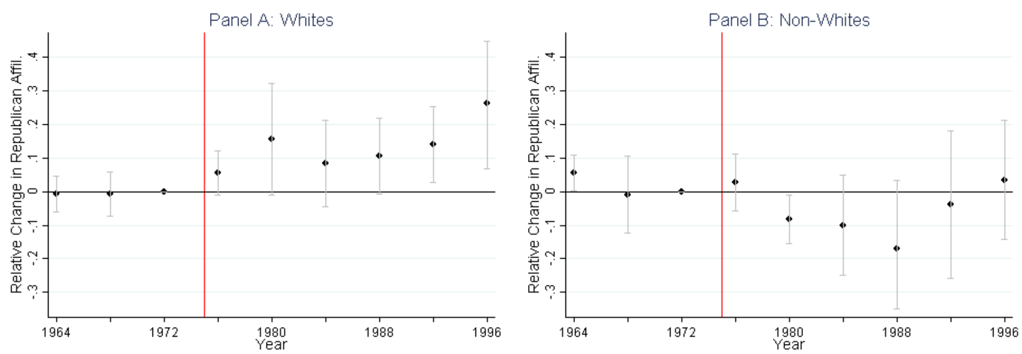
Notes: Data come from CPS Voting and Registration Supplement. Graphs show DD coefficients and 95 percent confidence intervals from estimation of Equation 2 by race. Observations are at the state-year level and are weighted by voting eligible population. Standard errors clustered at the state-level. Red vertical line represents passage of 1975 VRA. Estimates are unavailable for 1976 due to lack of state-level identifiers in 1976 CPS voter supplement. Full results displayed in Table 3, Columns 1 and 3.

Figure 5: Effect on Democratic Vote Share



Notes: Data come from ICPSR and Dave Leip's Election Atlas. Graph shows DD coefficients and 95 percent confidence intervals from estimation of Equation 1 including flexible controls for historical demographic and eligibility measures. Observations are at the county-year level and weighted by votes cast. Standard errors clustered at the state-level. Red vertical line represents passage of 1975 VRA. Full results displayed in Table 2, Column 5.

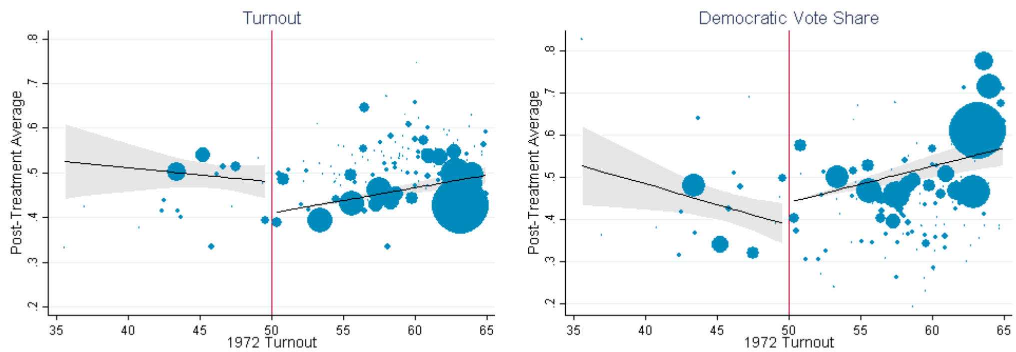
Figure 6: Effect on Republican Affiliation by Race



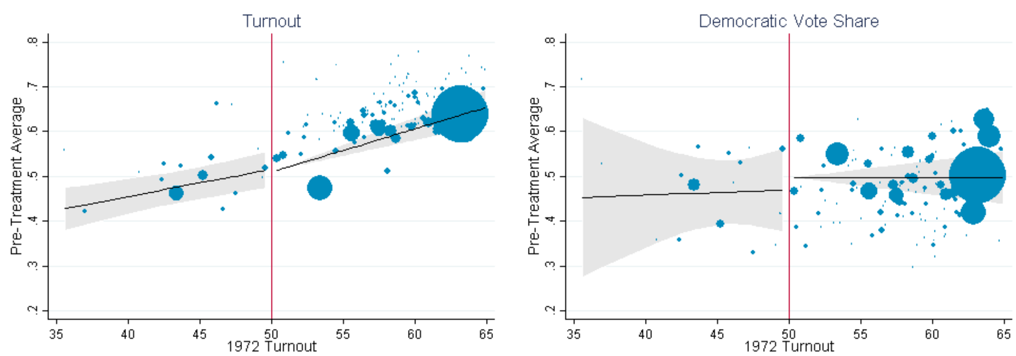
Notes: Data come from ANES. Graphs show DD coefficients and 95 percent confidence intervals from estimation of modified version of Equation 3 replacing $Post_t$ with a set of indicators for the most recent presidential election. Given insufficient treatment variation in some cells, Census division-year fixed effects are additionally replaced with Census region-year fixed effects. The outcome of interest is whether a respondent self-identified as strongly Republican. Observations are at individual-level and are weighted by ANES recommended survey weights. Standard errors clustered at the state-level. Red vertical line represents passage of 1975 VRA. Full results displayed in Table A6, Panel B, Columns 1 and 3.

Figure 7: Regression Discontinuity

Post-Treatment



Pre-Treatment

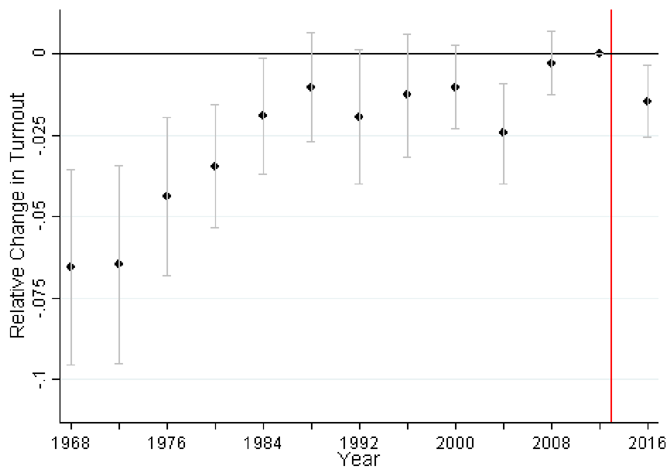


Notes: Data come from ICPSR and Dave Leip's Election Atlas. Graph shows fitted values and confidence intervals from estimation of Equation 5 and scatter plot of average post-treatment outcomes for each sample county, weighted by voting eligible population and major party votes cast. Standard errors are heteroskedasticity-robust. Full results displayed in Table A9, Column 3.

Figure 8: Newspaper Mentions by Party Affiliation

Notes: Data come from newspapers.com. Graph shows DD coefficients and 95 percent confidence intervals from estimation of Equation 1 modified with newspaper and Census region-year fixed effects and omitting year 2012. Left-panel includes newspapers that endorsed Nixon in 1972 according to data from Gentzkow et al. (2011), right-panel includes all other sample papers. Observations are at the newspaper-year level. Standard errors clustered at the state-level. Red vertical line represents passage of 1975 VRA. Full results are displayed in Table A16, Columns 2 and 3.

Figure 9: Post-Shelby: Voter Turnout



Notes: Data come from ICPSR and Dave Leip's Election Atlas. Graph shows DD coefficients and 95 percent confidence intervals from estimation of Equation 1 modified with Census division-year fixed effects and omitting year 2012. Standard errors clustered at the state-level. Observations are at the county-year level and weighted by voting eligible population. Treatment group is comprised of *all* counties freed from preclearance by Shelby ruling, including those brought under coverage by the 1965 and 1970 versions of the VRA. Red vertical line represents 2013 Shelby ruling. Full results displayed in Table 7, Column 1.

Table 1: Harris County Objections

Date	Proposed Change	Reason for Objection
Dec 1975	Purge all voters that fail to re-register by March 1, 1976[a]	Re-registration requirements would disproportionately burden minorities, given historical discrimination and poll tax
Jan 1976	Eliminate state-funded primaries for parties with 2- 20% of 1974 vote share[a]	Would only affect Raza Unida, a Mexican-American nationalist party that received 6% of vote share
Jan 1977	Create new school district in Houston suburbs	Minorities had only gained board majority in Houston after desegregation. Those living outside the city would have little chance at representation in new district
Mar 1978	Consolidate polling station for precincts 55 and 340	Voters in predominantly black precinct 340 would be required to cross a freeway with no pedestrian overpass to vote
Mar 1978	Change school district election date from April to August	Over 3,000 students and faculty at local black university would be out of county during election
May 1978	Change county school trustees election date to January	Would create two separate schooling elections in minority districts (as opposed to one joint election), while reducing the number of polling locations in those areas from 725 to 25
Jun 1979	Annexation of nearby area to Houston	Annexation of predominantly white area would reduce minority population share by 1.7 percentage points
Dec 1979	Implement majority vote requirement; re-draw city council districts	Would combine non-adjacent neighborhoods to create a district with less than 50% minority population share
Mar 1989	Implement anti-single-shot and majority vote requirement for at-large elections	Vote rules would make it difficult for minorities to coordinate to elect preferred candidates
Oct 1991	Redistrict Houston city's nine district-based council seats	Redistricting plan would result in only one majority Hispanic district despite Hispanics comprising 30% of population
Mar 1994	Create county criminal court with judge elected via at-large election	Vote rules would make it difficult for minorities to coordinate to elect preferred candidate
Feb 1995	Bilingual election materials contained numerous misspellings and inaccuracies[a]	Spanish registration card left out required information and would have resulted in invalid Hispanic registrations
Jan 1996	Authorize election officials to invalidate registrations based on citizenship information[a]	Reliance on out-of-date information would have threatened over 70,000 Hispanics and Asians with pending citizenship applications
Mar 1997	Annexation of nearby area to Webster city	Annexation of predominantly white area would have reduced minority population share by 2.8 p.p.
Nov 2001	Redistrict Texas state legislature[a]	Would reduce the number of Hispanic majority districts by 11%
May 2006	Reduce polling locations for community college district from 84 to 12	Location with fewest minorities would serve only 6,500 voters, while location with most minorities would serve 67,000 voters
Aug 2008	Require district supervisors to be land-owning registered voters[a]	Hispanics disproportionately did not own land
Mar 2012	Require state-issued identification in order to vote[a]	Among registered voters, Hispanics were twice as likely as whites to lack proper identification

Notes: Data comes from U.S. Department of Justice. Some changes were submitted and objected to multiple times, in which case the earliest submission is noted. In addition to those listed above, objections were also lodged against state, county and judicial redistricting plans in 1976, 1978, 1990 1991, 1992 and 2001.
[a] denotes state-level submissions.

Table 2: Effect on Voter Turnout and Democratic Vote Share

PC x Year	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: DV=Turnout			*Panel B: DV=Dem. Share*		
1960	0.012	0.008	-0.007	0.022	0.005	0.001
	(0.011)	(0.014)	(0.009)	(0.028)	(0.026)	(0.022)
1964	0.006	0.005	-0.007	0.037	0.018	0.012
	(0.006)	(0.008)	(0.007)	(0.019)	(0.019)	(0.017)
1968	0.013	0.012	0.002	0.014	-0.000	-0.012
	(0.002)	(0.003)	(0.004)	(0.023)	(0.020)	(0.014)
1972	-	-	-	-	-	-
	-	-	-	-	-	-
1976	0.021	0.015	0.011	-0.026	-0.026	-0.029
	(0.007)	(0.005)	(0.008)	(0.018)	(0.009)	(0.009)
1980	0.043	0.032	0.026	-0.018	-0.002	0.001
	(0.013)	(0.006)	(0.009)	(0.020)	(0.013)	(0.014)
1984	0.051	0.044	0.037	-0.052	-0.027	-0.018
	(0.009)	(0.008)	(0.013)	(0.026)	(0.017)	(0.018)
1988	0.066	0.054	0.050	-0.058	-0.031	-0.016
	(0.011)	(0.008)	(0.014)	(0.030)	(0.023)	(0.022)
1992	0.061	0.047	0.041	-0.072	-0.049	-0.033
	(0.015)	(0.010)	(0.016)	(0.029)	(0.023)	(0.019)
1996	0.079	0.058	0.044	-0.073	-0.048	-0.033
	(0.022)	(0.008)	(0.014)	(0.025)	(0.018)	(0.017)
2000	0.083	0.064	0.051	-0.081	-0.053	-0.039
	(0.023)	(0.010)	(0.017)	(0.024)	(0.019)	(0.020)
2004	0.081	0.057	0.041	-0.079	-0.046	-0.028
	(0.022)	(0.011)	(0.019)	(0.019)	(0.018)	(0.016)
2008	0.078	0.053	0.039	-0.064	-0.035	-0.016
	(0.022)	(0.012)	(0.021)	(0.016)	(0.019)	(0.013)
2012	0.081	0.057	0.046	-0.056	-0.025	-0.008
	(0.020)	(0.014)	(0.025)	(0.018)	(0.019)	(0.013)
2016	0.078	0.052	0.038	-0.045	-0.012	0.005
	(0.022)	(0.009)	(0.020)	(0.025)	(0.026)	(0.014)
Demo. Ctrls.	-	Yes	Yes	-	Yes	Yes
Near Cutoffs	-	-	Yes	-	-	Yes
Obs.	37,640	37,606	13,892	37610	37567	13884
R-sq.	0.921	0.930	0.892	0.867	0.918	0.920

Notes: Data come from ICPSR and Dave Leip's Election Atlas. DD coefficients from estimation of Equation 1 displayed. Standard errors clustered at the state-level in parentheses. Observations weighted by voting eligible population (turnout) and major party votes cast (vote share). Controls include interactions between year indicators and average income, average education, and county population shares of minorities, military personnel and 18-21 year olds. Near cutoffs restricts analysis to counties with 1972 turnout between 40-60% and 1970 minority share between 0-10%.

Table 3: Effect on Voter Turnout by Race

	Whites		Non-Whites	
PC x Year	(1)	(2)	(3)	(4)
	DV=Voter Turnout			
1968	0.032	0.029	0.010	0.014
	(0.026)	(0.023)	(0.012)	(0.011)
1972	-	-	-	-
	-	-	-	-
1980	-0.007	0.005	0.060	0.049
	(0.038)	(0.036)	(0.039)	(0.039)
1984	0.001	0.013	0.131***	0.123**
	(0.041)	(0.040)	(0.044)	(0.045)
1988	0.028	0.033	0.110***	0.106**
	(0.042)	(0.043)	(0.037)	(0.042)
1992	0.016	0.026	0.090	0.086
	(0.041)	(0.039)	(0.065)	(0.069)
1996	-0.019	-0.010	0.081	0.076
	(0.034)	(0.034)	(0.055)	(0.057)
2000	0.006	0.011	0.114**	0.111**
	(0.027)	(0.027)	(0.045)	(0.048)
2004	-0.006	0.003	0.140**	0.135**
	(0.045)	(0.043)	(0.055)	(0.058)
2008	0.007	0.015	0.203***	0.199***
	(0.044)	(0.043)	(0.066)	(0.069)
2012	-0.008	0.001	0.172***	0.184***
	(0.046)	(0.049)	(0.051)	(0.045)
2016	0.007	0.020	0.117**	0.129***
	(0.050)	(0.052)	(0.050)	(0.042)
Election Ctrls.	-	Yes	-	Yes
Obs.	528	528	365	365
R-sq.	0.875	0.877	0.829	0.837

Notes: Data come from CPS Voting and Registration Supplement. DD coefficients from estimation of Equation 2 displayed. Standard errors clustered at the state-level in parentheses. Observations weighted by voting eligible population. Election controls include indicators for the presence of gubernatorial and Senate elections as well as for bilingual restrictions within state-year.

Table 4: Party Identification by Race

	Whites			Non-Whites		
	Pre-Treat Mean	(1)	(2)	Pre-Treat Mean	(3)	(4)
Panel A: DV=Republican Affiliation (Indicator)						
Weak	0.366	0.074	0.048	0.082	-0.123	-0.103
		(0.035)	(0.033)		(0.011)	(0.011)
Strong	0.277	0.052	0.026	0.059	-0.054	-0.045
		(0.014)	(0.012)		(0.011)	(0.013)
Panel B: DV=Minority Aid Opposition (Scale: 1-6)						
Respondent	4.480	0.301	0.242	2.565	-0.126	-0.234
		(0.122)	(0.142)		(0.294)	(0.265)
Republicans	4.061	0.318	0.263	4.863	0.902	0.694
		(0.276)	(0.293)		(0.090)	(0.088)
Democrats	3.160	-0.180	-0.174	2.850	0.376	0.364
		(0.158)	(0.180)		(0.070)	(0.140)
Demo. Ctrls.		-	Yes		-	Yes
Obs.(Rep.)		22,612			3,831	
Obs.(Aid)		17,240			3,031	

Notes: Data come from ANES. DD coefficient from estimation of Equation 3 displayed. Standard errors clustered at the state-level in parentheses. Observations weighted using ANES recommended survey weights. Demographic controls include respondent's age, income, education and military status. Minority aid responses refer to the respondent's self-reported position regarding government aid to minorities as well as her reported beliefs about the positions of the Republican and Democratic parties.

Table 5: Republican Identification by Minority Aid Opposition

	Whites (1)	Whites (2)	Non-Whites (3)	Non-Whites (4)
	DV=Republican Affiliation			
PC x Post ($\beta 4$)	0.013	-0.059	-0.079	-0.087
	(0.053)	(0.044)	(0.018)	(0.031)
PC x Post x NoAid ($\beta 5$)	0.109	0.153	-0.134	-0.189
	(0.038)	(0.034)	(0.066)	(0.069)
$\beta 4 + \beta 5$	0.122	0.094	-0.213	-0.276
Conservatism Ctrl.	-	Yes	-	Yes
Obs.	17,240	16,046	3,031	2,884
R-sq.	0.118	0.211	0.201	0.234

Notes: Data come from ANES. DD and DDD coefficients of interest from estimation of Equation 4 controlling for demographic variables displayed. Conservatism controls include interactions between year and an indicator for whether the respondent self-identified as weakly conservative. Standard errors clustered at the state-level in parentheses. Observations weighted using ANES recommended survey weights.

Table 6: Effect on Campaign Exposure

	Whites		Non-Whites	
	Pre-Treat Mean	(1)	Pre-Treat Mean	(2)
Media Exposure	2.429	0.053	2.298	-0.212
		(0.133)		(0.063)
Contact (Republican)	0.158	0.132	0.085	-0.071
		(0.034)		(0.016)
Contact (Democrat)	0.153	0.061	0.138	0.009
		(0.030)		(0.031)
Obs. (Media)		12,288		1,892
Obs. (Contact)		21,421		3,710

Notes: Data come from ANES. DD coefficient from estimation of Equation 3 displayed. Standard errors clustered at the state-level in parentheses. Observations weighted using ANES recommended survey weights. Includes demographic controls. *Media exposure* refers to the number of media forms (zero to four) the respondent saw, read or heard about the political campaign. *Contact* is an indicator for whether the respondent was contacted by Republican (Democratic) party "to get them to vote for their candidate".

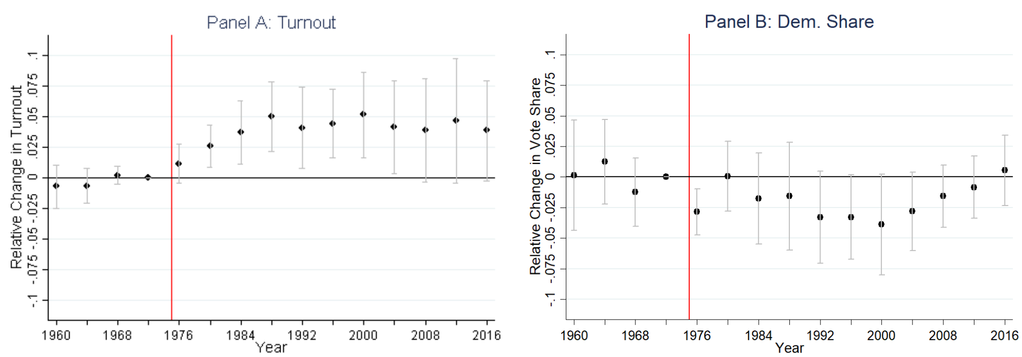
Table 7: Post-Shelby Analysis

PC x Year	(1)		(2)		(3)	
			DV=Voter Turnout			
1968	-0.066	(0.015)	-0.013	(0.029)	-0.098	(0.063)
1972	-0.065	(0.015)	-0.036	(0.028)	-0.097	(0.068)
1976	-0.044	(0.012)	-	-	-	-
1980	-0.035	(0.009)	-0.017	(0.016)	-0.074	(0.055)
1984	-0.019	(0.009)	-0.021	(0.020)	-0.072	(0.042)
1988	-0.010	(0.008)	-0.001	(0.017)	-0.063	(0.039)
1992	-0.019	(0.010)	0.005	(0.020)	-0.094	(0.034)
1996	-0.013	(0.009)	-0.019	(0.015)	-0.082	(0.043)
2000	-0.010	(0.006)	-0.012	(0.022)	-0.025	(0.041)
2004	-0.024	(0.008)	-0.025	(0.020)	-0.028	(0.028)
2008	-0.003	(0.005)	-0.006	(0.012)	0.010	(0.028)
2012	-	-	-	-	-	-
2016	-0.015	(0.005)	-0.003	(0.010)	-0.021	(0.029)
Sample	County		Whites (State)		Non-Whites (State)	
Obs.	40,402		600		437	
R-sq.	0.836		0.858		0.853	

Notes: Data come from ICPSR and Dave Leip's Election Atlas. DD coefficients from estimation of Equation 1 modified with Census division-year fixed effects and omitting year 2012 displayed. Standard errors clustered at the state-level in parentheses. Observations weighted by voting eligible population. Treatment group is comprised of *all* counties (states) freed from preclearance coverage by Shelby ruling, including those brought under coverage by earlier versions of the VRA.

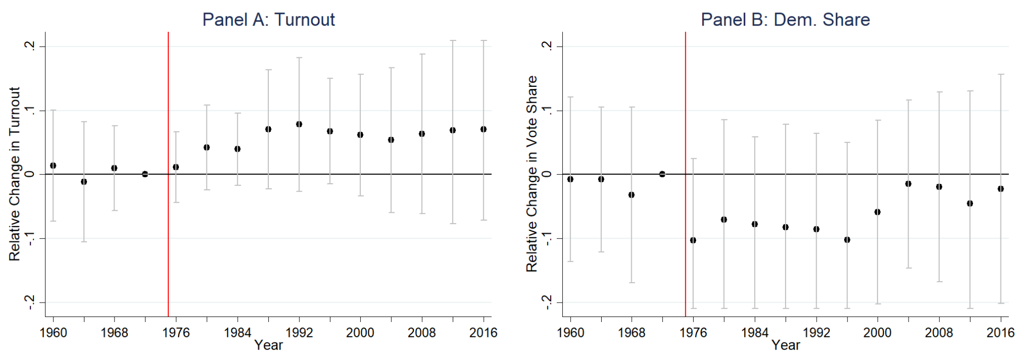
Appendix A. Supplementary figures and tables noted in text

Figure A1: Counties near the Coverage Triggers



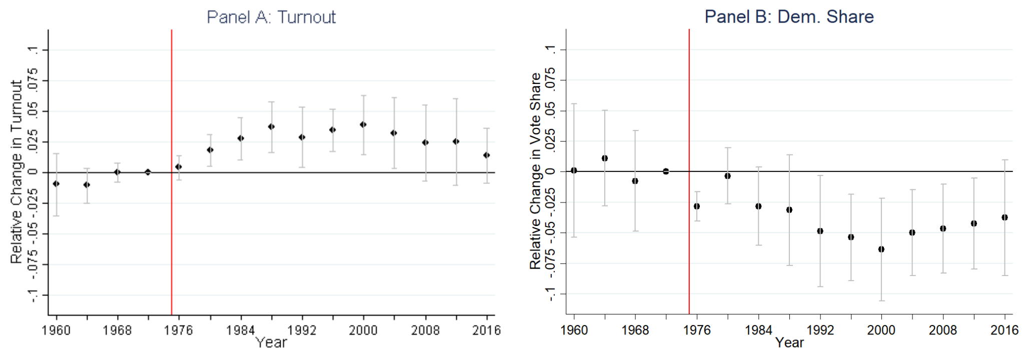
Notes: Data come from ICPSR and Dave Leip's Election Atlas. Graph shows DD coefficients and 95 percent confidence intervals from estimation of Equation 1 including flexible controls for historical demographic measures. Sample is restricted only to those counties with 40-60% turnout and 0-10% language minority share in 1972. Observations are at the county-year level and weighted by votes cast. Standard errors clustered at the state-level. Red vertical line represents passage of 1975 VRA. Full results displayed in Table 2, Columns 3 and 6.

Figure A2: Regression Discontinuity by Year



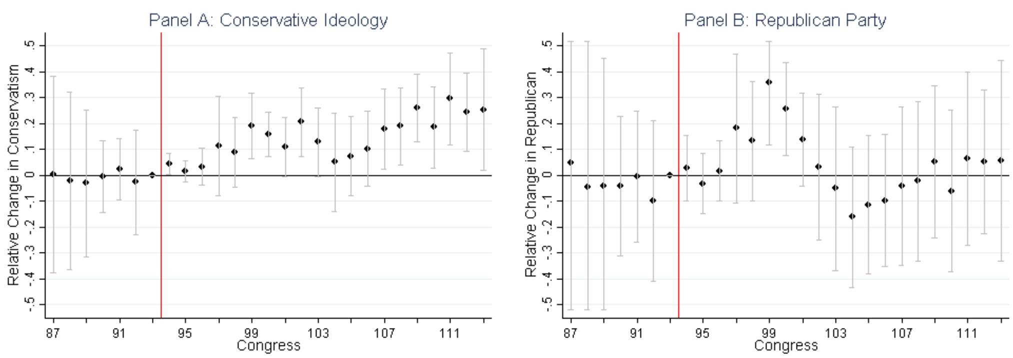
Notes: Data come from ICPSR and Dave Leip's Election Atlas. Graphs show RD coefficients and 95 percent confidence intervals from estimation of Equation 5, separately for each election from 1960 onwards. The sample includes counties with greater than 5% language minority share in 1970 and 1972 voter turnout between 30 and 70%, excluding those in Texas and Arizona. Observations weighted by voting eligible population (for turnout) and major party votes cast (for Dem. share). Heteroskedasticity-robust standard errors are included. Red vertical line represents passage of 1975 VRA. Full results displayed in Table A10.

Figure A3: Controlling for Historical Determination Measures



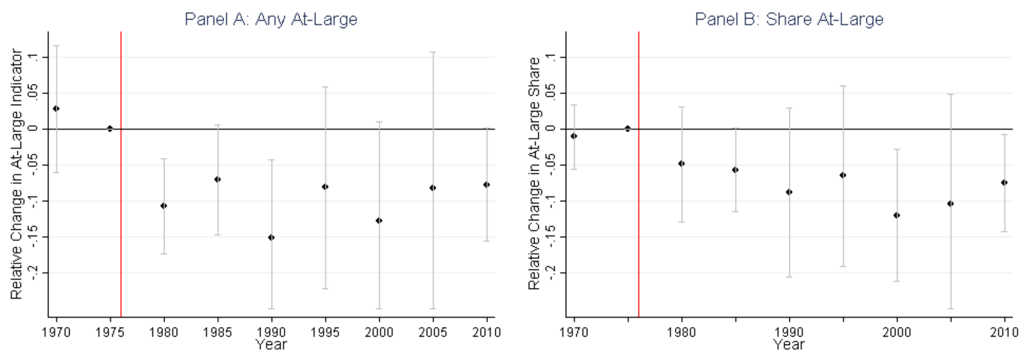
Notes: Data come from ICPSR and Dave Leip's Election Atlas. Graph shows DD coefficients and 95 percent confidence intervals from estimation of Equation 1 including interactions between year indicators and 1972 turnout and 1970 language minority share. Observations are at the county-year level and weighted by votes cast. Standard errors clustered at the state-level. Red vertical line represents passage of 1975 VRA. Full results displayed in Table A12, Column 1.

Figure A4: Effect on Congressional Ideology



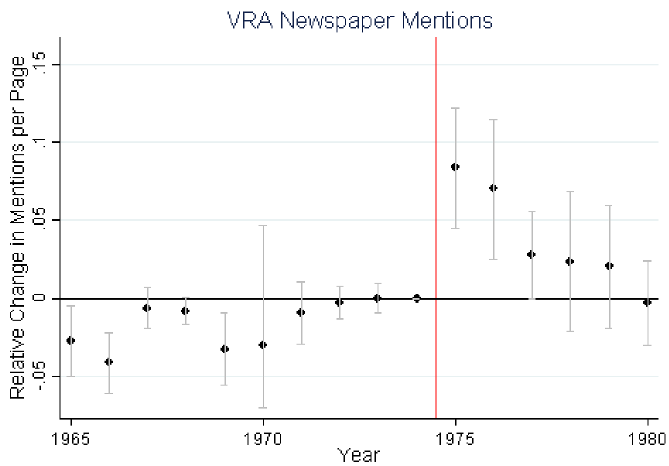
Notes: Data come from DW-NOMINATE. Graphs show DD coefficients and 95 percent confidence intervals from estimation of district-level analogue of Equation 1 on a Republican indicator and DW-NOMINATE first-dimension scores of conservatism (scaled from -1 to 1) including county and Census division-year fixed effects. The omitted group is the 93rd Congress, which ended in January 1975. Observations are at the district-Congress level. Standard errors clustered at the state-level. Red vertical line represents passage of 1975 VRA. Full results are displayed in Table A13.

Figure A5: Effect on At-Large Elections



Notes: Data come from ICMA Municipal Yearbooks. Graphs show DD coefficients and 95 percent confidence intervals from estimation of municipality-level analogue of Equation 1 substituting municipality fixed effects in place of county fixed effects. The treatment group is comprised of municipalities in covered counties, which were also subject to pre-clearance. Any At-Large is an indicator set to one if a municipality maintains any council seats elected through at-large, as opposed to district-based, elections in a given year. Share At-Large is the fraction of council seats elected by at-large elections. Observations are at the municipality-year level. Standard errors clustered at the state-level. Red vertical line represents passage of 1975 VRA. Full results are displayed in Table A14

Figure A6: Effect on Newspaper Mentions of the VRA



Notes: Data come from newspapers.com. Graph shows DD coefficients and 95 percent confidence intervals from estimation of Equation 1 modified with newspaper and Census region-year fixed effects and omitting year 2012. Observations are at the newspaper-year level. Standard errors clustered at the state-level. Red vertical line represents passage of 1975 VRA. Full results are displayed in Table A16, Column 1

Table A1: Summary Statistics: County Characteristics (1972)

	Control		Treatment	
	Mean	SD	Mean	SD
Total Pop.	73,316	254,507	53,908	171,322
Voting Eligible Pop.	48,611	174,234	34,165	110,878
Income (Avg.)	3,990	898	3,522	716
Black (percent)	3.574	7.848	8.542	10.024
Language Minority (percent)	2.239	7.105	15.303	18.387
College Educated (percent)	7.460	3.962	7.474	3.263
Age 18 to 21 (percent)	5.166	0.984	5.172	1.098
Active Military (percent)	0.388	2.043	0.754	2.924
White-Minority Income Ratio	2.967	7.257	2.121	1.941
Minority-White Poverty Diff.	12.143	20.087	27.430	18.111
Minority-White Uneducated Diff.	2.938	7.468	8.115	7.358
Voter Turnout	0.651	0.133	0.485	0.081
Democratic Vote Share	0.347	0.104	0.313	0.099
Counties	2,232		283	

Notes: Data come U.S. Census Bureau. from White-minority income ratio is the ratio of average white to average non-white income. Minority-white poverty differential is the difference between non-white and white poverty rates. Minority-white uneducated differential is the difference between percent of non-whites with no education and percent of whites with no education.

Table A2: Effect on Voter Turnout and Democratic Vote Share: Period Model

PC x Year	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: DV=Turnout			*Panel B: DV=Dem. Share*		
1960-1968	0.010	0.008	-0.004	0.025	0.008	0.001
	(0.005)	(0.007)	(0.004)	(0.023)	(0.021)	(0.018)
1972	-	-	-	-	-	-
	-	-	-	-	-	-
1976-1988	0.047	0.038	0.033	-0.040	-0.022	-0.015
	(0.009)	(0.005)	(0.009)	(0.023)	(0.015)	(0.016)
1992-2004	0.077	0.057	0.044	-0.077	-0.049	-0.033
	(0.021)	(0.010)	(0.016)	(0.023)	(0.019)	(0.017)
2008-2016	0.079	0.054	0.041	-0.055	-0.024	-0.006
	(0.021)	(0.011)	(0.022)	(0.019)	(0.021)	(0.013)
Demo. Ctrls.	-	Yes	Yes	-	Yes	Yes
Near Cutoffs	-	-	Yes	-	-	Yes
Obs.	37,640	37,606	13,892	37,610	37,567	13,884
R-sq.	0.921	0.930	0.892	0.867	0.918	0.920

Notes: Data come from ICPSR and Dave Leip's Election Atlas. Table shows coefficients from estimation of a period-level analogue of Equation 1, which replaces the full set of treatment-year interactions with interactions between treatment and a set of period indicators ($I_{\tau_1-\tau_2,t}$), where $I_{\tau_1-\tau_2,t}$ is set to 1 for years between τ_1 and τ_2 and 0 otherwise. Standard errors clustered at the state-level in parentheses. Observations weighted by voting eligible population (turnout) and major party votes cast (vote share). Controls include interactions between year indicators and average income, average education, and county population shares of minorities, military personnel and 18-21 year olds. Near cutoffs restricts analysis to counties with 1972 turnout between 40-60% and 1970 minority share between 0-10%.

Table A3: Voter Turnout: All Federal Elections

PC x Year	(1)		(2)		(3)	
			DV=Voter Turnout			
1960	-0.001	(0.012)	0.011	(0.019)	-0.009	(0.014)
1962	0.002	(0.008)	0.009	(0.011)	-0.000	(0.014)
1964	-0.007	(0.006)	0.007	(0.010)	-0.009	(0.009)
1966	0.004	(0.003)	0.010	(0.008)	-0.000	(0.007)
1968	-0.001	(0.008)	0.014	(0.007)	0.000	(0.006)
1970	0.004	(0.012)	0.011	(0.011)	0.004	(0.015)
1972	0.002	(0.013)	0.017	(0.013)	-0.004	(0.008)
1974	-	-	-	-	-	-
1976	0.007	(0.010)	0.019	(0.009)	0.013	(0.008)
1978	0.034	(0.013)	0.033	(0.011)	0.030	(0.009)
1980	0.029	(0.014)	0.033	(0.011)	0.024	(0.006)
1982	0.044	(0.014)	0.044	(0.014)	0.045	(0.013)
1984	0.039	(0.013)	0.048	(0.011)	0.036	(0.014)
1986	0.056	(0.012)	0.056	(0.011)	0.050	(0.010)
1988	0.049	(0.014)	0.052	(0.010)	0.045	(0.012)
1990	0.070	(0.015)	0.065	(0.011)	0.059	(0.010)
1992	0.058	(0.021)	0.058	(0.018)	0.050	(0.020)
1994	0.064	(0.019)	0.057	(0.013)	0.046	(0.013)
1996	0.065	(0.022)	0.059	(0.014)	0.042	(0.013)
1998	0.062	(0.017)	0.053	(0.012)	0.042	(0.015)
2000	0.068	(0.024)	0.063	(0.015)	0.046	(0.016)
2002	0.075	(0.016)	0.070	(0.009)	0.055	(0.015)
2004	0.067	(0.024)	0.057	(0.016)	0.037	(0.019)
2006	0.073	(0.019)	0.064	(0.012)	0.049	(0.018)
2008	0.063	(0.025)	0.053	(0.015)	0.035	(0.021)
2010	0.066	(0.018)	0.057	(0.013)	0.043	(0.024)
2012	0.066	(0.023)	0.057	(0.017)	0.042	(0.025)
2014	0.073	(0.018)	0.060	(0.010)	0.047	(0.020)
2016	0.064	(0.023)	0.052	(0.014)	0.033	(0.020)
Demo. Ctrls.	-		Yes		Yes	
Near Cutoffs	-		-		Yes	
Obs.	72,767		72,703		26,858	
R-sq.	0.939		0.946		0.933	

Notes: Data come from ICPSR and Dave Leip's Election Atlas. DD coefficients from estimation of Equation 1 including all federal and gubernatorial elections displayed. Standard errors clustered at the state-level in parentheses. Observations weighted by voting eligible population. Voter turnout measured from highest participation among presidential, gubernatorial, Senate and House elections in given county-year. Controls include interactions between year indicators and average income, average education, and county population shares of minorities, military personnel and 18-21 year olds. Near cutoffs restricts analysis to counties with 1972 turnout between 40-60% and 1970 minority share between 0-10%.

Table A4: Voter Turnout by Race: Period Model

	Whites		Non-Whites	
PC x Year	(1)	(2)	(3)	(4)
	DV=Voter Turnout			
1968	0.032	0.029	0.010	0.014
	(0.026)	(0.023)	(0.012)	(0.012)
1972	-	-	-	-
	-	-	-	-
1980-1988	0.008	0.018	0.108	0.102
	(0.039)	(0.038)	(0.039)	(0.040)
1992-2004	-0.001	0.007	0.109	0.105
	(0.036)	(0.035)	(0.053)	(0.057)
2008-2016	0.002	0.013	0.161	0.169
	(0.045)	(0.047)	(0.052)	(0.046)
Election Ctrls.	-	Yes	-	Yes
Obs.	528	528	365	365
R-sq.	0.874	0.876	0.826	0.835

Notes: Data come from CPS Voting and Registration Supplement. Table shows coefficients from estimation of a period-level analogue of Equation 2, which replaces the full set of treatment-year interactions with interactions between treatment and a set of period indicators ($I_{\tau_1-\tau_2,t}$), where $I_{\tau_1-\tau_2,t}$ is set to 1 for years between τ_1 and τ_2 and 0 otherwise. Standard errors clustered at the state-level in parentheses. Observations weighted by voting eligible population. Election controls include indicators for the presence of gubernatorial and Senate elections as well as for bilingual restrictions within state-year.

Table A5: Voter Turnout by Race: Treatment Intensity

	Whites		Non-Whites	
Intensity x Year	(1)	(2)	(3)	(4)
	DV=Voter Turnout			
1968	0.032	0.029	-0.018	-0.014
	(0.027)	(0.024)	(0.041)	(0.038)
1972	-	-	-	-
	-	-	-	-
1980	-0.007	0.005	0.031	0.025
	(0.039)	(0.037)	(0.068)	(0.067)
1984	-0.004	0.008	0.077	0.074
	(0.043)	(0.041)	(0.083)	(0.081)
1988	0.031	0.037	0.053	0.055
	(0.043)	(0.043)	(0.069)	(0.065)
1992	0.013	0.023	0.030	0.031
	(0.042)	(0.040)	(0.076)	(0.080)
1996	-0.019	-0.010	0.023	0.022
	(0.035)	(0.035)	(0.074)	(0.074)
2000	0.005	0.010	0.051	0.051
	(0.029)	(0.028)	(0.075)	(0.071)
2004	-0.006	0.003	0.075	0.073
	(0.046)	(0.045)	(0.080)	(0.083)
2008	0.008	0.016	0.143	0.141
	(0.045)	(0.045)	(0.081)	(0.084)
2012	-0.005	0.005	0.100	0.113
	(0.047)	(0.050)	(0.088)	(0.085)
2016	0.007	0.021	0.051	0.067
	(0.052)	(0.053)	(0.086)	(0.083)
Election Ctrls.	-	Yes	-	Yes
Obs.	528	528	365	365
R-sq.	0.875	0.877	0.827	0.835

Notes: Data come from ICPSR and Dave Leip's Election Atlas. DD coefficients from estimation of Equation 2 replacing treatment dummy PC_s with treatment intensity variable $PCintensity_s$ displayed. Standard errors clustered at the state-level in parentheses. Observations weighted by voting eligible population. Election controls include indicators for the presence of gubernatorial and Senate elections as well as for bilingual restrictions within state-year.

Table A6: Party Affiliation by Race over Time

	Whites		Non-Whites	
PC x Year	(1)	(2)	(3)	(4)
Panel A: DV=Republican Affiliation (Weak)				
1964	-0.026	-0.020	0.042	0.088
	(0.031)	(0.034)	(0.035)	(0.046)
1968	0.037	0.044	0.005	0.017
	(0.053)	(0.049)	(0.058)	(0.060)
1972	-	-	-	-
	-	-	-	-
1976	0.106	0.093	-0.008	0.029
	(0.056)	(0.053)	(0.051)	(0.052)
1980	0.148	0.162	-0.097	-0.063
	(0.125)	(0.117)	(0.056)	(0.061)
1984	0.088	0.104	-0.041	-0.019
	(0.114)	(0.113)	(0.062)	(0.070)
1988	0.188	0.190	-0.106	-0.078
	(0.098)	(0.094)	(0.098)	(0.100)
1992	0.198	0.189	0.029	0.051
	(0.103)	(0.104)	(0.115)	(0.116)
1996	0.233	0.224	0.111	0.137
	(0.119)	(0.119)	(0.087)	(0.086)
Panel B: DV=Republican Affiliation (Strong)				
1964	-0.007	-0.005	0.056	0.096
	(0.026)	(0.021)	(0.026)	(0.038)
1968	-0.007	-0.004	-0.009	0.005
	(0.032)	(0.031)	(0.056)	(0.059)
1972	-	-	-	-
	-	-	-	-
1976	0.055	0.038	0.026	0.051
	(0.032)	(0.031)	(0.042)	(0.046)
1980	0.155	0.158	-0.081	-0.053
	(0.082)	(0.077)	(0.036)	(0.040)
1984	0.085	0.106	-0.101	-0.087
	(0.064)	(0.062)	(0.073)	(0.083)
1988	0.106	0.118	-0.169	-0.152
	(0.056)	(0.053)	(0.100)	(0.105)
1992	0.142	0.142	-0.040	-0.029
	(0.056)	(0.058)	(0.108)	(0.111)
1996	0.265	0.259	0.035	0.051
	(0.097)	(0.101)	(0.088)	(0.090)
Demo. Ctrls.	-	Yes	-	Yes
Obs.	22,162		3,841	

Notes: Data come from ANES. DD coefficients from estimation of Equation 3 including Census region-year fixed effects and replacing $Post_t$ with a set of indicators for the most recent presidential election. Observations are weighted by ANES recommended survey weights. Standard errors clustered at the state-level.

Table A7: Party Affiliation by Race (Gallup)

Whites			Blacks		
Pre-Treat Mean	(1)	(2)	Pre-Treat Mean	(3)	(4)
Panel A: DV=Republican Affiliation (Indicator)					
0.293	0.149***	0.159***	0.131	-0.098***	-0.110**
	(0.031)	(0.029)		(0.014)	(0.040)
Panel B: DV=No Black President (Indicator)					
0.479	0.057	0.048	0.047	0.003	-0.072***
	(0.070)	(0.071)		-0.004	(0.025)
Demo. Ctrls.	-	Yes		-	Yes
Obs.(Rep.)	20,526			1,881	
Obs.(Pres.)	19,050			1,828	

Notes: Data come from Gallup, 1961 to 2003. DD coefficient from estimation of Equation 3 with state and divisn-year fixed effects displayed. Standard errors clustered at the state-level in parentheses. Observations weighted using Gallup recommended survey weights. Controls include respondent's age, income, education. *Republican Affiliation* is an indicator for whether the respondent self-identified as Republican, while *No Black President* is an indicator for whether the respondent reported being unwilling to vote for a hypothetical black president.

Table A8: Republican Identification by Racial Conservatism (Gallup)

	Whites		Blacks	
	(1)	(2)	(3)	(4)
	DV=Republican Affiliation			
PC x Post	0.100**	0.112**	-0.118***	-0.133***
	(0.045)	(0.045)	(0.007)	(0.038)
PC x Post x NoBlack	0.063*	0.074**	-	-
	(0.032)	(0.033)	-	-
Demographic Ctrls.	-	Yes	-	Yes
Obs.	19,050		1,828	
R-sq.	0.041	0.064	0.190	0.224

Notes: Data come from Gallup, 1961 to 2003. DD and DDD coefficients of interest from estimation of Equation 4 with state and division-year fixed effects displayed. Standard errors clustered at the state-level in parentheses. *NoBlack* is an indicator for whether the respondent was not willing to vote for a black president. Observations weighted using Gallup recommended survey weights.

Table A9: Regression Discontinuity

	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: DV=Voter Turnout					
Pre-Treat	0.006	0.006	0.006	0.010	0.006	0.009
	(0.025)	(0.020)	(0.021)	(0.017)	(0.021)	(0.017)
Post-Treat	0.031	0.041	0.069	0.067	0.069	0.068
	(0.023)	(0.018)	(0.021)	(0.017)	(0.021)	(0.017)
	Panel B: DV=Dem. Vote Share					
Pre-Treat	-0.002	-0.000	-0.028	-0.020	-0.027	-0.020
	(0.053)	(0.035)	(0.051)	(0.033)	(0.050)	(0.033)
Post-Treat	0.024	-0.003	-0.054	-0.061	-0.056	-0.064
	(0.030)	(0.026)	(0.027)	(0.024)	(0.026)	(0.024)
Controls	-	Yes	-	Yes	-	Yes
Bandwidth	35-65		30-70		25-75	
Obs. (Pre)	566	562	714	710	762	758
Obs. (Post)	1,555	1,544	1,962	1,951	2,105	2,094
R-sq. (Pre)	0.455	0.708	0.507	0.729	0.508	0.723
R-sq. (Post)	0.024	0.322	0.115	0.415	0.118	0.414

Notes: Data come from ICPSR and Dave Leip's Election Atlas. RD coefficients and standard errors from estimation of Equation 5, pooled across all post-treatment and pre-treatment years, respectively. The sample excludes counties in Texas and Arizona, as those areas were covered due to state-level determinations, as well as counties with less than 5% 1970 minority share or 1972 turnout outside of the bandwidths. Observations weighted by voting eligible population (for turnout) and major party votes cast (for Dem. share). Heteroskedasticity-robust standard errors are included.

Table A10: Regression Discontinuity by Year

Less50	(1)		(2)	
	DV=Turnout		*DV=Dem. Share*	
1960	0.014	(0.044)	-0.007	(0.065)
1964	-0.012	(0.048)	-0.008	(0.057)
1968	0.010	(0.034)	-0.032	(0.070)
1972	-	-	-	-
1976	0.012	(0.028)	-0.103	(0.065)
1980	0.042	(0.034)	-0.070	(0.079)
1984	0.040	(0.029)	-0.077	(0.069)
1988	0.071	(0.047)	-0.083	(0.082)
1992	0.078	(0.053)	-0.086	(0.076)
1996	0.068	(0.042)	-0.102	(0.077)
2000	0.062	(0.048)	-0.059	(0.073)
2004	0.054	(0.057)	-0.015	(0.067)
2008	0.064	(0.063)	-0.019	(0.075)
2012	0.069	(0.074)	-0.045	(0.089)
2016	0.070	(0.072)	-0.023	(0.091)
Bandwidth	30-70		30-70	
Counties	178		178	

Notes: Data come from ICPSR and Dave Leip's Election Atlas. RD coefficients and standard errors from estimation of Equation 5, separately for each election from 1960 onwards. The sample excludes counties in Texas and Arizona, as those areas were covered due to state-level determinations, as well as counties with less than 5% 1970 minority share or 1972 turnout outside of the bandwidths. Observations weighted by voting eligible population (for turnout) and major party votes cast (for Dem. share). Heteroskedasticity-robust standard errors are included.

Table A11: Restricted Samples

PC x Year	(1)		(2)		(3)	
	Panel A: DV=Voter Turnout					
1960	-0.009	(0.006)	0.026	(0.011)	0.007	(0.013)
1964	-0.003	(0.006)	0.011	(0.009)	0.005	(0.011)
1968	0.007	(0.004)	0.009	(0.006)	0.016	(0.004)
1972	-	-	-	-	-	-
1976	-0.011	(0.004)	0.017	(0.010)	0.005	(0.005)
1980	0.008	(0.008)	0.019	(0.009)	0.016	(0.012)
1984	0.024	(0.015)	0.030	(0.007)	0.036	(0.014)
1988	0.034	(0.016)	0.038	(0.009)	0.043	(0.019)
1992	0.037	(0.020)	0.030	(0.013)	0.037	(0.023)
1996	0.037	(0.016)	0.042	(0.011)	0.042	(0.018)
2000	0.041	(0.019)	0.052	(0.014)	0.048	(0.024)
2004	0.041	(0.019)	0.054	(0.017)	0.038	(0.025)
2008	0.035	(0.020)	0.045	(0.020)	0.037	(0.026)
2012	0.043	(0.026)	0.054	(0.022)	0.047	(0.026)
2016	0.039	(0.021)	0.051	(0.018)	0.039	(0.022)
	Panel B: DV=Dem. Vote Share					
1960	0.008	(0.015)	-0.003	(0.018)	0.030	(0.015)
1964	0.024	(0.011)	0.032	(0.012)	0.033	(0.008)
1968	0.002	(0.010)	0.006	(0.011)	0.014	(0.009)
1972	-	-	-	-	-	-
1976	-0.029	(0.010)	-0.011	(0.019)	-0.034	(0.012)
1980	-0.002	(0.015)	-0.002	(0.019)	-0.007	(0.015)
1984	-0.033	(0.013)	-0.024	(0.024)	-0.045	(0.018)
1988	-0.039	(0.018)	-0.022	(0.029)	-0.045	(0.020)
1992	-0.053	(0.017)	-0.039	(0.031)	-0.058	(0.014)
1996	-0.054	(0.008)	-0.035	(0.032)	-0.065	(0.015)
2000	-0.070	(0.005)	-0.047	(0.030)	-0.075	(0.014)
2004	-0.052	(0.007)	-0.035	(0.024)	-0.059	(0.014)
2008	-0.030	(0.009)	-0.022	(0.017)	-0.046	(0.016)
2012	-0.024	(0.011)	-0.016	(0.016)	-0.038	(0.016)
2016	-0.013	(0.014)	-0.007	(0.018)	-0.019	(0.015)
Sample	42-58% Turnout 1-9% Minority		Neighboring Counties		Prob(PC) > 75th Percentile	
Obs.	2,361		4,315		9,182	
R-sq.	0.931		0.942		0.881	

Notes: Data come from ICPSR and Dave Leip's Election Atlas. DD coefficients from estimation of Equation 1 on restricted samples displayed. Column 1 restricts sample to counties within 8 p.p. of the turnout cutoff and 4 p.p. of the minority share cutoff. Column 2 restricts sample to treatment counties bordering at least one control county and control counties bordering at least one treatment county. Column 3 restricts sample to counties in the 75th percentile or higher of predicted preclearance coverage, based on logit of treatment on 1968 turnout and 1960 minority share. Standard errors clustered at the state-level in parentheses. Observations weighted by voting eligible population. Includes demographic controls.

Table A12: Selection Controls

PC x Year	(1)		(2)		(3)	
			Panel A: DV=Voter Turnout			
1960	-0.010	(0.013)	0.008	(0.014)	0.007	(0.016)
1964	-0.011	(0.007)	0.005	(0.009)	0.007	(0.007)
1968	-0.000	(0.004)	0.011	(0.003)	0.021	(0.004)
1972	-	-	-	-	-	-
1976	0.004	(0.005)	0.014	(0.004)	0.012	(0.005)
1980	0.018	(0.006)	0.031	(0.005)	0.029	(0.006)
1984	0.028	(0.009)	0.043	(0.008)	0.041	(0.007)
1988	0.037	(0.010)	0.053	(0.008)	0.051	(0.009)
1992	0.028	(0.012)	0.046	(0.010)	0.047	(0.013)
1996	0.034	(0.008)	0.057	(0.008)	0.053	(0.010)
2000	0.038	(0.012)	0.064	(0.010)	0.054	(0.014)
2004	0.032	(0.014)	0.058	(0.011)	0.045	(0.015)
2008	0.024	(0.015)	0.054	(0.012)	0.037	(0.015)
2012	0.025	(0.018)	0.058	(0.015)	0.041	(0.017)
2016	0.014	(0.011)	0.053	(0.010)	0.029	(0.013)
			Panel B: DV=Dem. Vote Share			
1960	0.001	(0.027)	0.004	(0.028)	0.012	(0.030)
1964	0.011	(0.019)	0.017	(0.021)	0.017	(0.019)
1968	-0.008	(0.020)	0.001	(0.019)	0.002	(0.022)
1972	-	-	-	-	-	-
1976	-0.028	(0.006)	-0.028	(0.008)	-0.016	(0.005)
1980	-0.003	(0.011)	-0.004	(0.013)	0.008	(0.010)
1984	-0.028	(0.016)	-0.029	(0.017)	-0.020	(0.015)
1988	-0.031	(0.022)	-0.032	(0.023)	-0.024	(0.022)
1992	-0.049	(0.023)	-0.050	(0.022)	-0.041	(0.022)
1996	-0.054	(0.017)	-0.049	(0.017)	-0.048	(0.019)
2000	-0.064	(0.021)	-0.054	(0.019)	-0.063	(0.024)
2004	-0.050	(0.017)	-0.047	(0.017)	-0.054	(0.022)
2008	-0.047	(0.018)	-0.034	(0.020)	-0.057	(0.026)
2012	-0.042	(0.018)	-0.025	(0.020)	-0.053	(0.028)
2016	-0.038	(0.023)	-0.010	(0.028)	-0.052	(0.034)
Add'l Ctrls.	Turnout1972 x Year Minority1970 x Year		Dem.Share1972 x Year		Pr(PC)xYear	
Obs.	37,565		37,545		37,573	
R-sq.	0.936		0.931		0.932	

Notes: Data come from ICPSR and Dave Leip's Election Atlas. DD coefficients from estimation of Equation 1 with additional controls for selection. $Turnout1972$ and $Minority1970$ are historical voter turnout and language minority share measures used to determine preclearance coverage under 1975 VRA. $Pr(PC)$ is county's probability of preclearance coverage based based on logit of treatment on 1968 turnout and 1960 minority share. Standard errors clustered at the state-level in parentheses. Observations weighted by voting eligible population. Includes demographic controls.

Table A13: Effect on Congressional Ideology

PC x Cong.	(1)		(2)	
	DV=Conservatism		*DV=Republican*	
87	0.004	(0.188)	0.048	(0.313)
88	-0.020	(0.171)	-0.046	(0.292)
89	-0.031	(0.141)	-0.041	(0.245)
90	-0.003	(0.069)	-0.042	(0.133)
91	0.024	(0.059)	-0.006	(0.126)
92	-0.026	(0.100)	-0.097	(0.154)
93	-	-	-	-
94	0.046	(0.020)	0.027	(0.063)
95	0.017	(0.020)	-0.032	(0.058)
96	0.034	(0.035)	0.018	(0.057)
97	0.114	(0.096)	0.182	(0.142)
98	0.088	(0.067)	0.133	(0.114)
99	0.192	(0.062)	0.359	(0.120)
100	0.159	(0.043)	0.256	(0.089)
101	0.109	(0.056)	0.138	(0.089)
102	0.206	(0.066)	0.032	(0.141)
103	0.129	(0.066)	-0.051	(0.156)
104	0.051	(0.094)	-0.160	(0.135)
105	0.074	(0.076)	-0.113	(0.132)
106	0.102	(0.072)	-0.097	(0.126)
107	0.178	(0.077)	-0.042	(0.151)
108	0.191	(0.074)	-0.022	(0.153)
109	0.261	(0.064)	0.053	(0.145)
110	0.186	(0.078)	-0.060	(0.154)
111	0.297	(0.088)	0.064	(0.166)
112	0.243	(0.075)	0.051	(0.137)
113	0.254	(0.117)	0.058	(0.192)
Obs.	9,449		9,449	
R-sq.	0.721		0.664	

Notes: DD coefficients from estimation of district-level analogue of Equation 1 including county and Census division-year fixed effects displayed. Standard errors clustered at the state-level in parentheses. *Conservatism* is the first dimension DW-NOMINATE score of a district's representative in a given session of Congress, scaled from -1 (least conservative) to 1 (most conservative). *Republican* is dummy indicating whether a district's representative is Republican. The omitted period is the 93rd session of Congress, which ended in 1975.

Table A14: Effect on Election Rules

PC x Year	(1)		(2)	
	DV=Any At-Large		*DV=Share At-Large*	
1970	0.028	(0.044)	-0.010	(0.022)
1975	-	-	-	-
1980	-0.107	(0.033)	-0.049	(0.040)
1985	-0.070	(0.038)	-0.057	(0.029)
1990	-0.151	(0.054)	-0.087	(0.058)
1995	-0.081	(0.070)	-0.065	(0.063)
2000	-0.128	(0.069)	-0.119	(0.046)
2005	-0.082	(0.094)	-0.104	(0.076)
2010	-0.077	(0.039)	-0.075	(0.033)
Obs.	29,308		28,837	
R-sq.	0.638		0.758	

Notes: Data come from ICMA Municipal Yearbooks. DD coefficients from estimation of municipality-level analogue of Equation 1 including municipality and state-year fixed effects displayed. *Any At-Large* is a dummy indicating whether a city employed any at-large elections and *Share At-Large* is the share of city council seats elected at large. Standard errors clustered at the state-level in parentheses.

Table A15: Effect on Turnout by Historical Discrimination

	Income Gap (1)	Poverty Gap (2)	Education Gap (3)
		DV=Voter Turnout	
PCxPost	0.038***	0.033***	0.041***
	(0.013)	(0.011)	(0.001)
PCxPostxDisc.	0.004**	0.001**	0.001
	(0.002)	(0.000)	(0.001)
Discriminate=	$\frac{\text{avg. white inc.}}{\text{avg. minority inc.}}$	% minority pov. - % white pov.	% minority w/o edu. - % white w/o edu.
Obs.	25,196	25,946	31,133
R-sq.	0.931	0.932	0.931

Notes: Data come from ICPSR and Dave Leip's Election Atlas. DDD coefficient from estimation of Equation 6 displayed. Standard errors clustered at the state-level in parentheses. Observations weighted by voting eligible population. Includes demographic and eligibility controls.

Table A16: Effect on Newspaper Coverage

PC x Year	(1)		(2)		(2)	
	DV=VRA Mentions per Page					
1965	-0.028	(0.011)	-0.001	(0.011)	-0.042	(0.014)
1966	-0.041	(0.010)	-0.025	(0.018)	-0.046	(0.014)
1967	-0.006	(0.006)	-0.031	(0.013)	-0.001	(0.005)
1968	-0.008	(0.004)	-0.027	(0.009)	-0.003	(0.004)
1969	-0.032	(0.011)	-0.011	(0.008)	-0.038	(0.014)
1970	-0.030	(0.038)	0.004	(0.033)	-0.036	(0.044)
1971	-0.009	(0.010)	-0.009	(0.009)	-0.008	(0.012)
1972	-0.003	(0.005)	-0.016	(0.006)	0.002	(0.007)
1973	0.000	(0.004)	-0.010	(0.010)	0.002	(0.006)
1974	-	-	-	-	-	-
1975	0.083	(0.019)	0.146	(0.059)	0.063	(0.013)
1976	0.070	(0.022)	0.094	(0.049)	0.060	(0.014)
1977	0.028	(0.014)	0.080	(0.034)	0.008	(0.010)
1978	0.023	(0.022)	0.098	(0.061)	0.004	(0.016)
1979	0.020	(0.020)	0.060	(0.025)	0.009	(0.020)
1980	-0.003	(0.013)	-0.004	(0.014)	-0.000	(0.015)
Newspapers	All		Endorse Nixon		No Endorse Nixon	
Obs.	5,682		1,624		4,058	
R-sq.	0.129		0.139		0.556	

Notes: Data come from newspapers.com. DD coefficients from estimation of newspaper-level analogue of Equation 1 including newspaper and Census region-year fixed effects displayed. Dependent variable is the ratio of mentions of the phrase "Voting Rights Act" to the total number of digitized pages on newspapers.com in a newspaper-year. Standard errors clustered at the state-level in parentheses.

Do 40-Year-Old Facts Still Matter? Long-Run Effects of Federal Oversight under the Voting Rights Act

By Desmond Ang

Online Appendix

Reading this Appendix is not necessary to understanding the main analysis. All results discussed here are also referenced in the main paper. This section simply provides a more detailed explanation of the paper's robustness analyses and secondary data sources.

I. Data

A. Gallup Survey

ANES survey data is one of few available sources of historical information on political preferences identified by respondent race and county. However, given the limited sample size of the data, specifically among minorities, one may be concerned about the robustness of the party identification by race estimates. Thus, to complement those estimates, I replicate the party affiliation by race analysis presented in Section III using historical Gallup survey micro-data identified by respondent state and race. These results were displayed in Table A7 and Table A8.

Specifically, I responses regarding self-reported Republican affiliation and hypothetical opposition to a black presidential candidate. The latter was based on a question asking "If your party nominated a generally well-qualified man for president, would you vote for him if he happened to a Negro?" and coded as 0 for individuals responding "Yes" and 1 otherwise. Responses to the same question were employed by Kuziemko and Washington (2015) as a consistently measured proxy for racial conservatism, similar to the ANES question regarding government aid to minorities. Thus, micro-data were acquired from the Roper Center for all surveys administered in the sample period including the black president question (i.e., 1961, 1963, 1965, 1967, 1969, 1971, 1978, 1983, 1984, 1987, 1999, 2003) and contain between 1,000 and 3,500 responses in each year.

B. Electoral Rules

Municipality-level data employed in Figure A5 and Table A14 comes from International City/County Municipal Association (ICMA), specifically Municipal Yearbooks published in 1972, 1976, 1981, 1986, 1991, 1996, 2001, 2006 and 2011. These data are in turn based on mail-in surveys administered by the ICMA to municipalities in the one or two years prior to publication. Surveys contain questions regarding forms of government and were sent to cities, towns, townships, villages and boroughs with populations larger than 2,500 (5,000 prior to 1980), as well as other municipalities in the ICMA database. Data from 1981 onwards are available in electronic format and are identified by Census FIPS place codes. For earlier years, information was hand-coded from hard copies. Unfortunately, hard copies do not contain contain place codes. Thus, pre-treatment data was first mapped to place codes by name and state then merged with data from latter years by place code. The final dataset contains between 2,000 to 4,000 municipalities in each sample year.

C. Congressional Ideology

As complement to the Democratic vote share analysis presented in Section III, I estimate the effects of preclearance on Congressional ideology and affiliation using DW-NOMINATE data. DW-NOMINATE is a multidimensional scaling technique which collapses legislative roll-call voting into a two-dimensional ideal point. The first dimension is commonly thought of as a liberal-conservative measure (scaled from -1 to 1) and correctly classifies roll-call votes over 90% of the time. This score is also strongly correlated with ideological measures derived from campaign finances (Bonica, 2014) and Congressional speech, even when controlling for political party (Gentzkow et al., 2016). While the second dimension of DW-NOMINATE historically tracked to policy issues that cut across party lines, Congressional voting since the

1960's has been virtually unidimensional (McCarty et al., 1997). Importantly, DW-NOMINATE scores are specific to a representative-Congress and capture within-person changes over time.

Because preclearance coverage was determined at the county-level, I map each district in each Congress to the county or counties that comprise it. Because district borders frequently change, often drastically, this process was non-trivial. However, it was greatly facilitated by relationship files generously provided by James Snyder. For districts comprised of several counties, this data was supplemented with hand-coded information from the Congressional District Atlas, which publishes detailed district maps overlaid with county borders.

II. Robustness

A. Standard Errors

The paper's main analysis employs standard errors clustered at the state-level. This has been shown to correct for serial correlation in situations with close to 50 clusters (Bertrand et al., 2002). As my main analysis relies on 44 clusters, assumptions about asymptotic convergence may not hold. Thus, I test the robustness of my results to various corrections. In particular, I estimate Equation 1 using heteroskedasticity-robust standard errors, county clustered standard errors, state and year multi-way clustered standard errors, and wild-t cluster bootstrapped standard errors presented in Cameron et al. (2008). These results are shown in Table B2.

In nearly all cases, state-clustered standard errors (Column 1) are actually *smaller* than those produced from other methods for pre-treatment estimates. Thus, the 1968 DD estimate for both voter turnout and Democratic vote share is insignificant at the 5% level with all alternative specifications, providing support for the existence of parallel pre-trends. Importantly, post-treatment significance is maintained in nearly all cases.

B. Placebo Tests

B..1 Randomly-Assigned

The fact that standard errors are actually smaller when clustering by state than when clustering by county or not at all suggests that serial correlation is not an overriding concern. Nonetheless, for robustness' sake, I run a series of 500 simulations estimating Equation 1 with randomly generated treatment groups. Table B3 lists the estimated coefficients of interest for voter turnout in Column 1 and for Democratic vote share in Column 2. All coefficients are insignificant and near to zero.

B..2 Faux-Triggers

While the regression discontinuity analysis and flexible determination controls presented in Section IV suggest otherwise, one may be concerned that the cutoffs and variables used to determine preclearance are correlated with differential time trends. To examine this, I estimate "treatment" effects using placebo treatments around the actual coverage cutoffs. In particular, I restrict the analysis to counties with greater than 5% minority population share in 1970 and either 40-50% 1972 turnout or 50-60% 1972 turnout. For the first sample, I then assign treatment according to a "faux"-trigger at 45%. For the second, I assign treatment according to a false trigger at 55%. These estimates are displayed in Columns 1 and 2 of Table B4, respectively.

Notably, none of the post-treatment estimates on voter turnout are significant at the 5%-level and the majority are of the opposite sign (negative) of my main results. Regarding Democratic vote share, four of 22 post-treatment estimates are significant at the 5%-level. However, three of these four estimates, and 11 of the 22 total post-treatment estimates, are positive, while all of the paper's primary post-treatment estimates were negative. Taken together with the extremely limited sample size, these results suggest that my primary estimates are not confounded by differential trends around the true cutoffs.

C. Falsification Tests

In DD models, there is always concern that the estimated effects reflect some omitted, underlying trend orthogonal to the treatment itself. To test this, I use Equation 1 to estimate the impact of treatment on population size and minority share. Unlike other correlates of turnout like income or education which may be directly influenced through political representation, population growth and migration patterns are perhaps more likely to be "predetermined." But because voter turnout the ratio of votes cast to eligible voters, any unaccounted trends in population could directly bias the outcome of interest.

As shown in Table B3, Columns 3 and 4, this is likely not a concern here. I find little evidence of differential trends between groups in population size or composition, as all coefficients are insignificantly different from zero and small in magnitude.

D. Alternative Fixed Effects

One strength of this study's design is its ability to control for state-level shocks. In including state-year fixed effects, however, it is possible that the estimated treatment effects are not representative of counties in wholly-covered states (i.e., those in Texas and Arizona). I thus estimate Equation 1 replacing state-year indicators with alternative fixed effects.

Column 1 of Table B6 displays the estimated coefficients with a set of year indicators and a dummy variable for the historical use of poll taxes in certain states prior to their nationwide abolition in 1966. Column 2 instead replaces state-year fixed effects with division-year fixed effects. For both models, the results are nearly identical to those including state-year fixed effects, suggesting both the robustness of my findings and their validity with respect to counties in covered states.

E. Alternative Controls

I also demonstrate robustness to alternative controls for the language restrictions concurrently introduced in the 1975 VRA. Because the presence of bilingual election requirements spanned both treatment and control groups and varied between elections within counties, I included a simply policy indicator to account for their effects in my main estimation. However, one may be concerned that this dummy variable fails to capture the actual policy effect or somehow biases the DD estimates of interest. Thus, I estimate Equation 1 with two alternative specifications. One excludes the bilingual indicator entirely, the other includes a full set of bilingual-year interactions, allowing for time-varying effects of bilingual election requirements. As Columns 1 and 2 of Table B5 show, neither method demonstrably changes the sign, magnitude or significance of the coefficients of interest.

Alternatively, one may be concerned that the estimates are biased by unobserved demographic trends, not otherwise captured by the interactions between year and 1970 population measures. To account for this, I further include time-varying controls for these same demographic characteristics. These estimates, which are nearly identical to my main results, are displayed in Column 3.

F. Alternative Outcomes

This paper's main outcomes of interest are untransformed measures of voter turnout (calculated as the share of votes cast to the voting eligible population) and Democratic vote share (calculated as the share of Democratic votes cast to major party votes cast). Here, I estimate effects on an alternative measure of Democratic support — party vote share measured against *all* presidential votes cast (including those for third-party candidates) — as well as on logged voter turnout and Democratic vote share. These estimates are shown in Table B7. Notably, the direction and significance of all estimates are highly similar to those shown in the paper's primary analysis, supporting the existence of parallel pre-trends and large, persistent post-treatment effects.

III. Other Analysis

A. Voting Rights Act of 1965

As noted in Section II, identifying preclearance's effects from the 1965 VRA is problematic due to the Act's concurrent prohibition of literacy tests. Though excluding areas covered in 1965 from the analysis bolsters the internal validity of turnout estimates, one may be concerned that it also diminishes their external validity, particularly with relation to the Southern states most commonly associated with racial discrimination.

To address these concerns, I estimate treatment effects upon the universe of counties ever subject to preclearance by employing a "stacked" difference-in-differences model and controlling for the presence of literacy tests. In particular, I estimate:

$$\begin{aligned} y_{c,t} =& \delta_c + \delta_{d,t} + \sum_{\tau \neq -1} \beta_\tau relativetime_{\tau,t} \times PC_c \\ &+ \gamma_1 bilingual_{c,t} + \gamma_2 literacytest_{c,t} + \gamma_3 polltax_{c,t} + \epsilon_{c,t}, \end{aligned} \tag{7}$$

where $relativetime_{\tau,t}$ are relative time to treatment dummies equal to 1 if election t is τ elections from the treatment. For example, $relativetime_{1,t}$ is set to 1 in 1968 for counties subject to preclearance starting in 1965, in 1972 for counties subject to preclearance starting in 1970 and in 1976 for counties subject to preclearance starting in 1975, and is set to 0 otherwise. As the inclusion of state-year fixed effects would absorb all the treatment variation from the Southern states wholly-covered by the 1965 VRA, I instead include Census division-year effects $\delta_{d,t}$ and a $polltax_{c,t}$ indicator to control for the historical use of poll taxes in certain states. $literacytest_{c,t}$ accounts for the presence of literacy tests at time t among those discriminatory counties where they were later banned.[43] The coefficients of interest β_τ then represent the

[43]Specifically, $literacytest_{c,t}$ is defined as by Husted and Kenny (1997), such that it equals 1 for all counties covered by the 1965 and 1970 VRAs only in years prior to coverage and is set to 0 otherwise, even among those uncovered counties which employed literacy tests

average change between an election τ periods from treatment and the election just prior to treatment ($\tau = -1$) pooled across all treatment counties, relative to that same change over time among control counties.

The results from estimating Equation 7 with and without flexible demographic and eligibility controls are displayed in Table B8. Though some of the pre-treatment DD estimates ($\tau < 0$) are significant at the 10% level, they are inconsistent in sign and do not indicate obvious pre-treatment trends. Following treatment, I find that turnout in covered areas increased steadily over time. Despite examining three times as many treatment counties as in Section III, these effects are remarkably similar to those presented earlier, which also found maximum turnout gains of around 8 p.p.

Though this analysis does not control for idiosyncratic state-level shocks and adopts an admittedly simplistic approach to accounting for literacy test bans, the fact that these estimates so closely align with the paper's main results demonstrates both the robustness of my findings as well as the consistent and powerful effects of preclearance coverage as applied throughout the country.

(for example, those in Massachusetts and Maine). The reason for this is that literacy tests varied greatly between areas and were most restrictive in those areas specifically targeted by the earlier VRAs.

Table B1: Effect on DW-NOMINATE Scores

PC x Cong.	(2)		(3)	
	DV=1st Dim.		*DV=2nd Dim.*	
87	0.004	(0.188)	-0.171	(0.155)
88	-0.020	(0.171)	-0.036	(0.127)
89	-0.031	(0.141)	0.027	(0.129)
90	-0.003	(0.069)	0.059	(0.103)
91	0.024	(0.059)	0.023	(0.093)
92	-0.026	(0.100)	0.095	(0.078)
93	-	-	-	-
94	0.046	(0.020)	0.050	(0.048)
95	0.017	(0.020)	0.125	(0.050)
96	0.034	(0.035)	0.146	(0.047)
97	0.114	(0.096)	-0.000	(0.117)
98	0.088	(0.067)	0.018	(0.115)
99	0.192	(0.062)	-0.068	(0.123)
100	0.159	(0.043)	-0.099	(0.116)
101	0.109	(0.056)	-0.044	(0.116)
102	0.206	(0.066)	-0.083	(0.128)
103	0.129	(0.066)	-0.047	(0.127)
104	0.051	(0.094)	-0.064	(0.126)
105	0.074	(0.076)	-0.098	(0.142)
106	0.102	(0.072)	-0.069	(0.144)
107	0.178	(0.077)	0.132	(0.124)
108	0.191	(0.074)	0.131	(0.119)
109	0.261	(0.064)	0.146	(0.124)
110	0.186	(0.078)	0.228	(0.139)
111	0.297	(0.088)	0.193	(0.128)
112	0.243	(0.075)	0.043	(0.110)
113	0.254	(0.117)	0.133	(0.114)
Obs.	9,449		9,449	
R-sq.	0.721		0.739	

Notes: Data come from DW-NOMINATE. DD coefficients from estimation of district-level analogue of Equation 1 including county and Census division-year fixed effects displayed. Standard errors clustered at the state-level in parentheses. DW-NOMINATE scores collapse Congressional voting into a two-dimensional ideal point, scaled from -1 to 1. The first dimension broadly measures a district representative's conservatism in a given session of Congress (higher is more conservative). The second dimension historically captured attitudes surrounding policy issues that cut across party lines, but has provided little additional explanatory power since the 1960's. The omitted period is the 93rd session of Congress, which ended in 1975.

Table B2: Alternative Standard Errors

		Standard Errors				p-values
		cluster state	cluster state, year	cluster county	heterosk. robust	wild-t boot
PC x Year	Coef.	(1)	(2)	(3)	(4)	(5)
		Panel A: DV=Voter Turnout				
1960	0.012	(0.014)	(0.003)	(0.013)	(0.019)	0.116
1964	0.006	(0.008)	(0.010)	(0.010)	(0.022)	0.110
1968	0.013	(0.003)	(0.010)	(0.007)	(0.020)	0.068
1972	-	-	-	-	-	-
1976	0.021	(0.005)	(0.003)	(0.009)	(0.019)	0.000
1980	0.043	(0.006)	(0.006)	(0.013)	(0.017)	0.058
1984	0.051	(0.008)	(0.003)	(0.009)	(0.017)	0.054
1988	0.066	(0.008)	(0.003)	(0.011)	(0.016)	0.000
1992	0.061	(0.010)	(0.009)	(0.015)	(0.016)	0.086
1996	0.079	(0.008)	(0.016)	(0.020)	(0.017)	0.136
2000	0.083	(0.010)	(0.017)	(0.021)	(0.016)	0.014
2004	0.081	(0.011)	(0.017)	(0.021)	(0.016)	0.092
2008	0.078	(0.012)	(0.017)	(0.021)	(0.016)	0.132
2012	0.081	(0.014)	(0.014)	(0.018)	(0.016)	0.162
2016	0.078	(0.009)	(0.016)	(0.021)	(0.017)	0.096
		Panel B: DV=Dem. Vote Share				
1960	0.005	(0.026)	(0.018)	(0.024)	(0.032)	0.076
1964	0.018	(0.019)	(0.016)	(0.016)	(0.027)	0.068
1968	-0.000	(0.020)	(0.019)	(0.018)	(0.032)	0.064
1972	-	-	-	-	-	-
1976	-0.026	(0.009)	(0.009)	(0.012)	(0.022)	0.054
1980	-0.002	(0.013)	(0.007)	(0.015)	(0.019)	0.000
1984	-0.027	(0.017)	(0.014)	(0.019)	(0.020)	0.046
1988	-0.031	(0.023)	(0.018)	(0.025)	(0.021)	0.068
1992	-0.049	(0.023)	(0.018)	(0.025)	(0.022)	0.000
1996	-0.048	(0.018)	(0.014)	(0.022)	(0.020)	0.044
2000	-0.053	(0.019)	(0.017)	(0.021)	(0.020)	0.100
2004	-0.046	(0.018)	(0.014)	(0.020)	(0.018)	0.114
2008	-0.035	(0.019)	(0.015)	(0.021)	(0.017)	0.140
2012	-0.025	(0.019)	(0.015)	(0.024)	(0.019)	0.082
2016	-0.012	(0.026)	(0.020)	(0.030)	(0.024)	0.158

Notes: Data come from ICPSR and Dave Leip's Election Atlas. Standard errors calculated with various methodologies in parentheses, p-values from bootstrapping listed in Column (5). Coefficients and state-clusterd standard errors (shown in Column 1) are derived from main estimation results plotted in Figures 3 and 5.

Table B3: Placebo Tests: Random Assignment and Population Measures

	Treatment Placebos				Outcome Placebos			
PC x Year	(1)		(2)		(3)		(4)	
	DV=Turnout		*DV=Dem. Share*		*DV=ln(VAP)*		*DV=% Minority*	
1960	0.000	0.008	-0.001	0.008	-0.010	(0.082)	-0.005	(0.010)
1964	0.000	0.008	0.000	0.007	-0.007	(0.050)	-0.002	(0.006)
1968	0.000	0.005	-0.000	0.007	-0.005	(0.024)	-0.001	(0.003)
1972			-	-	-	-	-	-
1976	0.000	0.005	0.000	0.007	-0.004	(0.008)	-0.000	(0.002)
1980	0.001	0.007	0.000	0.008	-0.016	(0.017)	0.001	(0.004)
1984	0.001	0.007	0.000	0.007	-0.003	(0.017)	0.002	(0.003)
1988	0.001	0.008	0.000	0.008	0.011	(0.021)	0.002	(0.003)
1992	0.001	0.010	0.000	0.009	-0.019	(0.027)	0.005	(0.004)
1996	0.001	0.010	-0.001	0.010	-0.024	(0.033)	0.005	(0.003)
2000	0.001	0.009	-0.001	0.011	0.004	(0.025)	0.004	(0.005)
2004	0.001	0.010	-0.001	0.011	0.020	(0.027)	0.003	(0.004)
2008	0.000	0.010	-0.001	0.012	0.020	(0.028)	0.002	(0.004)
2012	0.001	0.009	-0.001	0.013	0.028	(0.026)	0.003	(0.005)
2016	0.000	0.010	-0.001	0.014	0.027	(0.028)	0.002	(0.006)
PC=	Assigned Randomly				Actual Treatment			
Obs.					37,605		37,605	
R-sq.	500 reps		500 reps		0.994		0.965	

Notes: Data come from ICPSR and Dave Leip's Election Atlas. DD coefficients from estimation of Equation 1 with randomly assigned treatment status and for outcome placebos displayed. Standard errors clustered at the state-level in parentheses. Observations weighted by voting eligible population. Includes demographic controls.

Table B4: Placebo Tests: Faux-Triggers

Less x Year	(1)		(2)	
	Panel A: DV=Voter Turnout			
1960	-0.020	(0.023)	-0.022	(0.015)
1964	-0.017	(0.014)	-0.014	(0.010)
1968	-0.027	(0.019)	-0.020	(0.010)
1972	-	-	-	-
1976	-0.018	(0.012)	0.006	(0.007)
1980	-0.007	(0.015)	0.004	(0.010)
1984	-0.015	(0.014)	0.009	(0.012)
1988	-0.011	(0.016)	0.015	(0.011)
1992	-0.010	(0.018)	0.023	(0.013)
1996	-0.015	(0.014)	0.016	(0.013)
2000	-0.018	(0.019)	-0.002	(0.015)
2004	-0.006	(0.020)	-0.001	(0.014)
2008	-0.001	(0.022)	-0.008	(0.014)
2012	-0.008	(0.025)	-0.019	(0.017)
2016	0.006	(0.025)	-0.007	(0.016)
	Panel B: DV=Dem. Vote Share			
1960	-0.018	(0.021)	0.010	(0.010)
1964	-0.034	(0.021)	-0.019	(0.013)
1968	-0.021	(0.016)	-0.008	(0.008)
1972	-	-	-	-
1976	-0.025	(0.014)	-0.013	(0.009)
1980	-0.006	(0.013)	0.006	(0.007)
1984	-0.012	(0.013)	-0.017	(0.009)
1988	-0.008	(0.015)	-0.026	(0.011)
1992	-0.012	(0.014)	-0.015	(0.012)
1996	0.008	(0.015)	0.021	(0.018)
2000	0.026	(0.014)	0.025	(0.020)
2004	0.027	(0.014)	0.016	(0.021)
2008	0.042	(0.017)	-0.008	(0.020)
2012	0.052	(0.019)	-0.000	(0.021)
2016	0.047	(0.022)	0.048	(0.025)
Turnout	40-50		50-60	
Faux-Trigger	<45		<55	
Obs.	1,530		1,805	
R-sq.	1.000		1.000	

Notes: Data come from ICPSR and Dave Leip's Election Atlas. DD coefficients from estimation of Equation 1 examining counties near the turnout cutoff and using "faux" treatment triggers. Column 1 restricts the sample to high-minority counties with 1972 turnout between 40-50% and sets a placebo trigger at 45%. Column 2 restricts the sample to high-minority counties with 1972 turnout between 50-60% and sets a placebo trigger at 55%. Standard errors clustered at the state-level in parentheses. Observations weighted by voting eligible population. Includes demographic controls.

Table B5: Alternative Controls

PC x Year	(1)		(2)		(3)	
	Panel A: DV=Voter Turnout					
1960	0.009	(0.015)	0.008	(0.015)	0.007	(0.014)
1964	0.005	(0.008)	0.005	(0.008)	0.003	(0.009)
1968	0.012	(0.003)	0.011	(0.003)	0.011	(0.003)
1972	-	-	-	-	-	-
1976	-0.003	(0.006)	-0.020	(0.014)	0.012	(0.005)
1980	0.014	(0.007)	0.012	(0.008)	0.029	(0.006)
1984	0.025	(0.004)	0.022	(0.006)	0.041	(0.007)
1988	0.035	(0.007)	0.041	(0.011)	0.051	(0.008)
1992	0.033	(0.008)	0.050	(0.011)	0.038	(0.009)
1996	0.044	(0.011)	0.061	(0.008)	0.051	(0.011)
2000	0.050	(0.011)	0.067	(0.011)	0.060	(0.012)
2004	0.043	(0.012)	0.065	(0.012)	0.052	(0.013)
2008	0.039	(0.011)	0.060	(0.013)	0.048	(0.013)
2012	0.046	(0.011)	0.062	(0.015)	0.052	(0.014)
2016	0.048	(0.010)	0.054	(0.009)	0.048	(0.011)
	Panel B: DV=Dem. Vote Share					
1960	0.006	(0.026)	0.006	(0.026)	0.003	(0.030)
1964	0.018	(0.019)	0.018	(0.019)	0.017	(0.021)
1968	0.001	(0.020)	0.000	(0.020)	-0.001	(0.021)
1972	-	-	-	-	-	-
1976	-0.010	(0.005)	-0.013	(0.005)	-0.021	(0.008)
1980	0.015	(0.008)	0.017	(0.009)	0.003	(0.013)
1984	-0.010	(0.013)	-0.011	(0.017)	-0.022	(0.016)
1988	-0.014	(0.022)	-0.011	(0.024)	-0.026	(0.022)
1992	-0.036	(0.022)	-0.038	(0.023)	-0.051	(0.023)
1996	-0.035	(0.016)	-0.048	(0.017)	-0.046	(0.018)
2000	-0.040	(0.017)	-0.060	(0.021)	-0.050	(0.020)
2004	-0.033	(0.019)	-0.045	(0.018)	-0.042	(0.018)
2008	-0.022	(0.022)	-0.033	(0.019)	-0.030	(0.019)
2012	-0.015	(0.021)	-0.031	(0.018)	-0.021	(0.020)
2016	-0.008	(0.027)	-0.023	(0.024)	-0.009	(0.026)
Add'l Ctrls.	-		Bilingual x Year		Demo. (Time-Varying)	
Omitted Ctrls.	Bilingual		-		-	
Obs.	37,606		37,606		37,604	
R-sq.	0.926		0.931		0.933	

Notes: Data come from ICPSR and Dave Leip's Election Atlas. DD coefficients from estimation of Equation 1 with alternative control specifications displayed. Standard errors clustered at the state-level in parentheses. Observations weighted by voting eligible population. Includes demographic controls.

Table B6: Alternative Fixed Effects

PC x Year	(1)		(2)	
	Panel A: DV=Voter Turnout			
1960	-0.027	(0.025)	0.015	(0.029)
1964	0.018	(0.024)	0.005	(0.019)
1968	-0.021	(0.010)	-0.015	(0.010)
1972	-	-	-	-
1976	0.062	(0.015)	0.040	(0.014)
1980	0.066	(0.015)	0.049	(0.016)
1984	0.099	(0.022)	0.066	(0.013)
1988	0.102	(0.017)	0.079	(0.014)
1992	0.076	(0.012)	0.061	(0.009)
1996	0.065	(0.014)	0.068	(0.013)
2000	0.061	(0.018)	0.075	(0.015)
2004	0.043	(0.015)	0.067	(0.012)
2008	0.037	(0.016)	0.072	(0.016)
2012	0.037	(0.018)	0.073	(0.021)
2016	0.053	(0.023)	0.069	(0.017)
	Panel B: DV=Dem. Vote Share			
1960	0.025	(0.024)	-0.017	(0.017)
1964	0.011	(0.031)	-0.008	(0.031)
1968	0.057	(0.020)	-0.005	(0.011)
1972	-	-	-	-
1976	0.013	(0.019)	-0.080	(0.023)
1980	-0.017	(0.021)	-0.052	(0.016)
1984	-0.039	(0.011)	-0.060	(0.011)
1988	-0.023	(0.011)	-0.068	(0.016)
1992	-0.046	(0.013)	-0.087	(0.026)
1996	-0.051	(0.020)	-0.076	(0.030)
2000	-0.080	(0.025)	-0.075	(0.032)
2004	-0.072	(0.023)	-0.066	(0.026)
2008	-0.072	(0.016)	-0.055	(0.013)
2012	-0.069	(0.019)	-0.045	(0.015)
2016	-0.016	(0.012)	-0.015	(0.014)
Add'l Ctrls.	Year, Polltax		Division x Year, Polltax	
Omitted Ctrls.	State x Year		State x Year	
Obs.	37,620		37,620	
R-sq.	0.760		0.838	

Notes: Data come from ICPSR and Dave Leip's Election Atlas. DD coefficients from estimation of Equation 1 with alternative control specifications displayed. Standard errors clustered at the state-level in parentheses. Observations weighted by voting eligible population. Includes demographic controls.

Table B7: Alternative Outcomes

PC x Year	(1)		(2)		(3)	
	DV=ln(Turnout)		*DV=ln(Dem. Share)*		*DV=Dem. Share (All)*	
1960	0.019	(0.027)	-0.005	(0.040)	0.005	(0.026)
1964	0.008	(0.016)	0.021	(0.015)	0.018	(0.019)
1968	0.020	(0.006)	0.001	(0.029)	-0.011	(0.011)
1972	-	-	-	-	-	-
1976	0.013	(0.011)	-0.066	(0.029)	-0.025	(0.009)
1980	0.048	(0.011)	-0.013	(0.038)	0.001	(0.013)
1984	0.073	(0.022)	-0.078	(0.048)	-0.028	(0.016)
1988	0.090	(0.026)	-0.082	(0.059)	-0.031	(0.022)
1992	0.079	(0.030)	-0.112	(0.056)	-0.039	(0.018)
1996	0.095	(0.020)	-0.106	(0.049)	-0.042	(0.018)
2000	0.105	(0.029)	-0.121	(0.052)	-0.051	(0.020)
2004	0.095	(0.032)	-0.101	(0.048)	-0.047	(0.017)
2008	0.090	(0.034)	-0.070	(0.044)	-0.036	(0.018)
2012	0.095	(0.041)	-0.054	(0.045)	-0.027	(0.019)
2016	0.090	(0.028)	-0.022	(0.053)	-0.013	(0.024)
Obs.	37,606		37,576		37,576	
R-sq.	0.998		0.9		0.921	

Notes: Data come from ICPSR and Dave Leip's Election Atlas. DD coefficients from estimation of Equation 1 for alternative outcomes displayed. Standard errors clustered at the state-level in parentheses. Observations weighted by voting eligible population. Includes demographic controls.

Table B8: Counties Covered under All VRA Versions

PC x Rel.Time	(1)		(2)	
	DV=Voter Turnout			
-4	-0.005	(0.029)	0.000	(0.028)
-3	0.012	(0.012)	0.032	(0.017)
-2	-0.040	(0.020)	-0.027	(0.017)
-1	-	-	-	-
0	0.020	(0.019)	0.035	(0.019)
1	0.006	(0.025)	0.022	(0.025)
2	0.024	(0.026)	0.038	(0.024)
3	0.038	(0.026)	0.048	(0.025)
4	0.041	(0.024)	0.045	(0.024)
5	0.040	(0.026)	0.046	(0.026)
6	0.050	(0.024)	0.058	(0.024)
7	0.056	(0.021)	0.060	(0.022)
8	0.052	(0.023)	0.057	(0.024)
9	0.045	(0.024)	0.048	(0.024)
10	0.059	(0.024)	0.056	(0.024)
11	0.067	(0.023)	0.064	(0.023)
12	0.065	(0.024)	0.068	(0.026)
Demo. Ctrls.	-		Yes	
Obs.	46,591		46,533	
R-sq.	0.837		0.847	

Notes: Data come from ICPSR and Dave Leip's Election Atlas. DD coefficients from estimation of Equation 7 displayed. Standard errors clustered at the state-level in parentheses. Observations weighted by voting eligible population. Includes all counties brought under preclearance coverage by the 1965, 1970 and 1975 versions of the VRA. Relative time corresponds to the number of presidential elections before/after treatment.