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

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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.* (*JEL* D72, J15, K16)

> *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 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 (Valelly 2004).

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

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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" (New York Times Editorial Board 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" (Thomas 2017). 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" (ABC News 2017). 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, Persson, and Sturm 2010; Washington 2012; Schuit and Rogowski 2017), 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 US 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, Aghion, and Alesina (2008) and Alesina, Baqir, and Hoxby (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 United States, where electoral rulemaking is highly decentralized and opaque.

This paper seeks to better understand the effects of such oversight. Using a flexible difference-in-difference 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 9 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 percentage points 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 percentage points across posttreatment 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 realignment 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 1960s (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 1949, 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 and Strömberg 2010, Gentzkow and Shapiro 2010, Chiang and Knight 2011, Gentzkow et al. 2015).

Perhaps most important, 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 Fourteenth and Fifteenth 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 2004). 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 to 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 percent of objections pertained to local or municipal, as opposed to state-level, proposals.[2]

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

[1] Changes could alternatively be submitted to the US District Court for DC. However, due to large relative material and time costs of pursuing "judicial" preclearance, over 99 percent of submissions were directed to the attorney general.

[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, Roland, and Tabellini 2000), Baqir (2002) finds that at-large election systems have little effect on the size of government.

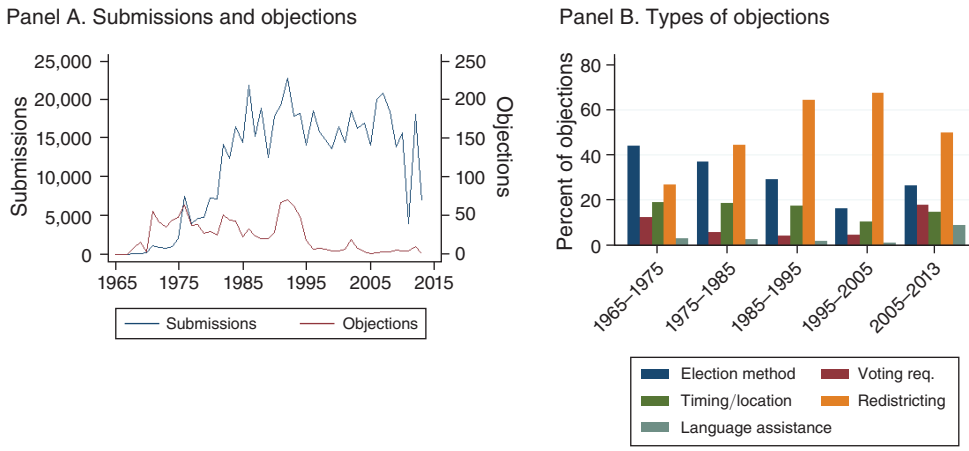

FIGURE 1. PRECLEARANCE ENFORCEMENT OVER TIME

Notes: Data come from US 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.

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

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]

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

[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

Table 1—Harris County Objections

Date	Proposed change	Reason for objection
Dec 1975	Purge all voters that fail to reregister by March 1, 1976[a]	Reregistration requirements would disproportionately burden minorities, given historical discrimination, and poll tax
Jan 1976	Eliminate state-funded primaries for parties with 2–20 percent of 1974 vote share[a]	Would only affect Raza Unida, a Mexican-American nationalist party that received 6 percent 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 2 separate schooling elections in minority districts (as opposed to 1 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; redraw city council districts	Would combine nonadjacent neighborhoods to create a district with less than 50 percent 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 1 majority Hispanic district despite Hispanics comprising 30 percent 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 percentage points
Nov 2001	Redistrict Texas state legislature[a]	Would reduce the number of Hispanic majority districts by 11 percent
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 landowning 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 come from US 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

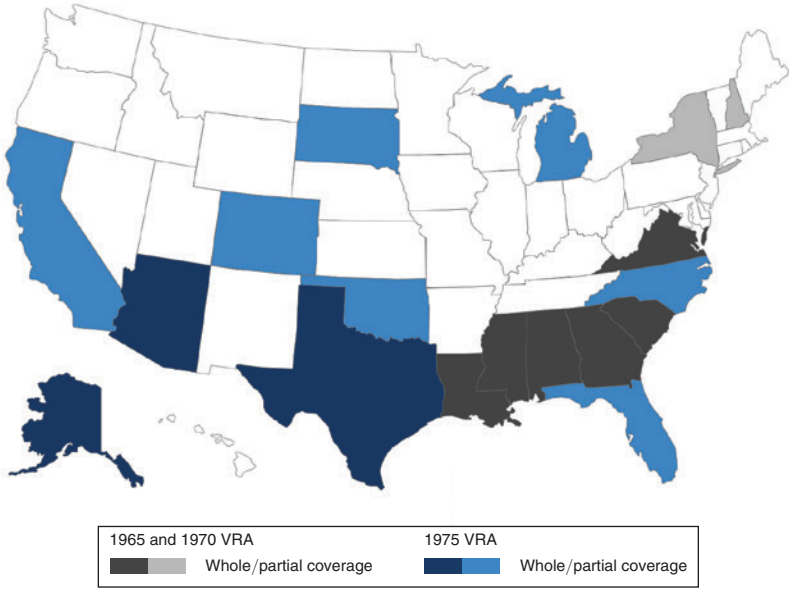

Figure 2. Jurisdictions by Year of Preclearance Coverage

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.

B. *Coverage*

Figure 2 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.

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 1972 was brought under federal

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-1980s.

oversight.[6] This resulted in the coverage of 283 counties across 9 states, only 3 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" (Seguin 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 flash point years after its initial implementation. While campaigning in 1980, President Ronald Reagan called the act "humiliating to 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

[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 "bailout" provisions, allowing areas to escape preclearance coverage after demonstrating nondiscrimination. However, these criteria were designed such that "bailout" was virtually impossible prior to 1985 and remained extremely onerous afterward. In over 40 years, only a handful of cities in Virginia successfully bailed out of coverage after proving nondiscrimination. 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–$1,000. 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).

[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).

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 1 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 5 hours long. This is part of a larger trend among previously covered counties, 43 percent 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).

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 9 states were brought under coverage in 1975, representing 3 of the nation's 4 census regions. This geographic heterogeneity bolsters the study's internal and external validity.

[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.

[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 be 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 are only available from 1968 onward.

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 US 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 eighty-seventh to one hundred and thirteenth Congresses from Poole and Rosenthal's DW-NOMINATE data (Poole and Rosenthal 1985, 2012).

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 contain individual-level self-reports of race and voting participation, which I aggregate to construct representative estimates of state-level turnout for whites and nonwhites from 1968–2016.[17] Unfortunately, the voting supplement only contains information on whether a respondent voted or 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

[14] Interpolated estimates were obtained from Gentzkow, Shapiro, and Sinkinson (2011) for 1960–2004 and were calculated by the author using the same methodology from 2004 onward.

[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 online 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.

[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 micro-data 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 posttreatment samples, nonwhites include any individual that did not identify as "white."

preferences.[18] As validation of the ANES data, I also analyze historical Gallup survey data from 1961 to 2003. These data are identified by respondent race and state and have 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 US municipal governments regarding the number and type of council seats in each city. These surveys—which have been employed by Baqir (2002) and Trebbi, Aghion, and Alesina (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 Rights Act."[20] To account for nonrandom 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, Shapiro, and Sinkinson (2011).

B. *Estimating Equation*

This paper employs a flexible difference-in-difference (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 antidiscrimination 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 US 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

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

[19] ICMA data from 1980 onward are available in electronic format, while data for 1970 and 1975 were hand-coded from hard copies. Further discussion of these data are included in the online Appendix.

[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 US papers nor the full set of pages in each sample paper.

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

9 states captured by the 1975 VRA 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 one 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 pretreatment 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

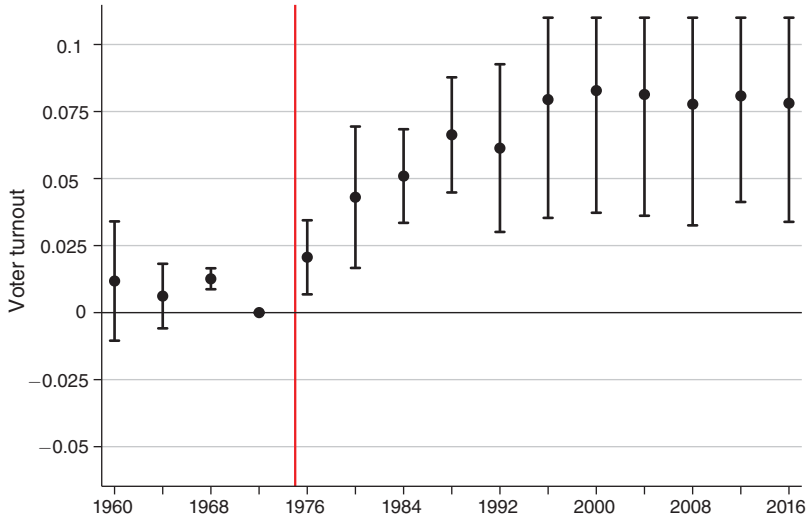

Figure 3. Effect on Voter Turnout

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

of parallel trends, the pretreatment 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 pretreatment estimates. As shown in Table B2 of the online 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: $1960 = 1968, p = 0.927$; $1960 = 1964, p = 0.585$; $1964 = 1968$, $p = 0.259$). Taken together, these estimates indicate the similarity of time trends between groups prior to treatment.

Following the implementation of oversight restrictions in 1975, average turnout among covered jurisdictions, relative to uncovered counties, gradually increased. These effects are modest at first (2.1 percentage points in 1976), peaking more than 20 years after implementation (8.3 percentage points in 2000) before leveling off. Notably, all posttreatment 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

[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 posttreatment effects.

Table 2—Effect on Voter Turnout and Democratic Vote Share

PC × 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)
Demographic controls	—	Yes	Yes	—	Yes	Yes
Near cutoffs	—	—	Yes	—	—	Yes
Observations	37,640	37,606	13,892	37,610	37,567	13,884
R^2	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 the estimation of equation (1) are displayed. Standard errors are clustered at the state level and are in parentheses. Observations are weighted by the 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 restrict the analysis to counties with the 1972 turnout between 40–60 percent and the 1970 minority share between 0–10 percent.

long-runs (2008 onward).[24] Again, analysis suggests that preclearance led to stable gains in turnout of nearly 8 percentage points over the past 2 decades.

[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 one for years between τ_1 and τ_2 and zero otherwise.

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 percentage points 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 percentage point to roughly 4 percentage points, the estimated treatment effects are in range of those identified by Filer, Kenny, and Morton (1991) and Highton (2004) for banning literacy tests (2 to 9 percentage points) and poll taxes (13 percentage points to 15 percentage points). As I will demonstrate later, I find larger effects among minority populations, demonstrating the sizable impact that preventative antidiscrimination 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 pretreatment differences in income, education, and minority share (i.e., by including interactions between a full set of year indicators and county-level measures of nonwhite 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 Twenty-Sixth 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 pretreatment coefficients. Though the posttreatment 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 percent and 1970 language minority share between 0–10 percent. These estimates are also plotted in Figure A1. Due to noise in the determination measures and nonlinearities in the coverage formula, treatment was plausibly exogeneous among this restricted sample. In support of this, all of the pretreatment 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

[25] These effects are also consistent with habit formation among those voters newly enfranchised by preclearance protections. See Gerber, Green, and Shachar (2003), Madestam et al. (2013), and Fujiwara, Meng, and Vogl (2016).

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 percentage points) is roughly 20 percent larger than the latter (5.2 percentage points).[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 percentage points.

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 nonwhite 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 (nonwhites) in state s at time t. The variable PC_s is a treatment state dummy set to one 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. 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 (nonwhites), 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 one if a state contains any county that is subject to bilingual elections in that year).

[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.

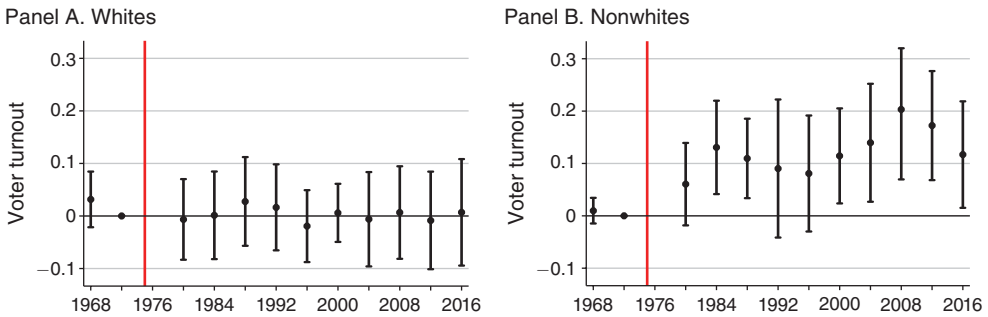

Figure 4. Effect on Turnout by Race

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

Estimates of β_τ from equation (2) are plotted in Figure 4 and displayed in Table 3, separately for whites and nonwhites.[27] As shown in Figure 4, panel B, 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 percentage points represents 30 percent of average minority turnout (48.5 percent) and is bounded below at 7 percentage points—and for their consistency—all posttreatment 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 percent of the population in treatment states, the average treatment estimate of 14.2 percentage points (averaging all posttreatment coefficients from column 3 in Table B6 of the online Appendix) translates to a net gain in turnout of 3.7 percentage points, roughly similar to the corresponding 4.8 percentage point increase recovered from the county-level analysis (column 1 of Table 2).[28]

As Figure 4, panel B shows, I find no effect on white turnout. None of the posttreatment 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 percent of city councils, it is also consistent with findings by

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

[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 of the online Appendix, demonstrates similar effects among the entire treatment group (i.e., including Texas and Arizona).

Table 3—Effect on Voter Turnout by Race

	Whites		Nonwhites	
PC × Year	(1)	(2)	(3)	(4)
DV = Voter turnout				
1968	0.032	0.029	0.010	0.014
	(0.026)	(0.023)	(0.012)	(0.0ß11)
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 controls	—	Yes	—	Yes
Observations	528	528	365	365
R^2	0.875	0.877	0.829	0.837

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

Trebbi, Aghion, and Alesina (2008) and Hajnal, Lajevardi, and Nielson (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 posttreatment estimates are inconsistent in

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

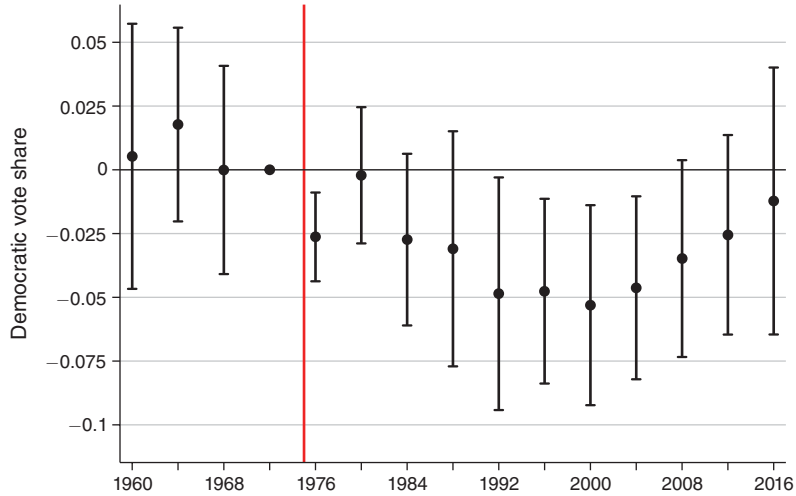

Figure 5. Effect on Democratic Vote Share

Notes: Data come from ICPSR and Dave Leip's Election Atlas. The graph shows DD coefficients and 95 percent confidence intervals from the 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 are clustered at the state level. The red vertical line represents the passage of the 1975 VRA. Full results are displayed in Table 2, column 5.

sign and never rise above 0.7 percentage points in magnitude. However, following treatment, I find gradual gains in minority turnout as large as 14 percentage points. 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.

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 the passage of the Civil Rights Act of 1964, minorities have overwhelmingly voted Democratic (Bositis 2012).[32] In the 1972 presidential

[30] $PCintensity_s$ is distributed with a mass point at 1 and all the remaining values clustered below 0.20, suggesting that the binary treatment variable may be more appropriate.

[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 1960s are scarce (Black 2004), and some historians have credited southern blacks with helping Dwight Eisenhower win the presidency in 1952 and 1956 (Strong 1971).

election, Democratic nominee George McGovern received 87 percent of nonwhite 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 3 pretreatment estimates are less than 1 percentage point in magnitude and all are insignificantly different from 0, even at the 35 percent level.

Following implementation, I find significant *decreases* in Democratic vote share among counties subject to Section 5 coverage. Roughly half of the posttreatment 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 Democratic support dropped by 2.6 percentage points in 1976 and by 5.3 percentage points in the tightly contested 2000 election before recovering in recent years. Though confidence intervals include more modest effects ranging from 0.8 percentage points in 1976 to 1.5 percentage points in 2000, these magnitudes are nonetheless politically relevant. The mean treatment estimate of 3.2 percentage points is greater than half the average presidential margin of victory since 1975 (5.7 percentage points).

As shown in Table 2, I find similar (though larger) effects even without demographic controls. Under the base model (column 4), all pretreatment 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 1 percentage point between treatment and control counties in each election prior to 1972. Rather than indicating differential trends, the pretreatment coefficients likely reflect large group differences (of 7 percentage points) in Democratic support during the omitted election of 1972, a historical outlier and the largest presidential landslide in US history.[33]

As shown in column 5, after accounting for the influence of the Vietnam War and Twenty-Sixth 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 nonwhite 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

[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 Twenty-Sixth 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 Twenty-Sixth Amendment allowed 10 million Americans between the ages of 18 to 21 to cast a presidential ballot for the first time in 1972.

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 changes in congressional representation. Taken together, these results suggest that preclearance protections meaningfully influenced party support in covered areas. Across the 3 models, I find immediate effects on Democratic vote share ranging from 2.5 to 3 percentage points 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 nonwhites—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 one 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 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 percentage points). Nonwhites, 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.

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 percentage point increase in Republican vote share among this group would

Table 4—Party Identification by Race

	Whites			Nonwhites		
	Pretreat. mean	(1)	(2)	Pretreat. 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)
Demographic controls		—	Yes		—	Yes
Observations (Rep.)		22,612			3,831	
Observations (aid)		17,240			3,031	

Notes: Data come from ANES. The DD coefficient from the estimation of equation (3) is displayed. Standard errors are clustered at the state level and are in parentheses. Observations are 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.

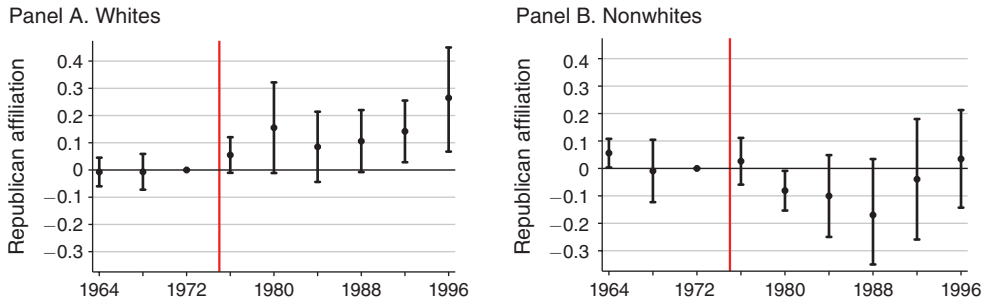

Figure 6. Effect on Republican Affiliation by Race

Notes: Data come from ANES. Graphs show DD coefficients and 95 percent confidence intervals from the estimation of the 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 the individual level and are weighted by ANES recommended survey weights. Standard errors are clustered at the state level. The red vertical line represents the passage of the 1975 VRA. Full results are displayed in Table A6, panel B, columns 1 and 3.

more than offset any concurrent Democratic gains from increased minority turnout or support. Indeed, the partisan shift among whites translates to a roughly 6 percentage point decrease in county-wide Democratic vote share, while the combined mobilization and party preference effects among minorities correspond to a 4 percentage

Table 5—Republican Identification by Minority Aid Opposition

	Whites		Nonwhites	
	(1)	(2)	(3)	(4)
DV = Republican affiliation				
PC × Post (β_4)	0.013	−0.059	−0.079	−0.087
	(0.053)	(0.044)	(0.018)	(0.031)
PC × Post × NoAid (β_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 control	—	Yes	—	Yes
Observations	17,240	16,046	3,031	2,884
R^2	0.118	0.211	0.201	0.234

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

point increase in Democratic support.[34] Thus, on net, the race-level estimates for turnout and party affiliation predict an approximately 2 percentage point decrease in Democratic vote share, in range of the average treatment effect of 3 percentage points 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. Respondents were asked to state their own preferences—scaled from one (government should help minorities) to six (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 nonwhites 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 toward 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 nonwhites:

$$(4) \quad Rep_i = \delta_c + \delta_{d,t} + \beta_1 NoAid_i + \beta_2 Post_t \times NoAid_i + \beta_3 PC_c \times NoAid_i + \beta_4 PC_c \times Post_t + \beta_5 PC_c \times Post_t \times NoAid_i + \epsilon_i,$$

[34] Roughly, the change in net vote share can be decomposed as the net change due to party-switching among whites ($0.80 \times -7.4 = -5.9$) and minorities ($0.20 \times 12 = 2.4$), holding turnout constant, plus the differential change due to increased minority turnout of approximately 1.5 percentage points. This last estimate uses the 8.8 percentage point (17 percent) average increase in minority turnout from 1976–1996 (column 4 of Table 3) and average pretreatment Democratic vote share of 35 percent (Table A1).

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 one 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 percentage points) to identify as Republican following coverage. On the other hand, I find that nonwhites 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 pretreatment sample includes only eight nonwhites in the PC_c-$NoAid_i$ cell, this last result is likely spurious.

Please consider these findings with caveats. As noted, the estimates, particularly for nonwhites, 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 antidiscrimination 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 which are

[35] Additional information regarding the Gallup data is included in the online 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 (1949), Carmines and Stimson (1989), Kousser (2010), Tesler and Sears (2010), and Enos (2016).

supported by consistent evidence of parallel pretreatment 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 percentage points 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 percent 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 percent 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 the 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 posttreatment years.

Panel A of Figure 7 displays the relationship between the running variable and average posttreatment turnout and Democratic vote share. Notably, crossing from just above the 50 percent cutoff to just below it is associated with a significant jump in posttreatment turnout. The point estimate for β_1 suggests a 6.9 percentage point 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 percentage points. In both cases, the local effects strongly resemble the average effects recovered from the difference-in-difference estimation.

Panel B estimates the same RD model across all pretreatment 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 posttreatment effects and little evidence of pretreatment 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

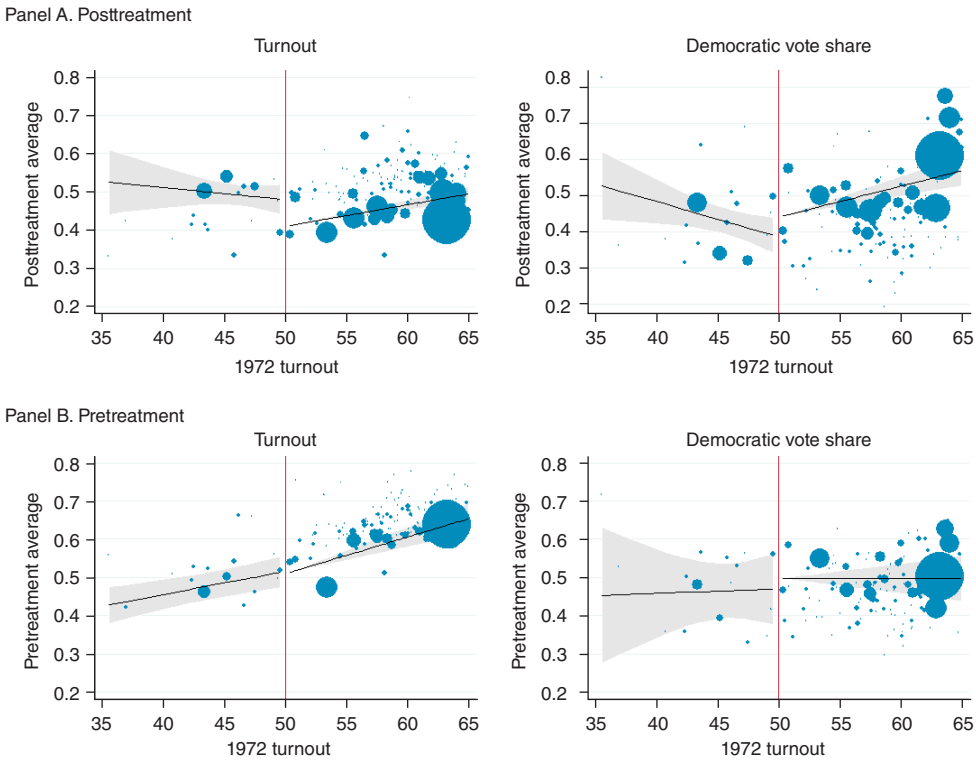

FIGURE 7. REGRESSION DISCONTINUITY

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

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 percentage points) and vote share (−2.3 percentage points) differ by just around 1 percentage point from their respective DD estimates (7.8 percentage points and −1.2 percentage points).

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-difference 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 percentage

points of the minority cutoff and 10 percentage points 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 percentage points of the turnout cutoff and 4 percentage points 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 nonrandom 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 1960s. 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 20 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, Shapiro, and Taddy 2016).[37]

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 percentage points. 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 policymaking.

D. *Other Robustness*

The online 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 of the online Appendix shows robustness to alternative methods of calculating standard errors. In Table B3, columns 1 and 2 of the online Appendix, I use randomized treatment placebos to provide evidence that the effects were not caused by serial correlation. Columns 3 and 4 of the same table further demonstrate that treatment and control areas did not experience differential trends in population or minority share over time. In Table B4 of the online Appendix, I find that placebo tests based on "faux" treatments cutoffs around the actual trigger fail to produce similar posttreatment estimates. Table B5 of the online Appendix 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 of the online Appendix shows that treatment effects are largely invariant to alternative area-time

[37] While the second dimension of DW-NOMINATE historically tracked policy issues that cut across party lines, such as bimetallism and slavery, congressional voting since the 1960s has been virtually unidimensional (McCarty, Poole, and Rosenthal 1997). As Bateman and Lapinski (2016) notes, "this measure bears little relationship to patterns of voting on civil rights," and during the 1970s, 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 online 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 online Appendix includes additional discussion of the DW-NOMINATE data and its construction.

fixed effects. Finally, Table B7 of the online Appendix 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, Aghion, and Alesina (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 citywide voting. As proposed changes to at-large systems comprised the plurality of Section 5 objections until the 1980s, 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-difference (DDD) model:

$$(6)\quad y_{c,t} = \delta_c + \delta_{s,t} + \beta_1 PC_c \times Post_t + \beta_2 PC_c \times Post_t \times discriminate_{c,1970} + \sum_{\tau \neq 1972} \lambda_\tau I_{\tau,t} \times discriminate_{c,1970} + \gamma_1 bilingual_{c,t} + \epsilon_{c,t},$$

where $discriminate_{c,1970}$ is defined as one of three predictors of pretreatment 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 posttreatment 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 percent, 83 percent, and 20 percent 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 twentieth century, newspapers were not only the primary source of information regarding local and state politics but also partisan agents themselves (Hamilton 2003; Strömberg 2004; Gentzkow, Glaeser, and Goldin 2006; Gentzkow, Shapiro, and Sinkinson 2011). Thus, if preclearance was an important political issue for 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

[39] This is based on the average treatment income ratio of 2.1, the poverty gap of 27.4 percentage points, and the illiteracy gap of 8.1 percentage points.

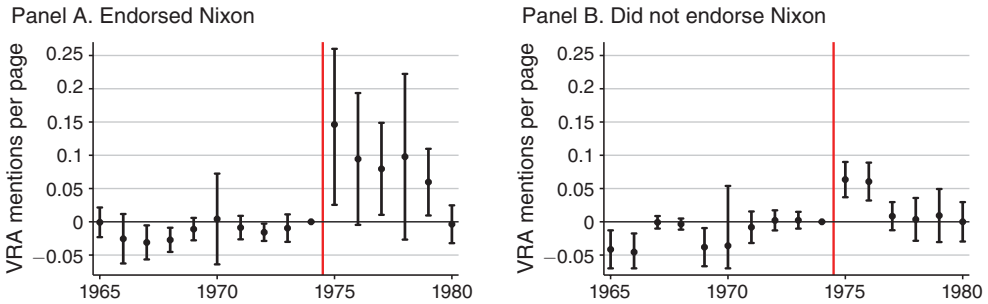

Figure 8. Newspaper Mentions by Party Affiliation

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

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 pretreatment 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 nontrivial issue. Second, the increased media coverage due to treatment was quite large, as the point estimate for 1975 (0.08) exceeds the pretreatment 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 examine 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

[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 toward zero, suggesting that the observed effects may represent a lower bound of actual disparities in local public and media focus on the VRA.

Table 6—Effect on Campaign Exposure

	Whites		Nonwhites	
	Pretreat. mean	(1)	Pretreat. 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)
Observations (media)		12,288		1,892
Observations (contact)		21,421		3,710

Notes: Data come from ANES. The DD coefficient from the estimation of equation (3) is displayed. Standard errors are clustered at the state level and are in parentheses. Observations are weighted using ANES recommended survey weights. Demographic controls are included. *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 the Republican (Democratic) Party "to get them to vote for their candidate."

restrictions entirely. Though the proposal ultimately failed, house Republicans overwhelmingly voted in its favor.[41]

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 0.146 and 0.063, 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, Shapiro, and Sinkinson 2014), these findings suggest that preclearance remained a political flash point 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.

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 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 percentage points) were more than twice as large as those in Democratic contact (6.1 percentage points).

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

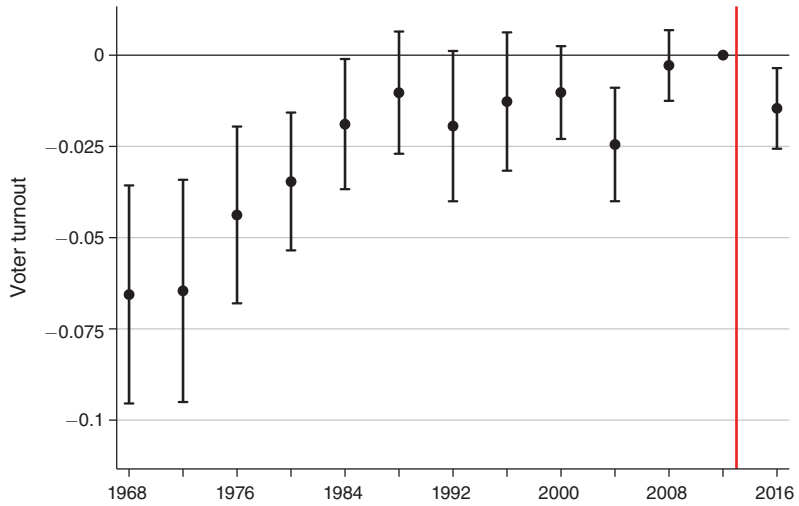

Figure 9. Post-Shelby: Voter Turnout

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

As column 2 demonstrates, I find no evidence of increased exposure to political targeting or media among minorities. Following treatment, nonwhites 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 nonwhites 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

Table 7—Post-Shelby Analysis

PC × 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)		Nonwhites (state)	
Observations	40,402		600		437	
R^2	0.836		0.858		0.853	

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

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 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.

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 online 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 percentage points 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

[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.

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 antidiscrimination 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.

Appendix A. Supplementary Figures and Tables Noted in Text

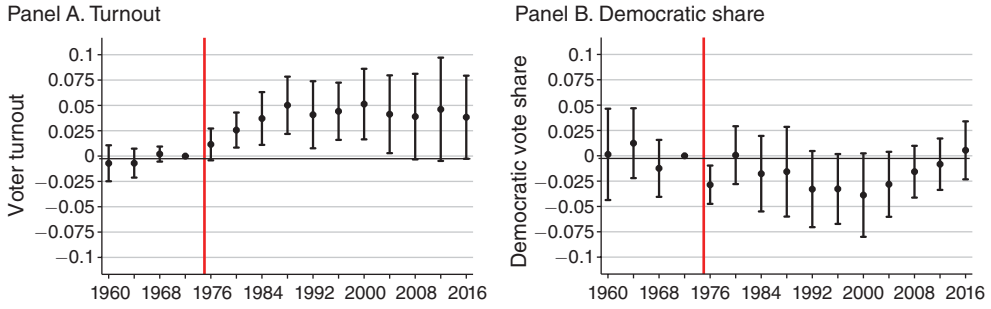

Figure A1. Counties near the Coverage Triggers

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

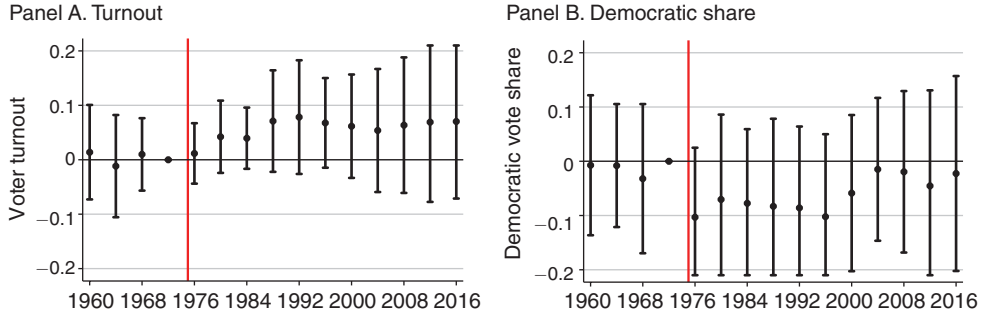

Figure A2. Regression Discontinuity by Year

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

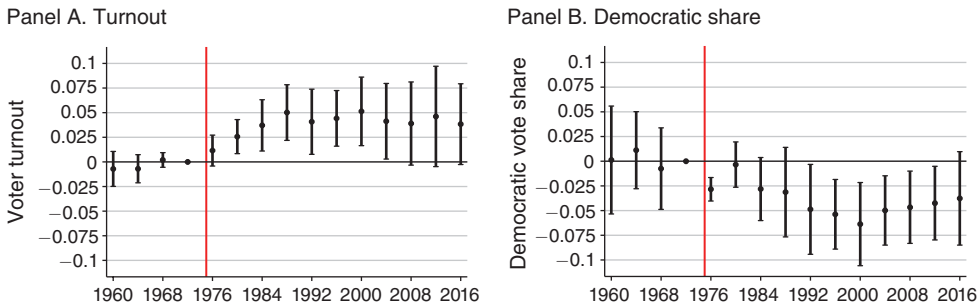

Figure A3. Controlling for Historical Determination Measures

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

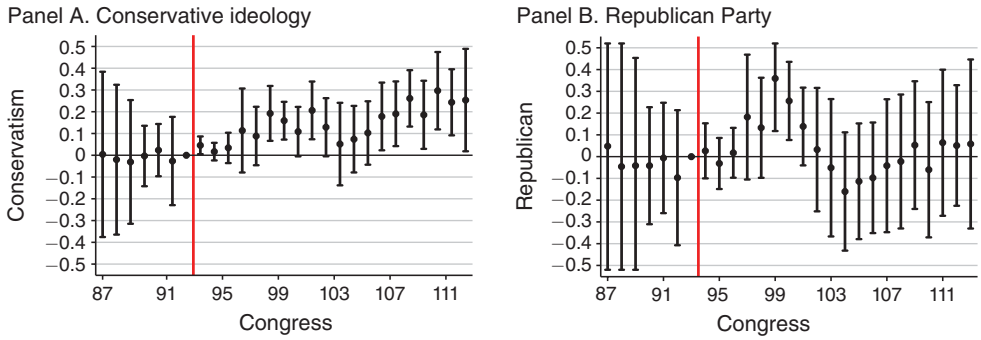

Figure A4. Effect on Congressional Ideology

Notes: Data come from DW-NOMINATE. Graphs show DD coefficients and 95 percent confidence intervals from the estimation of the 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 ninety-third Congress, which ended in January 1975. Observations are at the district-Congress level. Standard errors are clustered at the state level. The red vertical line represents the passage of the 1975 VRA. Full results are displayed in Table A13.

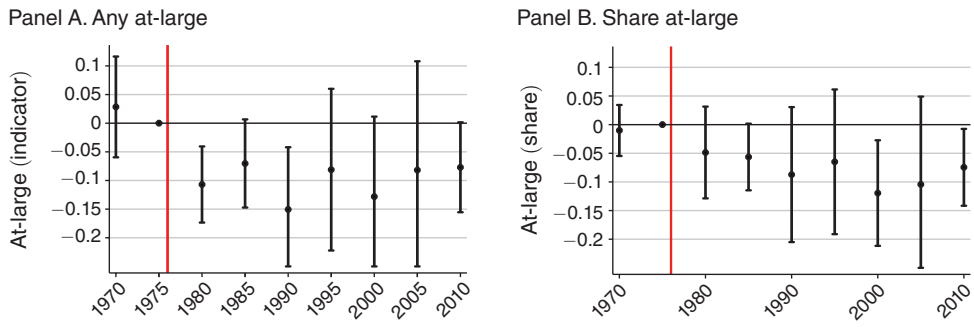

FIGURE A5. EFFECT ON AT-LARGE ELECTIONS

Notes: Data come from ICMA Municipal Yearbooks. Graphs show DD coefficients and 95 percent confidence intervals from the estimation of the 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 preclearance. 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 are clustered at the state level. The red vertical line represents the passage of the 1975 VRA. Full results are displayed in Table A14.

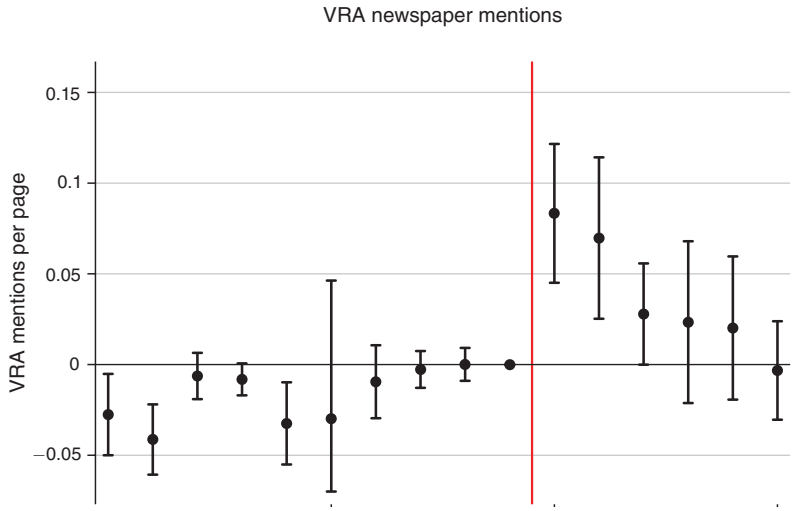

FIGURE A6. EFFECT ON NEWSPAPER MENTIONS OF THE VRA

Notes: Data come from newspapers.com. The graph shows DD coefficients and 95 percent confidence intervals from the 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 are clustered at the state level. The red vertical line represents the passage of the 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 population	73,316	254,507	53,908	171,322
Voting-eligible population	48,611	174,234	34,165	110,878
Income (average)	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 differential	12.143	20.087	27.430	18.111
Minority-white uneducated differential	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 from the US Census Bureau. White-minority income ratio is the ratio of the average white to average nonwhite income. Minority-white poverty differential is the difference between nonwhite and white poverty rates. Minority-white uneducated differential is the difference between the percent of nonwhites with no education and the percent of whites with no education.

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

PC × 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)
Demographic controls	—	Yes	Yes	—	Yes	Yes
Near cutoffs	—	—	Yes	—	—	Yes
Observations	37,640	37,606	13,892	37,610	37,567	13,884
R^2	0.921	0.930	0.892	0.867	0.918	0.920

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

Table A3—Voter Turnout: All Federal Elections

PC × 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)
Demographic controls	—		Yes		Yes	
Near cutoffs	—		—		Yes	
Observations	72,767		72,703		26,858	
R^2	0.939		0.946		0.933	

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

Table A4—Voter Turnout by Race: Period Model

	Whites		Nonwhites	
PC × 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 controls	—	Yes	—	Yes
Observations	528	528	365	365
R^2	0.874	0.876	0.826	0.835

Notes: Data come from CPS Voting and Registration Supplement. The table shows coefficients from the 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 one for years between τ_1 and τ_2 and zero otherwise. Standard errors are clustered at the state level and are in parentheses. Observations are weighted by the 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		Nonwhites	
Intensity × 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 controls	—	Yes	—	Yes
Observations	528	528	365	365
R^2	0.875	0.877	0.827	0.835

Notes: Data come from ICPSR and Dave Leip's Election Atlas. DD coefficients from the estimation of equation (2), replacing treatment dummy PC_s with the treatment intensity variable $PCintensity_s$, are displayed. Standard errors are clustered at the state level and are in parentheses. Observations are weighted by the 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		Nonwhites	
PC × 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)
Demographic controls	—	Yes	—	Yes
Observations	22,162		3,841	

Notes: Data come from ANES. DD coefficients from the 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, are displayed. Observations are weighted by ANES recommended survey weights. Standard errors are clustered at the state level.

Table A7—Party Affiliation by Race (*Gallup*)

	Whites			Blacks	
Pretreat. mean	(1)	(2)	Pretreat. 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)
Demographic controls	—	Yes		—	Yes
Observations (Rep.)	20,526			1,881	
Observations (Pres.)	19,050			1,828	

Notes: Data come from Gallup, 1961–2003. The DD coefficient from the estimation of equation (3) with state and divisn-year fixed effects is displayed. Standard errors are clustered at the state level and are in parentheses. Observations are weighted using Gallup-recommended survey weights. Controls include the respondent's age, income, and 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 × Post	0.100	0.112	−0.118	−0.133
	(0.045)	(0.045)	(0.007)	(0.038)
PC × Post × NoBlack	0.063	0.074	—	—
	(0.032)	(0.033)	—	—
Demographic controls	—	Yes	—	Yes
Observations	19,050		1,828	
R^2	0.041	0.064	0.190	0.224

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

Table A9—Regression Discontinuity

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. DV = Voter turnout						
Pretreat.	0.006	0.006	0.006	0.010	0.006	0.009
	(0.025)	(0.020)	(0.021)	(0.017)	(0.021)	(0.017)
Posttreat.	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						
Pretreat.	−0.002	−0.000	−0.028	−0.020	−0.027	−0.020
	(0.053)	(0.035)	(0.051)	(0.033)	(0.050)	(0.033)
Posttreat.	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	
Observations (pre)	566	562	714	710	762	758
Observations (post)	1,555	1,544	1,962	1,951	2,105	2,094
R^2 (pre)	0.455	0.708	0.507	0.729	0.508	0.723
R^2 (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 the estimation of equation (5) pooled across all posttreatment and pretreatment years, respectively, are displayed. 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 percent the 1970 minority share or the 1972 turnout outside of the bandwidths. Observations are weighted by the 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 the estimation of equation (5), separately for each election from 1960 onward, are displayed. 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 percent the 1970 minority share or the 1972 turnout outside of the bandwidths. Observations are weighted by the voting-eligible population (for turnout) and major party votes cast (for Democratic share). Heteroskedasticity-robust standard errors are included.

Table A11—Restricted Samples

PC × 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 percent turnout 1–9 percent minority		Neighboring counties		Pr(PC) > 75th percentile	
Observations	2,361		4,315		9,182	
R^2	0.931		0.942		0.881	

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

Table A12—Selection Controls

PC × 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)
Additional controls	Turnout1972 × Year Minority1970 × Year		Dem.Share1972 × Year		Pr(PC) × Year	
Observations	37,565		37,545		37,573	
R^2	0.936		0.931		0.932	

Notes: Data come from ICPSR and Dave Leip's Election Atlas. DD coefficients from the estimation of equation (1) with additional controls for the selection are displayed. *Turnout*1972 and *Minority*1970 are the historical voter turnout and language minority-share measures used to determine preclearance coverage under the 1975 VRA. $Pr(PC)$ is the county's probability of preclearance coverage based on the logit of the treatment on the 1968 turnout and the 1960 minority share. Standard errors are clustered at the state level and are in parentheses. Observations are weighted by the voting-eligible population. Demographic controls are included.

Table A13—Effect on Congressional Ideology

PC × Congress	(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)
Observations	9,449		9,449	
R^2	0.721		0.664	

Notes: DD coefficients from the estimation of the district-level analogue of equation (1), including county and census division-year fixed effects, are displayed. Standard errors are clustered at the state level and are 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 the dummy indicating whether a district's representative is Republican. The omitted period is the ninety-third session of Congress, which ended in 1975.

Table A14—Effect on Election Rules

PC × 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)
Observations	29,308		28,837	
R^2	0.638		0.758	

Notes: Data come from ICMA Municipal Yearbooks. DD coefficients from the estimation of the municipality-level analogue of equation (1), including municipality and state-year fixed effects, are 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 are clustered at the state level and are in parentheses.

Table A15—Effect on Turnout by Historical Discrimination

	Income gap (1)	Poverty gap (2)	Education gap (3)
DV = Voter turnout			
PC × Post	0.038	0.033	0.041
	(0.013)	(0.011)	(0.001)
PC × Post × Disc.	0.004	0.001	0.001
	(0.002)	(0.000)	(0.001)
Discriminate =	$\frac{\text{average white income}}{\text{average minority income}}$	% minority poverty − % white poverty	% minority w/o education − % white w/o education
Observations	25,196	25,946	31,133
R^2	0.931	0.932	0.931

Notes: Data come from ICPSR and Dave Leip's Election Atlas. DDD coefficient from the estimation of equation (6) is displayed. Standard errors are clustered at the state level and are in parentheses. Observations are weighted by the voting-eligible population. Demographic and eligibility controls are included.

Table A16—Effect on Newspaper Coverage

PC × Year	(1)		(2)		(3)	
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	
Observations	5,682		1,624		4,058	
R^2	0.129		0.139		0.556	

Notes: Data come from newspapers.com. DD coefficients from the estimation of the newspaper-level analogue of equation (1), including newspaper and census region-year fixed effects, are displayed. The 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 are clustered at the state level and are in parentheses.

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