

Voter Suppression?

Evidence from *Shelby County v. Holder**

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Abstract

Robust evidence shows that the Voting Rights Act increased voter turnout among citizens of color, with significant downstream effects on representation, school funding, and the black-white wage gap. In 2013, the Supreme Court’s ruling in *Shelby County v. Holder* and subsequent Congressional inaction ended preclearance—a key enforcement mechanism of the Voting Rights Act. To assess whether the removal of preclearance reduced voter turnout and support for Democratic presidential candidates, I leverage difference-in-differences variation in preclearance removal using county election returns and national election surveys. I find little evidence that preclearance removal reduced voter turnout, either overall or among voters of color. In contrast to journalistic accounts, I show that preclearance removal may have *increased* Democratic vote share.

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I Introduction

In *Shelby County v. Holder* (2013), the US Supreme Court invalidated the coverage formula governing a key enforcement mechanism of the Voting Rights Act. The mechanism—known as preclearance—required state and local jurisdictions identified by the coverage formula to secure federal approval for any policy change related to elections. To secure approval, a covered jurisdiction had to demonstrate that the policy change would not restrict voting rights based on race or membership in a language minority group. If federal authorities withheld approval, the jurisdiction could not legally implement the proposed change. The *Shelby County* decision and the subsequent failure of Congress to draft a new coverage formula concluded 48 years of preclearance oversight under the Voting Rights Act. Previously covered jurisdictions can now adopt new election policies without federal approval.

A primary concern is that the removal of preclearance enables officials in previously covered jurisdictions to suppress voting among citizens of color for political advantage. While white voters in covered states lean Republican, voters of color exhibit strong and enduring support for Democrats. All else being equal, a differential increase in the cost of voting for citizens of color would likely benefit Republican candidates at the expense of Democrats. Even if officials do not explicitly target citizens of color, policies that deter voting are likely to cost Democratic candidates votes. Quasi-experimental evidence from rainy elections shows that decreases in turnout hinder the election prospects of Democratic presidential candidates in Republican-leaning counties (Hansford and Gomez 2019). Such evidence suggests that the marginal voter in a typical covered jurisdiction favors Democratic candidates.

The removal of preclearance appears to have prompted previously covered jurisdictions to adopt policies that seek to increase the cost of voting, perhaps differentially so for citizens of color. Immediately following the removal of preclearance, legislatures in Alabama, Mississippi, and Texas enacted strict voter identification laws that had either previously failed to secure federal approval or were under review at the time of the ruling (Brennan Center for Justice 2019). Other previously covered states—such as North Carolina, a swing state—also enacted voter identification laws with additional restrictions on early voting and registration (US Commission on Civil Rights 2018). A federal court later held that North Carolina’s election

policy changes “target African Americans with almost surgical precision” (*N.C. State Conference of the NAACP v. Patrick McCrory*, 2016). As compared to uncovered jurisdictions, previously covered states were more likely to purge voters of color from voter rolls and previously covered counties were more likely to close polling places after the removal of preclearance (Brater et al. 2018; Arthur and McCann 2018). The proliferation of election policy changes in previously covered jurisdictions suggests that preclearance oversight was binding.

By spurring restrictive election policy changes, the removal of preclearance could attenuate the well-documented benefits of the Voting Rights Act. Through the elimination of poll taxes, literacy tests, and other discriminatory election policies, the Voting Rights Act of 1965 and its subsequent revisions increased voter registration and turnout among citizens of color (Ang forthcoming; Filer et al. 1991; Fresh 2018). Downstream consequences of these changes include sustained increases in black representation (Bernini et al. 2018; Schuit and Rogowski 2017), reallocation of public resources toward historically underserved black school districts (Cascio and Washington 2014), and a reduction in the black-white wage gap (Avenancio-León and Aneja 2019). Most of these changes are attributable to preclearance. To the extent that it erodes voting rights on the basis of race, the removal of preclearance could pose important distributional consequences.

While journalistic accounts of disenfranchisement abound, systematic evidence of voter suppression in response to the removal of preclearance remains elusive. To this end, I consider the impact of the removal of preclearance on voter turnout and support for Democratic presidential candidates. Specifically, I seek to address the following questions:

1. Did the removal of preclearance reduce voter turnout?
2. Did the removal of preclearance differentially reduce turnout among voters of color?
3. Did the removal of preclearance hinder the election prospects of Democratic candidates?

Using county election returns and two national election surveys from ten federal elections (2000-2018), I leverage difference-in-differences variation in the the removal of preclearance. My analysis suggests that preclearance removal had negligible effects on overall voter turnout rates. In contrast to media coverage, I find no evidence that the preclearance removal reduced

turnout among voters of color. Similarly, I estimate that Hillary Clinton would have won fewer votes had preclearance not ended.

II Background

II.A The Voting Rights Act

For its profound impact on the political participation and representation of people of color, the Voting Rights Act is often billed as “the most successful piece of civil rights legislation ever adopted” by Congress (US Department of Justice 2015). Before the Voting Rights Act was passed in 1965, many states required citizens of color to pass literacy tests and pay poll taxes prior to voting.¹ Unsurprisingly, states with these types of barriers exhibited vast racial disparities in voter registration and turnout (US Commission on Civil Rights 2018). To address these disparities, the Voting Rights Act and its subsequent revisions banned the use of literacy tests, provided the legal framework to challenge poll taxes and other discriminatory policies through litigation, and required federal approval of new state and local election policies in jurisdictions with histories of discrimination.

The two principal enforcement mechanisms of the Voting Rights Act are litigation and preclearance. While litigation may deter discriminatory election practices, it typically targets previously enacted policies. To overturn a policy through litigation, a plaintiff must demonstrate that the policy is intended to discriminate or that it exhibits discriminatory effects (Ho 2017). In contrast, preclearance targets policies before implementation. The scope of policies subject to routine preclearance includes redistricting, voting technology, voting hours, polling place closure, polling place location, and other seemingly minor changes (*Allen v. State Board of Elections*, 1969).² To implement a new policy, a covered jurisdiction must demonstrate to the US Attorney General that the proposed policy is free of both discriminatory intent and expected

¹Strictly speaking, literacy tests and poll taxes disenfranchised a broad class of otherwise eligible voters. However, grandfather clauses re-enfranchised those whose ancestors were eligible to vote prior to the passage of the 15th Amendment, which guaranteed of universal male suffrage. But few, if any, citizens of color had voting rights prior to the passage of the 15th Amendment, so grandfather clauses almost exclusively exempted white citizens from literacy tests and poll taxes. In practice, literacy tests and poll taxes selectively targeted citizens of color.

²Between 2000 and 2013, the Office of the Attorney General reviewed 215,000 proposed voting changes. See US Department of Justice, “[Section 5 Changes by Type and Year](#).”

discriminatory effects.³ A crucial distinction for the research design I outline in Section III is that preclearance enforcement was limited to covered jurisdictions concentrated in the South, whereas the right to litigate applies nationally.

The existing quasi-experimental literature leverages the coverage formula to identify causal effects of preclearance. Fresh (2018) exploits within-state variation in preclearance coverage to find that preclearance had substantial positive effects on black voter registration and overall turnout in North Carolina. Using a similar approach on a national scale, Ang (forthcoming) leverages the expansion of preclearance coverage in the 1975 renewal of the Voting Rights Act to show that preclearance had a significant and persistent positive effect on voter turnout, especially among citizens of color. Bernini et al. (2018) use difference-in-differences and geographic discontinuity approaches to show that preclearance coverage increased the number of black local elected officials, which led to increases in local education spending.

An emerging literature on the economic effects of enfranchisement suggests that changes in political participation and representation among well-defined socioeconomic groups with distinct policy preferences trigger differential changes in the distribution of public resources. Broadly speaking, the re-enfranchisement of relatively poor, non-white voters in the South appears to have increased government spending on redistributive welfare programs (Husted and Kenny 1997). Recent quasi-experimental work identifies other important downstream consequences of the Voting Rights Act. Using a triple differences design that leverages within-state variation in racial composition and the use of literacy tests, Cascio and Washington (2014) show that the abolition of literacy tests in the South increased state transfers to predominately black school districts. To identify the effects of enfranchisement on labor market disparities, Avenancio-León and Aneja (2019) use a triple differences design that compares racial differences in labor market outcomes within covered counties to those within neighboring uncovered counties. They find that preclearance increased the relative wages of black workers through additional government employment opportunities and enhanced anti-discrimination protections. Through re-enfranchisement and increased representation,

³As an alternative, a jurisdiction can seek preclearance from the US District Court for the District of Columbia.

the Voting Right Act generated substantial economic benefits for citizens of color.⁴

II.B *Shelby County v. Holder*

On June 25, 2013, the Supreme Court ruled in a 5-4 decision that the coverage formula governing preclearance is unconstitutional. In his majority opinion, Chief Justice Roberts argued that the coverage formula is “based on 40-year-old facts having no logical relation to the present day,” and thus violates equal sovereignty of the states. Moreover, preclearance was “intended to be temporary,” but Congress repeatedly renewed these sections without major revisions, most recently in 2006 (*Shelby County v. Holder*, 2013). The court did not rule on the constitutionality of preclearance itself, but the invalidation of the coverage formula rendered preclearance unenforceable. Congress has since failed to enact new coverage formula that would comply with the *Shelby County* ruling and restore preclearance oversight.

A small quasi-experimental literature documents mixed—albeit preliminary—evidence on the effects of preclearance removal. Ang (forthcoming) provides mixed evidence that the removal of preclearance decreased turnout in previously covered jurisdictions. Relative to turnout in uncovered counties, turnout in covered counties was lower in 2016 than in 2008 and 2012. However, his event study estimates also show that the difference in turnout between covered and uncovered counties in 2016 is statistically indistinguishable from the differences in 2000 and 2004. Considered together, his estimates suggest that the removal of preclearance did not have a statistically significant effect on turnout in previously covered counties.⁵ Shepherd et al. (2018) test for systematic changes in polling place locations within North Carolina following the removal of preclearance and find that voters in previously covered counties were no more likely than voters in uncovered counties to experience changes in polling place location during the 2016 election. Finally, Aneja and Avenancio-León (2019) consider the downstream

⁴Existing literature on enfranchisement in other settings highlights similar distributional consequences. For instance, Miller (2008) shows that the enfranchisement of women in the US during the late 19th and early 20th centuries increased government spending on public health initiatives related to children’s health. Outside the US, Fujiwara (2015) finds that the de facto enfranchisement of relatively less-educated citizens in Brazil increased government spending on public healthcare services, which are disproportionately used by low-income and less-educated Brazilians.

⁵More precisely, the implied difference-in-differences coefficient for the 1988-2016 subset of his sample of presidential elections is statistically indistinguishable from zero. For the full sample (1968-2016), the implied difference-in-differences coefficient is positive.

effects of preclearance removal on labor market outcomes. While they do not provide first stage estimates of political effects, their triple differences estimates suggest that the removal of preclearance increased the black-white wage gap in previously covered counties.

I contribute to this growing literature by providing additional evidence that preclearance removal had little impact on voter turnout and new evidence that it may have increased support for Democratic candidates. In contrast to existing work, I consider both presidential and midterm elections and conduct heterogeneity analysis that accommodates nonlinear effects by county racial composition.

III Data and Research Design

I seek to assess whether preclearance removal reduced voter turnout and Democratic vote share in previously covered counties. To this end, I leverage variation in preclearance coverage and the timing of preclearance removal using difference-differences comparisons of county election returns and self-reported turnout from two national election surveys. While turnout and support for Democratic presidential candidates are the principal outcomes of interest, I also consider voter registration, early voting, and vote-by-mail to explore how election officials in previously covered counties responded to preclearance removal.

III.A Preclearance Coverage

Preclearance coverage provides a crucial source of identifying variation. The Civil Rights Division of the US Department of Justice maintains a list of jurisdictions previously subject to preclearance.⁶ **Figure 1** illustrates preclearance coverage when preclearance enforcement ended in 2013.

Fully covered counties are explicitly identified by the coverage formula. While most of these counties are concentrated in the South, there are fully covered counties in Alaska, Arizona, and parts of California, New York, and South Dakota. Before preclearance ended in 2013, any state or local authority governing elections in these counties were required to secure

⁶See US Department of Justice, “[Jurisdictions Previously Covered By Section 5](#).”

federal preclearance for any policy change related to elections.

Partially covered counties are those in states with at least one fully covered jurisdiction. The presence of an explicitly covered jurisdiction in an otherwise uncovered state effectively extends coverage to the state government, as a statewide policy change would likely affect voting in the covered jurisdiction. Before 2013, state officials overseeing elections in partially covered counties were required to obtain preclearance for any election policy change, but local officials were not. For example, Michigan has two fully covered townships. A new statewide voter identification law would require voters in those townships to provide identification as a precondition to voting, so the state would have to obtain federal approval for the policy. In contrast, local officials in Wayne County, MI—an otherwise uncovered jurisdiction—could move polling places or change voting hours without federal approval. Most counties in California, Florida, New York, North Carolina, and South Dakota are partially covered.

Uncovered counties are those in states without jurisdictions explicitly identified by the coverage formula. Neither state nor local officials overseeing elections in these counties were required to obtain preclearance for election policy changes. While the West, Midwest, and Northeast census regions contain most uncovered states, there are seven uncovered states in the South. In the analyses that follow, I use uncovered counties to construct counterfactual outcomes for fully covered and partially covered counties. Given the differences in the intensity of preclearance oversight, I estimate separate effects for fully covered and partially covered counties.

III.B Data

County election returns include voter turnout and registration data from Dave Leip’s Atlas of US Presidential Elections (Leip 2018) and presidential election results from the MIT Election Data and Science Lab (2018). I merge these data with county socio-demographics from the US Census Bureau and county voting-age population by race from the National Center for Health Statistics to construct a county-year panel of turnout rates, active voter registration rates, and Democratic presidential candidate vote shares, each measured as a fraction of voting-age

citizens.⁷ The panel covers the District of Columbia and counties in 49 states over ten federal elections between 2000 and 2018.⁸

To test whether the end of preclearance differentially affected voters of color, I rely on the baseline racial composition of counties and self-reported turnout from two national election surveys that include indicators for race and ethnicity. Using data from 2012, I measure baseline racial composition as the percentage of the voting-age population that is not white non-Hispanic. I use these baseline measurements to explore the potentially heterogeneous effects of preclearance removal in both the county election returns and the national election surveys.

National election surveys include repeated cross sections from the the Cooperative Congressional Election Study (CCES; Kuriwaki 2018) and the Voting and Registration Supplement of the November Current Population Survey (CPS; Flood et al. 2018). Both surveys provide nationally representative samples of the population of non-institutionalized voting-age citizens. The CCES sample includes six federal elections between 2006 and 2016 and the CPS sample includes ten federal elections between 2000 and 2018.⁹ As previously mentioned, coverage is defined at the county level. To avoid measurement error, I assign respondents to comparison groups using county identifiers. The CPS sample only includes county identifiers for respondents living in a select group of mostly urban areas. Over the course of the sample period, 409 different counties appear in the CPS sample. The CCES sample includes identifiers for all respondents in all years. In addition to self-reported turnout, both samples include indicators for active voter registration. The CPS also includes indicators for the timing of voting (early vs. on election day) and the mode of voting (by mail vs. at the polls).

Table 1 presents baseline statistics for each comparison group. A notable difference between the comparison groups is that covered counties have a significantly higher concentration

⁷Because the number of voting-age citizens is measured with error, several observations from rural counties exhibit turnout rates in excess of 100 percent. While I include these observations, my results are not sensitive to their exclusion.

⁸I omit Alaska because it conducts elections at the legislative district level. Unlike counties, legislative district boundaries change over time in a way that is likely endogenous to preclearance coverage.

⁹An extensive literature documents that CPS respondents over-report voting (DeBell et al. 2018). In contrast, the CCES validates self-reported turnout against state voter files. Provided that over-reporting is not correlated with preclearance coverage and the timing preclearance removal, over-reporting should not affect the difference-in-differences comparisons that follow.

of nonwhite voters than uncovered counties. In spite of this difference, [Figure 2](#) demonstrates that covered and uncovered counties exhibit common support in baseline racial composition.

Panel A of [Figure 3](#) shows that voter turnout rates are persistently lower in fully covered counties than in uncovered counties. Panel B demonstrates a similar pattern within race. Within each survey, the implied turnout rate of white respondents generally exceeds that of nonwhite respondents. Compared to white respondents before preclearance removal, nonwhite respondents appear to exhibit a more pronounced fluctuation in turnout between adjacent presidential and midterm elections.

[Figure 4](#) shows that eligible voters in fully covered counties exhibit less support for Democratic candidates than those in partially covered and uncovered counties. Most fully covered counties are located in states that consistently swing Republican. In contrast, partially covered counties are located in states that either consistently favor Democrats or swing from one party to the other in subsequent elections. Uncovered counties include both Republican-leaning and Democratic-leaning counties, but the vote share statistics give more weight to populous counties, which tend to lean Democratic.

III.C Empirical Strategy

Challenges to causal inference include persistent dissimilarities between covered and uncovered counties and unobserved changes that affect voting patterns over time, such as the salience of elections. I address these challenges by using uncovered counties to construct counterfactual outcomes that inform how the outcomes of covered counties would have evolved if preclearance removal had not occurred. Under common trends assumptions outlined in [Section III.D](#), my difference-in-differences comparisons identify causal effects of preclearance removal.

First, I compare the election returns of covered counties to those of uncovered counties, before and after preclearance removal. The goal of this approach is to isolate the effect of preclearance removal on voter turnout and Democratic presidential candidate vote shares. A naive cross-sectional comparison of election outcomes between covered and uncovered counties after preclearance removal would likely fail to account for pre-existing differences in voter participation and political preferences. Likewise, comparisons of outcomes in covered counties

before and after preclearance removal would likely fail to account for confounding factors that affect voting behaviors, such as time series variation in the salience of presidential races. By comparing changes in outcomes within covered counties to those within uncovered counties, my difference-in-differences comparisons are better-positioned than naive cross-sectional or before-and-after comparisons to control for pre-existing differences between covered and uncovered counties and unobserved changes that affect all counties.

The motivating difference-in-differences specification relates election outcomes to preclearance coverage and the timing of preclearance removal:

$$y_{ct} = \beta_1(\text{Covered}_c^{\text{Full}} \times \text{Post}_t) + \beta_2(\text{Covered}_c^{\text{Partial}} \times \text{Post}_t) + X'_{ct}\Gamma + \alpha_c + \lambda_t + \varepsilon_{ct}. \quad (1)$$

Depending on the regression, y_{ct} is either the turnout rate or the Democratic presidential vote share of county c during election t . The interaction $\text{Covered}_c^{\text{Full}} \times \text{Post}_t$ is a treatment indicator equal to one if county c is explicitly covered under the preclearance coverage formula and year t is after 2013. The interaction $\text{Covered}_c^{\text{Partial}} \times \text{Post}_t$ is a treatment indicator for partially covered counties. County fixed effects (α_c) absorb the coverage indicators and control for time-invariant county characteristics that affect election outcomes. Year fixed effects (λ_t) absorb the Post_t indicator and control for secular changes that affect election outcomes in all counties. To improve precision and to control for compositional changes within counties, I include time-varying demographic controls (X'_{ct}) for age distribution, racial composition, educational attainment rates, median income, poverty rates, and indicators for the presence of senatorial and gubernatorial elections. The causal parameters of interest are the difference-in-differences coefficients for fully covered counties (β_1) and partially covered counties (β_2).¹⁰

To assess whether preclearance removal reduced turnout among voters of color, I conduct similar comparisons using election surveys. In this set of comparisons, I condition the sample on race. For each survey, I report results for nonwhite respondents—which include those who identify as Asian, Black, Latino, Native American, or mixed race—and non-Hispanic white respondents. The survey specification compares the self-reported turnout of survey

¹⁰To facilitate inference statements about voters in covered counties—as opposed to counties themselves—I weight county-year observations by the number of voting-age citizens.

respondents in covered and uncovered counties, before and after preclearance removal:

$$y_{ict} = \phi_1(\text{Covered}_c^{\text{Full}} \times \text{Post}_t) + \phi_2(\text{Covered}_c^{\text{Partial}} \times \text{Post}_t) + X_i' \Psi + X_{ct}' \Gamma + \alpha_c + \lambda_t + \varepsilon_{ict}, \quad (2)$$

where y_{ict} is an indicator equal to one if respondent i in county c voted during election year t or zero if the respondent did not vote. The interaction $\text{Covered}_c^{\text{Full}} \times \text{Post}_t$ is a treatment indicator equal to one if the respondent resides in a fully covered county after preclearance removal. The interaction $\text{Covered}_c^{\text{Partial}} \times \text{Post}_t$ is a treatment indicator equal to one if the respondent resides in a partially covered county after preclearance removal. County fixed effects (α_c) control for race-specific, time-invariant factors that affect voting in each county. Year fixed effects (λ_t) control for race-specific secular changes that affect voting nationally. The fixed effects absorb the lower-order terms of the treatment interactions. Respondent controls (X_i') include age, sex, educational attainment, household income, and interview characteristics. Time-varying county controls (X_{ct}') include indicators for the presence of senatorial and gubernatorial elections. The parameters ϕ_1 and ϕ_2 characterize the effects of preclearance removal on eligible voters in fully and partially covered counties.¹¹

I estimate standard errors that allow for clustering at the county level because coverage is defined at the county level. The inferences made in Section IV are robust to standard errors that allow for clustering at the state level.

III.D Identification

The identifying assumption posits that covered and uncovered counties exhibit common trends in election outcomes without preclearance, conditional on fixed effects and time-varying controls. Put differently, the causal interpretation of my difference-in-differences estimates rests on the assumption that the election outcomes of covered counties would have evolved similarly to those of uncovered counties had preclearance not ended. If there are unobservables correlated with coverage and the timing of preclearance removal—such as variation in regional support for Democratic presidential candidates over time—then difference-in-differences comparisons

¹¹To facilitate inference statements about the population of eligible voters, I weight survey respondents using recommended survey weights.

may fail to isolate the causal effect of preclearance removal on election outcomes. To allay these concerns, I report results from specifications that absorb region-specific shocks to voting with census-division-by-year fixed effects.

IV Results

In this section, I present the results of the difference-in-differences and triple differences analyses outlined in Section III. To assess the validity of common trends and test for trend breaks in election outcomes following the removal of preclearance, I report estimates from event studies in addition to a variety of alternative difference-in-differences specifications.

IV.A Main Results

Difference-in-differences estimates in Table 2 provide evidence that is inconsistent with widespread reductions in turnout in previously covered counties. The point estimates in column 1 suggest that preclearance removal increased voter turnout by 1.7 percentage points (statistically distinguishable from zero at the 1 percent level) in fully covered counties and 0.2 percentage points (statistically indistinguishable from zero at conventional significance levels) in partially covered counties. While these estimates are robust to the inclusion of time-varying controls in column 2, the accommodation of region-specific shocks in columns 3 through 6 yields meaningfully different estimates. Estimates from the most flexible specification (column 6) suggest that turnout increased by 0.1 percentage points (statistically indistinguishable from zero at conventional significance levels) in fully covered counties and decreased by 0.5 percentage points (statistically distinguishable from zero at the 10 percent level) in partially covered counties. While the most flexible specifications suggest that turnout decreased in partially covered counties, the magnitude of this effect is small relative to pre-removal turnout rates.

The event studies in Figure 5 provide additional, albeit noisy, evidence that preclearance removal did not meaningfully affect voter turnout in covered counties. Event study coefficients based on Equation 1 compare regression-adjusted differences in turnout between covered

and uncovered counties, relative to the difference in 2012. The estimates reported in [Figure 5](#) are from specifications that include controls and census-division-by-year fixed effects, and thus relate to the results I report in column 6 of [Table 2](#). While there are several negative and statistically significant estimates in the years leading up to preclearance removal for fully covered counties, three out of four of these estimates describe pre-removal differences in midterm election years. As shown in [Table 1](#), the concentration of nonwhite adults in fully covered counties is twice that of uncovered counties. [Figure 3](#) suggests that turnout among nonwhite voters exhibits a more pronounced cyclic fluctuation between presidential and midterm elections. The negative pre-removal estimates are likely the result of those two facts.¹² The within-race event studies reported in [Figure 6](#) support this conjecture. Within election type, the estimates in [Figure 5](#) provide little evidence of a significant decrease in turnout following preclearance removal.

Panel A of [Table 3](#) reports difference-in-differences for nonwhite survey respondents. The CPS sample provides little evidence that preclearance removal decreased turnout among respondents of color. Estimates from the most flexible specification in column 8 suggest that preclearance removal increased turnout among those in fully covered counties by 0.1 percentage points and decreased turnout among those in partially covered counties by 0.9 percentage points. However, both estimates are statistically indistinguishable from zero at conventional significance levels. The CCES sample provides similar results for those in partially covered counties. Based on estimates from the most flexible specification in column 4, preclearance increased turnout among nonwhite respondents in partially covered counties by a statistically insignificant 1.2 percentage points. In contrast, all four CCES estimates for those fully covered counties are positive and statistically distinguishable from zero at the 5 percent level. Estimates from column 4 suggest that preclearance removal increased turnout among nonwhite respondent in fully covered counties by 6 percentage points, but the corresponding event study in Panel A of [Figure 6](#) suggests that this estimate is inflated.

The other event study estimates in Panel A of [Figure 6](#) fail to falsify the common trends assumption the for remaining difference-in-differences comparisons of nonwhite survey

¹²The inclusion of county-by-election-type fixed effects—to be considered in the next iteration of this paper—would likely absorb the differences that generate the negative event study estimates in pre-removal years.

respondents. All pre-removal event study estimates for nonwhite CPS respondents are statistically indistinguishable from the adjusted base-year difference at the 5 percent level and do not exhibit discernible trends leading into preclearance removal. Likewise, all event study estimates for nonwhite CCES respondents in partially covered counties are numerically stable and statistically indistinguishable from the adjusted base-year difference at the 5 percent level. The positive and statistically significant difference-in-differences estimate for nonwhite CCES respondents in fully covered counties appears to be an artifact of upward-trending event study coefficients leading into preclearance removal. The event study estimates lack a break in trend after 2013, which suggests that preclearance removal had little effect on turnout among nonwhite CCES respondents in fully covered counties.

Panel B of [Table 3](#) reports difference-in-differences for white survey respondents. Specifications with controls and census-division-by-year fixed effects provide little evidence of an effect of preclearance removal on turnout among white respondents in fully covered counties. Event studies corroborate these results. For those in partially covered counties, the CCES and CPS samples yield statistically significant, but opposite-signed estimates. Across several alternative specifications, the CCES estimates for white respondents in partially covered counties are negative and the CPS estimates are positive. The corresponding event studies in Panel B of [Figure 6](#) show that the negative effect for CCES respondents is driven by a single positive and statistically significant event study coefficient in 2006; all other coefficients are numerically stable and statistically indistinguishable from the base-year coefficient. In contrast, the CPS event study features numerically stable event study estimates in the pre-removal period and all but one are statistically indistinguishable from the base-year coefficient, which supports a causal interpretation of its corresponding difference-in-differences estimate.

The difference-in-differences estimates in [Table 4](#) show evidence that is inconsistent with the notion that preclearance removal hindered the election prospects of Democratic presidential candidates. Across a variety of specifications, the estimated effect on Democratic presidential vote share in fully covered candidates is positive and statistically significant. While the magnitude and statistical significance of the effect in partially covered counties diminishes with the inclusion of census-division-by-year fixed effects, the estimated relationship remains

non-negative. [Figure 7](#) shows the the pre-removal coefficients for fully covered counties exhibit a slight upward trend, which may indicate that the corresponding difference-in-differences coefficient is inflated. However, the general pattern gives no impression that preclearance removal reduced votes for Democratic presidential candidates. If support for presidential candidates is any indication, preclearance removal does not appear to have reduced support for Democratic candidates in fully covered counties. If anything, preclearance removal is associated with increased support for Democrats.

IV.B Heterogeneity

The difference-in-differences estimates presented above characterize how preclearance removal affected turnout and Democratic vote share in covered counties, *on average*. These averages could mask differential effects by county racial composition. To explore the potentially heterogeneous effects of preclearance removal, I interact the treatment indicators in [Equation 1](#) and [Equation 2](#) with baseline county racial composition. I allow for nonlinearities by using restricted cubic spline interactions with three knots placed at population-weighted quartiles of baseline racial composition.¹³ For ease of interpretation, I present marginal effects of preclearance removal.

[Figure 8](#) plots the marginal effect of preclearance removal by fraction nonwhite. For the least nonwhite fully covered counties, marginal effects are near zero. At around 30 percent nonwhite, marginal effects for fully covered counties begin to slope upward, but at no point are they statistically distinguishable from zero based on a 95 percent confidence interval. Marginal effects for partially covered counties begin to slope downward at around 30 percent nonwhite and become negative at around 50 percent. However, these negative effects are statistically indistinguishable from zero based on a 95 percent confidence interval.

Panel A of [Figure 9](#) explores whether effects differ for nonwhite survey respondents in relatively white counties versus those in relatively nonwhite counties. The marginal effects appear constant and near zero for survey respondents in fully covered and partially covered counties. Panel B reveals a similar pattern for white respondents. Within respondent race, I

¹³Compared to polynomial interactions, restricted cubic splines exhibit less erratic behavior in the tails of the racial composition distribution.

find little evidence of heterogeneous effects by county racial composition.

Figure 10 shows that the positive effects of preclearance removal on Democratic vote share are driven by relatively nonwhite counties. Marginal effects for fully covered counties are positive across the distribution of county racial composition and statistically distinguishable from zero for counties that are at least 40 percent nonwhite at baseline. Point estimates suggest that—for counties over 75 percent nonwhite—preclearance removal increased Democratic vote share by 2.5 to 3.5 percentage points, on average. Relatively nonwhite partially covered counties appear to experience similar, but less pronounced effects. A notable exception emerges from partially covered counties around 25 percent nonwhite. In these counties, preclearance removal decreased Democratic vote share by approximately 1 percentage point.

IV.C Policy Channels

Preclearance removal had little effect on turnout, but this does not necessarily imply that officials overseeing elections in covered counties did not adopt new policies. To explore policy responses to preclearance removal, I conduct difference-in-differences comparisons of active voter registration, self-reported timing of voting, and self-reported mode of voting. **Figure 11** shows that preclearance removal had statistically insignificant effects on self-reported voter registration among nonwhite and white survey respondents in both fully covered and partially covered counties.¹⁴ In contrast to the survey analysis, the analysis of county registration records suggests that preclearance removal increased voter registration by nearly two percentage points in fully covered counties. **Figure 12** shows that preclearance removal reduced early voting among nonwhite CPS respondents in fully covered counties by 3.4 percentage points (statistically distinguishable from zero based on a 95 percent confidence interval). However, this decrease in early voting was fully offset by a 3.4 percentage-point increase in election day voting (statistically indistinguishable from zero based on a 95 percent confidence interval). Broadly speaking, these results suggest that officials made policy changes in response to preclearance removal.

¹⁴Note that voter purges target inactive voters, so those results do not speak to whether covered jurisdictions increased the frequency of purges following preclearance removal.

V Discussion

I find that preclearance removal had little impact on voter turnout in federal elections. While journalistic accounts emphasize differential targeting of voters of color by officials in previously covered jurisdictions, I provide evidence that is inconsistent with decreased turnout among voters of color. Moreover, it does not appear that preclearance removal cost Hillary Clinton votes in the 2016 presidential election. If anything, preclearance removal increased her vote share, especially in communities of color.

While my results indicate that the removal of preclearance had a relatively small impact on federal elections, they do not rule out the possibility that previously covered states pursued policies that increase the cost of voting. Even if previously covered states adopted restrictive policies, there are several mechanisms that could explain the non-negative turnout response to the removal of preclearance. First, voters mobilize in response to perceived threats to the franchise (Biggers and Smith 2018; Valentino and Neuner 2017). Second, previously covered states bundled restrictive policies with policies that increase voting access, such as online voter registration (Fuller 2014). Third, an increase in voting rights litigation after preclearance removal blocked some restrictive policies and may have deterred others (US Commission on Civil Rights 2018). Fourth, substitution toward absentee or early voting could partially offset negative turnout responses to polling place changes (Clinton et al. 2019). Finally, the most credible evidence on voter identification laws suggests that they have little impact on turnout (Cantoni and Pons 2019; Highton 2017).

My results speak only to differential changes between covered and uncovered jurisdictions following preclearance removal. They do not rule out the existence of voter suppression in federal elections. Nor do they generalize to local elections. Although federal elections contain a multitude of state and local races, many school boards, cities, and other local jurisdictions run separate elections. Local elections generally receive less national scrutiny, but their outcomes pose important consequences for the provision of public resources and the enforcement of anti-discrimination laws. The persistence of racial divides in the local politics of previously covered states raises concerns that local officials could leverage the removal of federal oversight to manipulate election outcomes. Future research should examine the impact of preclearance

removal in these settings.

Table 1: Baseline Descriptive Statistics

	Fully Covered		Partially Covered		Uncovered	
	Mean (1)	Std. Dev. (2)	Mean (3)	Std. Dev. (4)	Mean (5)	Std. Dev. (6)
<i>Panel A: Counties</i>						
Turnout Rate	0.46	0.13	0.52	0.12	0.55	0.12
Democratic Vote Share	0.25	0.10	0.32	0.08	0.31	0.10
Registration Rate	0.82	0.11	0.84	0.10	0.86	0.10
Fraction Nonwhite	0.40	0.18	0.36	0.21	0.20	0.17
Poverty Rate	0.16	0.06	0.13	0.04	0.12	0.05
County Observations	844		411		1856	
<i>Panel B: CPS (Nonwhite)</i>						
Voted?	0.53	0.50	0.57	0.50	0.60	0.49
Registered?	0.72	0.45	0.72	0.45	0.74	0.44
Voted by Mail?	0.04	0.20	0.11	0.31	0.08	0.27
Voted at Polls?	0.49	0.50	0.45	0.50	0.52	0.50
Voted Early?	0.13	0.33	0.16	0.36	0.14	0.35
Voted on Election Day?	0.42	0.49	0.43	0.49	0.48	0.50
County Observations	99		105		202	
<i>Panel C: CPS (White)</i>						
Voted?	0.65	0.48	0.69	0.46	0.68	0.47
Registered?	0.81	0.40	0.84	0.37	0.82	0.38
Voted by Mail?	0.10	0.30	0.16	0.37	0.12	0.32
Voted at Polls?	0.54	0.50	0.52	0.50	0.56	0.50
Voted Early?	0.19	0.39	0.22	0.41	0.17	0.38
Voted on Election Day?	0.47	0.50	0.48	0.50	0.52	0.50
County Observations	99		105		205	
<i>Panel D: CCES (Nonwhite)</i>						
Voted?	0.38	0.49	0.46	0.50	0.48	0.50
Registered?	0.83	0.37	0.85	0.35	0.85	0.36
County Observations	717		344		1227	
<i>Panel E: CCES (White)</i>						
Voted?	0.50	0.50	0.61	0.49	0.58	0.49
Registered?	0.86	0.35	0.88	0.32	0.88	0.33
County Observations	807		400		1802	

Notes: Statistics from federal elections before preclearance ended in 2013. County statistics are weighted by the number of voting-age citizens. Survey statistics are weighted using recommended survey weights. The denominator for each county election statistic and the implicit denominator for each survey statistic is the number of voting-age citizens. All registration statistics refer to active voter registrations. *Sources:* Dave Leip's Atlas of US Presidential Elections, MIT Election Data and Science Lab, Integrated Public Use Microdata Series (Current Population Survey), Cooperative Congressional Election Study, US Census Bureau, and the National Center for Health Statistics.

Table 2: Effect of Preclearance Removal on Voter Turnout

	Turnout					
	(1)	(2)	(3)	(4)	(5)	(6)
Fully Covered \times Post	0.017*** (0.002)	0.016*** (0.002)	0.002 (0.003)	0.004 (0.003)	0.003 (0.003)	0.001 (0.003)
Partially Covered \times Post	0.002 (0.004)	−0.0001 (0.004)	−0.002 (0.003)	−0.003 (0.003)	−0.004* (0.003)	−0.005* (0.003)
Observations	30,929	30,929	30,929	30,929	30,929	30,929
Controls		✓		✓		✓
Year FE	✓	✓				
Region-Year FE			✓	✓		
Division-Year FE					✓	✓
County FE	✓	✓	✓	✓	✓	✓

Notes: OLS estimates from difference-in-differences regressions based on [Equation 1](#). An observation is a county in a federal election year. The turnout rate is the number of ballots cast divided by the number of voting-age citizens. Controls include county demographic composition (age, race, sex), educational attainment rates, poverty rates, median income, and indicators for the presence of senatorial and gubernatorial races. All regressions are weighted by the number of voting-age citizens. Standard errors (in parentheses) allow for clustering at the county level. ***, **, and * denote statistical significance at the 1, 5, and 10 percent levels.

Table 3: Effect of Preclearance Removal on Voter Turnout by Race

	Voted?							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>Panel A: Nonwhite</i>								
Fully Covered \times Post	0.071*** (0.019)	0.078*** (0.020)	0.057** (0.023)	0.060*** (0.023)	0.021 (0.018)	0.014 (0.016)	0.005 (0.022)	0.001 (0.019)
Partially Covered \times Post	0.010 (0.014)	0.009 (0.013)	0.014 (0.021)	0.012 (0.019)	-0.001 (0.016)	-0.005 (0.015)	-0.001 (0.015)	-0.009 (0.014)
Observations	75,853	75,581	75,853	75,581	93,659	93,659	93,659	93,659
<i>Panel B: White</i>								
Fully Covered \times Post	0.030* (0.015)	0.038*** (0.015)	0.001 (0.019)	0.010 (0.016)	0.039*** (0.012)	0.036*** (0.011)	0.006 (0.013)	0.004 (0.012)
Partially Covered \times Post	-0.035*** (0.011)	-0.030*** (0.010)	-0.044*** (0.012)	-0.040*** (0.011)	0.024*** (0.009)	0.018** (0.008)	0.023** (0.009)	0.017* (0.009)
Observations	224,006	223,035	224,006	223,035	213,284	213,284	213,284	213,284
Survey	CCES	CCES	CCES	CCES	CPS	CPS	CPS	CPS
Controls		✓		✓		✓		✓
Year FE	✓	✓			✓	✓		
Division-Year FE			✓	✓			✓	✓
County FE	✓	✓	✓	✓	✓	✓	✓	✓

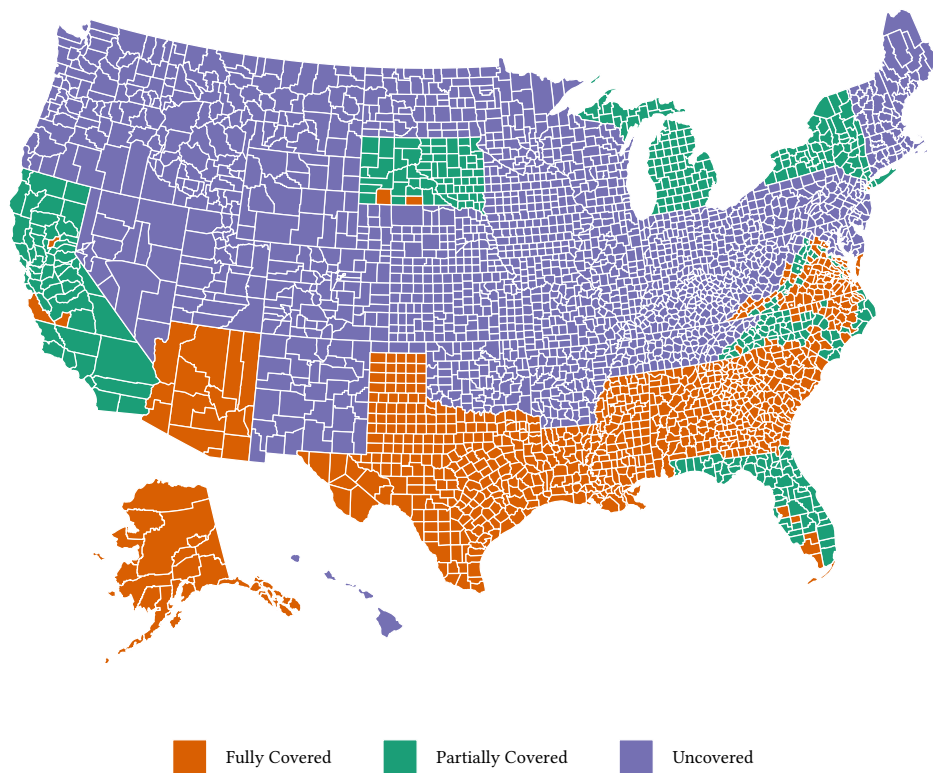
Notes: OLS estimates from difference-in-differences regressions based on Equation 2. An observation is a respondent in a federal election year. The outcome is an indicator equal to one if the respondent voted and zero if otherwise. Controls include respondent characteristics (age, sex, income, educational attainment, and interview characteristics) and indicators for the presence of senatorial and gubernatorial races. All regressions are weighted using recommended survey weights. Standard errors (in parentheses) allow for clustering at the county level. ***, **, and * denote statistical significance at the 1, 5, and 10 percent levels.

Table 4: Effect of Preclearance Removal on Vote Share for Democratic Presidential Candidates

	Democratic Vote Share					
	(1)	(2)	(3)	(4)	(5)	(6)
Fully Covered \times Post	0.031*** (0.004)	0.032*** (0.003)	0.025*** (0.004)	0.026*** (0.003)	0.024*** (0.004)	0.023*** (0.003)
Partially Covered \times Post	0.022*** (0.005)	0.016*** (0.005)	0.014*** (0.004)	0.011*** (0.004)	0.006 (0.004)	0.005 (0.004)
Observations	15,196	15,196	15,196	15,196	15,196	15,196
Controls		✓		✓		✓
Year FE	✓	✓				
Region-Year FE			✓	✓		
Division-Year FE					✓	✓
County FE	✓	✓	✓	✓	✓	✓

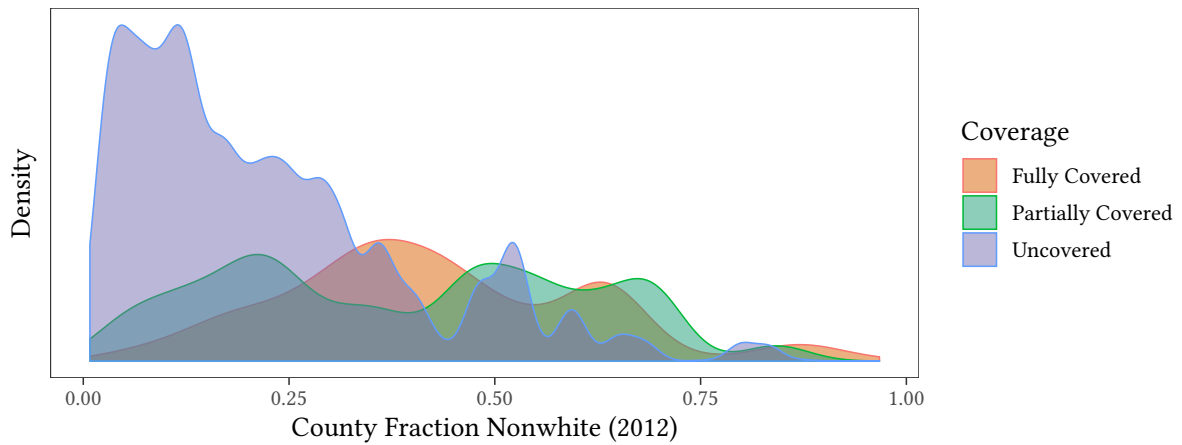
Notes: OLS estimates from difference-in-differences regressions based on [Equation 1](#). An observation is a county in a federal election year. Democratic vote share is the number of votes for the democratic presidential candidate divided by the number of voting-age citizens. Controls include county demographic composition (age, race, sex), educational attainment rates, poverty rates, median income, and indicators for the presence of senatorial and gubernatorial races. All regressions are weighted by the number of voting-age citizens. Standard errors (in parentheses) allow for clustering at the county level. ***, **, and * denote statistical significance at the 1, 5, and 10 percent levels.

Figure 1: Preclearance Coverage



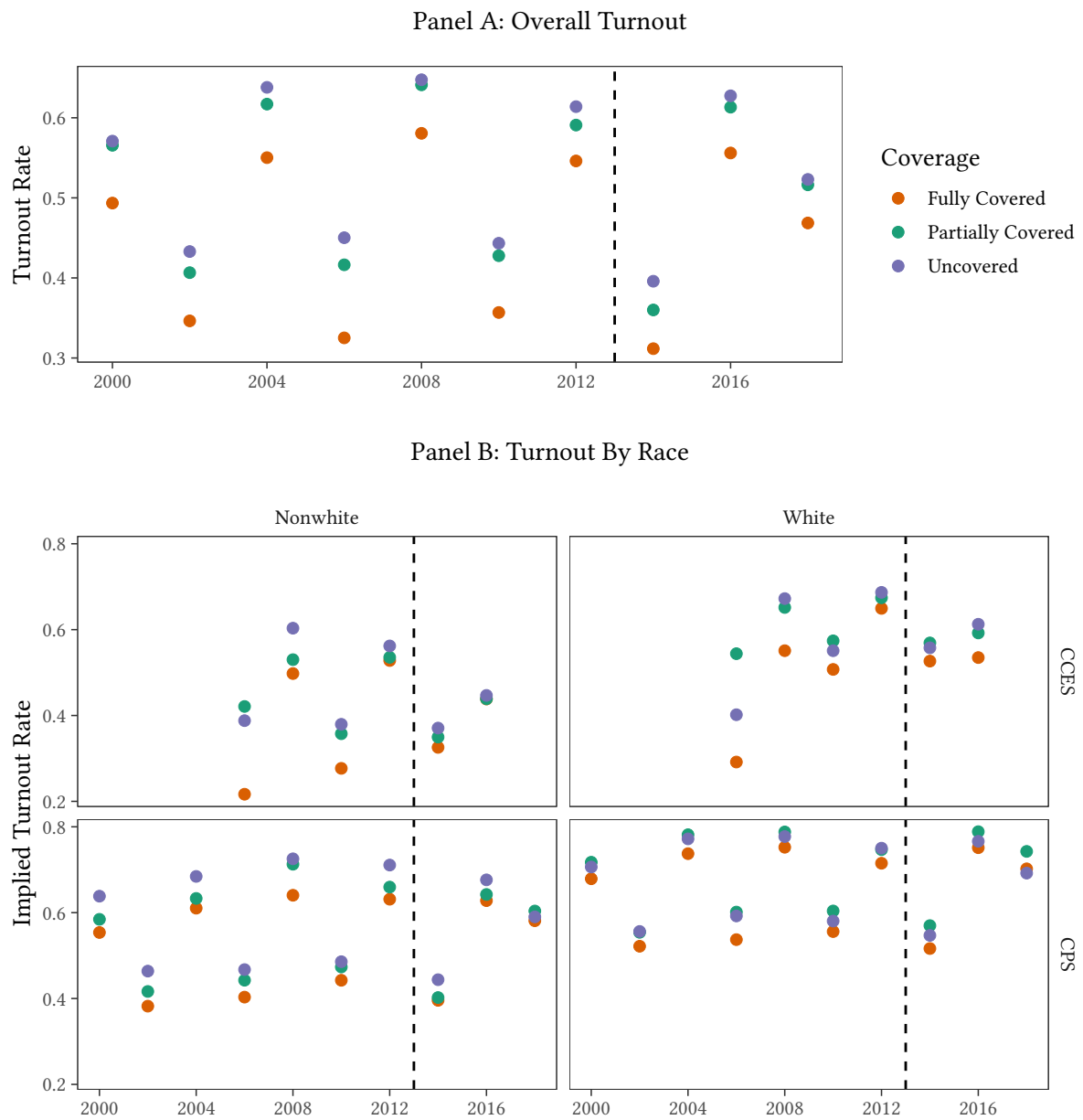
Notes: Before the *Shelby County* decision invalidated the preclearance coverage formula in 2013, jurisdictions identified by the formula were required to obtain preclearance for all election policy changes. Any state policy affecting elections in an explicitly covered jurisdiction also required preclearance, which effectively extended coverage to some unidentified jurisdictions. In fully covered counties, both state and local policies were subject to preclearance. In partially covered counties, state policies were subject to preclearance, but local policies were not. In uncovered counties, neither state nor local policies were subject to preclearance. Three New York City boroughs—Manhattan, Brooklyn, and the Bronx—are fully covered.

Figure 2: Baseline County Racial Composition



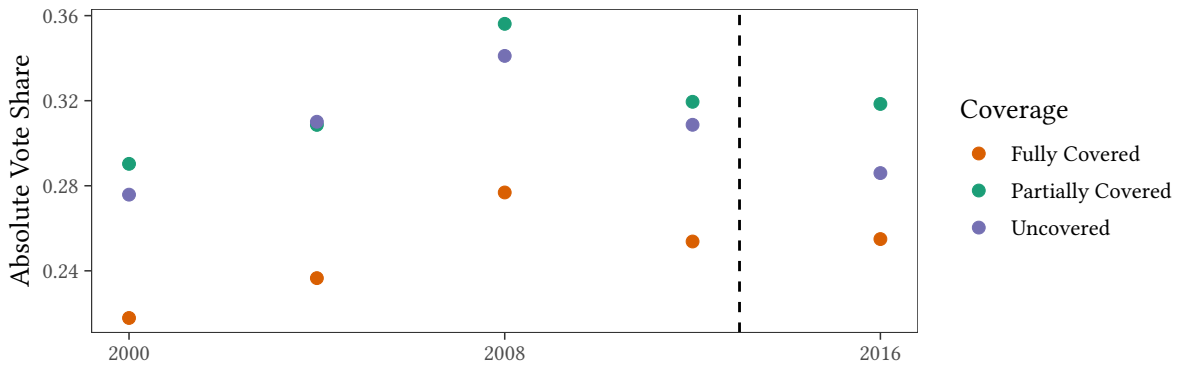
Notes: Kernel density estimates of county racial composition in 2012, weighted by the number of voting-age citizens. *Sources:* US Census Bureau and the National Center for Health Statistics.

Figure 3: Voter Turnout Rates



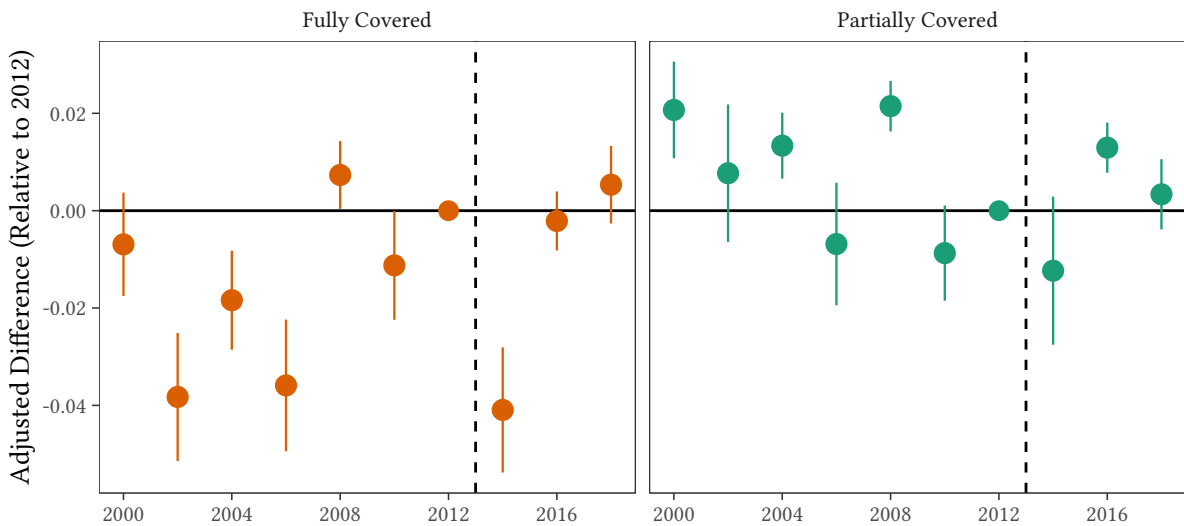
Notes: Panel A plots turnout in ten federal elections as a fraction of voting-age citizens. Panel B plots self-reported turnout by race from two national election surveys. Dashed lines mark the year preclearance ended. *Sources:* Dave Leip's Atlas of US Presidential Elections, Integrated Public Use Microdata Series (Current Population Survey), Cooperative Congressional Election Study, US Census Bureau, and the National Center for Health Statistics.

Figure 4: Democratic Vote Share



Notes: Votes for the Democratic presidential candidate in the general election as a fraction of voting-age citizens. The dashed line marks the year preclearance ended. *Sources:* MIT Election Data and Science Lab, US Census Bureau, and the National Center for Health Statistics.

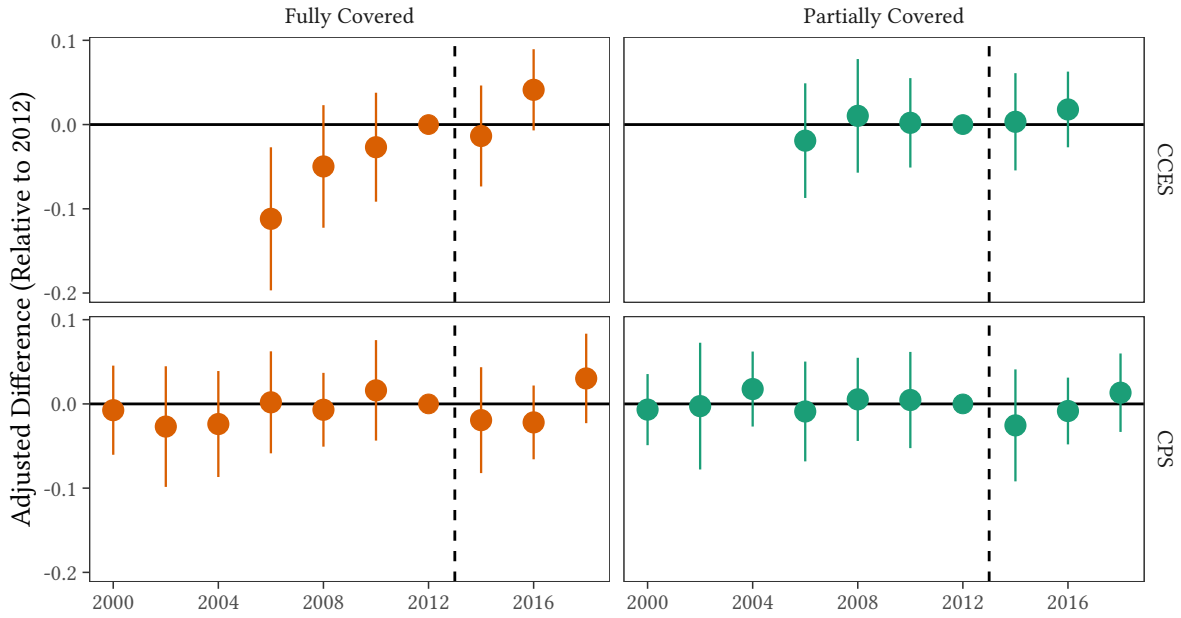
Figure 5: Effect of Preclearance Removal on Voter Turnout—Event Study



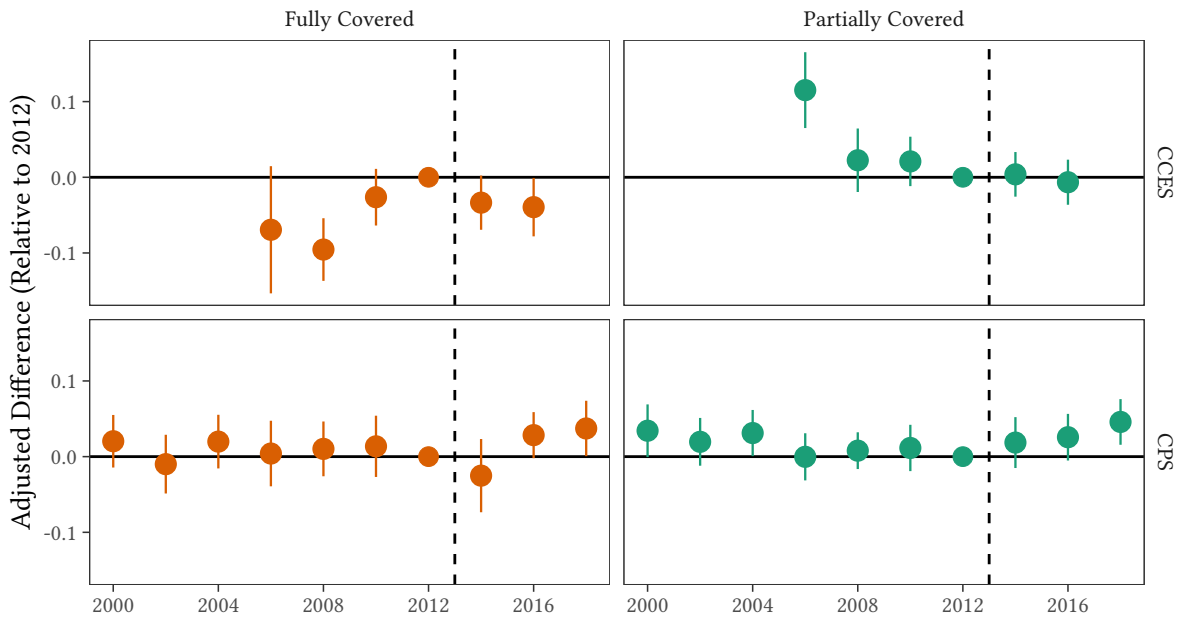
Notes: OLS estimates from an event study based on Equation 1. The dashed line marks the year preclearance ended. An observation is a county in a federal election year. The turnout rate is the number of ballots cast divided by the number of voting-age citizens. Controls include county demographic composition (age, race, sex), educational attainment rates, poverty rates, median income, and indicators for the presence of senatorial and gubernatorial races. The regression includes county and census-division-by-year fixed effects and is weighted by the number of voting-age citizens. Vertical bars outline 95 percent confidence intervals based on standard errors that allow for clustering at the county level.

Figure 6: Effect of Preclearance Removal on Voter Turnout by Race—Event Study

Panel A: Nonwhite Turnout

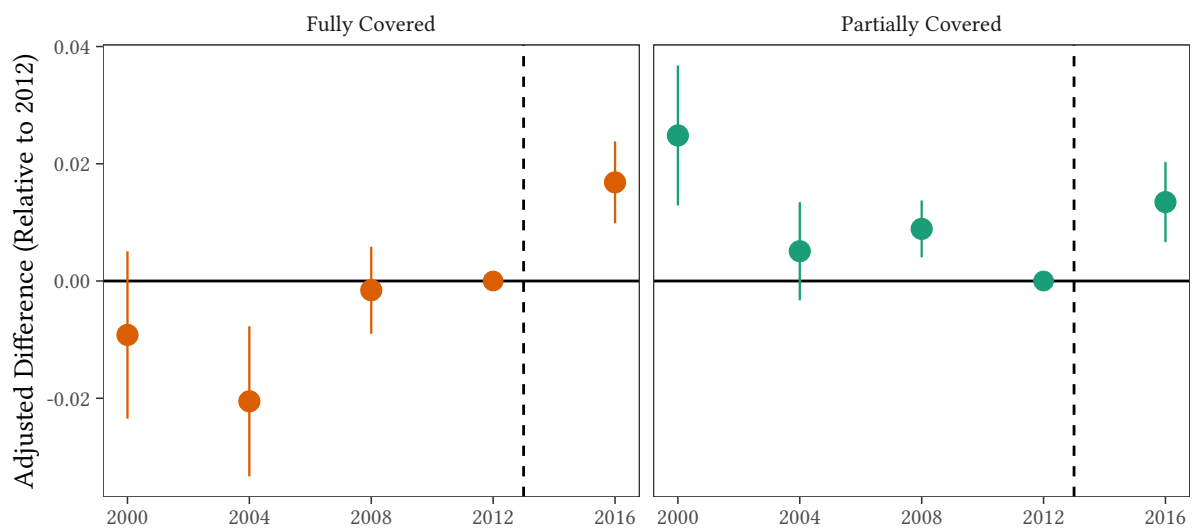


Panel B: White Turnout



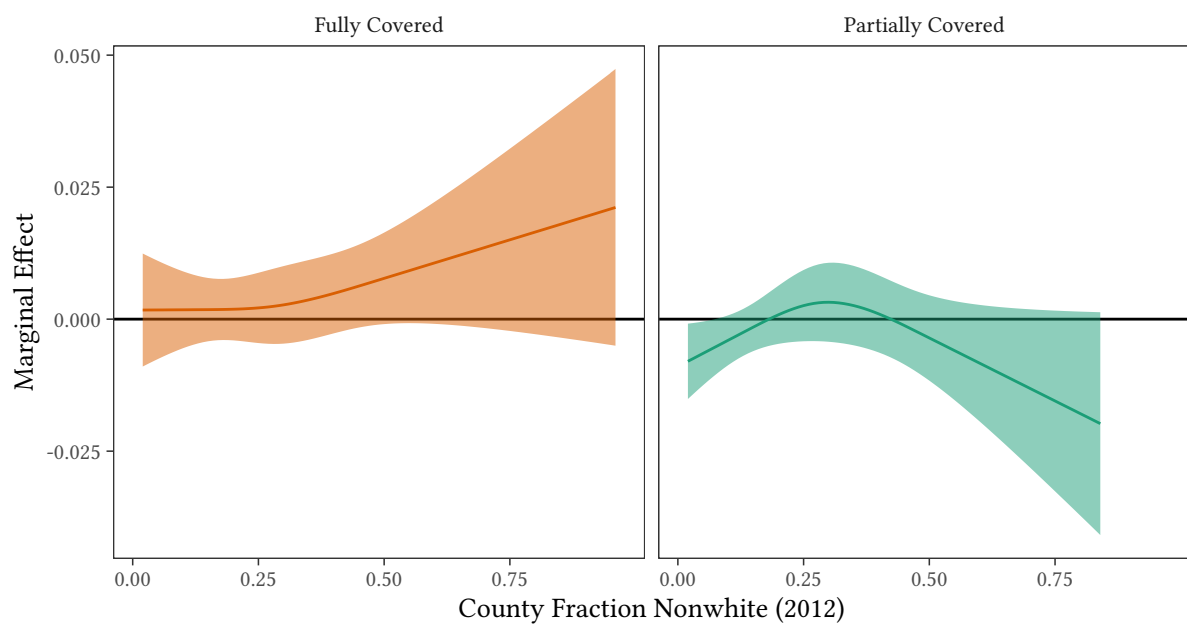
Notes: OLS estimates from event studies based on Equation 2. The dashed line marks the year preclearance ended. An observation is a respondent in a federal election year. The outcome is an indicator equal to one if the respondent voted and zero if otherwise. Controls include respondent characteristics (age, sex, income, educational attainment, and interview characteristics) and indicators for the presence of senatorial and gubernatorial races. All regressions include county and census-division-by-year fixed effects and are weighted using recommended survey weights. Vertical bars outline 95 percent confidence intervals based on standard errors that allow for clustering at the county level.

Figure 7: Effect of Preclearance Removal on Democratic Vote Share—Event Study



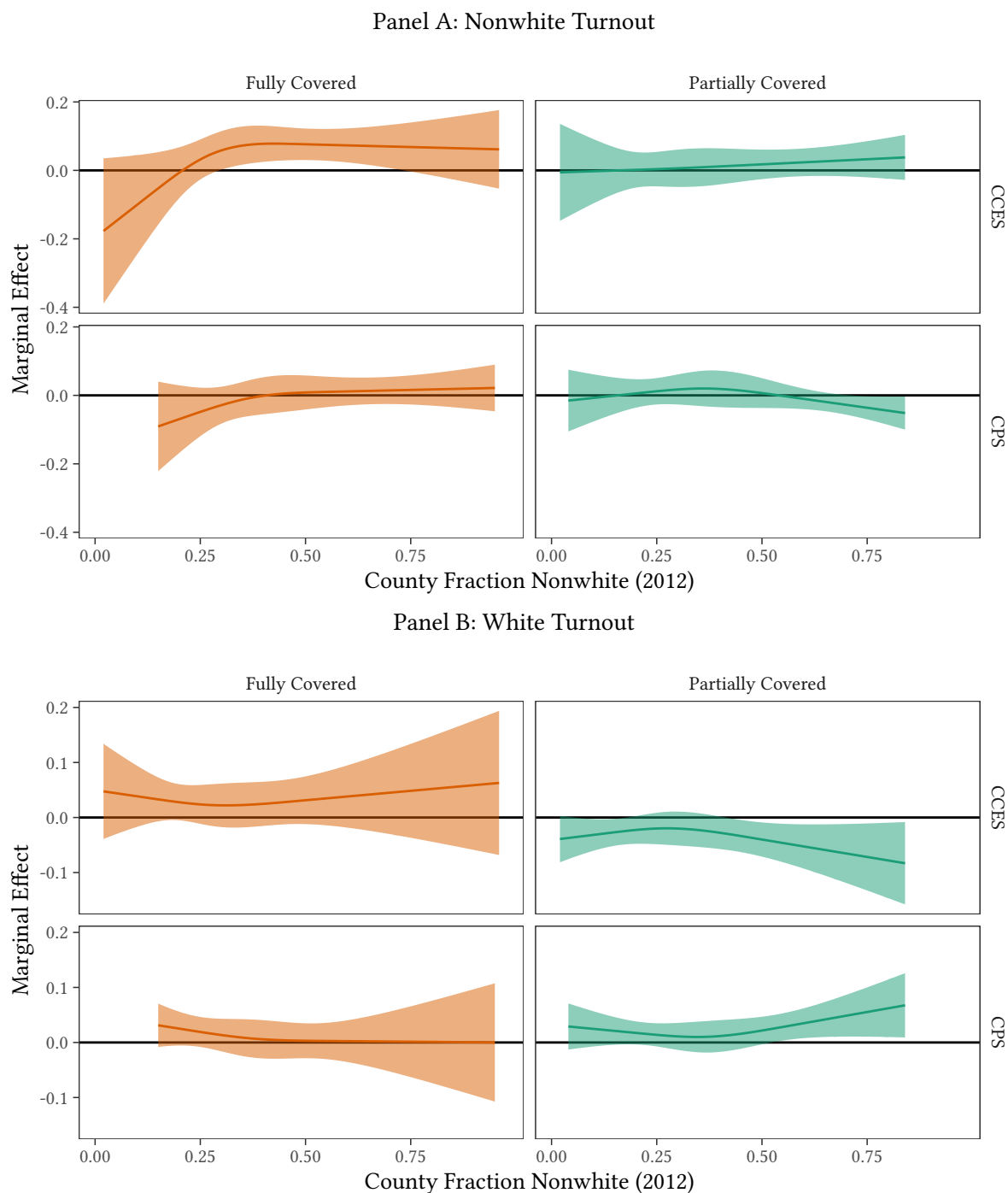
Notes: OLS estimates from an event study based on Equation 1. The dashed line marks the year preclearance ended. An observation is a county in a federal election year. Democratic vote share is the number of votes for the democratic presidential candidate divided by the number of voting-age citizens. Controls include county demographic composition (age, race, sex), educational attainment rates, poverty rates, median income, and indicators for the presence of senatorial and gubernatorial races. The regression includes county and census-division-by-year fixed effects and is weighted by the number of voting-age citizens. Vertical bars outline 95 percent confidence intervals based on standard errors that allow for clustering at the county level.

Figure 8: Heterogeneous Effects of Preclearance Removal on on Voter Turnout by Racial Composition



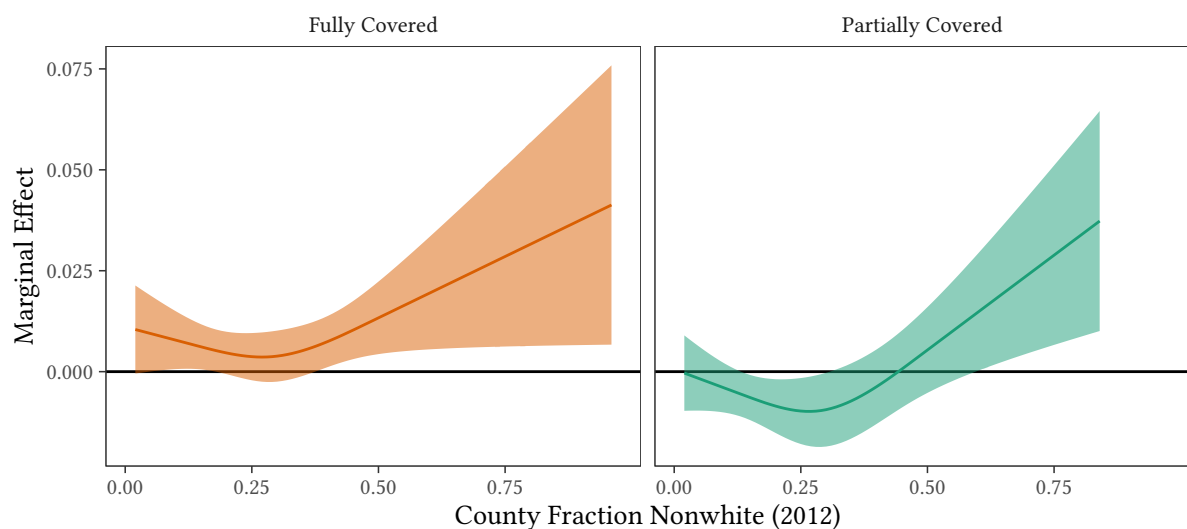
Notes: Marginal effects from a restricted-cubic-spline-interacted difference-in-differences regression based on Equation 1. The spline includes three knots placed at population-weighted quartiles of county fraction nonwhite. An observation is a county in a federal election year. The turnout rate is the number of ballots cast divided by the number of voting-age citizens. Controls include county demographic composition (age, race, sex), educational attainment rates, poverty rates, median income, and indicators for the presence of senatorial and gubernatorial races. The regression includes county and census-division-by-year fixed effects and is weighted by the number of voting-age citizens. Shaded areas outline 95 percent confidence intervals based on standard errors that allow for clustering at the county level.

Figure 9: Heterogeneous Effects of Preclearance Removal on Voter Turnout by Respondent Race and County Racial Composition



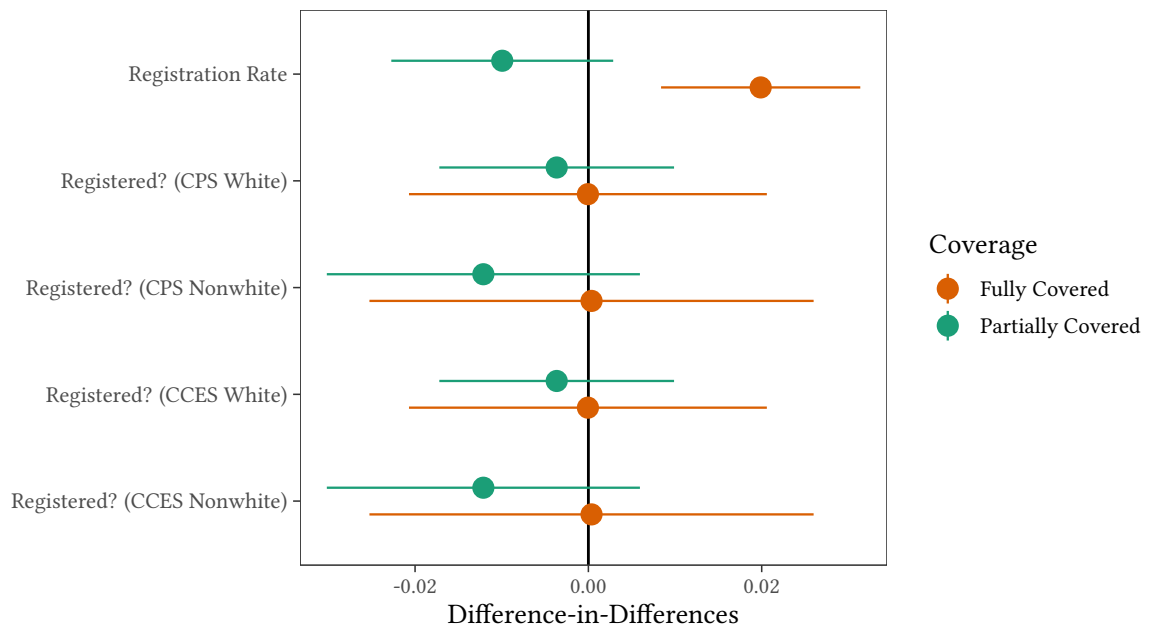
Notes: Marginal effects from restricted-cubic-spline-interacted difference-in-differences regressions based on Equation 2. The spline includes three knots placed at survey-sample-weighted quartiles of county fraction nonwhite. An observation is a respondent in a federal election year. The outcome is an indicator equal to one if the respondent voted and zero if otherwise. Controls include respondent characteristics (age, sex, income, educational attainment, and interview characteristics) and indicators for the presence of senatorial and gubernatorial races. Regressions include county and census-division-by-year fixed effects and are weighted using recommended survey weights. Shaded areas outline 95 percent confidence intervals based on standard errors that allow for clustering at the county level.

Figure 10: Heterogeneous Effects of Preclearance Removal on Democratic Vote Share by Racial Composition



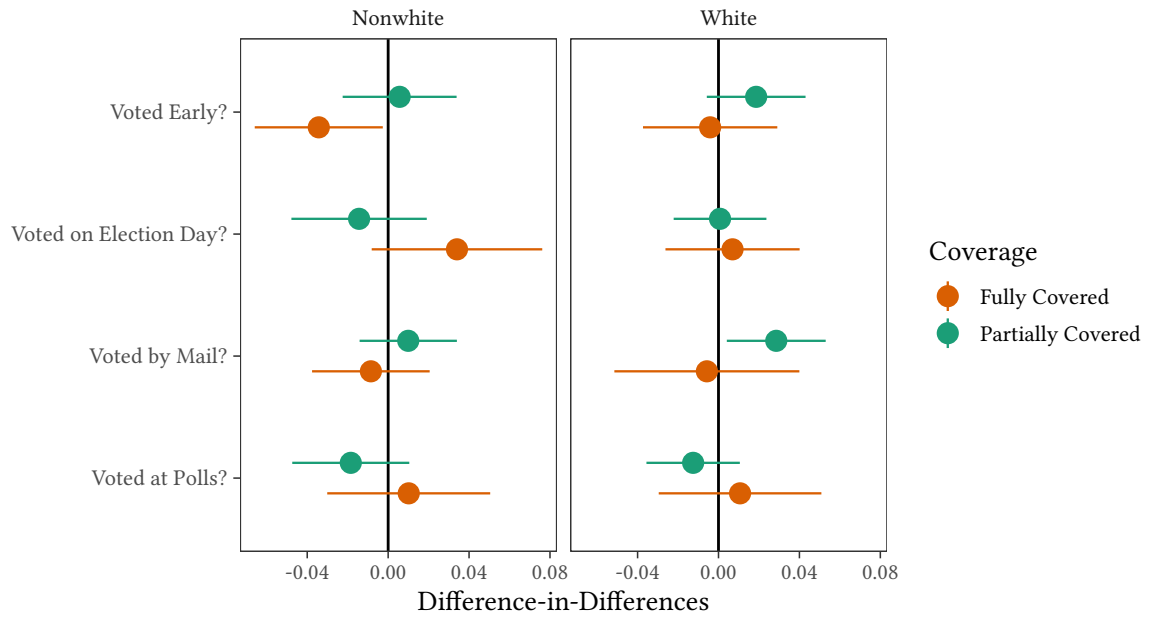
Notes: Marginal effects from a restricted-cubic-spline-interacted difference-in-differences regression based on Equation 1. The spline includes three knots placed at population-weighted quartiles of county fraction nonwhite. An observation is a county in a federal election year. Democratic vote share is the number of votes for the democratic presidential candidate divided by the number of voting-age citizens. Controls include county demographic composition (age, race, sex), educational attainment rates, poverty rates, median income, and indicators for the presence of senatorial and gubernatorial races. The regression includes county and census-division-by-year fixed effects and is weighted by the number of voting-age citizens. Shaded areas outline 95 percent confidence intervals based on standard errors that allow for clustering at the county level.

Figure 11: Effect of Preclearance Removal on Voter Registration



Notes: OLS estimates from difference-in-differences regressions based on [Equation 1](#) and [Equation 2](#). The registration rate is the number of ballots cast divided by the number of voting-age citizens. The survey outcomes are indicators equal to one if the respondent is registered to vote. The registration rate regression includes controls include county demographic composition (age, race, sex), educational attainment rates, poverty rates, and median income, and is weighted by the number of voting-age citizens. The self-reported registration regressions include controls for respondent characteristics (age, sex, income, educational attainment, and interview characteristics) and are weighted using recommended survey weights. All regressions include indicators for the presence of senatorial and gubernatorial races, county fixed effects, and census-division-by-year fixed effects. Horizontal bars outline 95 percent confidence intervals that allow for clustering at the county level.

Figure 12: Effect of Preclearance Removal on the Timing and Mode of Voting



Notes: OLS estimates from difference-in-differences regressions based on Equation 2. The analysis sample includes CPS respondents with county identifiers. *Voted Early?* is an indicator equal to one if the respondent voted before election day or zero if the respondent voted on election day or did not vote. *Voted on Election Day?* is an indicator equal to one if the respondent voted on election day or zero if the respondent voted early or did not vote. *Voted by Mail?* is an indicator equal to one if the respondent voted by mail or zero if the respondent voted at the polls or did not vote. *Voted at Polls?* is an indicator equal to one if the respondent voted in person or zero if the respondent voted by mail or did not vote. The survey outcomes are indicators equal to one if the respondent is registered to vote. All regressions include respondent characteristics (age, sex, income, educational attainment, and interview characteristics), indicators for the presence of senatorial and gubernatorial races, county fixed effects, and census-division-by-year fixed effects. All regressions are weighted using recommended survey weights. Horizontal bars outline 95 percent confidence intervals that allow for clustering at the county level.

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