

Does Federal Preclearance Make a Difference?
Examining the Effects of *Shelby* on the Minority Voting Gap and Countermobilization*

Brendan Cirillo
University of Colorado

Jeffrey Milyo
University of Missouri

Abstract:

In *Shelby County v. Holder* (2013), the Supreme Court suspended federal preclearance restrictions on changes to voting laws in several states with a history of racial or ethnic discrimination and disparities in voting. We analyze the impact of removing federal preclearance of state election reforms on minority voter turnout by estimating the triple difference in minority versus non-minority turnout in affected and unaffected states before and after *Shelby*. We employ data from the 2008-2024 Cooperative Election Study, which includes both validated voter turnout and registration, as well as several measures of political mobilization. We find no evidence that the end of federal preclearance has had detrimental effects on minority voter turnout or registration. We also show that revised federal preclearance procedures in the proposed John Lewis Voting Rights Advancement Act are not better targeted at states where minority voting retrogressed after *Shelby*. Finally, we do not find support for the hypothesis that countermobilization among minority voters mitigates or confounds what would otherwise have been an increase in the minority voting gap post-*Shelby*.

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Throwing out preclearance when it has worked and is continuing to work to stop discriminatory changes is like throwing away your umbrella in a rainstorm because you are not getting wet.

--- Justice Ruth Bader Ginsburg, dissenting in *Shelby County v. Holder* (2013)¹

1. Introduction:

The 2013 United States Supreme Court decision in *Shelby County v. Holder* struck down the criteria used to subject state and local jurisdictions to federal preclearance of changes in voting laws. Several states and counties, mostly but not exclusively in the South, had been subject to preclearance (or “covered”) since the passage of the original Voting Rights Act (VRA) in 1965, or at least since major amendments to the VRA in 1975. And in the decade preceding *Shelby*, the Department of Justice (DOJ) actively exercised its oversight authority, reviewing between 14,000 and 20,000 changes to state and local voting procedures annually.²

Preclearance essentially flipped the burden of proof for determining whether reforms to election administration have a discriminatory intent or effect; covered jurisdictions were required to seek prior approval from the DOJ or a three-judge panel of the United States District Court for the District of Columbia before implementing any voting reforms. However, the Supreme Court did not find in *Shelby* that federal preclearance was itself unconstitutional, but rather that the existing formula for identifying covered jurisdictions was based on overly distant historical circumstances that may not target problematic jurisdictions in the present. The recently proposed John Lewis Voting Rights Advancement Act (VRAA) accordingly includes a revised coverage formula for identifying jurisdictions subject to federal preclearance. We therefore analyze both the effects of suspending preclearance on the minority voting gap in formerly covered states, and the implications of restored preclearance coverage under the proposed John Lewis VRAA.

In the wake of *Shelby*, a wave of voting reforms championed mainly by Republican officials in Republican-controlled (or “red”) states, from voter identification laws to restrictive rules for early and absentee voting, has been proposed or implemented in previously covered jurisdictions (e.g., see Lopez 2014 and Morris 2022). These reform efforts have been denounced as a resurgence of racially motivated “voter suppression”³ or “Jim Crow 2.0”⁴; and the *Shelby* decision has been characterized as having “gutted the Voting Rights Act” (Berman 2021), and as having “broke[n] America” (Newkirk 2018).⁵ Yet, despite intense rhetoric from many politicians, advocates, and media commentators, there has been until very recently little scientific evidence on the causal impact (or “treatment effect”) of the *Shelby*

¹ *Shelby County v. Holder*, 570 U.S. 529, 590 (2013) (Ginsburg, J., dissenting).

² “About Section 5 of the Voting Rights Act,” U.S. Department of Justice webpage (<https://www.justice.gov/crt/about-section-5-voting-rights-act>, viewed August 20, 2025).

³ “Voter Suppression,” The Brennan Center webpage (<https://www.brennancenter.org/topics/voting-elections/voter-suppression>, viewed June 7, 2026).

⁴ “Remarks by President Biden on Protecting the Right to Vote,” January 11, 2022 (<https://bidenwhitehouse.archives.gov/briefing-room/speeches-remarks/2022/01/11/remarks-by-president-biden-on-protecting-the-right-to-vote/>, viewed June 7, 2026).

⁵ Also see commentary from the ACLU (Lakin and McGrath 2023); the Brennan Center for Justice (Perez 2020; and Singh and Carter 2023); the Center for Public Integrity (Mendelson 2023); Common Cause (“How Voting Laws Have Changed...” 2025); the League of Women Voters (“Fighting Voter Suppression,” n.d.); and the Legal Defense Fund (“How *Shelby County v. Holder* Broke Democracy,” n.d.).

decision on minority voting (Raze 2022; Billings et al. 2024; Komisarchik and White 2025; and Morris and Miller 2025).⁶

Of course, there is a substantial scholarly literature on the historical importance of the VRA for improvements in minority (especially Black) political participation, representation, and political influence (e.g., Husted and Kenny 1997; Grofman and Handley 1998; Marschall et al. 2010; Cascio and Washington 2014; Schuit and Rogowski 2017; Ang 2019; and Bernini et al. 2023). And while the gap between minority and non-minority voting rates has narrowed dramatically since the passage of the VRA (Grofman et al. 1992), a significant difference remains today (Morris and Grange 2024).

For example, verified voting data from the Cooperative Election Study (CES) show that turnout among minority voting-age citizens has averaged just 43% since 2008, compared with 60% for non-minority voting-age citizens. Demographic differences contribute to this 17 percentage-point gap, but even after controlling for age, education, and sex, a difference of 9 percentage points remains (authors' calculations). In addition, improvement in the minority voting gap has stalled in recent years: apart from two blips corresponding to Barack Obama's historic presidential campaigns, the gap has remained stubbornly large (see Figure 1). Moreover, a persistent voting gap is observed across subgroups of minority voters, so the overall minority gap is not driven solely by Black or Hispanic voting patterns (see Figures A1 and A2). However, what is not clear from these figures is whether Justice Ginsburg's umbrella was indeed "working"; that determination is the focus of this study.

The *Shelby* decision in 2013 provides a natural experiment of sorts for testing whether federal preclearance worked to stymie state practices that would otherwise exacerbate the minority voting gap. Removal of preclearance not only allowed states to adopt new laws but also to resume previously blocked practices, so that prior to the 2014 general election, a dozen formerly covered states had implemented what were decried in several quarters as "restrictive voting laws" (e.g., Lopez 2014).⁷ Apart from the direct impact of removing preclearance on legislative reforms, Komisarchik and White (2025) note that *Shelby* may carry symbolic weight, emboldening election administrators in their exercise of potentially discriminatory discretion (e.g., interactions with citizens, training of poll workers, or dissemination of information about registration and voting). Consequently, the treatment effect of *Shelby* may not be realized only through explicit changes in legislation but also through diffuse and indirect channels; the full impact might not manifest immediately but may emerge only gradually.

Accordingly, we examine the intent-to-treat effect, where the focus is on the opportunity for states to alter election procedures without federal preclearance. At issue, then, is not whether states reform election practices, but whether the ability to do so without federal oversight has an observable impact

⁶ In contrast, studies of specific state voting reforms, such as voter identification laws, have received more attention. Early findings were mixed, owing in part to methodological flaws (see Highton 2017; and Grimmer et al. 2018); more recent comprehensive and methodologically sophisticated studies find that strict voter identification requirements do not suppress minority turnout (e.g., Cantoni and Pons 2021; Hoekstra and Koppa 2022; Grimmer and Hersh 2024).

⁷ Also see Feder and Miller (2020), who report increased purges of voter rolls in previously covered jurisdictions immediately post-*Shelby*; however, Komisarchik and White (2025) find no significant evidence of changes to several measures of state election administration (including voter purge rates once standard errors are clustered appropriately) in formerly covered jurisdictions, with the exception of an increased probability of passing photo identification requirements in states formerly explicitly covered (see the discussion below for the distinction between explicitly and implicitly covered states).

on the minority voting gap over the six ensuing election cycles. We employ triple difference models to identify the treatment effect of removing preclearance.⁸ In doing so, we follow several recent studies that take similar approaches but yield very different results: Raze (2022) finds evidence of an *increase* in relative Black turnout post-*Shelby*; Billings et al. (2024) and Morris and Miller (2025) report the opposite; and Komisarchik and White (2025) find negligible effects of *Shelby* on minority turnout. These studies employ different data sources, units of observation, and even different conceptualizations of the treated groups; however, all but Morris and Miller (2025) also emphasize the potential confounding effect of countermobilization among minority voters in response to perceived attacks on voting rights post-*Shelby*. Consequently, we also test whether *Shelby* generated an increase in political mobilization among minority voters relative to non-minority voters.

Our analysis is most similar to Raze (2022) in that we examine individual-level turnout using nationwide survey data matched to state voter files and we conceptualize the relevant treatment effect at the state level. However, unlike earlier studies that focus on Black-White voting gaps (and to a lesser extent, Hispanic-White), we examine changes in the overall minority voting gap, as well as the disaggregated gap for Black, Hispanic, and other minority voters. We also have an advantage over earlier studies in that we can incorporate additional election cycles post-*Shelby*. And we include a more comprehensive set of relevant individual-level controls, including party identification; the latter is particularly important, since previous analyses of the impact of *Shelby* on minority turnout do not control for the influence of partisan electoral tides in their main specifications. In addition, we test whether the effects of removing preclearance are concentrated in Republican-controlled states and whether the “fix” to Section 4 in the proposed John Lewis VRAA is better targeted than the pre-*Shelby* coverage formula.

Overall, we find no significant detrimental effect on minority voting or registration from the removal of federal preclearance of election reforms in previously covered states; nor do we find evidence that the proposed John Lewis VRAA’s coverage formula is better targeted at states where minority voting has retrogressed than the formula struck down in *Shelby*. We also find no support for the contention that increased political mobilization among minority voters counters what would otherwise be a negative effect of *Shelby* on Black and Hispanic voting and registration. Taken together, these findings stand in contrast to dire predictions that followed *Shelby* and cast doubt on claims that federal preclearance of state election laws is necessary to protect minority voter turnout and registration.

2. The Voting Rights Act and Preclearance

President Johnson described the 1965 Voting Rights Act (VRA) as “one of the most monumental laws in the entire history of American freedom.”⁹ More recently, the VRA has been described as “possibly the most consequential federal law in our nation’s history” (Consovoy and McCarthy 2013), and Gerken (2013) calls Section 5 of the VRA the “crown jewel” of the Civil Rights Movement. Section 5 established a preclearance procedure whereby states and counties with a history of discriminatory voting practices were required to get approval from federal authorities before implementing changes to voting laws.

⁸ The *Shelby* decision affects all treated states simultaneously, so does not raise the same methodological concerns associated with identifying staggered treatment effects (e.g., Baker et al. 2022); see also Komisarchik and White (2025) on this point.

⁹ Quoted by Assistant Attorney General Kristen Clarke (2023) in “Reflecting on the 10th Anniversary of *Shelby County v. Holder*” (<https://www.justice.gov/archives/opa/blog/reflecting-10th-anniversary-shelby-county-v-holder>; last viewed June 1, 2025).

Section 4 of the VRA established the criteria for determining which jurisdictions would be covered by the preclearance requirements in Section 5. A jurisdiction was covered if it maintained a “test or device” restricting the opportunity to register or vote as of November 1, 1964, and either had registration rates below 50% as of that date or voter turnout rates under 50% in the November 1964 presidential election.¹⁰

The special provisions of Sections 4 and 5 of the VRA were originally set to expire after 5 years, but Congress extended them for another 5 years in 1970, adding a parallel coverage formula benchmarked to 1968. Sections 4 and 5 were extended again in 1975, this time for 7 years; these amendments added protections for language minorities and the coverage formula was again expanded to include jurisdictions with low registration and voting rates as of November 1972. The application of Section 5 preclearance was then extended for 25 years in 1982, and for another 25 years in 2006, although the coverage formula in Section 4 was never updated after 1975.¹¹

In 2013, the Supreme Court ruled in *Shelby* that the criteria for identifying covered jurisdictions in Section 4 of the VRA were unconstitutional.¹² Preclearance was an extraordinary and ostensibly temporary exercise of federal authority that flipped the burden of proof for adjudicating whether an election reform constituted illegal discrimination (albeit only in covered jurisdictions, a departure from the principle of equal sovereignty). The majority in *Shelby* determined that such an extraordinary measure must be justified by current conditions, and that a coverage formula based on decades-old benchmarks was not sufficient to provide that justification.

As shown in Figure 2, at the time of the *Shelby* decision, nine states were explicitly “covered as a whole” (Alabama, Alaska, Arizona, Georgia, Louisiana, Mississippi, South Carolina, Texas, and Virginia). This meant that changes in election procedures in those states or any local jurisdictions in those states would be subject to federal preclearance. In addition, after the 1999 Supreme Court decision in *Lopez v. Monterey County*, another six states (California, Florida, Michigan, New York, North Carolina, and South Dakota) were “implicitly covered” because at least one of their local jurisdictions was covered.¹³ The Court ruled in *Lopez* that changes to state election law could not be implemented in covered jurisdictions until precleared; in effect, statewide election reforms in implicitly covered states were therefore also subject to federal preclearance (Katz 2001).

This means that the 2013 *Shelby* decision had a broader scope than has been recognized in several studies that ignore implicit state coverage after the 1999 decision in *Lopez* (e.g., Ang 2019; Billings et al. 2024; and Komisarchik and White 2025). Moreover, based on recent population estimates, about 22% of the U.S. population (and 28% of the minority population) reside in states that were explicitly “covered as a whole,” while about 53% of the U.S. population (and 65% of the minority population) reside in states

¹⁰ “About Section 5 of the Voting Rights Act,” U.S. Department of Justice webpage (<https://www.justice.gov/crt/about-section-5-voting-rights-act>; viewed August 20, 2025).

¹¹ This discussion of the history of preclearance follows the account in Consovoy and McCarthy (2013) and “About Section 5 of the Voting Rights Act,” U.S. Department of Justice (cited above).

¹² Chief Justice Roberts noted that in the 2006 extension of preclearance, “Congress did not use the record it compiled to shape a coverage formula grounded in current conditions. It instead reenacted a formula based on 40-year-old facts having no logical relation to the present day.” *Shelby County*, 570 U.S. at 554.

¹³ *Lopez v. Monterey County*, 525 U.S. 266 (1999).

that formerly were covered in any way by the VRA.¹⁴ Consequently, it is important for evaluations of preclearance to acknowledge the existence of implicitly covered states (Raze 2022; Malmberg 2025; and Morris and Miller 2025).¹⁵

By striking the coverage formula in Section 4, but leaving the preclearance process in Section 5 intact, the Court effectively paused federal preclearance, leaving the door open for Congress to propose a new coverage formula. Subsequent efforts to “fix” Section 4 and restore federal preclearance have been hotly contested partisan scuffles. In both 2021 and 2022, the Democratic-controlled U.S. House passed the John Lewis Voting Rights Advancement Act, which would reestablish federal preclearance requirements in newly defined covered jurisdictions, but the bill was deemed “unnecessary” by Senate Minority Leader Mitch McConnell (R-KY), who successfully led filibusters to block passage of the bill in the Senate.¹⁶ In 2022, with the support of President Biden, Senate Democrats tried to use the “nuclear option” and suspend the filibuster for the purpose of passing voting rights reforms, but this effort was stymied by the defections of Senators Manchin (D-WV) and Sinema (D-AZ).¹⁷ While restoration of federal preclearance is unlikely under President Trump, the close margins of party control in Congress and the visceral nature of the public debate over alleged suppression of minority voters all but assure continued calls to pass the John Lewis Voting Rights Advancement Act.

3. Recent Estimates of the Effects of Preclearance on the Minority Voting Gap

Ang (2019) presents evidence that federal preclearance constrained attempts by state and local election officials to change voting laws in covered jurisdictions prior to *Shelby* and examines difference-in-difference models of aggregate county-level voter turnout in presidential elections from 1960-2016 for counties in states not covered as a whole prior to 1975 (thereby omitting most of the explicitly covered states shown in Figure 2).¹⁸ Starting in 1976, he finds a steady increase in turnout in covered versus uncovered counties through the mid-1990s, after which the difference levels off, persisting at 4-8 percentage points through 2016. To test whether preclearance affected turnout by race, Ang (2019) aggregates individual-level self-reported turnout from the Current Population Survey (CPS) to construct state-level turnout rates for the White and non-White voting-age population from 1968-2016. Separate difference-in-difference models show a significant increase in state-level non-White turnout after the 1975 amendments to the VRA, but no such increase for White turnout.

¹⁴ United States Census Bureau, “State Population Characteristics: 2020-2024,” available at: <https://www.census.gov/data/datasets/time-series/demo/popest/2020s-state-detail.html>; viewed June 1, 2025.

¹⁵ These authors refer to what we call “implicitly covered” states as “partially covered”; however, that designation may obscure the key point: state-level election changes in these states were fully subject to preclearance, not merely changes in their covered counties.

¹⁶ Hutzler, Alexandra. 2021. “Mitch McConnell Comes Out Against ‘Unnecessary’ John Lewis Voting Rights Act,” *Newsweek* (<https://www.newsweek.com/mitch-mcconnell-comes-out-against-unnecessary-john-lewis-voting-rights-act-1598741>; viewed August 20, 2025).

¹⁷ Mascaro, Lisa. 2022. “Voting Bill Collapses, Democrats Unable to Change Filibuster,” AP News (<https://apnews.com/article/biden-voting-rights-bill-collapses-27c888b4f9bf876520913d7036a942b0>; viewed June 1, 2025).

¹⁸ This approach is similar in spirit to comparing newly treated counties to never-treated counties; however, it also embeds an assumption that implicit state coverage had no effect, since any county not explicitly covered is assigned to the control group. Omitted from the analysis are Alaska, six states explicitly covered prior to 1975 (Alabama, Georgia, Louisiana, Mississippi, South Carolina, and Virginia), and localities covered prior to 1975; Ang (2019) reports that including these areas yields consistent results.

To explore the possible impact of *Shelby*, Ang (2019) conducts a supplementary event study over presidential elections in which the treatment group is expanded to all counties freed from preclearance. This exercise shows a relative decline in turnout of 1.5 percentage points in previously covered counties in the 2016 election (compared to 2012), although the author concedes that this is not a causal analysis. Because whole states now enter the treatment group, the event study specification substitutes division-year effects for state-year effects, leaving time-varying state shocks uncontrolled. Further, given only one post-*Shelby* election observation, this specification cannot distinguish an effect of *Shelby* from an idiosyncratic shock attributable to Barack Obama not being on the ballot in 2016. Finally, the assumption embedded in the main analysis, that implicit state coverage has no effect, carries over to this event study, since uncovered counties in implicitly covered states remain in the control group.

Ang (2019) argues that these findings suggest a potential negative impact of *Shelby* on minority political participation; however, several additional caveats are in order with respect to this conjecture. First, the estimated impact of the 1975 VRA amendments levels off and remains essentially unchanged for two decades. This is consistent with Senator McConnell's assertion that fixes to the VRA are unnecessary, Justice Ginsburg's umbrella notwithstanding. Second, Ang (2019) does not analyze individual-level CPS data, due to concerns with the reliability of early CPS micro-data below the state level. Third, the reliance on CPS self-reported voting to construct race-specific state-level turnout is unfortunate, as it is well-known that survey respondents in general misreport their voting behavior, and that over-reporting rates vary by race (e.g., Ansolabehere and Hersh 2012; Ansolabehere et al. 2022).

Raze (2022) conducts the first comprehensive evaluation study of the removal of federal preclearance by examining the triple difference in (non-Hispanic) Black versus White turnout, in (fully or partially) covered versus uncovered states, both before and after *Shelby*.¹⁹ He employs validated voter data from the Cooperative Election Study (CES); the CES is a large national survey whose respondents are matched to voter file records in each election year since 2008. This approach permits Raze (2022) to analyze *individual-level* voting and registration records from 2008-2018, which in turn allows him to control for all "lower-order interactions" in his triple difference regressions (i.e., race $\times$ state; race $\times$ year; and state $\times$ year). This specification cleanly isolates the treatment effect of *Shelby*, which in this case is a statistically significant and large *increase* in relative Black voter turnout after the removal of preclearance.

To explain this counterintuitive result, Raze (2022) conjectures that effective countermobilization efforts swamp what might otherwise be a depressing effect of *Shelby* on Black turnout. But Grimmer and Hersh (2024) argue that the estimated increase in relative Black turnout is implausibly large (roughly 4 percentage points on average, and 6-8 percentage points in 2016) given existing estimates of the efficacy of countermobilization (also see Green and Gerber 2023). Our analysis of *Shelby* follows the approach in Raze (2022) in many respects, but we do not find such large effects on Black or minority turnout after including additional election years and individual-level controls beyond just age, education, and sex. We also explicitly test the countermobilization conjecture, given it looms so large in interpreting the estimated treatment effect of *Shelby*.

¹⁹ Gibson (2020) analyzes the effects of *Shelby* on county-level turnout in North Carolina and finds no negative effects on turnout in formerly covered versus uncovered counties in that state. However, because North Carolina's statewide election laws were themselves subject to preclearance, such within-state comparisons cannot capture the effects of removing federal oversight of state legislation.

Billings et al. (2024) also employ administrative data from state voter files to test the treatment effect of *Shelby*; but rather than using validated survey data, the authors construct block-level turnout rates *conditional on pre-2013 registration* and analyze changes in this measure across census blocks with varying shares of Black and Hispanic population, in covered and uncovered counties, before and after *Shelby* (2006-2018). They find that blocks in the top quartile of Black or Hispanic population share (which average just over 50% Black or Hispanic population) experienced a decline in voter turnout among registered voters of about 0.9 percentage points post-*Shelby*. Billings et al. (2024) caution that inferring a decrease in individual minority turnout from these block-level results risks the ecological fallacy, but several other caveats are in order, as well.

First, the data are a snapshot of registration rolls as of late 2020. Individuals removed from the rolls before then, whether due to moving, non-voting, or purging, are unobservable, and list removal is also an administrative procedure subject to federal oversight prior to *Shelby*. In addition, most states do not record race or ethnicity in voter files. Rather than rely on the imputed values provided by their data vendor (L2), which the authors use only in supplementary individual-level analyses, the main analysis classifies blocks by their 2010 Census racial composition. The cost of this choice is that race enters the analysis only as neighborhood composition, which is the source of the ecological inference problem the authors acknowledge. Moreover, because turnout is measured conditional on registration, and indeed conditional on registration prior to 2013, any effect of *Shelby* operating through registration itself, including restrictions on registration drives or same-day registration, is excluded by construction. For this reason, evaluations of the effects of state voting reforms on disparities in turnout typically measure the turnout gap as a share of voting-age or voting-eligible population (e.g., Fraga 2018).

Second, these 2010 block characteristics are assumed fixed throughout the 2006–2018 panel, which is a concern if neighborhood composition changes at different rates across groups or geographies. Further, Billings et al. (2024) define treatment intensity only by Black or Hispanic shares, so blocks with substantial populations of other minority groups are sorted into comparison categories, absorbing any differential effects of *Shelby* on those voters. Finally, block-level counts for small populations are noisy, and minority counts in small geographies are especially vulnerable to errors introduced by the Census Bureau's privacy-protection algorithms (Kenny et al. 2024).

Third, like Ang (2019), the authors define treatment by explicit coverage alone and ignore implicit state coverage. This restriction is particularly consequential because Billings et al. (2024) analyze census blocks only in explicitly covered states and their bordering states. The treatment group comprises counties in explicitly covered states and explicitly covered counties in implicitly covered states; all other counties, including those in implicitly covered states like California and Florida (whose statewide election laws were themselves subject to preclearance), are classified as part of their control group. Consequently, unlike Raze (2022), the analysis in Billings et al. (2024) does not cleanly identify the triple difference of interest.

Komisarchik and White (2025) also evaluate the treatment effect of *Shelby* on minority voting by examining repeated cross-sections of state voter files. The authors construct county-level differences in registration and turnout rates (for Black versus White, and Hispanic versus White) by dividing race-specific county-level counts of registrants and voters (obtained from Catalist) by Census Bureau five-year estimates of the county-level citizen voting-age population (CVAP), for each federal election year from

2008-2020. For states that do not record race in voter files (which are mostly states in the control group), they rely on Catalist's imputed race and Hispanic ethnicity for individual observations.

Komisarchik and White (2025) then estimate difference-in-difference models with the county-level Black-White (or Hispanic-White) gap as the dependent variable, and report statistically insignificant and small declines in relative Black registration and turnout (less than 1 percentage point).²⁰ In contrast, specifications that include state $\times$ year fixed effects generate estimates that are statistically significant and positive (1.5 to 3 percentage points). The inclusion of state $\times$ year effects is in general appropriate since statewide elections or presidential battleground contests in a particular election year impact turnout differently across states in that year. However, the authors discount these specifications because the treatment effects in their models are then identified only from within-state variation in the eight partially covered states. This concern underscores the value of treating implicit state coverage as part of the treatment rather than as a source of control units. Komisarchik and White (2025) ultimately attribute their null results to limited state and local policy activity, while speculating that suppressive policies may take longer than four election cycles to develop and manifest.

Finally, Komisarchik and White (2025) thus join Ang (2019) and Billings et al. (2024) in assigning the uncovered counties of implicitly covered states to the control group, despite those states' statewide election laws having been subject to preclearance. In addition, because their dependent variable is itself a racial gap, race is differenced out before estimation: no specification can include the race-interacted fixed effects (race $\times$ state, race $\times$ year) that individual-level data accommodate, and the gap construction instead imposes the assumption that state and year shocks affect Black and White participation identically. Consequently, these authors also do not cleanly identify the triple difference of interest, unlike Raze (2022).

Most recently, Morris and Miller (2025) utilize repeated cross-sections of state voter files to construct county-level measures of the Black-White (and minority) voting gap for the period 2008-2022.²¹ Importantly, these authors do account for implicitly covered states, and their study stands out for its focus on county election administration. Morris and Miller (2025) report that the minority voting gap grew larger in formerly covered counties post-*Shelby*, but they find no significant effect from the removal of preclearance over state election laws (the treatment margin we examine). Instead, their estimated effects are concentrated where Section 5 constrained local practices. Several caveats nonetheless apply.

These authors balance control counties to treated ones on pre-treatment covariates but do not weight by county population;²² because their dependent variable is itself a racial gap, race-interacted fixed

²⁰ Komisarchik and White (2025) also explore the “downstream” impacts of *Shelby* on legislative representation but they find no evidence of a decline in Black elected members of the U.S. House post-*Shelby* (also see Stephanopoulos et al. 2024 on legislative redistricting). Consistent with this, we note that half of the Black (or Hispanic) U.S. Senators elected since 2013 were from formerly covered states.

²¹ Morris and Miller employ voter file data from Catalist for the pre-*Shelby* years and from L2 for post-*Shelby* years; they also impute race and ethnicity independently using a modified Bayesian Improved Surname Geocoding approach (Imai and Khanna 2016). However, this method utilizes race and ethnicity characteristics of Census blocks, which may be problematic given changing data privacy methods employed by the Census (Kenny et al. 2024).

²² Komisarchik and White (2025) attribute the differing findings primarily to Morris and Miller's choice not to weight by county population.

effects are again unavailable; and their specifications omit state $\times$ year effects, leaving time-varying state-specific shocks (statewide elections, controversial ballot measures, presidential battleground status) as potential confounders. Consequently, this study also does not cleanly identify the triple difference of interest.

Setting aside methodological quibbles, the mixed findings across these five studies do not appear to bear out critics' worst fears about *Shelby*'s effect on state election reforms (see also Grimmer and Hersh 2024, and Malmberg 2025). This is consistent with other recent evidence on the effects of specific state election reforms, such as voter identification laws, on minority turnout and registration (e.g., Cantoni and Pons 2021). But the studies reviewed here leave several loose ends, including the treatment of implicit coverage and partisan election tides. Moreover, only Morris and Miller (2025) examine elections after 2018. The question of whether removing preclearance has any detrimental impact on minority voting remains open.

The analysis by Raze (2022) stands out as the only evaluation to date that cleanly identifies the treatment effect of removing federal preclearance of state election practices in a fully specified triple difference model with individual-level observations. However, his analysis examines just three general elections post-*Shelby* and is limited to non-Hispanic Black and white citizens. Our analysis follows his in conceptualizing the treatment of *Shelby* at the state level and analyzing individual-level data in a fully specified triple difference model, but includes twice as many post-*Shelby* general elections and encompasses Hispanic and other minority voters (while omitting non-citizens).

Also of note, none of the previous evaluation studies of *Shelby* includes controls for partisan tides affecting voter turnout across elections (i.e., party $\times$ year) in their main specifications, despite the central importance of partisanship as a motivator of individual turnout decisions and of party competition as a driver of mobilization efforts (e.g., Downs 1957; Fiorina 1976; Aldrich 1993, 1995; and Green and Gerber 2023). As we show below, accounting for individual party identification and partisan electoral tides materially affects estimates of disparities in voter turnout.

Previous evaluation studies have also paid little attention to partisan control of state government.²³ When the *Shelby* decision was announced in 2013, every explicitly covered state was under unified Republican control of state government (both legislative chambers and the governorship),²⁴ as were the implicitly covered states with the exceptions of California and New York. As noted above, many of these Republican-controlled (or "red") states implemented voting reforms in the immediate aftermath of *Shelby*, and the impact on minority voting rights in these states was the primary concern of critics of the decision (e.g., Singh and Carter 2023). For this reason, we also examine the estimated impact of *Shelby* on the minority voting gap in red states.

Countermobilization

One explanation offered for the absence of detrimental effects from removing federal preclearance on minority voting and registration is countermobilization. Civil rights groups and political campaigns may have responded to *Shelby* with targeted and extraordinary voter mobilization efforts that generated

²³ Morris and Miller (2025) examine heterogeneity by county-level partisanship, but not by the party controlling the state governments whose post-*Shelby* reforms drew critics' concern.

²⁴ In 2013, Virginia's state senate was evenly divided, but tie-breaking votes were cast by the Republican Lieutenant Governor.

countervailing increases in minority turnout and registration (e.g., Raze 2022; Billings et al. 2024; Komisarchik and White 2025). And while targeted in-person get-out-the-vote appeals can raise turnout among those contacted, the effect sizes documented in the mobilization literature are modest (Green and Gerber 2023), and systematic evidence of countermobilization at the scale required to offset a suppressive *Shelby* effect is lacking.

Komisarchik and White (2025) employ survey data from the Cooperative Election Study to test whether *Shelby* increased minority respondents' reported contact by parties or political organizations; they find only small (albeit positive) and statistically insignificant effects. Indeed, those authors conclude that countermobilization does not explain the resilience of minority voter participation post-*Shelby*. This is also consistent with evidence on the effects of particular state election reforms, such as voter identification laws, on reports of campaign contact (e.g., Cantoni and Pons 2021, and Grimmer and Hersh 2024). However, countervailing political mobilization need not come only from the organized efforts of outside groups.

In general, election administration procedures may involve some tradeoff between ease of voting and perceptions of election security (Ferroni and Milyo 2025). For example, voting by mail lowers the hassle costs of voting, but it also creates breaks in the chain of custody for ballots. Similarly, while strict voter identification laws increase hassle costs for some voters, they may also improve public confidence in the integrity of elections (Endres and Panagopoulos 2021; Milyo 2025). So, to the extent that *Shelby* permitted states to adopt reforms perceived as improving election security, those reforms may have increased confidence in elections, and hence turnout, among some voters. Consistent with this possibility, Citrin et al. (2014) conduct large-scale field experiments and find that informing voters about a new state voter identification law did not depress turnout, and if anything increased it, in heavily Black precincts in Virginia and Tennessee (see also Bright and Lynch 2017; Endres and Panagopoulos 2023).

On the other hand, to the extent that minority voters perceive state election reforms as an attempt to suppress their ability to vote, this may incite anger and create a backlash that instead results in higher turnout among minority voters. For example, Valentino and Neuner (2017) find that framing a hypothetical change in voter identification requirements as motivated by voter suppression increases reported intentions to vote among Democrats, the party with which most minority voters identify. However, evidence for this backlash effect from field experiments is mixed (Biggers 2021; Biggers and Smith 2020). Even so, the potential for offsetting increases in minority turnout, whether through organized countermobilization, intrinsic backlash, or increased confidence in election integrity, implies that, in theory, voting reforms in general and *Shelby* in particular have ambiguous effects on turnout and relative minority turnout.

Given this concern, we also examine whether there is any evidence of increased political mobilization among minority voters that might mitigate what would otherwise be a negative impact of *Shelby* on the minority voting gap. In contrast to earlier *Shelby* studies, which examine only reported campaign contact, we employ three different measures of countermobilization: interest in public affairs and political news; political activities other than voting; and whether an individual reports being contacted by a political candidate or campaign.²⁵ We also examine the subset of survey respondents who

²⁵ We combine these three measures into a single “political mobilization index” to reduce measurement error in any individual item and to limit multiple comparisons; results for each component measure are reported in the Appendix.

accurately report whether they voted, reasoning that accurate reporting of turnout signals reliable reporting of other political activities.

4. Data and Methods

We examine voting, registration, and political mobilization data from the Cooperative Election Study (CES) for all federal election years from 2008 to 2024.²⁶ The CES is a large nationally representative survey of American political attitudes and behavior; in even-numbered years, the survey is administered to over 50,000 respondents in the weeks prior to the general election (with a post-election follow-up wave).²⁷ The CES is comparable in sample size to the November Voting and Registration Supplement of the Current Population Survey (CPS), but the CES also includes information on relevant individual attributes not included in the CPS, such as party identification and political engagement.

A major advantage of the CES over the CPS is that self-reported voting and voter registration are checked against state administrative records. All respondents are subsequently matched by the survey's data vendor against a national database of state voter files to verify whether they are registered and whether they voted in the most recent November general election. This "validated vote" (or "verified vote") feature of the CES is particularly important, since self-reported voter turnout and registration is notoriously overstated in surveys, and at different rates for minority versus non-minority respondents (e.g., Ansolabehere and Hersh 2012).²⁸ The CES therefore combines the advantage of administrative data on voter turnout and registration with that of surveys that include detailed information on relevant individual attributes not available in state voter files (e.g., education, or in most states, race/ethnicity).

Some caveats regarding the CES validation process are in order. Respondents who are not matched to voter registration records are coded as unregistered non-voters. A failure to match may reflect misreporting by the respondent, but it may also reflect error in the matching process itself, in which case some actual registrants and voters are misclassified as unregistered non-voters. Nevertheless, we follow the recommendation of Ansolabehere and Schaffner (2017) and treat all respondents not matched to state voter files as non-registered (and hence non-voters).²⁹ This is also by far the most common approach in the literature (e.g., Raze 2022). As a robustness check, we also analyze the effects of *Shelby* on minority turnout among validated registered voters, which should be less sensitive to matching errors. Grimmer et al. (2018) raise a different concern regarding variation in matching rates across states and years, especially for minority respondents. However, all model specifications we

²⁶ The survey was known as the Cooperative Congressional Election Study (CCES) through 2019; we use the current name throughout.

²⁷ The CES had over 30,000 respondents in 2008 (see Table A1 in the Appendix). In contrast, the American National Election Study had under 10,000 respondents in 2020, while the General Social Survey had fewer than 4,000 respondents. The November supplement of the Current Population Survey has about 60,000 respondents but does not contain political content beyond self-reported voting and registration.

²⁸ For the time period we examine, 89% of CES respondents self-report voting, but only 60% are validated voters; among minority respondents, 84% self-report voting, but only 47% are validated voters.

²⁹ North Dakota does not require registration to vote (but does maintain a voter file); we code all North Dakota respondents as registered to vote when analyzing validated voter registration. Omitting North Dakota from our analysis has no appreciable impact on reported results.

examine include race $\times$ state and race $\times$ year interactions; these indicators absorb differential match rates that vary across these dimensions.³⁰

To identify the causal effect of federal oversight of state election administration on disparities in voting, we estimate the following triple difference (DDD): minority versus non-minority turnout in covered versus uncovered states, before and after *Shelby*. We also examine DDD models with minority respondents broken out into Black, Hispanic, and Other race.³¹ After omitting non-citizens and respondents not residing in the 50 states,³² we observe 492,354 individuals in the CES, although as individual-level controls are added to the baseline DDD model, some observations are dropped due to missing values. Descriptive statistics for all variables used in our analysis are provided in Tables A2-A3.

Identification of the treatment effect in these DDD models does not require that turnout evolve in parallel across treated and control states; it requires only the weaker assumption that the minority/non-minority turnout gap would have evolved in parallel absent treatment (Olden and Møen 2022). We assess the plausibility of this assumption by testing whether the race $\times$ covered state $\times$ year interactions are jointly significant in the pre-treatment period. As shown in the event study figures below, in no case are these pre-treatment differentials jointly significant.

Like the earlier evaluation studies reviewed above, we do not include controls for differences in state election institutions over time, as these are themselves potential outcomes of the treatment. Post-*Shelby* changes in election law are a primary channel through which removing preclearance could affect the minority voting gap, and conditioning on them would absorb the effect of interest. Following the literature, we examine the reduced-form (intent-to-treat) effect of *Shelby* on the minority voting gap. Because this is a mixed-level analysis with treatment variation at the state level and voting outcomes measured at the individual level, all standard errors are adjusted for clustering at the state level (Primo et al., 2007).

Our approach is most similar to Raze (2022), who also examines data from the CES, albeit only through 2018. Unlike Raze, we include all minority respondents in our analyses and omit non-citizens. We also examine models that include controls for individual attributes other than age, education, race, and sex (e.g., employment, income, and marital status), as well as party identification and partisan tides in turnout. Like Raze (2022), we examine fully specified triple difference models that control for all lower-order interactions of race, state, and year; we also show below that failure to include these pairwise interactions (e.g., state $\times$ year) materially changes the estimated effect of *Shelby* on minority turnout. However, because we include these lower-order interactions in all models, effects that vary only at the

³⁰ If instead match rates for minority respondents change differentially in covered states after *Shelby*, for example through more aggressive list maintenance, this would code removed registrants as non-voters and thus bias our estimates toward finding a negative effect of *Shelby*. Our null results are conservative with respect to this form of measurement error.

³¹ The CES records Hispanic identity both as a response to the race item and as a separate ethnicity item; we code respondents as Hispanic if they identify as such on either. The Black, Hispanic, and Other Race indicators are therefore not mutually exclusive; a respondent may appear in more than one subgroup. However, the overall minority indicator counts each respondent once.

³² Vote validation was not possible for respondents in Virginia in 2008 and 2010; we follow Cantoni and Pons (2021) and Raze (2022) and omit these observations from our dataset.

state-year level, such as the impact of *Shelby* on overall voter turnout in treated states, are absorbed and cannot be separately identified.

Note that *Shelby* freed covered jurisdictions at two margins: statewide election law in all covered states, and local election practices in explicitly covered jurisdictions. Our state-level treatment therefore captures the total effect of *Shelby* on residents of covered states, through both channels; the implicitly covered states, where only statewide law was subject to preclearance, isolate the state-law channel, and the covered-county differentials reported below speak to the local channel directly.

A limitation of any individual-level survey, including the CES, is that within-county samples are too small to reliably estimate models with county fixed effects, let alone interactions of race $\times$ county $\times$ year.³³ For this reason, our data are not appropriate for analyzing county-level treatments in a fully specified triple difference model, and we focus instead on the state-level treatment of *Shelby*. However, as part of our sensitivity checks, we do test for differential effects on minority turnout in formerly covered counties.

We present all of our findings for two different interpretations of the “treatment” imposed by the Court’s decision in *Shelby*. We provide estimates of the treatment effect of *Shelby* for only the subset of “explicitly covered” states, as well as for all covered states. In general, there is no substantive difference in our findings across the two definitions; we nonetheless present both, since explicitly covered states were covered as a whole due to their histories of discriminatory practices, a circumstance not shared by implicitly covered states. We also demonstrate that our findings are robust to allowing differential effects for covered counties within implicitly covered states, and to restricting the treatment effect to Republican-controlled (“red”) states as of the *Shelby* decision.

After presenting our findings for voter turnout among voting-eligible respondents, we employ the same estimation strategy to examine the treatment effect of *Shelby* on the minority gap in voter registration and in turnout conditional on registration. We then examine whether the states that would be covered under the proposed John Lewis VRAA exhibit retrogression in relative minority voting post-*Shelby*; this counterfactual indicates whether the proposed “fix” to Section 4’s coverage formula is better targeted at such retrogression than the formula struck down in *Shelby*.³⁴

Finally, we employ the same model specifications to test the countermobilization hypothesis, replacing only the dependent variable in our regression models. Several previous studies have employed a question from the CES on whether a respondent was contacted by a candidate or campaign as a proxy for organized mobilization efforts aimed at the respondent (e.g., Cantoni and Pons 2021; Komisarchik and White 2025). However, this question was not asked in the 2008 or 2018 waves of the CES, so models employing this measure, including the combined index described below, exclude those years. Moreover, political mobilization may also spring from more intrinsic motivation. For this reason, we utilize two additional proxies for political mobilization, namely political interest and participation in political activities other than voting, and combine all three variables into a single index.

³³ For example, of the 3,077 counties sampled in the CES over the period 2008–2024, only 46 large counties are included in every election year, and there are on average fewer than six minority respondents per county per year in the full sample.

³⁴ We follow Barber (2019), who identifies the states that would have been covered under the John Lewis VRAA’s proposed coverage formula; see Figure 4.

In every election year, the CES includes a question on respondents' interest in public affairs and political news on a four-point scale.³⁵ Respondents also report their political activities other than voting; we construct a count of four such activities asked in every election year, which are attending political meetings, putting up a political sign, working for a political campaign, and donating to a political candidate or group. We then normalize both the "political interest" and "other political activity" measures to a 0–1 scale, which facilitates comparison with the binary indicator for "campaign contact" discussed above. We examine each of these three measures separately in the Appendix, but because our findings are very similar for each, in the text below we present results for a simple "political mobilization index," which is the mean of the three measures.

In addition to examining multiple measures, we depart from previous studies in two ways. First, these measures are self-reported and therefore subject to misreporting, like self-reported voting. Consequently, we also conduct our analysis after restricting the sample to "accurate reporters," defined as individuals who correctly self-report whether they voted (based on the validated vote). Second, since the countermobilization hypothesis ultimately concerns increases in turnout among those mobilized, we also examine mobilization among those who actually voted. We therefore examine the effects of *Shelby* on political mobilization for four samples of CES respondents, namely all respondents, accurate reporters, validated voters, and accurate reporters who are also validated voters. This affords a more thorough evaluation of the countermobilization hypothesis than earlier efforts.

5. Results

In Table 1 we report estimates of the triple difference treatment effect of *Shelby* on minority turnout for several different model specifications;³⁶ the format of this table is replicated when we examine other dependent variables (registration, turnout conditional on registration, and political mobilization) below. The top panel of Table 1 displays the coefficient of interest when the treatment is characterized as the removal of preclearance in explicitly covered states only, while the second panel includes all formerly covered states in the treatment group. Within each panel, we report estimates for four nested specifications (columns 1–4), with successive models adding covariates. Our preferred specification is the model with all covariates (column 4), but we include the other models to show how adding or removing covariates affects our estimates.

Looking across the columns of Table 1, the estimated effect of *Shelby* is consistently positive, indicating a small reduction in the minority voting gap, albeit statistically insignificant in specifications with added controls. Our preferred model yields an estimate of 0.45 percentage points for explicitly covered states, with a 95% confidence interval of approximately –1.2 to 2.1 points. When the treatment group includes all covered states (Panel 2), the positive estimates are somewhat larger; indeed, the baseline estimate of 2.11 points is statistically significant, but it attenuates and loses significance as demographic controls

³⁵ "Some people seem to follow what's going on in government and public affairs most of the time, whether there's an election going on or not. Others aren't that interested. Would you say you follow what's going on in government and public affairs: (Most of the time; Some of the time; Only now and then; or Hardly at all)."

³⁶ Table B1 in the Appendix reports the analogous estimates when lower-order interactions are omitted from the regression models, as in most prior studies. In these misspecified models, the estimated effect of *Shelby* on relative minority turnout is negative, and statistically significant when all covered states are included in the treatment (Panel 2), suggesting that *Shelby* widened the minority voting gap. As discussed in Sections 3 and 4, such specifications confound the treatment effect with differential state-level trends; including the lower-order interactions reverses the sign on the estimated coefficients of interest.

and partisan tides are added to the model. Our preferred specification yields 1.25 points, with a 95% confidence interval of approximately -0.3 to 2.8. Further, in no case do we find a statistically significant differential effect in explicitly covered states beyond that in all covered states (bottom row of Table 1).

These null findings do not rule out very small negative treatment effects on minority turnout, but neither do they rule out larger positive effects. In particular, our preferred estimates allow us to rule out declines in relative minority turnout greater than about 1.2 percentage points in explicitly covered states, and greater than about 0.3 points across all covered states, with 95% confidence. The upper bounds of these same intervals, however, admit relative gains in minority turnout of up to 2.1 and 2.8 percentage points, respectively. This asymmetry underscores the importance of our subsequent examination of the countermobilization hypothesis, since positive effects of up to 2.1 to 2.8 percentage points, if real, would more plausibly reflect mobilization in response to *Shelby* than the absence of any response at all. In any case, the dire consequences predicted by critics of *Shelby* are simply not manifest in our analysis of voter turnout over the six election cycles since the Court's decision.

Event Study for Minority Voting Turnout

As discussed in Section 4, the triple difference estimation strategy requires that the minority versus non-minority turnout gap would have evolved in parallel in treated and control states absent treatment. Consequently, we conduct an event study analysis for both conceptualizations of the treated group. Figure 3 plots the estimated minority $\times$ treated state differentials for each election year, relative to 2012. Not only is there no obvious pre-trend apparent in Figure 3, but we also fail to reject the null hypothesis that the pre-treatment differentials are jointly zero. Figure 3 likewise shows no trend in the post-treatment differentials; in particular, the estimates do not become more negative over time, as would be expected if the effects of *Shelby* emerged only gradually. The only notable post-treatment estimate is a positive differential in 2016, consistent with the event-study patterns reported in Raze (2022). Figures B1–B3 in the Appendix present analogous event studies for Black, Hispanic, and Other Race respondents separately; in each case, the pre-treatment differentials are likewise small and not jointly significant.

Differential Effects for Covered Counties in Implicitly Covered States

Morris and Miller (2025) find that the effects of *Shelby* are concentrated where Section 5 constrained county and local election practices, raising the question of whether the relevant treatment operates at the county level. As discussed in Section 4, our data cannot support counties as the treatment unit in a fully specified triple difference model. We can, however, estimate pooled and differential treatment effects for covered counties in implicitly covered states (Table B2 in the Appendix).³⁷ We find no evidence of a significant differential effect on minority turnout for respondents in covered versus uncovered counties within implicitly covered states.

However, we do find a modest decline in overall turnout in these covered counties post-*Shelby* (roughly 2.8 percentage points in our preferred specification, albeit only marginally significant). This decline is shared across racial groups, consistent with a county-level channel for *Shelby* effects on overall participation, but not with a differential effect on minority voters. Thus, within the states where *Shelby*

³⁷ The coefficient of interest in Table B2 is "Minority $\times$ Covered County in an Implicitly Covered State $\times$ Post-*Shelby*" ($\beta = 0.08$, $SE = 1.68$, in our preferred specification).

removed only statewide preclearance, minority respondents in counties that additionally lost oversight of local election practices fared no differently than their neighbors, although our survey data have limited power at this margin.

Treatment Effects by Race/Ethnicity

Our analysis to this point has lumped all minority respondents into one group. Table 2 reports the coefficients of interest when we break out the treatment effect of *Shelby* for Black, Hispanic, and Other Race respondents (all relative to non-minority respondents). The estimated triple difference is small and positive for Black and Hispanic respondents, but fairly large and statistically significant for Other Race respondents, indicating a relative increase in turnout of roughly 2–3 percentage points. Once again, findings are similar across both panels, and not surprisingly, we cannot reject the null of identical treatment effects in explicitly covered states versus all covered states.³⁸ Event studies for each group are presented in the Appendix (Figures B1–B3). These figures also make clear the absence of any notable pre- or post-treatment trends; in each case, we cannot reject the null that the pre-treatment differentials are jointly zero.

Treatment in Red States

As discussed in Section 3, none of the prior evaluations examines whether the treatment effect of *Shelby* is concentrated in states under Republican control. This concern suggests limiting the treatment group to red states as of the *Shelby* decision. However, all of the explicitly covered states were under unified Republican control at that time, so the estimates in Panel 1 of Tables 1 and 2 already show the treatment effects for red states, and those estimates are uniformly small and insignificant for all groups except Other Race respondents. For the specifications that include all covered states, only California and New York were not red states in 2013. Consequently, we reexamine the treatment effects on the minority voting gap and its racial/ethnic components after dropping observations from California and New York. Our findings are unchanged: the estimates remain small and statistically insignificant for the overall minority gap and for the Black and Hispanic gaps, while the Other Race estimate remains positive and significant (see Table B3 in the Appendix).

Treatment Effects on Registration and Turnout among Registered Voters

We estimate the treatment effect of *Shelby* on validated minority voter registration in DDD models of the same sort employed above for validated turnout. This analysis also finds generally small, positive, and statistically insignificant effects for minorities overall and for each minority group (see Tables B4 and B5 in the Appendix). As with turnout, the estimated effect on registration is largest for Other Race respondents, though it is not statistically significant (Table B5). Given the absence of large or significant negative effects on either turnout or registration, it is not surprising that we also find null effects of *Shelby* on minority turnout among validated registrants (Tables B6 and B7 in the Appendix). Recall from Section 4 that turnout among validated registrants is less sensitive to errors in the vote-validation matching process; these null findings thus also reinforce our conclusion that the turnout results are not artifacts of differential match quality.

³⁸ Results are very similar when we restrict the treatment group to red states (see Panel 2 of Table B3 in the Appendix).

Counterfactual Analysis of the Proposed John Lewis VRAA

The proposed John Lewis VRAA includes a "fix" to the coverage formula voided in *Shelby*; however, the set of states that would be covered under this proposal is a subset of the states covered prior to *Shelby*, differing only by the exclusion of Alaska, Arizona, Michigan, and South Dakota (Figure 4). We investigate whether the proposed formula better targets states that have retrogressed with respect to minority voting or registration by re-estimating our triple difference models using the states that would be covered under the proposal as the treatment group. Under this counterfactual, significant negative "treatment" effects would indicate that the proposed fix is better targeted than the formula struck down in *Shelby* at identifying such retrogression.

Given that the set of states covered under the proposal is so similar to those covered in 2013, it is not surprising that we observe small, positive, and statistically insignificant treatment effects for voter turnout (Table 3), voter registration (Table B8), and turnout among registered voters (Table B9). The proposed John Lewis VRAA, in short, is not targeted at states that have experienced retrogression in minority voting or registration post-*Shelby*.

Treatment Effects on Political Mobilization

As discussed above, several scholars have posited that countermobilization of minority voters may confound the observed treatment effect of *Shelby* on voter turnout (see especially Raze 2022). And while Komisarchik and White (2025) find suggestive evidence of increased campaign contact among minorities post-*Shelby*, the estimated effects are substantively small, and existing benchmarks for the effects of mobilization on turnout imply that effects of this size are too small to offset meaningful suppression (Grimmer and Hersh 2024).

In contrast to studies that examine only campaign contact, we examine three proxies for political mobilization: interest in political news, other political activity, and contact by candidates or campaigns. We first demonstrate that each of these proxies is positively and significantly associated with voter turnout, even after controlling for all covariates in our preferred specification (Table 4; sample sizes vary across proxies for the reasons noted in Section 4). Thus the premise of the countermobilization hypothesis, that increased mobilization is associated with increased turnout, is strongly supported in our data.

Next, we estimate the treatment effect of *Shelby* on each measure of political mobilization employing the same DDD framework (see Tables C1–C6 in the Appendix). However, given that these separate analyses yield substantively similar results, we report here the results of interest for the political mobilization index, the mean of these three measures.

Table 5 shows the estimated coefficients of interest for this index for our preferred specification, across four samples of respondents. For the all-covered treatment group (Panel 2), we find small, positive, and statistically significant effects of *Shelby* on political mobilization in the full sample and among accurate reporters (columns 1 and 2 of Table 5); the estimates for explicitly covered states are null throughout. However, we find no significant effects among validated voters (the event study for the model in column 4 is shown in Figure C1). Moreover, even the statistically significant estimates are substantively tiny, on the order of 0.01 to 0.02 on a 0–1 index whose standard deviation is roughly 0.26, which is well under a tenth of a standard deviation. The pattern is similar when we break out the treatment effect by

race/ethnicity (Table 6). Taken together, mobilization effects that are an order of magnitude too small to generate offsetting turnout flows, and no detectable effects among validated voters, are difficult to reconcile with the countermobilization hypothesis; we address a potential selection concern with the validated-voter comparison below.

6. Discussion

Our examination of the treatment effect of *Shelby* on voter turnout, registration, and political mobilization yields no evidence that the removal of federal preclearance of state election reforms has harmed minority voters in previously covered states. In most instances, our point estimates run in the opposite direction. We also show that the revised coverage formula in the proposed John Lewis VRAA is not better targeted than the formula struck down in *Shelby* at identifying states where minority voting has retrogressed, and we find no support for the contention that countermobilization masks what would otherwise be a negative effect of *Shelby*.

Null results invite the question, “null relative to what?” Our preferred estimates rule out, with 95% confidence, declines in relative minority turnout larger than about 0.3 percentage points across all formerly covered states (1.2 points for explicitly covered states). For comparison, Morris and Miller (2025) report that the racial turnout gap grew by 2 to 2.6 percentage points on average, and by 4 to 5 points by 2022, in formerly covered counties. Effects of that magnitude at the state level lie well outside our confidence intervals. Our null results are consistent with small effects in either direction, but not with retrogression on the scale that critics of *Shelby* predicted or that county-level studies report.

One estimate is consistently positive and statistically significant: relative turnout among Other Race respondents rose by 2 to 3 percentage points in treated states post-*Shelby* (the corresponding registration estimates are positive but insignificant). This finding may not be coincidental. Alaska, Arizona, and South Dakota were brought under coverage in part because of language-minority protections affecting Native voters, so formerly covered states are disproportionately home to the groups composing this category. We are reluctant to over-interpret a positive effect we did not predict; we note only that it is robust across specifications and that it compounds, rather than relieves, the difficulty for the hypothesis that *Shelby* suppressed minority participation.

Our findings contrast most directly with Billings et al. (2024) and Morris and Miller (2025), who report increases in the racial turnout gap in formerly covered counties, but the studies estimate different things. Both conceptualize treatment at the county level and analyze aggregate turnout among registered voters with imputed race. We assign treatment at the state level, capturing, as discussed in Section 4, the total effect of *Shelby* on residents of covered states through both the state-law and local channels, and analyze validated individual-level turnout among the voting-eligible population with self-reported race, full lower-order interactions, and partisan controls. In fact, Morris and Miller (2025) themselves find no significant effect from removing preclearance of *state* election administration, and our own county-level check points the same direction; within implicitly covered states, formerly covered counties show a modest post-*Shelby* decline in overall turnout, but no racial differential. The evidence is thus consistent with a reading under which any post-*Shelby* retrogression operates through local administration, and on participation generally rather than minority participation differentially, rather than through the state election reforms that have dominated public controversy. Whether county-level gap estimates survive individual-level validation is a question our data cannot answer.

Two limitations of our countermobilization analysis deserve mention. First, the validated-voter test conditions on a post-treatment variable. If *Shelby*-era mobilization converted marginal non-voters into voters, average mobilization among voters could fall even as overall mobilization rose. We therefore emphasize the unconditioned evidence; in the full sample, where selection is not at issue, the estimated effects on our mobilization index are 0.01 to 0.02 on a 0-1 index with a standard deviation of 0.26, an order of magnitude too small to generate offsetting turnout flows, and relative minority registration shows no movement consistent with such flows. The offsetting hypothesis thus requires effects invisible on every margin where they would independently register. Second, our mobilization measures are self-reported; restricting the analysis to accurate reporters mitigates but cannot eliminate misreporting.

The scope of our conclusions should be stated plainly. Preclearance governed far more than turnout-relevant election administration; redistricting, annexations, polling place changes, and at-large election schemes were all subject to federal review, and vote dilution, not turnout suppression, was historically the doctrine's central concern. Our evidence speaks to participation, not representation, although we note that Komisarchik and White (2025) find no post-*Shelby* decline in Black membership in the U.S. House, and that half of the Black and Hispanic U.S. Senators elected since 2013 represent formerly covered states. These findings bear directly on the John Lewis VRAA debate. Proponents present the bill as restoring protections whose absence has demonstrably harmed minority voters, but with respect to state election reforms and voter participation, the harm is not demonstrable. Further, the proposed formula would reimpose preclearance on states that have experienced no measurable retrogression. None of this establishes that preclearance of local administration or redistricting lacks value; it establishes that the empirical case for federal oversight of *state* election laws cannot rest on post-*Shelby* trends in minority participation.

In *Shelby*, Chief Justice Roberts faulted Congress for retaining a coverage formula "based on 40-year-old facts having no logical relation to the present day"; Justice Ginsburg replied that discarding preclearance while it was still working was like discarding an umbrella in a rainstorm. A decade of evidence permits an empirical reading of the metaphor. With respect to state election reforms and minority participation, we find no sign of rain. Minority voters in formerly covered states are no worse off (relative to their counterparts elsewhere) than before the umbrella was put away.

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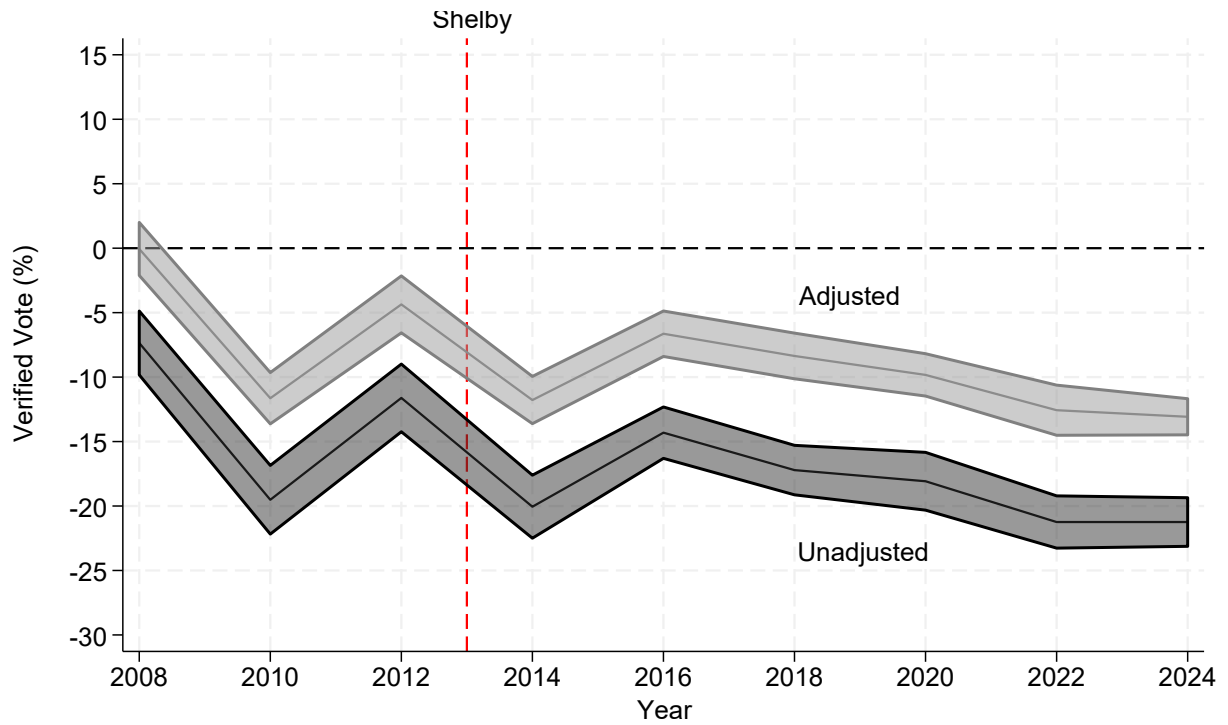
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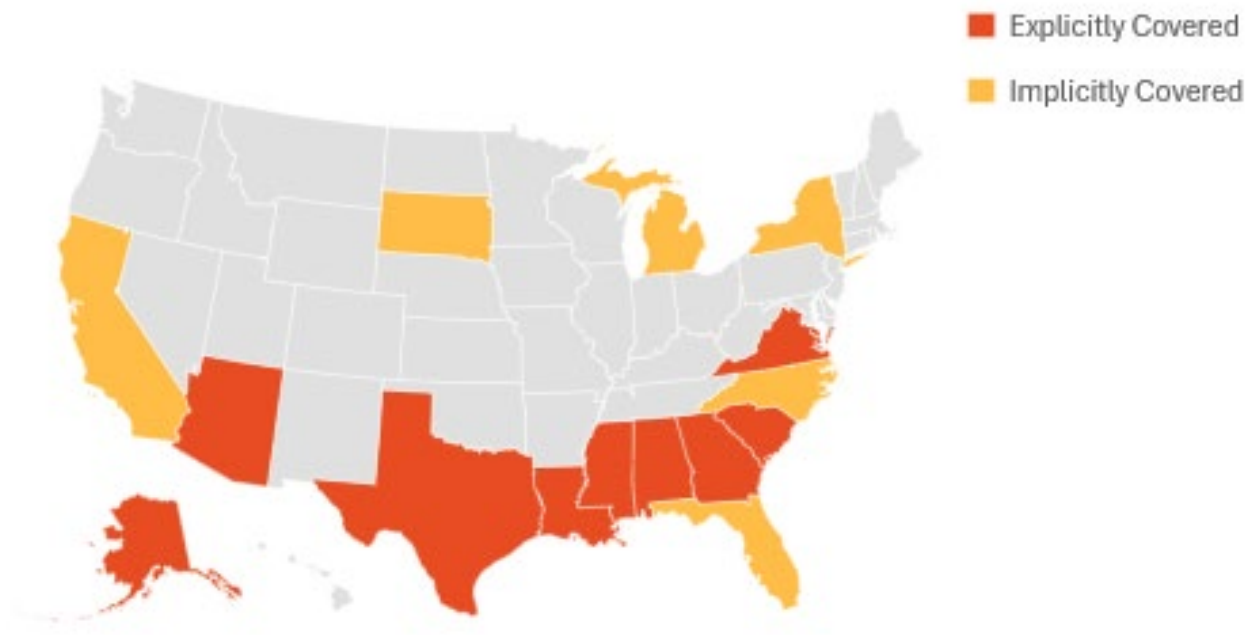
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Figure 1: Minority Voting Gap in the United States



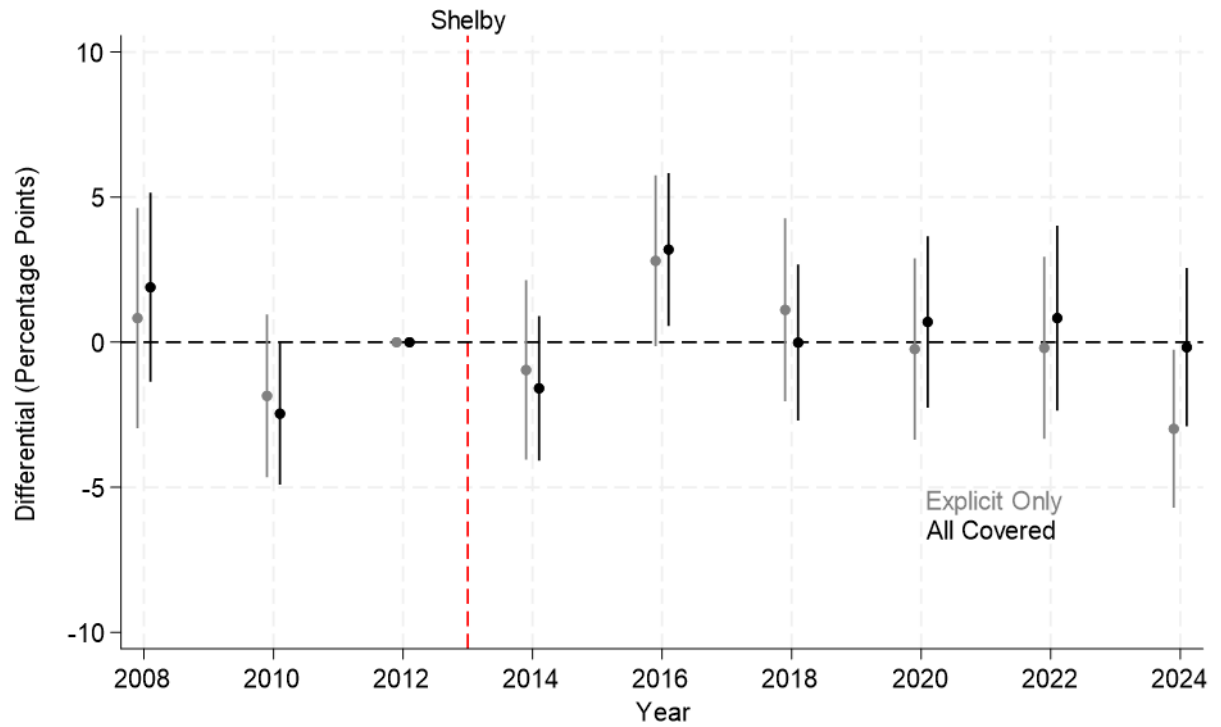
Notes: Difference in verified vote percentage for minority respondents compared to non-minority respondents from the Cooperative Election Study (adjusted for age, education, and sex).

Figure 2: States Subject to Preclearance Prior to *Shelby*



Note: The figure is adopted from Raze (2022).

Figure 3: Event Study of the Effect of *Shelby* on the Minority Voting Gap



Notes: Estimated minority $\times$ treated state differentials in validated turnout by election year (with 95% confidence intervals) based on the specification in Column 4 of Table 1. Pre-treatment differentials are not jointly significant.

Figure 4: States Subject to Preclearance Under the Proposed John Lewis VRAA



Notes: The figure is adopted from Barber (2019). States covered under the proposed John Lewis VRAA include all states explicitly and implicitly covered prior to *Shelby*, except Alaska, Arizona, Michigan, and South Dakota; no previously uncovered state would become covered.

Table 1: Effects of *Shelby* on Minority Voting Gap (%)

	(1)	(2)	(3)	(4)
Mean	59.95	59.97	59.98	60.08
(Standard Deviation)	(49.00)	(49.00)	(48.99)	(48.97)

Panel 1: Explicitly Covered States vs Other States

Minority × Explicitly Covered State	0.87	1.03	0.36	0.45
× Post- <i>Shelby</i>	(0.93)	(0.83)	(0.81)	(0.86)

Panel 2: All Covered States vs Other States

Minority × Covered State	2.11**	1.44*	1.24	1.25
× Post- <i>Shelby</i>	(1.00)	(0.78)	(0.81)	(0.81)

Included Controls in Each Panel:

Race, State, Year, and Interactions	Yes	Yes	Yes	Yes
Age, Education, and Sex		Yes	Yes	Yes
Party × Year			Yes	Yes
Other Demographic Characteristics				Yes
Observations	492,354	491,384	491,272	485,698
Test of Differential Effect in Explicitly Covered v. All Covered States	n.s.	n.s.	n.s.	n.s.

Notes: ***p<.01; **p<.05; and *p<.10; standard errors are clustered at the state-level. Other demographic controls include employment status, household characteristics, income, and marital status.

Table 2: Effects of *Shelby* on Racial/Ethnic Voting Gaps (%)

	(1)	(2)	(3)	(4)
Mean	59.95	59.97	59.98	60.08
(Standard Deviation)	(49.00)	(49.00)	(48.99)	(48.97)

Panel 1: Explicitly Covered States vs Other States

Black × Explicitly Covered State × Post- <i>Shelby</i>	1.47 (1.52)	1.11 (1.55)	0.36 (1.55)	0.50 (1.59)
Hispanic × Explicitly Covered State × Post- <i>Shelby</i>	-0.57 (0.89)	0.38 (0.87)	-0.05 (0.96)	0.24 (0.96)
Other × Explicitly Covered State × Post- <i>Shelby</i>	2.75* (1.46)	3.56*** (1.25)	3.39*** (1.20)	2.66** (1.27)

Panel 2: All Covered States vs Other States

Black × Covered State × Post- <i>Shelby</i>	1.46 (1.55)	1.02 (1.33)	0.81 (1.34)	0.77 (1.36)
Hispanic × Covered State × Post- <i>Shelby</i>	0.70 (1.78)	0.51 (1.57)	0.40 (1.58)	0.57 (1.53)
Other × Covered State × Post- <i>Shelby</i>	3.39*** (1.15)	2.84*** (0.93)	2.69*** (0.93)	2.33** (0.94)

Included Controls in Each Panel:

Race, State, Year, and Interactions	Yes	Yes	Yes	Yes
Age, Education, and Sex		Yes	Yes	Yes
Party × Year			Yes	Yes
Other Demographic Characteristics				Yes
Observations	492,354	491,384	491,272	485,698
Test of Differential Effects in Explicitly Covered v. All Covered States	$p < .10$	n.s.	n.s.	n.s.

Notes: *** $p < .01$; ** $p < .05$; and * $p < .10$; standard errors are clustered at the state-level. Other demographic controls include employment status, household characteristics, income, and marital status.

Table 3: Counterfactual Effects on Voting Gaps (%) from Removing Preclearance as Proposed in the John Lewis VRAA

	(1)	(2)	(3)	(4)
Mean	59.95	59.97	59.98	60.08
(Standard Deviation)	(49.00)	(49.00)	(48.99)	(48.97)
<i>Panel 1: Minority Voting Gap</i>				
Minority × Covered State × Post- <i>Shelby</i>	2.44** (0.94)	1.42* (0.76)	1.22 (.80)	1.14 (0.81)
<i>Panel 2: Racial/Ethnic Voting Gaps</i>				
Black × Covered State × Post- <i>Shelby</i>	1.73 (1.51)	1.01 (1.33)	0.82 (1.34)	0.66 (1.35)
Hispanic × Covered State × Post- <i>Shelby</i>	1.09 (1.56)	0.46 (1.36)	0.38 (1.37)	0.42 (1.33)
Other × Covered State × Post- <i>Shelby</i>	4.20*** (0.96)	3.28*** (0.92)	3.14*** (0.90)	2.91*** (0.89)
Included Controls in Each Panel:				
Race, State, Year, and Interactions	Yes	Yes	Yes	Yes
Age, Education, and Sex		Yes	Yes	Yes
Party × Year			Yes	Yes
Other Demographic Characteristics				Yes
Observations	492,354	491,384	491,272	485,698

Notes: ***p<.01; **p<.05; and *p<.10; standard errors are clustered at the state-level. Other demographic controls include employment status, household characteristics, income, and marital status.

Table 4: Effects of Political Mobilization on Verified Vote (%)

	(1)	(2)	(3)	(4)
Mean	61.17	64.41	63.98	64.74
(Standard Deviation)	(48.74)	(47.88)	(48.01)	(47.78)
Interest in Political News	28.39*** (0.47)			
Other Political Activity		13.17*** (0.66)		
Contacted by Candidate or Campaign			14.54*** (0.35)	
Political Mobilization Index				37.81*** (0.82)
Included Controls in Each Panel:				
Race, State, Year, and Interactions	Yes	Yes	Yes	Yes
Age, Education, and Sex	Yes	Yes	Yes	Yes
Party × Year	Yes	Yes	Yes	Yes
Other Demographic Characteristics	Yes	Yes	Yes	Yes
Observations	473,095	410,272	334,149	327,999

Notes: *** $p < .01$; ** $p < .05$; and * $p < .10$; standard errors are clustered at the state-level. Interest in Political News is asked of all respondents in the CES. All regression models include the controls listed in Column 4 of Table 1. “Other Political Activity” is asked in every election year, but only in the post-election wave of the CES, so includes fewer responses due to drop-off. “Contacted by a Candidate or Campaign” is also asked only in the post-election wave, but was not asked in 2008 or 2018, so has fewer responses. The “Political Mobilization Index” is the mean of all three indicators.

Table 5: Effects of *Shelby* on the Minority Gap in Political Mobilization Index

Sample:	All	Accurate Reporter	Verified Voter	Accurate & Verified
	(1)	(2)	(3)	(4)
Mean	0.52	0.55	0.58	0.59
(Standard Deviation)	(0.26)	(0.26)	(0.23)	(0.23)

Panel 1: Explicitly Covered States vs Other States

Minority × Explicitly Covered State × Post- <i>Shelby</i>	0.01 (0.01)	0.00 (0.01)	-0.00 (0.01)	-0.00 (0.01)
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Panel 2: All Covered States vs Other States

Minority × Covered State × Post- <i>Shelby</i>	0.01*** (0.00)	0.02*** (0.01)	0.01 (0.01)	0.01 (0.01)
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Included Controls in Each Panel:

Race, State, Year, and Interactions	Yes	Yes	Yes	Yes
Age, Education, and Sex		Yes	Yes	Yes
Party × Year			Yes	Yes
Other Demographic Characteristics				Yes

Observations	327,999	242,265	212,333	210,456
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Test of Differential Effect in Explicitly Covered v. All Covered States	n.s.	n.s.	n.s.	n.s.
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Notes: ***p<.01; **p<.05; and *p<.10; standard errors are clustered at the state-level. All regression models include the controls listed in Column 4 of Table 1. “Accurate Reporter” indicates that self-reported voting behavior (including not voting) is consistent with the verified vote record.

Table 6: Effects of *Shelby* on Racial/Ethnic Gap in Political Mobilization Index

Sample:	All	Accurate Reporter	Verified Voter	Accurate & Verified
	(1)	(2)	(3)	(4)
Mean	0.52	0.55	0.58	0.59
(Standard Deviation)	(0.26)	(0.26)	(0.23)	(0.23)

Panel 1: Explicitly Covered States vs Other States

Black × Explicitly Covered State × Post- <i>Shelby</i>	0.01 (0.01)	-0.00 (0.01)	-0.01 (0.01)	-0.01 (0.01)
Hispanic × Explicitly Covered State × Post- <i>Shelby</i>	0.01 (0.01)	0.01* (0.01)	0.00 (0.01)	0.00 (0.01)
Other × Explicitly Covered State × Post- <i>Shelby</i>	0.01 (0.01)	0.00 (0.01)	0.01 (0.01)	0.01 (0.01)

Panel 2: All Covered States vs Other States

Black × Covered State × Post- <i>Shelby</i>	0.01** (0.01)	0.01 (0.01)	0.00 (0.01)	0.00 (0.01)
Hispanic × Covered State × Post- <i>Shelby</i>	0.01* (0.01)	0.01 (0.01)	0.00 (0.01)	-0.00 (0.01)
Other × Covered State × Post- <i>Shelby</i>	0.00 (0.01)	0.02** (0.01)	0.01 (0.01)	0.01 (0.01)

Included Controls in Each Panel:

Race, State, Year, and Interactions	Yes	Yes	Yes	Yes
Age, Education, and Sex		Yes	Yes	Yes
Party × Year			Yes	Yes
Other Demographic Characteristics				Yes
Observations	327,999	242,265	212,333	210,456
Test of Differential Effects in Explicitly Covered v. All Covered States	n.s	n.s	n.s	n.s

Notes: ***p<.01; **p<.05; and *p<.10; standard errors are clustered at the state-level. All regression models include the controls listed in Column 4 of Table 1. “Accurate Reporter” indicates that self-reported voting behavior (including not voting) is consistent with the verified vote record.