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

Brendan Cirillo and Jeffrey Milyo*
University of Missouri

Abstract:

The 2013 *Shelby* decision 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 turnout versus white 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 to states where preclearance did make a difference prior to *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*.

**The authors are grateful for helpful comments from participants at the annual meetings of the Public Choice Society (especially John Aldrich), the Southern Political Science Association, and for financial assistance from the Ulysses S. Grant Institute for the Study of Democracy and the University of Missouri.*

Throwing out pre-clearance when it… is continuing to work… is like throwing away your umbrella in a rainstorm because you are not getting wet.

--- Justice Ruth Bader Ginsberg, dissenting in *Shelby v Holder* (2013)[1]

1. Introduction:

The 2013 United States Supreme Court decision in *Shelby* 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 the major amendments to the VRA in 1975. And prior to *Shelby*, the DOJ vigorously exercised its oversight authority, reviewing between 14,000 and 20,000 changes to state and local voting procedures each year.[2]

Pre-clearance essentially flips the burden of proof for determining whether reforms to election administration have a discriminatory intent or affect; covered jurisdictions are required to seek prior approval from the Department of Justice (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. For this reason, the recently proposed John Lewis Voting Rights Advancement Act (VRAA) includes a revised procedure for "bailing-in" jurisdictions subject to federal preclearance. Consequently, we analyze both the effects *Shelby*, which removes preclearance, on the minority voting gap in covered states, as well as the implications of potential federal 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, have 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 "with a vengeance"[3] or "Jim Crow 2.0"[4]; and the *Shelby* decision has been characterized by voting rights advocates and liberal commentators as "gutting the VRA", "ushering in widespread voter suppression", and "breaking America" (e.g. Berman 2021; Newkirk 2018).[5] And while the most indignant invective of this sort may be attributed to partisan theatrics reflecting less the status of minority voting rights and more the degradation of policy debate in an increasingly polarized polity, there is nonetheless a legitimate empirical question regarding the impact of *Shelby* on disparities in

[1] Shelby County. v. Holder, 570 U.S., 133 S. Ct. 2612 (2013) at 2650 (Ginsberg, J., dissenting).
[2] "About Section 5 of the Voting Rights Act," Department of Justice webpage at: https://www.justice.gov/crt/about-section-5-voting-rights-act, viewed August 20, 2025.
[3] "Vote Suppression," The Brennan Center webpage at https://www.brennancenter.org/issues/ensure-every-american-can-vote/vote-suppression, viewed March 1, 2023.
[4] "Remarks by President Biden on Protecting the Right to Vote," January 11, 2022; text available at: https://www.whitehouse.gov/briefing-room/speeches-remarks/2022/01/11/remarks-by-president-biden-on-protecting-the-right-to-vote/, viewed March 1, 2023.
[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.).

voter turnout. 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* decision on minority voting.[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 dramatically improved since the passage of the VRA (Grofman et al. 1992), a significant difference remains today (Morris and Grange 2024).

For example, using verified voting data from the Cooperative Election Study, turnout among minority voting-age citizens has averaged just 43% since 2008, compared to 60% for non-minority voting-age citizens. Demographic differences contribute to this significant 17 percentage-point gap, but even after controlling for age, education, and sex, a significant difference of 9 percentage points remains (authors' calculations). In addition, the pace of improvements in the minority voting gap has stalled out in recent years; other than two blips corresponding to Barak Obama's historic campaigns for President, the minority voting gap has remained stubbornly large (See Figure 1). Moreover, a persistent voting gap is observed across focal 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 isn't clear from these figures is whether Justice Ginsberg'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 hold in abeyance state practices that would otherwise exacerbate the minority voting gap. Removal of preclearance not only allowed states to adopt new laws but also permitted states to engage in previously blocked practices, so that prior to the 2014 general election a dozen formerly covered states (or 75% of all previously 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 also hold symbolic importance that serves to embolden 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 may well operate through diffuse and indirect channels; and this impact may not be manifest in full immediately but may evolve over time.

[6] In contrast, studies of selective state voting reforms, such as voter identification laws, have received more attention; and while early findings were decidedly mixed --- and early studies marred by 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 (Cantoni and Pons 2021; and Hoekstra and Koppa 2021).

[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 in formerly covered jurisdictions, with the exception of an increased probability of passing photo identification requirements in former explicitly covered states (see the subsequent discussion below for the distinction between explicitly and implicitly covered states).

As such, we examine the intent-to-treat, where the focus is on the opportunity for states to alter state 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 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*; while Billings et al. (2024) and Morris and Miller (2025) find the opposite; and Komisarchik and White (2025) report negligible effects from 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 nation-wide 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 partisan electoral tides on voter turnout. In addition, we test whether the removal of preclearance is only relevant for contemporaneous Republican-controlled states and whether the "fix" to Section 4 in the proposed John Lewis Voting Rights Advancement Act better targets states than the VRA pre-*Shelby*.

Overall, we find no significant detrimental effect on minority voting or registration from the removal of federal preclearance of state election reforms in previously covered states; nor do we find evidence that the states covered under the John Lewis VRA would be better targeted than those previously covered by the VRA. Moreover, we also find no support for the contention that increased political mobilization among minority voters counters an otherwise negative effect of *Shelby* on Black and Hispanic voting and registration. Taken together, these findings stand in stark contrast to the alarmist rhetoric sparked by *Shelby* and cast serious doubt on the efficacy of continued federal oversight of state election administration.

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 called the "crown jewel" of the Civil Rights Movement (Gerken 2013) and "possibly the most consequential federal law in our nation's history" (Consovoy and McCarthy 2013). Section 5 of the VRA established a pre-clearance

[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).

[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; l;ast viewed June 1st; 2025).

procedure whereby states and counties with a history of discriminatory voting practices were required to get approval from the Department of Justice (DOJ) before implementing changes to voting laws. Section 4 of the VRA established the basis for which jurisdictions would be covered by the pre-clearance requirements in Section 5; these criteria included having a “test or device” that limited the opportunity to register or vote as of November 1st, 1964; and having voter registration or voter turnout rates under 50% in the presidential election of 1964.[10]

The special provisions of Sections 4 and 5 of the VRA were originally set to expire after 5 years, but this time limit was subsequently extended for another 5 years by Congress in 1970, then again in 1972. Sections 4 and 5 were again amended in 1975 to include protections for language minorities and the coverage formula was expanded to include jurisdictions with low registration and voting rates as of November 1972. The application of Section 5 preclearance was extended for 25 years in 1982, and for another 25 years in 2006, although the coverage criteria in Section 4 was not updated.[11]

In 2013, the Supreme Court ruled in *Shelby* that the criteria for identifying covered jurisdictions in Section 4 of the VRA was outdated and therefore unconstitutional.[12] This is because 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); however, the majority in *Shelby* determined that the coverage formula imposed by the VRA for several decades was no longer sufficiently targeted based on contemporaneous conditions to justify the expediency of violating the fundamental principle of equal sovereignty among the states.

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 DOJ review. However, 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” due to at least one local jurisdiction being covered.[13] The Court ruled in *Lopez* that since changes in state election administration would also affect any covered locality, then changes to election laws in those states would also be subject to preclearance (Katz 2001).

This means that the 2013 *Shelby* decision had a larger impact than has been recognized in several studies that ignore the existence of implicit state coverage after the 1999 decision in *Lopez v Monterey* (e.g., Ang 2019; Billings et al. 2024; and Komisarchik and White 2025). About 22% of U.S. population (and 28% of the minority population) currently 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 that formerly were covered in any way by the VRA.[14] Consequently, it is important for evaluations of

[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 June 1st, 2025).
[11] This discussion of the history of preclearance is based on the account in Consovoy and McCarthy (2013).
[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*. v. *Holder*, 570 U.S., 133 S. Ct. 2612 (2013) at 2629.
[13] *Lopez v. Monterey County*, 525 U.S. 266 (1999).
[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.

preclearance to acknowledge the existence of these implicitly covered states (e.g., 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 pre-clearance have been hotly contested and partisan scuffles. In both 2021 and 2022, the Democrat-controlled U.S. House passed the John Lewis Voting Rights Advancement Act, which would re-establish federal pre-clearance requirements in newly defined covered jurisdictions, but the bill was deemed "unnecessary" by Senate Minority leader Mitch McConnel (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 to occur under President Trump, given the close margins for party control of Congress and the Presidency, and the visceral nature of the public debate over alleged suppression of minority voters, continuing calls to pass the John Lewis Voting Rights Advancement Act seem assured in coming years.

3. Recent Estimates of the Effects of Pre-Clearance on the Minority Voting Gap

Ang (2019) presents evidence that federal pre-clearance constrained attempts by state and local election officials to change voting laws in covered jurisdictions prior to *Shelby;* and examines difference-difference models for aggregate voter turnout in presidential elections from 1968-2016 for counties in states not covered as a whole prior to 1975 (i.e., all of the explicitly covered states in Figure 2 are omitted from the analysis).[18] Ang (2019) finds that starting in 1976, there is a steady increase in county-level turnout for covered versus "uncovered" jurisdictions from 1976 to 1994, but this levels off afterward. In order to test whether preclearance post-1976 impacts turnout by race, Ang (2019) aggregates individual-level self-reported turnout from the Current Population Survey (CPS) to create state-level turnout rates for white and non-white voting age population. He then examines separate difference-in-difference models and finds a significant increase in state-level non-white turnout for post-the 1975 amendments to the VRA, but no such increase for state-level white turnout.

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 amendment levels off and remains essentially unchanged for more

[15] These authors refer to what we call "implicitly covered" states as "partially covered"; however, the latter designation may generate confusion since it does not clearly convey that changes to state election administration in implicitly covered states are in fact covered by preclearance (rather than just some counties in those states).
[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] Masaro, Lisa. 2022. "Voting Bill Collapses, Democrats Unable to Change Filibuster," AP News (https://apnews.com/article/biden-voting-rights-bill-collapses-27c888b4f9bf876520913d7036a942b0; viewed June 1st, 2025).
[18] This is similar in spirit to comparing newly treated counties to only never-treated counties; however, this also imposes a restriction that implicit state coverage is ineffectual, since Ang (2019) treats any county not explicitly covered as part of the control group.

than a decade, which is also consistent with the contention of Senator McConnell (R-KY) that erstwhile fixes to the VRA are unnecessary, Justice Ginsberg's umbrella notwithstanding. Moreover, Ang (2019) does not analyze individual-level CPS data, which would obviate the need to impute state-level turnout rates by race. Even so, the reliance on CPS self-reported turnout to estimate the impact on minority voting 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 Black versus White turnout, in covered versus uncovered states, both before and after *Shelby*.[19] Raze (2022) employs validated voter data from the Cooperative Election Study (CES); the CES is a large national survey that also matches respondents to state voter files for 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 X state; race X year; and state X year). This specification cleanly isolates the treatment effect of *Shelby*, which in this case is a significant *increase* in relative Black voter turnout after the removal of pre-clearance.

To explain this counter-intuitive 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 increase in relative Black turnout reported in Raze (2022) is implausibly large (e.g., 4 percentage points; 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 and counterintuitive 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*.

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 census-block level turnout rates *conditional on registration* and analyze the change in this measure of turnout in predominantly Black or Hispanic versus other blocks before and after *Shelby* (from 2006-2018). They find that census blocks with more than about 50% Black or 50% Hispanic population realized a nearly 1% drop in voter turnout among registered voters post-*Shelby*. Billings et al (2024) caution that inferring from this that minority turnout decreased as a consequence of *Shelby* is subject to the ecological fallacy, but several other caveats are in order, as well.

First, as noted by Billings et al (2024), they observe only voters registered in 2020 and so can analyze only those individuals' voting history (back to 2006); this omits any individuals that were registered prior to 2020 but removed from the state voter file (e.g. due to moving or non-voting). In addition, most states do not record race or ethnicity in voter files, leaving the authors' to rely on imputed values provided by their data vendor (L2) for these key variables. Moreover, some of the relevant action in state election administration post-*Shelby* may primarily impact unregistered voters as of 2020 (e.g.,

[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.

prior purging voter rolls or restrictions on same-day voting registration); for this reason, evaluations 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, the census block characteristics employed by Billings et al (2024) are based on the 2010 Census and assumed fixed in subsequent years. This is a concern if such characteristics change at different rates over time for different groups or geographies. Also, any blocks with more than 50% minority voters combined, but not more than 50% Black or 50% Hispanic, are lumped in with the control blocks in their analysis. The use of Census block-level data for measuring relative turnout rates by minority race and Hispanic ethnicity is also problematic given the use of "disclosure avoidance systems" (DAS) by the U.S. Census Bureau; these privacy-protection algorithms may introduce substantial errors in counts for minority groups and/or geographies with small populations (Kenny et al 2024).

Third, like Ang (2019) the authors examine only the subset of explicitly covered jurisdictions and ignore implicit state coverage. This oversight is particularly problematic because Billings et al (2024) analyze census blocks only in states that are explicitly covered and their bordering states. The authors then characterize as the treatment group counties in explicitly covered states and explicitly covered counties in implicitly covered states, while all other counties – including those in implicitly covered states like California and Florida are all treated as part of the 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) conduct the most recently published evaluation of the treatment effect of Shelby on minority voting. These authors examine repeated cross-sections of state voter files and construct county-level turnout differences in Black versus White (and Hispanic versus White) turnout rates based on annual ACS estimates of county-level citizen-voting age population (CVAP) for each year from 2008-2020. As with Billings et al (2024), this approach necessitates employing their data vendor's (Catalist) imputed race and ethnicity for individual observations in state voter files. Komisarchik and White (2025) then estimate difference-in-difference models using the county-level Black-White (or Hispanic-White) voting gap as their dependent variable. The authors report a statistically insignificant and small (less than 1 percentage point) decrease in relative Black registration and turnout. [20] However, after controlling for year-by-state interactions, these estimates become positive, large (on the order of 2-3 percentage points), and statistically significant. And while the inclusion of year-by-state effects is appropriate since statewide elections or presidential battleground contests in a given election year impact turnout differently across states in any given year, Komisarchik and White (2025) downplay these findings. Instead, they speculate that racist election officials may need to experiment with policies for suppressing minority votes, so that four election cycles is not enough time for the long-term impact of *Shelby* to be manifest.

Finally, Komisarchik and White (2025), like Ang (2019) and Billings et al (2024) before them, do not account in their analysis for the fact that uncovered counties in implicitly covered states are different from counties in states not covered in any manner. Also, because Komisarchik and White (2025) analyze county-level observations, they can only control for year and county fixed effects, but not lower-order

[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.

interactions of race, county, and year (nor do they control for all lower order interactions of race, state, and year in any specifications). Consequently, Komisarchik and White (2025) also do not cleanly identify the triple difference of interest, unlike Raze (2022).

In a new working paper, Morris and Miller (2025) also 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 consider implicitly covered states in their analysis; and this study stands out for its focus on county election administration. Overall, Morris and Miller (2025) report that the Black and minority voting gap grew larger in formerly covered *counties* post-*Shelby*, but they also find no significant impact from the removal of preclearance of *state* election administration post-*Shelby*. However, Morris and Miller (2025) do not employ county population weights despite working with aggregate county-level data;[22] and they do not control for lower order interactions of race-state-year (and cannot control for lower order interactions of race-county-year due to analyzing aggregate data). So for example, time-varying and state-specific variations in turnout due to statewide elections, controversial state ballot measures, or presidential battleground contests are potential unobserved confounders. Finally, as with all of the prior studies above, these authors do not control for party identification or partisan electoral tides. Consequently, this study also does not cleanly identify the triple difference of interest.

Setting aside methodological quibbles, the mixed findings across these studies do not appear to bear out the worst fears of critics of the *Shelby* decision, at least as it pertains to state election reforms (also see, Grimmer and Hersh 2024; and Malmberg 2025). This is consistent with other recent evidence regarding the effects of specific state election reforms, such as voter identification laws, on minority turnout and registration (e.g., Cantoni and Pons 2021; and Grimmer and Hersh 2024). But it is also true that the studies reviewed here leave several loose ends and do not include recent elections, so 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 of *Shelby* that cleanly identifies the treatment effect of removing federal preclearance of state election practices in a fully specified triple difference model employing individual-level observations. However, Raze (2022) is limited to examining just three general elections post-*Shelby*. Our analysis follows Raze (2022) in conceptualizing the treatment effect of Shelby at the state-level and analyzing individual-level data in a full-specified triple difference model. However, we have the benefit of including twice as many general elections post-*Shelby* (and unlike Raze 2022, we omit non-citizens from our analysis).

Also of note, none of the previous evaluation studies of *Shelby* include controls for partisan tides affecting voter turnout across elections (i.e., party X year), despite the well-established central importance of partisanship as a motivator of individual turnout decisions and party competition as a driver of voter mobilization efforts (e.g., Downs 1957, Fiorina 1976; Aldrich 1993, 1995; and Green and

---

[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 Bayesian Improved Surname Geocoding (Imai and Khanna, 2016). However, this method utilizes race and ethnicity characteristics of Census blocks, which may be problematic give changing data privacy methods employed by the Census (Kenny et al. 2024).

[22] Komisarchik and White (2025) state that the differing findings in Morris and Miller (2025) are artifacts of not weighting by county population.

Gerber 2023). Our subsequent findings illustrate the importance of including controls for individual party identification and partisan tides across election years when analyzing disparities in voter turnout.

Previous evaluation studies have also ignored another potentially important aspect of partisan politics with respect to state election administration. When the *Shelby* decision was announced in 2013, all of the explicitly covered states had state governments with unified Republican control of both chambers of the state legislature and the governorship.[23] And with the exception of California and New York, this was also true of the implicitly covered states at that time, as well. 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 Shelby 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 that has been offered for the apparent muted effect of removing federal preclearance on minority voting and registration is that civil rights groups and political campaigns may engage in targeted and extraordinary voter mobilization efforts that generate countervailing increases in turnout and registration among minority voters in the wake of *Shelby* (e.g., Raze 2022; Billings et al 2024). And while it is conceptually plausible that targeted in-person get-out-the-vote appeals could have such an effect (Green and Gerber 2019), systematic evidence of sufficiently large-scale countermobilization in the wake of *Shelby* is lacking.

Komisarchik and White (2025) employ survey data from the Cooperative Election Study to test whether there is a treatment effect from *Shelby* on minority respondents reporting contact by political groups; however, they find only small (albeit positive) and insignificant effects of *Shelby* on reported contact. This is also consistent with evidence regarding 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 organized efforts of outside groups.

In general, election administration procedures may necessitate some tradeoff between ease of voting and perceptions of election security (Ferroni and Milyo 2025). For example, voting-by-mail may lower the hassle cost of voting, but it also creates breaks in the chain-of-custody for ballots. Similarly, while strict voter identification laws increase the hassle costs for some voters, strict voter identification laws also improve public confidence in the integrity of elections (Endres and Panagopoulos 2021; and Milyo 2025). So, to the extent *Shelby* permits states to adopt reforms aimed at improving election security, and to the extent increased public confidence results in higher turnout, this may increase confidence in elections and hence turnout. For example, Citrin et al. (2014) conduct large scale field experiments and find that informing voters about a new state voter identification law increased turnout in heavily Black precincts in Virginia and Tennessee (also, see Bright et al 2017; and 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*

[23] In 2013, the state senate in Virginia had an equal number of seats held by Democrats and Republicans, but tie-breaking votes in the senate were cast by the Republican Lieutenant Governor.

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 the reported intention to vote among minority respondents. However, for this backlash effect from field experiments is mixed (Biggers 2021; and Biggers and Smith 2020). Even so, the potential for countermobilization, whether generated by outside groups or intrinsic motivation (and whether inspired by increased anger or increased confidence), implies that voting reforms in general, and *Shelby* in particular, in theory 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 an otherwise more negative impact of *Shelby* on the minority voting gap. In doing so, we improve over earlier efforts in three ways. First, 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.[24] Second, we examine the subset of survey respondents that accurately report whether they voted or not, reasoning that this subset may be more reliable reporters of other political activities. And finally, we check whether there is any increase in political mobilization among *verified voters*, since the relevant concern is whether countermobilization generates a countervailing increase in turnout.

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. 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 two waves immediately before and after the general election.[25] The CES is therefore 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 is checked against state administrative records. All respondents in even years are subsequently checked against state voter files to confirm whether they are registered to vote and if they indeed voted in the most recent November general election. This "validated 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).[26] 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).

---

[24] For expositional convenience, we also combine these three measures into a single "political mobilization index"; however, we report results for each measure in the Appendix.

[25] 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 2022, 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.

[26] 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.

However, a caveat regarding the CES validated vote data is in order: there may be errors in the matching process for determining whether a given survey respondent actually voted, which would mean that some non-validated voters may be misclassified as 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).[27] 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 and find similar results.[28]

In order 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 vs. non-covered states, before and after *Shelby*. We also examine DDD models with minority respondents broken out into Black, Hispanic, and other race. After omitting non-citizens and non-state residents,[29] we observe over 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 is premised on the assumption that turnout in the control and treated groups evolves similarly pre-treatment. We confirm the plausibility of this assumption by testing whether differential year-effects for the ever-treated versus control states are jointly significant in the pre-treatment period. As is apparent from the event study figures presented below, in no case are these pre-treatment differentials jointly significant.

Like all the previous evaluation studies reviewed above, we do not include controls for differences in state election institutions over time, as these are endogenous; instead, we examine the reduced-form (intent-to-treat) effect of *Shelby* on the minority voting gap. And because this is a mixed-level analysis estimating the effects of state-level treatment (removal of pre-clearance) on individual-level voting outcomes, all standard errors are adjusted for clustering at the state-level.

As noted, our approach is similar to Raze (2022), who also examines data from the CES, albeit only through 2018. Unlike Raze (2022), 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 that failure to include

---

[27] North Dakota does not require registration to vote, so we code all residents of North Dakota as registered to vote when analyzing voter registration. Alternatively, dropping North Dakota from our analysis has no appreciable impact on reported results.

[28] Another drawback of using validated voter data from the CES is that there are not enough observations within counties to reliably estimate models with county-fixed effects (and all-the-more-so interactions of race-year-county). 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. Moreover, there are on average fewer than six minority respondents per-county per-year in the full sample. Consequently, the CES validated voter data are not appropriate for analyzing county-level treatments in a fully specified triple difference model. For this reason, we focus 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.

[29] 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, as well.

these pair-wise interactions (e.g., state X year) does influence the estimated effect of *Shelby* on minority turnout. However, because we include these lower order interactions in all models, we cannot separately identify other effects of potential interest in these models, such as the impact of *Shelby* on overall voter turnout in treated states.

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. However, in general, there is no substantive difference in our findings whether we examine only explicitly covered states or all covered states. Nevertheless, we present results for both interpretations of the treatment, since explicitly covered states are covered for historical circumstances not shared by other "implicitly" covered states (i.e., past state-level discriminatory practices). We also demonstrate that our findings are robust to including differentials for covered counties in implicitly covered states, or to restricting the treatment effect to Republican-controlled ("red") states in 2013, just before *Shelby*.

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 turnout conditional on registration. We then check whether the states that would be covered by the proposed John Lewis VRAA have undergone a different treatment post-*Shelby*; this counterfactual is informative for understanding whether the proposed John Lewis VRAA "fix" to the VRA better targets states that have had retrogression in minority voting than did the VRA pre-*Shelby*.[30]

Finally, we employ the same model specifications to test the countermobilization hypothesis (i.e, just swapping out 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 indicator for organized mobilization efforts aimed at the respondent (e.g., Cantoni and Pons 2021; and Komisarchik and White 2025). However, this question was not asked in the 2008 or 2018 versions of the CES. Moreover, political mobilization may also spring from more intrinsic motivation. For this reason, we utilize two additional proxies for political mobilization and (for convenience) combine all three variables into a single index of political mobilization.

In every year, the CES includes a question on respondents' interest in in public affairs and political news on a four-point scale.[31] And in every federal election year, respondents also report their political activities other than voting; we construct a count of four "other" political activities that were asked of respondents in every election year: 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 of political mobilization separately in the Appendix, but because our findings are very similar for each, in the text below, we simply present our analysis for a simple "political mobilization index," which is just the mean of these three measures.

---

[30] Barber (2019) identifies states that would be covered by the John Lewis VRAA.

[31] "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)."

In addition to examining multiple measures of political mobilization, we depart from earlier efforts to examine countermobilization in two important ways. First, these measures of political mobilization 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 accurately self-report whether or not they voted (based on the verified vote). Second, we improve on earlier efforts by recognizing that in order for political mobilization to be a driving force behind increased turnout, mobilization measures should increase among those individuals that actually voted. Consequently, we examine the effects of *Shelby* on political mobilization for four different samples of CES respondents: all respondents; accurate reporters; verified voters; and both accurate reporters and verified 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;[32] the format of this table is replicated when we examine other dependent variables (e.g., registration and political mobilization) below. The top panel of Table 1 displays the coefficient of interest when the treatment is characterized as only the removal of preclearance in explicitly covered states, while the second panel includes all formerly covered states in the treatment group. Within each panel, we report estimates for four nested specifications (corresponding to columns 1-4) where successive models include additional 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 impacts our findings.

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 this effect is statistically insignificant in specifications with added controls (including our preferred model). When considering all covered states as the treated group, the positive effect of *Shelby* on minority turnout is a bit larger, but in no case is there a statistically significant differential effect in explicitly covered states beyond that in all covered states. Of course, these null findings do not rule out the presence of very small negative treatment effects on minority turnout, but neither can they rule out even larger positive effects. Regardless, the dire consequences predicted by critics of *Shelby* are simply not manifest in our analysis of voter turnout in the decade since the Court's decision.

Event Study for Minority Voting Turnout

As noted above, the triple difference estimation strategy is predicated on there being parallel trends in the treated and controls states prior to *Shelby*. Consequently, we conduct an event study analysis for both conceptualizations of the treated group; Figure 3 shows the differential effect for minorities in treated states for each year before and after *Shelby*. Not only is there no obvious pre-trend apparent in Figure 3, we also fail to reject the null hypothesis that the differential year effects for minority turnout in

[32] Table B1 in the Appendix reports the "triple difference" estimates of interest when lower order interactions are not included in the regression models (as in some prior studies). In short, including lower order interaction terms changes the sign of the estimated coefficient of interest.

treated states pre-treatment are jointly zero. Figure 3 also demonstrates that there is no obvious trend in the post-treatment effect, as well.

Differential Effects for Covered Counties in Implicitly Covered States

One concern raised by Morris and Miller (2025) is whether the relevant treatment effect is at the county-level. Unfortunately, we do not have sufficient observations to estimate the triple-difference with counties as the treatment unit; however, we can estimate the pooled and differential treatment for covered counties in implicitly covered state (see Table B2 in the Appendix).[33] We find no evidence of any significant differential effect on minority turnout for respondents in a covered county (versus an uncovered county) in an implicitly covered state.

Treatment in Red States

An additional concern that we raise with respect to all prior analyses is whether the treatment effect of Shelby is manifest only in red states. This suggests limiting the treatment group to states controlled by Republicans in 2013, when *Shelby* was decided. However, all of the explicitly covered states were controlled by Republicans in 2013, so the estimates in Panel 1 of Table 1 already show the treatment effect for red states. For the specifications that include all covered states (Panel 2), only California and New York were not red states in 2013. Consequently, we re-examine the treatment effect on the minority voting gap after dropping observations from California and New York. This has a negligible impact on our findings (see Table B3 in the Appendix).

Treatment Effects by Race/Ethnicity

Our analysis to this point has lumped all minority respondents into one group. Table 2 reports 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 now fairly large and statistically significant for Other Race, indicating an *increase* in the probability of voting of around 2-3 percentage points. Once again, findings are similar across both panels; and not surprisingly, we cannot reject the null of an identical treatment effects in explicit states versus all states.[34] Event studies for each race/ethnicity are found in the Appendix (Figures B1-B3). These figures also make clear the absence of any significant pre- or post-treatment trends (and again, we cannot reject the null of identical pre-treatment year effects).

Treatment Effects on Registration and Turnout among Registered Voters

We estimated the treatment effect of *Shelby* on verified minority voter registration in DDD models of the same sort we employed for verified minority 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 voter turnout, the treatment effect on registration is largest for Other Race (Table B5). Given the dearth of large, negative, or significant treatment effects for both verified turnout and verified registration, it should be no surprise that we observe very similar null

[33] The coefficient of interest in Table B2 is “Minority x Covered County in an Implicit State x Post-Shelby”).
[34] Results are very similar when we restrict the treatment group to red states (see Panel 2 of Table B3 in the Appendix).

effects for the treatment effect of Shelby on minority turnout among registered voters, as well (see Tables B6 and B7 in the Appendix).

Counterfactual Analysis of the Proposed John R. Lewis VRAA

The proposed John R. Lewis VRAA includes a "fix" to the coverage formula voided in *Shelby*; however the set of states that would be covered by this proposal are a subset of most of the covered states prior to *Shelby* (Figure 3). We investigate whether the John R. Lewis Act better targets states that have retrogressed with respect to minority voting or voter registration by re-estimating our triple difference models but substituting the covered states in the proposed John R. Lewis Act for the treatment group. This counterfactual analysis will indicate that the proposed fix to the coverage formula is better targeted than the VRA just prior to *Shelby* if we observe significant and negative "treatment" effects.

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

Treatment Effects on Political Mobilization

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 some studies find modest evidence of increased contact from political candidates and campaigns among minorities as a result of Shelby, the magnitude of these estimated effects are substantively small (Komisarchik and White 2025; Grimmer and Hersch 2024).

In contrast to these studies, we examine three different 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 (see Table 4). Consequently, there is some corroborating evidence consistent with the premise of the countermobilization hypothesis, that increased mobilization is at least associated with increased voter turnout.

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 these separate analyses yield substantively similar results, we report here the results of interest for a "political mobilization index," which is simply the mean of these three measures.

Table 5 shows the estimated coefficients of interest for this index for our preferred specification, but for four different samples of respondents. At least for all covered states, we find small, positive, and significant effects of *Shelby* on political mobilization when examining the full sample of respondents or even for accurate reporters (see columns 1 and 2 of Table 5). However, we find no significant effects among verified voters (the event study for the model in column 4 is shown in Figure C1). Moreover, the estimated coefficients of interest are quite small relative to the standard deviation of the political mobilization index. This pattern of findings is very similar when we break out the treatment effect by race/ethnicity, as well (Table 6). The absence of sizeable or significant treatment effects of Shelby on minority political mobilization among *voters* is strongly contradictory to the countermobilization hypothesis.

5. Discussion

Our examination of the treatment effect of *Shelby* on voter turnout, registration, and political mobilization has generated no evidence that the removal of federal preclearance of state election administration has had any detrimental effects for minorities in previously covered states. In most instances, our point estimates run in the opposite direction and instead suggest small improvements in minority turnout and voter registration in treated states post-*Shelby*. We also show that revised federal preclearance formula in the proposed John Lewis Voting Rights Advancement Act does not better target states where preclearance did make a difference prior to *Shelby.* Finally, we find no evidence consistent with confounding effects from countermobilization of minority voters.

Our analysis has examined more years than prior studies and includes more individual covariates than prior studies. We also examine more indicators of political mobilization than prior studies. In addition, our analysis is the first to control for the influences of political parties on either turnout, registration, political mobilization, or even the effectively treated group of states. Our findings are also remarkably consistent across a variety of model specifications and robustness checks.

References:

Aldrich, John H. 1993. “Rational Choice and Turnout,” *American Journal of Political Science*, 37: 246-78.

Aldrich, John H. 1995. *Why Parties? The Origin and Transformation of Political Parties in America* (University of Chicago Press; Chicago, IL).

Ang, Desmond. 2019. ”Do 40-Year-Old Facts Still Matter? Long-Run effects of Federal Oversight Under the Voting Rights Act." *American Economic Journal: Applied Economic*s, 11(3): 1-53.

Ansolabehere, Stephen and Brian Schaffner. 2017. “Cooperative Election Study: Guide for the 2014 Vote Validation Data,” Available at the Harvard Dataverse: https://dataverse.harvard.edu/file.xhtml?persistentId=doi:10.7910/DVN/XFXJVY/ECKEP7&version=6.0.

Ansolabehere, Stephen, Bernard Fraga, and Brian Schaffner. 2022. “The Current Population Survey Voting and Registration Supplement Overstates Minority Turnout,” *Journal of Politics*, 84(3): 1850-1855.

Ansolabehere, Stephen and Eitan Hersh. 2012. “Validation: What Big Data Reveal About Survey Misreporting and the Real Electorate." *Political Analysis*, 20: 437-459.

Baker, Andrew C., David F. Larcker, and Charles C.Y. Wang 2022. “How Much Should We Trust Staggered Difference-in-Difference Estimates?” *Journal of Financial Economics*, 144: 370-395.

Barber, Benjamin. 2019. “The States Facing Federal Preclearance Under Proposed Voting Rights Act Fix,” Facing South (available at: https://www.facingsouth.org/2019/03/states-facing-federal-preclearance-under-proposed-voting-rights-act-fix; last viewed January 6th, 2026).

Berman, Ari. 2021. “Eight Years Ago, the Supreme Court Gutted the Voting Rights Act. Widespread Voter Suppression Resulted.” *Mother Jones* (https://www.motherjones.com/politics/2021/06/eight-years-ago-the-supreme-court-gutted-the-voting-rights-act-widespread-voter-suppression-resulted/; viewed March 1, 2023).

Bernini, Andrea, Giovanni Facchini, and Cecilia Testa. 2023. “Race, Representation, and Local Governments in the US South: The Effect of the Voting Rights Act,” *Journal of Political Economy*, 131(4): 994‑1056.

Biggers, Daniel R. 2021. “Can the Backlash Against Voter ID Laws Activate Minority Voters? Experimental Evidence Examining Voter Mobilization Through Psychological Reactance,” *Political Behavior*, 43:1161-1179.

Biggers, Daniel R. and Daniel A. Smith. 2020. “Does Threatening Their Franchise Make Registered Voters More Likely to Participate? Evidence from an Aborted Voter Purge,” *British Journal of Political Science*, 50: 933-954.

Billings, Stephen B., Noah Braun, Daniel B. Jones, and Ying Shi. 2024. “Disparate Racial Impacts of *Shelby County v. Holder* on Voter Turnout,” *Journal of Public Economics*, 230.

Bright, Chelsie L. M. and Michael S. Lynch. 2017. “Kansas Voter ID Laws: Advertising and Its Effects on Turnout,” *Political Research Quarterly*, 70(2): 340-347.

Cantoni, Enrico and Vincent Pons. 2021. "Strict ID Laws Don't Stop Voters: Evidence from a U.S. Nationwide Panel, 2008–2018." *Quarterly Journal of Economics*, 136: 2615-2660.

Cascio, Elizabeth U. and Ebonya Washington. 2014. "Valuing the Vote: The Redistribution of Voting Rights and State Funds Following the Voting Rights Act of 1965," *Quarterly Journal of Economics*, 129 (1): 379‑433.

Citrin, J., D. P. Green, and L. Morris. 2014. "The Effects of Voter ID Notification on Voter Turnout: Results from a Large-Scale Field Experience." *Election Law Journal: Rules, Politics, and Policy*, 13(2): 228–242.

Conovoy, William and Thomas McCarthy. 2013. "Shelby County v Holder: The Restoration of Constitutional Order." *The Cato Supreme Court Review* 2012-2013, pp. 31-62.

Downs, Anthony. 1957. *An Economic Theory of Democracy* (Harper and Row: New York, NY).

Endres, Kyle and Costas Panagopoulos. 2021. "Photo Identification Laws and Perceptions of Electoral Fraud," *Research and Politics*, July-September 2021: 1-7.

Endres, Kyle and Costas Panagopoulos. 2023. "Who is Mobilized to Vote by Information about Voter ID Laws?" *Politics, Groups, and Identities*, 11(1): 143-157.

Ferroni, Matteo and Jeffrey Milyo. 2025. "A Dynamic Cost of Voting Index: How Do State Election Administration Rules Impact Voter Turnout, the Racial Gap in Turnout, and Public Perceptions of the Integrity of Elections?" Manuscript (University of Missouri).

Feder, Catalina and Michael G. Miller. 2020 "Voter Purges After *Shelby*," *American Politics Research*, 48(6): 687-692.

"Fighting Voter Suppression," n.d. League of Women Voters (https://www.lwv.org/voting-rights/fighting-voter-suppression; last viewed June 1st, 2025)

Fiorina, Morris P. 1976. "The Voting Decision: Instrumental and Expressive Aspects," *Journal of Politics*, 38: 390-415.

Fraga, Bernard. 2018. *The Turnout Gap: Race, Ethnicity, and Political Inequality in a Diversifying America*. Cambridge University Press (Cambridge, UK).

Green, Donald P. and Alan S. Gerber. 2023. *Get Out the Vote: How to Increase Voter Turnout*; 5th edition (Brookings Institution Press; Washington, D.C.)

Gerken, Heather. 2013 "Goodbye to the Crown Jewel of the Civil Rights Movement," Slate (https://slate.com/news-and-politics/2013/06/supreme-court-and-the-voting-rights-act-goodbye-to-section-5.html; viewed March 1st, 2023).

Gibson, Nadine Suzanne. 2020. "Moving Forward or Backsliding: A Causal Inference Analysis of the Effects of the Shelby Decision in North Carolina," *American Politics Research*, 48(5): 649-662.

Grimmer, Justin and Eitan Hersh. 2024. "How Election Rules Affect Who Wins," *Journal of Legal Analysis*, 2024, 16 (1), 1–25.

Grimmer, Justin, Eitan Hersh, Marc Meredith, Jonathan Mummolo and Clayton Nall. 2018. "Obstacles to Estimating Voter ID Laws' Effect on Turnout." *The Journal of Politics*, 80(3): 1045-1051.

Grofman, Bernard and Lisa Handley. 1998. "Voting Rights in the 1990s: An Overview," in *Race and Redistricting in the 1990s*, Ed. By Bernard Grofman (Agathon Press, New York, NY); pp. 69‑79.

Grofman, Bernard, Lisa Handley, and Richard G. Niemi. 1992. *Minority Representation and the Quest for Voting Equality* (Cambridge University Press: New York, NY).

Highton, Benjamin 2017. "Voter Identification Laws and Turnout in the United States." *Annual Review of Political Science*, 20: 149-167.

Hoekstra, Mark and Vijetha Koppa. 2021. "(Almost) No One Votes Without ID, Even When They Can." *Economics Letters*, 2021, 205: 1-3.

"How Voting Laws Have Changed Since the Supreme court Weakened the Voting rights Act," 2025. Common Cause (https://www.commoncause.org/articles/how-voting-laws-have-changed-since-the-supreme-court-weakened-the-voting-rights-act/; last viewed June 1st, 2025).

"How Shelby County v. Holder Broke Democracy" Legal Defense Fund (https://www.naacpldf.org/shelby-v-holder-impact/; last viewed June 1st, 2025).

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

Katz, Ellen D. 2001. "Federalism, Preclearance, and the Rehnquist Court," *Villanova Law Review*, 46: 1179-1218.

Komisarchik, Mayya and Ariel White. 2025. "Throwing Away the Umbrella: Minority Voting after the Supreme Court's Shelby Decision," *Quarterly Journal of Political Science*, 20(2): 269–305.

Lakin, Sophia Lin and Moly McGrath. 2023. "How We Are Protecting the Right to Vote on the Anniversary of Shelby v. Holder," American Civil Liberties Union (https://www.aclu.org/news/voting-rights/how-we-are-protecting-the-right-to-vote-on-the-anniversary-of-shelby-v-holder; last viewed June 1st, 2025).

Lopez, Tomas. 2014. "`Shelby County': One Year Later." Brennan Center for Justice (https://www.brennancenter.org/our-work/research-reports/shelby-county-one-year-later; viewed March 1, 2025).

Malmberg, Alice. 2025. "Did Changes to the Voting Rights Act Cause Electoral Backsliding in the States?" Election Law Journal, 24(3): 266-276.

Marschall, Melissa J, Anirudh V.S. Ruhil, and Paru R. Shah. 2010. "The New Racial Calculus: Electoral Institutions and Black Representation in Local Legislatures," *American Journal of Political Science*, 54(1): 107–124.

Mendelson, Aaron. 2023. "What the Court Misunderstood Is Just How Fragile Democracy Is," The Center for Public Integrity (https://publicintegrity.org/politics/what-the-court-misunderstood-is-just-how-fragile-our-democracy-is/; last viewed June 1st, 2025).

Morris, Kevin. 2022. "The Impact of Restrictive Voting Legislation." Brennan Center for Justice (https://www.brennancenter.org/our-work/research-reports/impact-restrictive-voting-legislation; viewed March 1, 2025).

Morris, Kevin T. and Coryn Grange. 2024. "Growing Racial Disparities in Voter Turnout, 2008-2022." Brennan Center for Justice (https://www.brennancenter.org/our-work/research-reports/growing-racial-disparities-voter-turnout-2008-2022; last viewed January 2nd, 2026).

Morris, Kevin T. and Michael G. Miller. 2025. Did *Shelby County v. Holder* Increase the Racial Turnout Gap? Manuscript (Barnard College, Columbia University).

Milyo, Jeffrey. 2025. "Does Voter ID Improve Public Confidence in the Integrity of Elections?" Manuscript (University of Missouri).

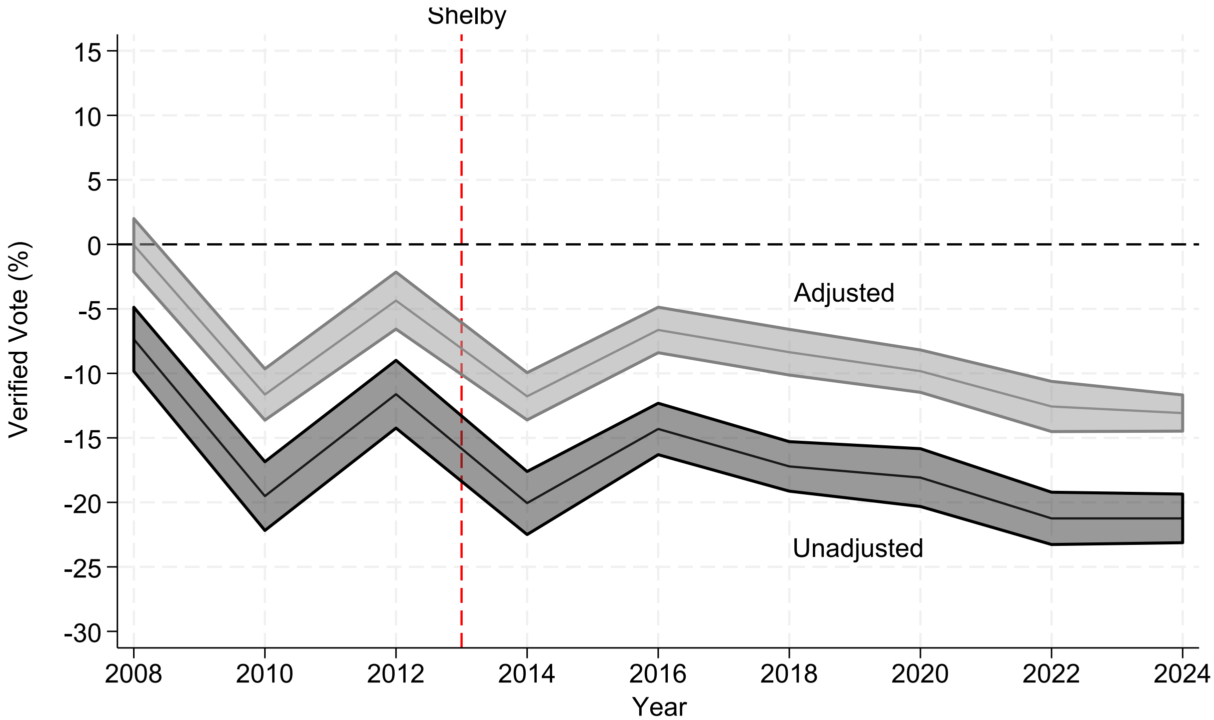
Newkirk, Vann. 2018. "How *Shelby County v. Holder* Broke America." *The Atlantic* (https://www.theatlantic.com/politics/archive/2018/07/how-shelby-county-broke-america/564707/; viewed March 1, 2025).

Perez, Myrna. 2020. "7 Years of Gutting Voting Rights," Brennan Center for Justice (https://www.brennancenter.org/our-work/analysis-opinion/7-years-gutting-voting-rights; last viewed June 1st, 2025).

Singh, Jasleen and Sara Carter. 2023. "States have added nearly 100 Restrictive Laws since SCOTUS Gutted the Voting Rights Act 10 Years Ago," Brennan Center for Justice (https://www.brennancenter.org/our-work/analysis-opinion/states-have-added-nearly-100-restrictive-laws-scotus-gutted-voting-rights; last viewed June 1st, 2025).

Stephanopoulos, Nicholas O., Eric McGhee, and Christopher Warshaw. 2024. "Non-Retrogression Without Law," University of Chicago Legal Forum, 2023: Article 11.

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*

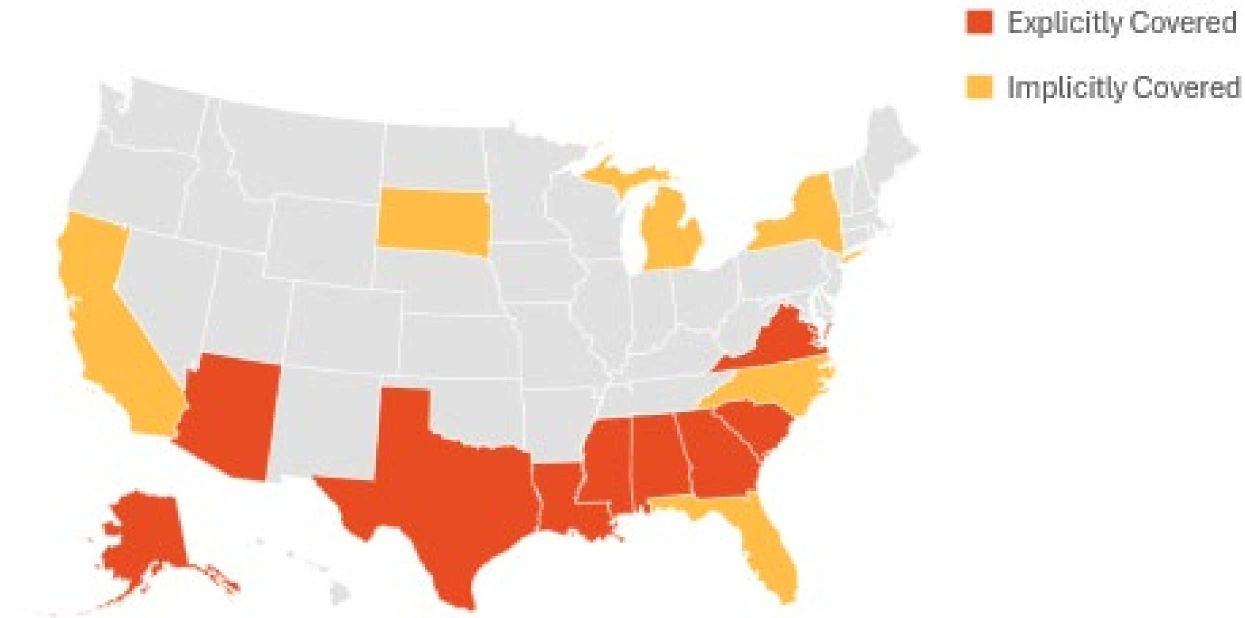
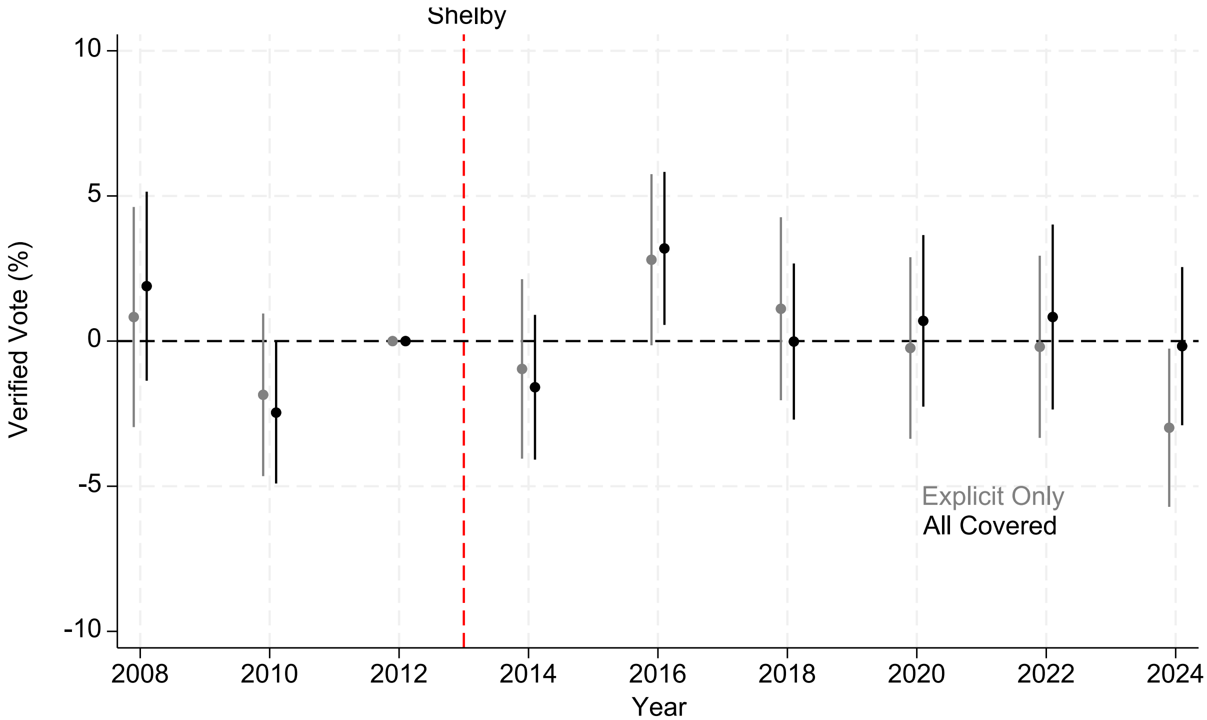


Figure 3: Difference in Minority Voting Gap (%)



Note: Point estimates and 95% confidence intervals; regression model includes all controls listed in Column 4 of Table 1. The estimated year differentials prior to *Shelby* are not jointly significant.

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

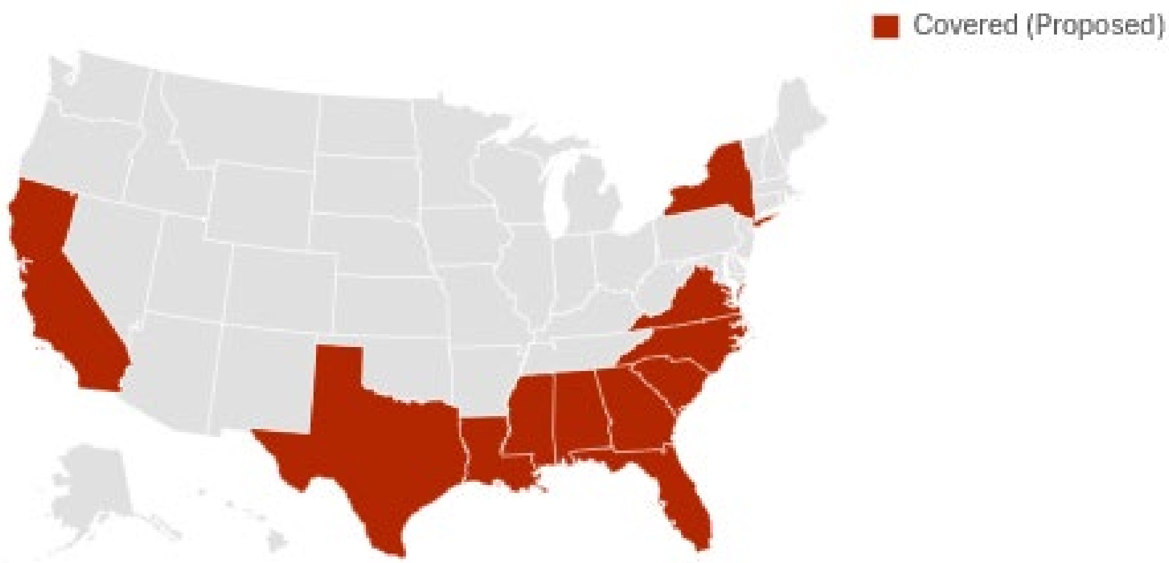


Table 1: Effects of *Shelby* on Minority Voting Gap (%)

	(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 x Explicitly Covered State	0.87	1.03	0.36	0.45
x Post-*Shelby*	(0.93)	(0.83)	(0.81)	(0.86)
Panel 2: All Covered States vs Other States				
Minority x Covered State	2.11**	1.44*	1.24	1.25
x Post-*Shelby*	(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 x 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 x Explicitly Covered State	1.47	1.11	0.36	0.50
x Post-*Shelby*	(1.52)	(1.55)	(1.55)	(1.59)
Hispanic x Explicitly Covered State	-0.57	0.38	-0.05	0.24
x Post-*Shelby*	(0.89)	(0.87)	(0.96)	(0.96)
Other x Explicitly Covered State	2.75*	3.56***	3.39***	2.66**
x Post-*Shelby*	(1.46)	(1.25)	(1.20)	(1.27)
Panel 2: All Covered States vs Other States				
Black x Covered State	1.46	1.02	0.81	0.77
x Post-*Shelby*	(1.55)	(1.33)	(1.34)	(1.36)
Hispanic x Covered State	0.70	0.51	0.40	0.57
x Post-*Shelby*	(1.78)	(1.57)	(1.58)	(1.53)
Other x Covered State	3.39***	2.84***	2.69***	2.33**
x Post-*Shelby*	(1.15)	(0.93)	(0.93)	(0.94)
Included Controls in Each Panel:				
Race, State, Year, and Interactions	Yes	Yes	Yes	Yes
Age, Education, and Sex		Yes	Yes	Yes
Party x 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 R. 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)
Panel 1: Minority Voting Gap				
Minority x Covered State	2.44**	1.42*	1.22	1.14
x Post-*Shelby*	(0.94)	(0.76)	(.80)	(0.81)
Panel 2: Racial/Ethnic Voting Gaps				
Black x Covered State	1.73	1.01	0.82	0.66
x Post-*Shelby*	(1.51)	(1.33)	(1.34)	(1.35)
Hispanic x Covered State	1.09	0.46	0.38	0.42
x Post-*Shelby*	(1.56)	(1.36)	(1.37)	(1.33)
Other x Covered State	4.20***	3.28***	3.14***	2.91***
x Post-*Shelby*	(0.96)	(0.92)	(0.90)	(0.89)
Included Controls in Each Panel:				
Race, State, Year, and Interactions	Yes	Yes	Yes	Yes
Age, Education, and Sex		Yes	Yes	Yes
Party x 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 x 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 x Explicitly Covered State	0.01	0.00	-0.00	-0.00
x Post-*Shelby*	(0.01)	(0.01)	(0.01)	(0.01)
Panel 2: All Covered States vs Other States				
Minority x Covered State	0.01***	0.02***	0.01	0.01
x Post-*Shelby*	(0.00)	(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 x Year			Yes	Yes
Other Demographic Characteristics				Yes
Observations	327,999	242,265	212,333	210,456
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. 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 (1)	Accurate Reporter (2)	Verified Voter (3)	Accurate & Verified (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 x Explicitly Covered State	0.01	-0.00	-0.01	-0.01
x Post-*Shelby*	(0.01)	(0.01)	(0.01)	(0.01)
Hispanic x Explicitly Covered State	0.01	0.01*	0.00	0.00
x Post-*Shelby*	(0.01)	(0.01)	(0.01)	(0.01)
Other x Explicitly Covered State	0.01	0.00	0.01	0.01
x Post-*Shelby*	(0.01)	(0.01)	(0.01)	(0.01)
Panel 2: All Covered States vs Other States				
Black x Covered State	0.01**	0.01	0.00	0.00
x Post-*Shelby*	(0.01)	(0.01)	(0.01)	(0.01)
Hispanic x Covered State	0.01*	0.01	0.00	-0.00
x Post-*Shelby*	(0.01)	(0.01)	(0.01)	(0.01)
Other x Covered State	0.00	0.02**	0.01	0.01
x Post-*Shelby*	(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 x 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.