Shelby Done Right: The End of VRA Preclearance, Countermobilization, and the John Lewis Voting Rights Advancement Act

Brendan Cirillo

September 29, 2025

I examine *Shelby County v. Holder (2013)*, which restored states' ability to change election rules. Through an intention-to-treat framework, I estimate the effect on the black-white and the hispanic-white vote gap using a triple difference-in-differences model and Cooperative Election Study data (2008–2022). In addition to the previously identified states in the VRA, I also examine the impact within a subset of states, potential 'bad actors', as determined by the John Lewis Voting Rights Advancement Act (JLA). Across models and samples, I find no widening of the black-white voting gap post-2013. The states identified in the JLA, results are similar, but there is some evidence of a widening of the hispanic-white gap relative to the hispanic-white gap in control states (4.9pp increase at 10% levels among CES verified individuals). In examining the possibility of countermobilization efforts by political campaigns, campaign contact rates during an election cycle were not significantly different from those in non-treated states or across races.

1 Introduction

Sections 4 and 5 of the Voting Rights Act (VRA) are among the most influential laws for understanding how rules shape political power through political participation, government representation, and the distribution of public resources.[1] These two sections combined to provide federal oversight to voting law changes to states with a history of discrimination based on race or language accessibility.[2] A large literature documents gains for minority communities following the VRA's adoption and extensions, from turnout and registration to officeholding and public spending, followed by a long-run plateau in the 1990s and 2000s for relative participation rates (Ang, 2019; Bernini et al., 2023; Aneja and Avenancio-León, 2019a).

The VRA identified, based on the Section 4 formula, a set of states that would be required to seek Justice Department approval, as outlined in Section 5, before changing any voting laws or rules. The *Shelby County v. Holder* (2013) decision removed the formula in Section 4, on the basis of it being outdated, that had triggered federal preclearance in Section 5. This institutional change was sudden, unexpected, and affected only a handful of states. By removing federal oversight, previously covered jurisdictions had the ability to alter election rules without prior federal approval. That ability creates an intent-to-treat (ITT) environment in which policy changes may differ across states, but the constraint itself shifts starting in 2013. This policy change creates a natural question: are the documented benefits from the VRA's initial implementation at risk of being reversed, or was the formula outdated, as suggested by the Supreme Court?

The expected impact, a priori, could be a decrease in general voting rates if the treated states pass laws or make changes that cause the cost of voting to increase. If new rules raise participation costs disproportionately for black or hispanic voters, the implication is a widening of the black-white and hispanic-white gap. It is also possible that a subset of states are 'bad actors' and that these states could have differential impacts compared to the whole set of states covered by the old Section 4 formula. If there are negative effects, political campaigns would likely try and mobilize their base, perhaps even increasing their efforts if they think that the previously covered areas have new barriers implemented. The potential differential effects across race, the subset of 'bad actors', and the competing forces of voting rule changes and campaign countermobilization are the focus of this paper.

To explore the effects of the repeal of Section 4 of the VRA, this paper largely follows

[1]E.g. Ang (2019); Bernini et al. (2023); Aneja and Avenancio-León (2019a) address the results from and measures of increased political power. Congressional Research Service (2021) shows the historical restructuring and significance of the VRA.

[2]Language accessibility was added during an expansion

the methodology from Raze (2022), while adding political party controls. Using data from 2008 to 2022, I examine both the original VRA-identified states and the possibility of 'bad actors' using individual voting data and survey responses across treated and untreated states. Treated states are states formerly covered by Section 4 of the VRA prior to *Shelby County v. Holder* or potential 'bad actors'. The 'bad actors' are identified using the reported coverage of the proposed John Lewis Voting Rights Advancement Act (JLA). If there are adverse effects, they could be masked by countermobilization efforts by political campaigns. I examine self-reported campaign contact rates, restricting the sample to voters and internally consistent survey respondents, and investigate the possibility of an increased effort. For all outcomes, I examine the differential effects between black and white respondents, or black, hispanic, and white, to analyze the racial disparities that ending preclearance might have. To address the racial disparities, I employ a triple Difference-in-Differences approach to estimate the causal effects.

I find the black-white voting gap does not widen in formerly covered states after 2013, relative to control states, when using the preferred specification with party-by-year fixed effects. Results are stable across the full sample and matched respondents. Relative to Raze (2022), whose estimates show a positive black-specific effect implying a closing of the black-white voting gap relative to control states, my findings suggest null effects once political party-by-year fixed effects are added. I then adopt the JLA-based 'bad actor' treatment and find that the black differential effect estimates remain no different than zero.

For countermobilization efforts, as measured by campaign contact, I find minimal evidence of a countermobilization. While most of the results suggest a null effect, there is evidence of a counter-mobilization effort among the black and hispanic respondents. Once looking at `truthtellers`, the subset of respondents with internally consistent survey responses, in implicitly covered states, black and hispanic respondents show higher contact rates after the *Shelby County v. Holder* decision (4.86% and 6.27%, respectively and significant at the 10% level). The rest of the results suggest no differential impact on minority respondents that would offset an otherwise negative effect. When restricting to the JLA states, there is no statistically significant impact on contact rates after the *Shelby County v. Holder* decision.

This paper contributes to the literature in three ways. First, I look at the subset of states identified by the proposed John Lewis Voting Rights Advancement Act (JLA) as modern 'bad actors'. The JLA provides a subset of states that are plausibly more likely to exploit the restored discretion on the basis that a more modern formula identified them as 'bad actors'. Second, using CES, I restrict the analysis to matched respondents to check the robustness of the results to verifiable survey respondents. This differs from other papers that only treat unmatched respondents as non-voters. Third, I include political party-by-year fixed effects to absorb national annual changes correlated with partisanship and address the change in sampling that occurs in explicitly covered states (Figure **??**).

Taken together, the evidence suggests that removing preclearance did not systematically widen the black-white or the hispanic-white voting gap under the historical coverage definition or the JLA subset. I also find that countermobilization, through campaign contact, does not explain any potential offset for negative impact among black voters. There is some evidence, depending on specification, of such an effort for hispanic voters.

The remainder of the paper is as follows. Section 2 goes into more detail surrounding the VRA and the *Shelby County v. Holder* decision. Section 2.1 synthesizes prior research and clarifies definitions of turnout, treatment, and controls. Section 3 explains the CES data and the methodology of the paper. Section 4 goes over the results, first looking at the impact on voting, checking the robustness of Raze (2022), before looking at the potential impact of 'bad actors' and countermobilization. Section 5 concludes.

2 Background

The 1965 Voting Rights Act (VRA) was called the crown jewel of the civil rights movement (Crayton (2023)). It addressed racial discrimination in voting through federal oversight. Section 4(b) of the VRA established a coverage formula based on the historical use of literacy tests, voter turnout, and voter registration. Section 5 required jurisdictions covered under this formula to obtain federal preclearance before changing voting laws. The original coverage formula covered states as a whole, but as the VRA was extended and expanded, it covered individual counties and townships. If a county or township was covered, the whole state required Justice Department approval to pass a law that would affect the covered jurisdictions. In effect, the whole state was covered by preclearance. The states covered as a whole (explicit states) were Alabama, Alaska, Arizona, Georgia, Louisiana, Mississippi, South Carolina, and Virginia. The states with covered jurisdictions (implicit states) were California, Florida, Michigan, North Carolina, New York, and South Dakota (U.S. Department of Justice, 2023). The implicit states are limited as a whole state if the law or rule change affects a covered jurisdiction.

Section 5 of the VRA was originally set to expire in 1970, but was renewed for 5 years in 1970. In 1975, the act was expanded to include discrimination based on language and was extended for 7 years. The act was extended two more times, in 1982 and 2006, for 25 years. Sections were added that required non-English ballots were made available for non-English speakers. These sections were originally enacted for 7 years in 1982, extended for 15 years in 1992, and extended for 25 years in 2006. All together, the VRA provided federal oversight for townships, counties, and states with a history of racial or linguistic discrimination.

The VRA had profound impacts on the black community in the covered states. The protection of voting rights and the federal consequences from discriminatory practices increased political power for black communities. This political power was a result of an increase in black registration and voting rates (e.g. Grofman and Handley (1998), Cascio and Washington (2014), Ang (2019), Bernini et al. (2023)). Political theory suggests that the elected officials will respond to the electorate. As a result of the increased voting power, black political representation increased. Not only was there an increase in black officeholders (e.g. Marschall et al. (2010), Shah et al. (2013), Bernini et al. (2023)), but there was also a change in state resources and behavior toward the black community. State transfers, which are state spending sent to specific counties or local governments, increased proportionately to the black community size after the VRA (Cascio and Washington, 2014). There was also an increase in local government spending and/or a change in the spending preferences (e.g. Bernini et al. (2023), Chaudhry (2023)). As a result of the increased representation and responsiveness from the state government to the black community's preferences, other outcomes were improved. In areas covered by the VRA, black arrest rates decreased (Facchini et al., 2020), the black community held more public sector jobs, and the black-white wage gap closed (Aneja and Avenancio-León, 2019a).

The positive impacts on the black community are significant and consistent in the literature. However, while the first-order effects on voting increased initially, this progress slowed and eventually leveled off in the 1980s and has remained constant (Ang, 2019). This leveling off has led to a concern that VRA has succeeded, but now imposes a burden on certain states based on an outdated coverage formula. As a result, almost all of the expansions or extensions prompted legal action, some of which made it to the Supreme Court. Following the 2006 expansion, in 2013, the Supreme Court ruled in *Shelby County v. Holder* that the existing Section 4(b) coverage formula was unconstitutional. The Supreme Court did not rule that coverage formulas were unconstitutional. Instead, it ruled that the existing formula from 1965 and subsequent expansions were outdated. This left the door open for Congress to create a new coverage formula. Under the Biden administration, the John Lewis Voting Rights Advancement Act (JLA) was introduced in the House and received in the Senate. Similar to the original formula in the VRA, this new formula covered states with racial disparities in voting. This new formula, presumably, would be allowed under the Supreme Court ruling. The new coverage formula identified these 12 states: Alabama, California, Florida, Georgia, Louisiana, Mississippi, New York, North Carolina, North Dakota, South Carolina, Texas, and Virginia.

Following the Supreme Court ruling on *Shelby County v. Holder*, previously covered states and counties were permitted to change laws without Department of Justice approval. Looking at the three federal election cycles from 2014-2018, which gave states from 2011 to 2018 to pass and implement voting law changes, and still be attributed to the *Shelby County v. Holder* decision[3], six of the fifteen covered states made changes to their voter ID laws. Relative to the 40% of original treated states that changed their voter ID laws (as mentioned above), and half of the JLA states changed their voter ID laws in the three election cycles following the *Shelby County v. Holder* decision. This indicates the JLA states could be the 'bad actors' of the original VRA states. A Brennan Center report found that ten of the fifteen states previously covered by preclearance made some change to their voting laws (Lopez, 2014). Komisarchik et al. (2025) find that previously covered states are more likely to have changed their voter ID laws following *Shelby County v. Holder*. In contrast, only six of the 36 uncovered states (including DC) changed their voting ID laws during that same window. Changing voting laws is not the only way to affect voting rates. A Brennan Center report focused on purges in 2014 and found that Florida, Virginia, and Mississippi all purged their voter rolls in the year following the repeal (Lopez, 2014).

In this paper, I treat the repeal of Section 4 of the VRA as the treatment timing and focus on the state's newfound ability to change its voting laws and rules as the mechanism for change in voter behavior. This intent-to-treat approach allows me to capture other policies (voter roll purges, changing polling locations and number, absentee ballots, early voting, etc.) instead of focusing on a single subset of laws that were passed. Since some view the Shelby decision as the catalyst for voter suppression, with some calling the surge of voting laws 'Jim Crow 2.0' (Caucus, 2021), I focus on the differential effect on the black or the hispanic community, compared to the white community. This differential will help identify the impact on the black-white voting gap between previously covered states and their uncovered counterparts following the repeal of Section 4 of the VRA.

2.1 Literature Review

2.1.1 *Shelby County v. Holder and the VRA*

The *Shelby County v. Holder* decision has been the subject of a growing empirical literature, but existing evidence is mixed and often limited by design choices. There are a few sources of data that could be used to examine the effects on voting as a result of the Supreme Court decision. First, there are national surveys. National surveys, such as the Current Population Survey (CPS), often overreport the voting and registration rates (Ansolabehere et al., 2022). However, the upside is the use of self-reported race and other demographic information that

[3]laws were passed in 2011 and 2012 would have had to have been implemented in 2013 or later

improves the richness of the data. A second source of data is state voter data, provided by private companies such as L2 and Catalist, which track complete voter rolls nationwide. This data is based on state voter rolls and contains some demographic information, but does not include actual race unless a state tracks it. Instead, these companies compute the race variable. The upside is that the voting records are based on state voter data and not on self-reported voting. Lastly, there is the combination of the two, where survey results are combined with voter records to verify voting outcomes. The Cooperative Election Survey is a national survey issued during federal election years that combines the survey responses with Catalist voting rolls. This combines the benefits of the two: reported race and other demographics combined with verified voting records. The downside is smaller samples and the use of weights to approximate national demographics. Any of these can be used for analysis, but doing so requires careful consideration of how voting is defined.

Starting with the voter roll data provided by L2 and Catalist, Billings et al. (2024) examines the effect of the *Shelby County v. Holder* decision. Using L2 data, their outcome variable is defined as the number of votes and registrations divided by the block-level voting age population. The voting data only includes the voter history of a 2020 snapshot of registration records. They then compare blocks with high-black or high-hispanic population shares to those that are almost entirely white, estimating a triple-difference specification in which covered versus uncovered counties serve as the treatment contrast before and after the Supreme Court decision. The block-level analysis shows a 1% decrease in high-hispanic and less than 1% decrease in high-black blocks compared to high-white blocks in the post-*Shelby* period. They extend this analysis using the imputed race variable provided by L2.[4] They find that turnout among registered black voters declines by about one percentage point relative to Whites in the post-*Shelby* period. The results for hispanic voters disappear compared to the block-level analysis.

The design, however, rests on problematic foundations. First, the definition of turnout relies on the registered voters as observed in 2020. Because the data are a snapshot of all registered voters in that year, anyone who had been purged from the rolls, moved, or otherwise left the registration list prior to 2020 is absent. This choice conditions on continued registration and ignores the direct effect of registration restrictions and purges. Therefore, the paper can only speak to turnout conditional on registration in 2020, and cannot distinguish between registration changes driving the effect on voting.

Second, the analysis is geographically restrictive. Rather than using all covered and uncovered counties nationwide, they limit their sample to fully covered southern states and their border states. While this restriction is intended to improve comparability, it has the

[4]Some of the individuals are not imputed since a few states track and report race.

side effect of excluding large numbers of implicitly covered states and explicitly covered counties. The choice of geographic restriction raises questions about the external validity of their results. Taken together, the paper represents an important empirical advance by exploiting the granularity of L2 records and block-level demographics, but its reliance on a 2020 snapshot of registered voters introduces a fatal flaw. As a result, the findings should be interpreted narrowly as changes in turnout among the registered population in 2020, not as evidence on overall participation or access to the ballot due to the end of preclearance.

Using voter roll data and not relying on a 2020 snapshot of registered people, Komisarchik et al. (2025) look at the *Shelby County v. Holder* decision by using Catalist. This individual voter data contains the voting record of all registered voters and their racial identity estimate for all election years. The authors work with Catalist to create an estimate of the number of registered people and voters by race. They then use U.S. Census county-level population data to create voting rate and registration rate by race in each county. These rates are the estimated number of voters/registered people divided by the county-level voting age population by race. They then subtract the white voting/registration rate from each race's voting/registration rate to create, for example, the black-white gap for each county. This is their outcome of interest for voting.

For voting, Komisarchik et al. (2025) use difference-in-differences voting and registration from 2008-2020. Their findings are mixed: some evidence of increased registration, little systematic effect on turnout, but clear increases in voter ID legislation after 2013. Since these are aggregated results instead of individual-level analysis, they are unable to use county-by-year fixed effects as they would in a triple-DID approach. Furthermore, their results for registration and voting are based on estimated race instead of self-reported race. Nevertheless, this work highlights the state-level legislation responses to the end of preclearance and connects it to electoral behavior.

Using the same underlying voter file sources but with methodological improvements, Morris and Miller (2024) analyze nearly one billion individual-level vote records from 2008–2022. Voting is defined directly from administrative records of whether a registered voter cast a ballot, while race is estimated using a modified Bayesian Improved Surname Geocoding procedure that incorporates the citizen voting-age population, allowing them to probabilistically assign race rather than rely on vendor classifications/probabilities. Their outcome is the white–nonwhite and white–black turnout gap at the county level. These gaps are constructed by taking the probability of the individual's race and assigning a proportion of their vote to each race based on that probability. In other words, if someone's probability of being white is 75% and their probability of being black is 25%, their vote is counted as 0.75 white votes and 0.25 black votes. Unlike Komisarchik et al. (2025), they treat uncovered counties in partially covered states as affected by Shelby and weight all counties equally rather than by black population, arguing that this better captures the discretion of local election officials. Their results show a post-Shelby increase in racial turnout gaps, concentrated in counties with prior DOJ objections and in Republican-leaning jurisdictions. This study provides the evidence that *Shelby County v. Holder* widened racial disparities in participation.

Taken together, these three studies illustrate how sensitive Shelby estimates are to data source, race classification, and weighting. Billings et al. (2024) rely on L2's imputed race and a 2020 snapshot of voter rolls, producing block-level turnout comparisons that hinge on continued registration. Komisarchik et al. (2025) instead use Catalist to construct county-level registration and turnout rates by race, weighting counties by black population, and focusing on aggregate gaps. Morris and Miller (2024) depart from both approaches and assign race probabilistically while weighting counties equally and assigning partial votes based on race probabilities. Each design choice carries its own logic, but the result is an ambiguous empirical record, where inferences about whether Shelby widened racial turnout gaps depend heavily on how race, turnout, and the black-white gap are defined.

Using CES data, which is a combination of national survey and Catalist verification, Raze (2022) employs a triple-difference specification using 2008–2018 to study black-white differences in verified vote and registration in counties previously covered by Section 4's formula. Voting is measured using a verified vote variable, which matches the survey results to voter rolls. They report relative increases in black voting rates, concentrated in presidential years. However, the paper includes non-citizens and fails to account for changes in political party enthusiasm and effort over time. Additionally, as reflected in Figure 1, there is a change in party identification heading into treatment, potentially biasing the estimate. Even so, this study is central for establishing CES-based estimates of Shelby's impact. This paper builds upon Raze (2022) and implements several improvements.

Ang (2019) use county-level turnout from 1960–2016 and state-level race-disaggregated turnout from 1968–2016 in long-run difference-in-differences models. They document a decline in overall turnout in 2016 relative to 2012, though concerning pretrends are visible as early as 2004, and no race-specific Shelby estimates are provided. Their contribution is primarily descriptive, situating Shelby within a longer arc of VRA-related participation patterns. Evidence from single states provides different perspectives. Gibson (2020) focuses on North Carolina, which had extensive preclearance coverage, and suggests changes in Democratic primary participation between 2012 and 2016 but little effect on general-election vote share.

Other studies extend beyond voting outcomes. Aneja and Avenancio-León (2019b) apply a triple-difference design to CPS wage data in the public sector. Building on their earlier paper that found a positive impact on wages as a result of the VRA (Aneja and Avenancio-León (2019a)), they check if the VRA benefits were eroded as a result of the *Shelby County v. Holder*. They find suggestive evidence of a decrease in wages, however, most of the point estimates are statistically insignificant and the causal link between voting oversight and labor-market outcomes remains tenuous, post-*Shelby*. Institutional channels have also been explored. Stephanopoulos et al. (2023) evaluate minority 'ability districts', districts where minority candidates usually win, before and after the 2020 redistricting cycle. They find no systematic change, with only three states exhibiting any distinguishable shift. Similarly, Zhang (2024) examines municipal annexations as a potential mechanism for vote dilution. Annexation is absorbing a Census block into a municipality, which can be used to dilute voter power for minority groups. They find no increase in annexations in formerly covered jurisdictions. If anything, annexations diluting black voting power declined after *Shelby* in formerly covered areas. These studies counter more alarmist narratives of 'Jim Crow 2.0' but remain somewhat removed from the immediate questions of turnout and registration.

The literature on voting and downstream effects since *Shelby County v. Holder* highlights both the importance and the challenges of identifying Shelby's consequences. Ambiguous treatment definitions (states versus counties), limited fixed-effects structures, and short temporal windows weaken causal claims. Still, three consistent insights emerge. First, legislatures in formerly covered states enacted voting laws (Komisarchik et al. (2025)), purged voter rolls (Feder and Miller (2020)), and closed polling places (De Rienzo (2022)). Second, institutional outcomes such as redistricting and annexation show less dramatic change than initially feared. Third, the most recent voter-file analyses provide stronger evidence of widening racial turnout gaps, particularly in locally covered and Republican-leaning counties (Morris and Miller (2024)). Yet even this work has limitations: it depends on probabilistic race estimates rather than self-reports and contrasts with other studies reporting null or modest effects (Komisarchik et al. (2025), Billings et al. (2024)). Taken together, the findings remain ambiguous, pointing to real but uneven consequences of Shelby, underscoring the value of designs that integrate survey and administrative data with more precise measures of race and participation. The present study builds on this foundation by county-by-year and party-by-year fixed effects, and uses a combination of voter roll data and national survey data through CES. These refinements address the methodological gaps in prior work and provide a clearer assessment of Shelby's impact on racial disparities in political participation without relying on imputation, block-level demographics, or restricting the controls to southern states.

2.1.2 Mobilization and Countermobilization

There are many ways to measure mobilization efforts and their effects during an election cycle. Some studies capture emotional or psychological responses among voters, others track shifts in confidence or attitudes generated by social media messaging, and others look directly at campaign outreach or records of political participation. Non-traditional measures focus on psychological and informational responses. A line of work emphasizes that anger in response to restrictive laws can itself be mobilizing. Valentino and Neuner (2017) show experimentally that when voter ID laws are framed as racially targeted, respondents, especially Democrats, report stronger anger and greater intention to participate. Biggers (2021) similarly demonstrates an increase in anger from a similar racial presentation about voter ID laws, but these field experiments find limited effects on realized turnout.

Another non-traditional mobilization is online communications. Haenschen et al. (2024) find that exposure to suppression-related tweets without concrete solutions reduces confidence in elections across groups but has no impact on stated vote intent. Green et al. (2022) study Twitter 'superusers' during the 2021 Georgia runoffs and find that those engaging with fraud-affirming content subsequently turned out at lower rates, while engagement with fraud-rejecting content produced only modest positive associations. These findings suggest that while suppression cues can generate anger or alter perceptions of legitimacy, translating those reactions into consistent electoral participation is difficult. Since these effects seem minor, focusing on traditional mobilization efforts might be more fruitful.

Moving from expressive reactions to more direct measures of political activity, other studies examine campaign contact. Countermobilization refers to behavioral and organizational responses that arise when groups perceive attempts to suppress participation. Therefore, rather than reducing engagement, restrictions can sometimes provoke outreach and stimulate voter activity. Countermobilization, then, can partially or fully offset the demobilization effects of restrictive laws. Evidence from the voter ID context illustrates the mechanisms. Cantoni and Pons (2021) show that strict identification laws, while not reducing aggregate turnout, are associated with increases in campaign contact directed toward minority voters. They measure the countermobilization by CES-reported campaign contact. They also created an index of voter activity that incorporates campaign activities (attending a meeting, volunteering, posting a sign, donations, and the size of donations). They argue that this mobilization may have offset negative effects modestly. However, this mobilization effort is not reflected in the voter activity index, suggesting that the contact may not change willingness to be involved in a campaign.

In the *Shelby* context, countermobilization has been studied explicitly. Komisarchik et al.

(2025) focus on the CES national survey response about campaign contact. Using a triple-DID approach, increases in campaign contact were not disproportionately targeted toward minority voters. They do find evidence of an overall increase in contact in areas previously covered by preclearance, which they interpret as suggestive of compensatory outreach. They leave out $Party \times Year$, potentially missing national partisan dynamics in mobilization, and potentially biasing the estimate. Aside from this paper, countermobilization in response to *Shelby County v. Holder* is largely unstudied, leaving open the question of whether mobilization efforts in the wake of the decision were broad enough, targeted enough, or sustained enough to meaningfully offset suppressive changes.

These results, taken together, suggest the limited effect of such efforts. While mobilization may have helped stabilize participation rates in the short term, most estimates imply that the magnitude of these responses is too small to alter electoral outcomes. Grimmer and Hersh (2024), drawing on estimates from Green and Gerber (2015) about the effectiveness of in-person mobilization, calculate that the effect of countermobilization in Arizona in 2020 would have been only about one-tenth of the size necessary to change the election result. This perspective suggests that countermobilization is important for interpreting null average effects in the wake of preclearance repeal, but the effect is likely too small in magnitude to change electoral outcomes. My analysis focuses on verified behavioral outcomes by using survey responses to identify a subset of respondents as `truthtellers`, providing a more precise assessment of the balance between suppression and mobilization in the post-*Shelby* period.

3 Data and Methodology

3.1 Data

To examine the impact of the *Shelby County v. Holder* court decision on voting and campaign contact, I use individual-level survey data from the Cooperative Election Study (CES). The CES is a biennial survey conducted in every federal election year. The sample includes data from 2008 to 2022. The sample is restricted to the survey respondents who are voting-age adults residing in the United States, excluding Washington, D.C. Observations with missing state or racial identity are dropped, as are Virginia respondents in 2008 and 2010 due to missing vote validation data. Following Raze (2020), I include demographic controls for gender, age, state of residency, and race, all self-reported by CES respondents. An advantage of using the CES over administrative data is access to self-identified race rather than relying on Census block-level imputation or race estimates for each individual.

An improvement upon Raze (2020) is excluding non-citizens and incorporating self-reported political ideology. I incorporate party-by-year to control for national differences in voting rates that have a differential effect depending on someone's political . Heading into 2013, the treatment year, there is an increase in respondents identifying as Democrats in fully covered states relative to control and implicitly covered states.[5] While the proportion of Democrats remains flat in the implicitly covered and untreated states, in fully treated states, there is an increase in the proportion of democrats. While the Republican proportion remains parallel, it decreases going into treatment.

The outcome variables of interest are verified vote and campaign contact, both from the CES. Verified vote takes advantage of the CES's vote validation process, in which respondent identities are matched to state files using name, address, and other identifiers. This process is used to construct a validated vote variable and a registered variable. Respondents are coded as either verified voters, verified non-voters, or unverified. For the main specification, following Raze (2020) and others, unverified respondents are coded as non-voters, which is also consistent with one of the recommended practices in the official CES documentation.[6]

Campaign contact is a self-reported binary variable indicating whether the respondent was contacted by a candidate or political campaign during the election cycle. This variable is available for the 2010–2016, 2020, and 2022 waves. When examining campaign contact, I create a variable `truthteller`. Respondents whose self-reported voting and registered responses match their official voting and reregistration record are labeled as `truthteller`. This restriction does two things. First, in addition to specifications that only include verified voters, `truthteller` keeps non-voters and voters in the sample. Second, it focuses on survey respondents who appear to answer the survey most accurately, hopefully reducing measurement error in the outcome variable by giving credibility to this subset of survey respondents.

Treatment is defined at the state level.[7] There are two treatment definitions, one for the states formerly covered by Section 5 (preclearance) at the time of the *Shelby County v. Holder* decision and one for the states identified by the John Lewis Voting Rights Advancement Act. For preclearance treatment states, the states are classified as explicitly covered, implicitly covered, or uncovered under Section 5 of the Voting Rights Act.

Explicitly covered states include those where the entire state was subject to preclearance

[5]Fully covered states are states that were originally identified by the VRA Section 4 formula as needing coverage in 1965 and the expansion in 1975. Implicitly covered states are those that have counties or townships covered, effectively covering the whole state if law changes affect those covered areas.

[6]`https://dataverse.harvard.edu/file.xhtml?fileId=10803737&version=5.0`

[7]County-level results are presented in the appendix but are limited in statistical power (Grimmer et al., 2018).

requirements. These entered in two waves: the original 1965 coverage (Alabama, Georgia, Louisiana, Mississippi, South Carolina) and the 1975 expansion (Alaska, Arizona, Texas). Implicitly covered states had one or more counties or townships covered under preclearance. In these cases, the whole state was still constrained in policymaking if the covered counties/townships were affected by changes to voting procedure (through law or other policy). These include California, Florida, Michigan, New York, North Carolina, and South Dakota.[8] To look at a potential set of 'bad actors', a second treatment definition is based on states identified by the new coverage formula proposed in the JLA. This would re-establish federal preclearance. These states include Alabama, California, Florida, Georgia, Louisiana, Mississippi, New York, North Carolina, South Carolina, Texas, and Virginia. This alternative captures a narrower set of jurisdictions. All treatments started in 2013 following the *Shelby County v. Holder* decision, and each treatment interacts with race and post-2013 indicators in a triple-difference framework.

3.2 Methodology

To estimate the effect of the *Shelby County v. Holder* decision on voting behavior and campaign contact, I use a triple difference-in-differences (triple-DID). This compares outcomes before and after the 2013 decision, across treated and untreated states, and between racial groups. White respondents serve as the control group, following the approach in Raze (2022), and state treatment is defined using two approaches described above: Shelby coverage and John-Lewis targeted states.

The main specification compares the black respondents to the white respondents:

$$Y_{ist} = \beta_1(\text{Post}_t \times \text{Treat}_s \times \text{Black}_i) + X'_{irst}\gamma + \lambda_{st} + \lambda_{rt} + \lambda_{rs} + P_{it} + \epsilon_{irst} \tag{1}$$

where Y_{ist} is the outcome for respondent i, of race r, in state s and year t. Outcomes include verified turnout and self-reported campaign contact. The indicator Post_t equals one in years after 2013, Treat_s indicates whether the respondent resides in a treated state (either preclearance of the subset covered by JLA), and Black_i is a binary variable for racial identity. All regressions include individual-level controls X_{ist} for age, age squared, gender, state of residence, and party (as a lower-order term of the fixed effect described below). Fixed effects λ_{st}, λ_{rt}, and λ_{rs} control for state-year, race-year, and race-state, respectively. The fixed effects absorb the lower-order terms for the triple-DID specification P_{it} represents political party-year fixed effects. Standard errors are clustered at the state level.

[8] In the county-level specification, treatment is coded at the county level for both explicit and implicit coverage.

A treated state is explicitly covered by the Voting Rights Act, implicitly covered by the Voting Rights Act, or identified as a new state to be covered by preclearance in the JLA. The JLA-targeted states isolate a smaller group of jurisdictions potentially identified as persistent violators. This approach may reduce attenuation bias due to previously covered states that are not persistent violators.

The coefficient of interest is β_1, which captures the change in the black-white gap in states that lost federal oversight, relative to the change in the black-white gap in control states. For voting, a positive coefficient would indicate that the black-white gap got smaller in treated states relative to control states, whereas a negative coefficient would indicate a widening of the black-white gap relative to controls. If the removal of preclearance increased restrictions on voting that impacted both racial groups equally, then the expected coefficient would be no different than zero. However, these policies are often accompanied by concerns over racial bias, as suggested by the use of the term 'Jim Crow 2.0'. If this racial bias is happening, the expected coefficient would be negative. For campaign contact, the expected sign of the coefficient is ambiguous. However, if there are negative effects from ending preclearance, then countermobilization might counteract those effects. If that is the case, β_1 would be positive and suggest an increase in countermobilization efforts in these previously covered areas.

The identifying assumption for the triple-difference design is the parallel trends assumption between control and treated states, in the absence of treatment. Specifically, we assume that in the absence of the policy change, the gap between black and white respondents would have been the same across treated and untreated states. This assumption implies that any differential trends in outcomes between races that occur over time are common across treated and control states, absent treatment. In other words, the black-white gap in treatment and control states would have remained parallel without treatment. To provide evidence supporting this assumption, event study analysis examines the coefficient leading up to the 2013 change (Figures 2-4).

The main analysis is conducted on the full sample of eligible respondents and in subsamples restricted to black and white reporting respondents (non-hispanic). Other analyses include a comparison of each race compared to white, and all non-white races grouped together compared to white. I also look at a subsample of just `truthteller` respondents for campaign contact. Lastly, a restriction to just matched and registered respondents repeats the main analysis on the subset of verified individuals. When included, unverified voters are treated as non-voters, following earlier work. Following earlier work, campaign contact is analyzed using the self-reported indicator of outreach by candidates or political parties, which is available from 2010–2016 and from 2020-2022 (Cantoni and Pons (2021), Komisarchik et al.

(2025)).

4 Results

The following section addresses the impact of the *Shelby County v. Holder* decision by looking at the impact of the repeal of preclearance. Section 4.1 looks at the explicitly and implicitly covered states, showing the main specification that contains party-by-year fixed effects and compares it to Raze (2022). Section 4.1 looks at the breakdown between black and white, all races and white, and checks robustness to including only CES matched respondents. Section 4.2 looks at alternative explanations for the results. First, I check whether there are 'bad actors' by looking at the subset of states that would have been covered by the JLA. Second, I check whether countermobilization efforts are present and if those results change among truthtellers and CES-verified voters.

4.1 *Shelby County v. Holder* Decision's Impact on Voting

Following the repeal of preclearance, there were a number of states that changed their voter ID laws, purged voter rolls, and made other election changes. Before examining the racial differences between voting rates post-*Shelby*, I look at the states as a whole and use the CES verified vote variable. Table 1 looks at the impact of the whole state. Panel A examines the *Shelby County v. Holder* and starts with no controls in column (1) and adds fixed effects for race-by-state and race-by-year in column (2), demographic controls in column (3), and party-by-year fixed effects in column (4), mimicking the main specification but in a difference-in-differences framework instead of a triple-DID. As such, these regressions do not include state-by-year fixed effects. These results show that, once adding controls, the repeal of preclearance didn't have a noticeable impact on states as a whole.

As discussed earlier, there is concern about the differential racial impacts that might occur as a result of the *Shelby County v. Holder* decision based on the state histories that led to preclearance coverage. To examine the racial disparities, I start by looking at the estimated difference leading up to treatment. Figure 2, panels A and B show the dynamic triple-DID coefficient of interest, effectively estimating the change in the black-white gap in the treated state relative to the control states. A positive coefficient would be a narrowing of the relative black-white gap, while a negative coefficient would indicate the widening of the black-white gap. These graphs support the identifying assumption by showing there is no evidence of early divergence between the control and either of the treated groups (explicit or implicit).

Table 2 only looks at the black and white voters. Panel A looks at the grouped effect for any treatment under the VRA, and panel B examines the differential impact between implicit and explicit states. Similar to Table 1, each column adds new controls, starting with only lower-order terms and progressing to the full model. These two panels are triple-DID and follow the methodology described in Section 3. State-by-year fixed effects are included in every year column. Column (3) is the main specification for Raze (2022) while column (4) is the main specification for this paper.[9] Statistically, Raze (2022) and the main specification show similar results, but my main model has smaller point estimates. For a complete comparison with Raze (2022), see Section 4.1.1.

The political and economic theory suggests that the laws passed and the changes made would have made the cost of voting higher[10] and lowered the voting rates. In Table 1, we see that there is no evidence of state-wide impacts relative to control states. Political pundits often suggest that these laws disproportionately affect minority communities. This would imply the cost is higher for these communities, or these communities would be more responsive to the changes, leading to a negative coefficient. Column (1) shows that with a negative coefficient. However, once we add controls for race and other demographic controls, there is a switch in the sign. This would go against the theory but would be consistent with Raze (2022). Raze (2022) found significant and positive responses in the black community relative to the white community (which is the same specification as column (3)). However, these results are not robust to one or two additional federal election cycles. Column (4) is the main specification, and there is no statistically significant effect on the black voting rate relative to the white voting rate as a result of the *Shelby County v. Holder* decision.

Next, I add all races and report the impact on black, voters along with hispanic voters.[11] First, looking at the estimates over time, Figure 2 shows evidence for the identifying assumption for both black and hispanic coefficients, as there is no early divergence. For full regression results, Table 3 expands on column (4) from Table 2 in two ways. The first addition is the inclusion of a breakdown of each race in panel B. Black is not the only race that could have been disproportionately affected by the *Shelby County v. Holder* decision. In the main specification (columns (1) and (2)), the results for hispanic are similar to the black and white comparison results. While the coefficient is negative instead of positive, it is still not statistically different than zero at conventional levels.

[9] Raze (2022) included non-citizens, which have been removed for all columns in this paper

[10] E.g. urging voter rolls requires additional effort to re-register, voter ID laws require extra cost to get an ID and keep it up-to-date.

[11] As mentioned in the methodology, other race contains mixed race, asian, native american, middle eastern, and other. As the VRA was implemented for Spanish language barriers and racial disparities, I report hispanic and black results but control for other race by year (grouping all non-white, non-black, and non-hispanic respondents). For full results, see the online appendix.

Columns (3) and (4) from Table 2 limit the sample to matched respondents only. Matched respondents are responses that CES confirms exist based on voter rolls, names, and other identifiers. Rather than assuming all unmatched respondents are non-voters, this specification removes them from the sample. This increases the average voting rate and allows for a comparison of verifiable individuals. When restricting the sample to matched only, the coefficients decrease. From columns (3) and (4), the coefficient for black is smaller. The hispanic group is now more negative but still not statistically different from zero.

Looking at the impact on the black and hispanic voting rates relative to white, these results are statistically no different than zero. There are a number of reasons why this could be the case. First, it could be the case that the laws or other changes made by states are not as impactful as previously suggested. This is the subject of other papers. Second, there could be a subset of previously covered states that are bad actors, and the non-bad actors attenuate the estimates of the effects. Third, there could be campaign efforts that are counteracting the adverse effects of the increased cost of voting. I test the second and third explanations in Section 4.2.

4.1.1 Raze (2022) Apples-to-Apples Comparison

As noted above, I believe political party-by-year is an essential inclusion over other work. Another improvement is removing non-citizens from the sample. First, as shown in Figure 1, there is a non-parallel change in political party identification between treatment groups heading into treatment. Furthermore, there are election-specific effects, such as enthusiasm and engagement, that differ between political parties. The inclusion of political party-by-year fixed effects helps address both the potential identifying assumption issue and the election effects for political party identifiers. Removing non-citizens is an improvement since including them adds non-voters that should not be included in the group of all possible voters. This section examines the extent to which these changes impact the results presented in Raze (2022).

Table 4 includes the original Raze specification in panel A. This includes gender, age, age-squared, and lower-order terms from the difference-in-differences. The sample only includes 2008-2018 data and includes the District of Columbia, which is excluded in the main specification. To test whether the two improvements, political party-by-year and removing non-citizens, make a difference, I compare the original Raze specification to each correction separately and then both corrected together. As seen in Table 2, the positive coefficient found in Raze (2022) is not robust to the 2020 and 2022 elections. In the Online Appendix, I show that the positive coefficient results are not robust to just adding 2020 data either.

In panel B, I only remove non-citizens from the sample. There are roughly 3000 included, and based on their location, they can change the coefficients in either direction. There are some small changes to the coefficients, but the results are robust to removing non-citizens. In panel C, I only add the political party-by-year fixed effects and leave non-citizens in the sample. The grouped coefficient is still positive, which is not reported in Raze (2022), but the coefficient is a bit smaller. However, when breaking out treatment between explicit and implicit coverage, I lose the statistical significance on explicitly covered states. The explicitly covered state coefficient is also smaller than the original specification. Implicit states are still significant and positive at a 5% level and an almost 3 p.p increase in the black voting rates relative to white. In panel D, I make both changes, removing non-citizens and adding political party-by-year fixed effects. The results largely resemble the party-by-year changes, but now the implicit treatment is only significant at a 10% level. The Raze (2022) results do not stand up to both changes and weaken or eliminate the results. However, I cannot rule out the point estimates from Raze (2022), but I want to bring attention to the results and how robust they are to any change in the specification. More data, matched subsample, weighting, and adding party-by-year fixed effects all result in less or no statistical significance. The corrections to the methodology create results that better align with the conventional theory. I explore alternative explanations in the next section, including countermobilization, which could explain a positive coefficient.

4.2 John Lewis Act and Countermobilization

In this section, I explore two alternative explanations for the null main results presented above. A null result does not necessarily mean that there was no impact of the *Shelby County v. Holder* decision on voting rates. One possible explanation is that there is a subgroup of bad actors that were identified in the JLA. As discussed earlier, there was a higher percentage of voter ID law changes in JLA states than in the *Shelby County v. Holder* states. For example, Virginia, Mississippi, and Florida were identified as states that took actions to purge their voter rolls following the decision. All three of these states are covered by the JLA formula.

To check if there is a set of bad actors, I start by looking at the dynamic estimates. Figure 4, panels A and B show the triple-DID coefficient of interest over time. This shows the effect on the black-white and black-hispanic gap in the JLA-identified states. These graphs show evidence of the identifying assumption since there is no deviation before treatment. In 2016, there is evidence of a closing of the black-white gap, similar to Raze (2022), however, all other years are no different than zero. For hispanic, there is suggestive evidence that the gap widened in 2014.

Table 5 shows the grouped estimates and confirms what the dynamic graphs show. For the main result, column (1), there is no evidence of an impact on voting in the JLA states on the black or hispanic communities. When restricting the sample to matched respondents, those who could be verified by CES, the hispanic coefficient shows a widening of the hispanic-white gap of 4.97 p.p. (significant at 10% level) in JLA-covered states after the *Shelby County v. Holder* decision. Other results show a suggestive widening, but this is the first to be significant at conventional levels. These results largely point to the JLA not targeting bad actors effectively, or at least the results for this subset of states are similar to the complete set of *Shelby County v. Holder* states. There is suggestive evidence that the JLA effectively identifies bad actors with respect to adverse effects in the hispanic community.

A second possible explanation is that countermobilization efforts offset the negative impacts. While there are different forms of countermobilization, I focus on direct campaign contact, as reported by survey respondents in the CES. A change in this outcome would suggest an increase in campaign efforts to reach potential voters. To examine this possibility, I start by looking at the dynamic graphs for racial disparities in contact rates between treated and untreated. For *Shelby County v. Holder* states, Figure 3 shows the breakdown between treatment type for hispanic and black. While there is only one year before treatment, the graphs show evidence for the identifying assumption. There is an early deviation for hispanic in implicit states, but not if the treatment types are grouped together. For the JLA states, Figure 4, panels B and C show evidence of the identifying assumption leading into treatment.

All coefficients in the post-treatment period are not statistically different than zero using the 95% confidence interval bands. Focusing on the *Shelby County v. Holder* states, Table 6 has the grouped coefficient. All models are the main specification and have race-by-state, race-by-year, and state-by-year fixed effects, along with the demographic controls. Columns (1) and (2) are the main specifications, with panel A focusing on black and white only and panel B showing all respondents. The coefficients are not statistically different than zero at conventional levels.

Columns (3) and (4) restrict the sample to `truthtellers`, those respondents whose verified registration and voting match their self-reported responses. In this specification, there is evidence of an increase in contact rates for black and hispanic voters in implicitly covered states. For black voters, there is a 4.97 p.p. increase (10% level), suggesting an increase in black contact rates relative to white compared to black relative to white in untreated states. For hispanic voters, there is a 6.27 p.p. increase (10% level). Columns (5) and (6) restrict the sample to just voters in an attempt to measure the rate of contact among actual voters. Similarly to the main specification, there is no evidence of an increase in contact rates. Columns (7) and (8) combines the specification by restricting to `truthtellers` and verified voters. The results are similar to columns (5) and (6).

Looking at the JLA states, Table 7 repeats the analysis that was done on *Shelby County v. Holder* states. Once restricted to the JLA states, none of the coefficients are significantly different than zero at conventional levels. Most notably, the effects on hispanic and black voters in implicitly covered states are no longer positive.

5 Conclusion

The end of federal preclearance in 2013 did not, on average, widen the black-white voting gap in previously covered states relative to control states. The main estimates in Section 4.1 are near zero across the full CES sample and in the matched restriction. When treatment is focused on the JLA, the results remain the same. Examining countermobilization, most specifications show no change in campaign contact for black respondents. Taken together, the pattern is most consistent with limited or insignificant average impacts of the end of preclearance on turnout or contact.

If protecting equal participation is the goal, oversight tools should be targeted and adaptive rather than uniform and static. A modernized formula could retain the core logic of preclearance but move from legacy, statewide lists to operate at finer geographic levels (county or sub-county) and be keyed to observable policy changes with the potential to alter costs (e.g. polling place relocation or closures, reductions in early-vote hours, voter ID changes, purge practices, changes to mail-in or provisional ballots). While some laws increase the cost and reduce participation, there are notable advantages to some of these laws, such as increased trust in elections. Balancing the increase in trust with the potential rise in voter costs is an ongoing policy debate. The statistically insignificant effects on the black-white gap here provide evidence for scarce enforcement but not necessarily for the abandonment of oversight.

There are implications for research and evaluation that should be highlighted by the lack of robustness of past CES-related work (Raze (2022)). The contrast with prior CES work underscores that conclusions about post-*Shelby* effects depend on treatment categorization, fixed-effect structure, and subsample. In line with some previous work, but not all, I find that the end of federal preclearance in 2013 did not widen the black-white gap under the historical coverage map or within the JLA subset. That pattern, together with limited average changes in campaign contact for black or hispanic respondents, suggests that any shift in cost of voting as a result of the end of federal oversight was not significant enough to affect turnout, or turnout differentially by race. This paper also suggests that, if there are 'bad actors', the JLA formula has not successfully identified these states, at least since 2013.

Bibliography

Aneja, Abhay and Carlos F. Avenancio-León, "The Effect of Political Power on Labor Market Inequality: Evidence from the 1965 Voting Rights Act," *Working Paper*, 2019. Available at SSRN: https://ssrn.com/abstract=3458369. 1, 1, 2, 2.1.1

Aneja, Abhay P. and Carlos F. Avenancio-León, "Disenfranchisement and Economic Inequality: Downstream Effects of *Shelby County v. Holder*," *AEA Papers and Proceedings*, May 2019, *109*, 161–165. 2.1.1

Ang, Desmond, "Do 40-Year-Old Facts Still Matter? Long-Run Effects of Federal Oversight Under the Voting Rights Act," *American Economic Journal: Applied Economics*, 2019, *11* (3), 1–53. 1, 1, 2, 2.1.1

Ansolabehere, Stephen, Bernard L. Fraga, and Brian F. Schaffner, "The Current Population Survey Voting and Registration Supplement Overstates Minority Turnout," *The Journal of Politics*, 2022, *84* (3), 1850–1855. 2.1.1

Bernini, Andrea, Giovanni Facchini, and Cecilia Testa, "Race, Representation, and Local Governments in the US South: The Effect of the Voting Rights Act," *Journal of Political Economy*, 2023, *131* (4), 994–1056. 1, 1, 2

Biggers, Daniel R, "Can the Backlash Against Voter ID Laws Activate Minority Voters? Experimental Evidence Examining Voter Mobilization through Psychological Reactance," *Political Behavior*, 2021, *43* (3), 1161–1179. 2.1.2

Billings, Stephen B, Noah Braun, Daniel B Jones, and Ying Shi, "Disparate Racial Impacts of Shelby County v. Holder on Voter Turnout," *Journal of Public Economics*, 2024, *230*, 105047. 2.1.1

Cantoni, Enrico and Vincent Pons, "Strict ID Laws Don't Stop Voters: Evidence from a US Nationwide Panel, 2008–2018," *The Quarterly Journal of Economics*, 2021, *136* (4), 2615–2660. 2.1.2, 3.2

Cascio, Elizabeth U and Ebonya Washington, "Valuing the Vote: The Redistribution of Voting Rights and State Funds Following the Voting Rights Act of 1965," *The Quarterly Journal of Economics*, 2014, *129* (1), 379–433. 2

Caucus, Congressional Voting Rights, "Congressional Voting Rights Caucus Leaders Condemn Rise of Jim Crow 2.0 Voter Suppression Laws," Press release April 2021. Accessed: 2025-09-25. 2

Chaudhry, Raheem, "Minority Enfranchisement and Local Preferences for Public Goods: Evidence from the Voting Rights Act," in "2023 APPAM Fall Research Conference" APPAM 2023. 2

Congressional Research Service, "The Voting Rights Act: Historical Development and

Policy Background," Technical Report R47520, Congressional Research Service 2021. Accessed: 2025-09-23. 1

Crayton, Kareem, "The Voting Rights Act Explained," Brennan Center for Justice July 2023. Accessed: 2025-09-23. 2

Facchini, Giovanni, Brian G Knight, and Cecilia Testa, "The Franchise, Policing, and Race: Evidence from Arrests Data and the Voting Rights Act," NBER Working Paper 27613, National Bureau of Economic Research 2020. 2

Feder, Catalina and Michael G Miller, "Voter Purges after Shelby: Part of Special Symposium on Election Sciences," *American Politics Research*, 2020, *48* (6), 687–692. 2.1.1

Gibson, Nadine Suzanne, "Moving Forward or Backsliding: A Causal Inference Analysis of the Effects of the Shelby Decision in North Carolina," *American Politics Research*, 2020, *48* (5), 649–662. 2.1.1

Green, Donald P. and Alan S. Gerber, *Get Out the Vote: How to Increase Voter Turnout*, 3 ed., Brookings Institution Press, 2015. 2.1.2

Green, Jon, William Hobbs, Stefan McCabe, and David Lazer, "Online Engagement with 2020 Election Misinformation and Turnout in the 2021 Georgia Runoff Election," *Proceedings of the National Academy of Sciences*, 2022, *119* (34), e2115900119. 2.1.2

Grimmer, Justin and Eitan Hersh, "How Election Rules Affect Who Wins," *Journal of Legal Analysis*, 2024, *16* (1), 1–25. 2.1.2

Grofman, Bernard and Lisa Handley, "Voting Rights in the 1990s: An Overview," *Race and Redistricting in the 1990s*, 1998, pp. 69–79. 2

Haenschen, Katherine, Bethany Albertson, and Sharon Jarvis, "Tweet No Harm: Offer Solutions when Alerting the Public to Voter Suppression Efforts," *Communication and the Public*, 2024, *9* (1), 5–27. 2.1.2

Komisarchik, Mayya, Ariel White et al., "Throwing Away the Umbrella: Minority Voting after the Supreme Court's Shelby Decision," *Quarterly Journal of Political Science*, 2025, *20* (2), 269–305. 2, 2.1.1, 2.1.2, 3.2

Lopez, Tomas, "'Shelby County': One Year Later," June 2014. 2

Marschall, Melissa J, Anirudh VS Ruhil, and Paru R Shah, "The New Racial Calculus: Electoral Institutions and Black Representation in Local Legislatures," *American Journal of Political Science*, 2010, *54* (1), 107–124. 2

Morris, Kevin and Michael Miller, "Did Shelby County v. Holder Increase the Racial Turnout Gap?," *SSRN Electronic Journal*, 2024. 2.1.1

of Justice, Civil Rights Division U.S. Department, "Section 4 of the Voting Rights Act," Online resource 2023. Accessed: 2025-09-23. 2

Raze, Kyle, "Voting Rights and the Resilience of Black Turnout," *Economic Inquiry*, 2022, *60* (3), 1127–1141. 1, 2.1.1, 4, 4.1, 9, 4.1.1, 4.2, 5, 2, 3, 4, 1, 2, 3, 4, 5, 6, 7

Rienzo, Salvatore M De, "Shelby County v. Holder and Changes in Voting Behavior," *The American Economist*, 2022, *67* (2), 195–210. 2.1.1

Shah, Paru R, Melissa J Marschall, and Anirudh VS Ruhil, "Are We There Yet? The Voting Rights Act and Black Representation on City Councils, 1981–2006," *The Journal of Politics*, 2013, *75* (4), 993–1008. 2

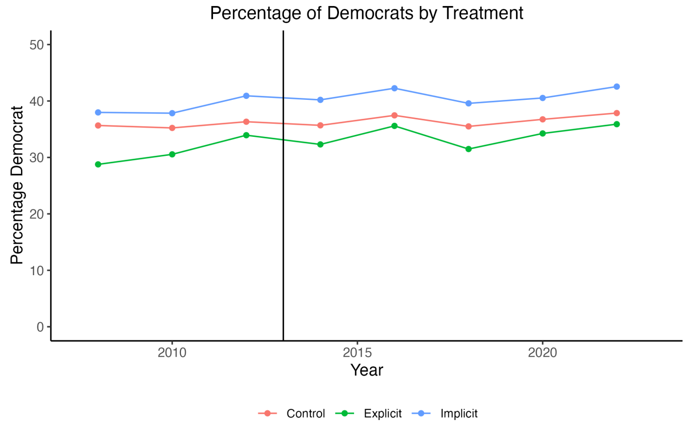
Stephanopoulos, Nicholas, Eric McGhee, and Christopher Warshaw, "Non-Retrogression without Law," *University of Chicago Legal Forum*, 2023, *2023* (1), 267–314. 2.1.1

Valentino, Nicholas A. and Fabian G. Neuner, "Why the Sky Didn't Fall: Mobilizing Anger in Reaction to Voter ID Laws," *Political Psychology*, 2017, *38* (2), 331–350. 2.1.2

Zhang, Iris H, "The Limits of Preclearance: Municipal Annexations Before and After Shelby County v. Holder," *Du Bois Review: Social Science Research on Race*, 2024, *21* (1), 24–49. 2.1.1

6 Main Body Figures

(a) Proportion Democrat by Treatment Group



(b) Proportion Republican by Treatment Group

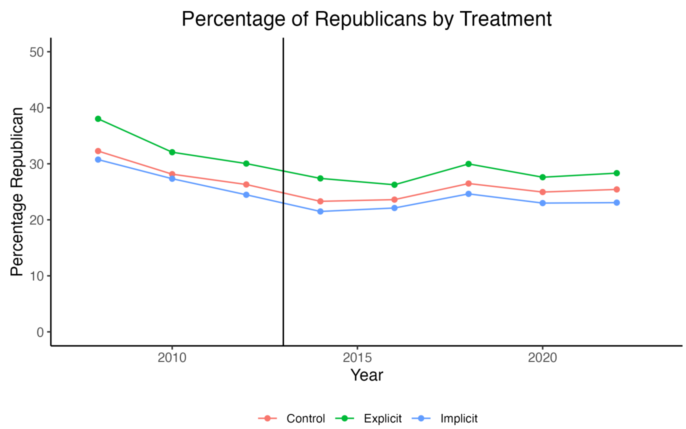
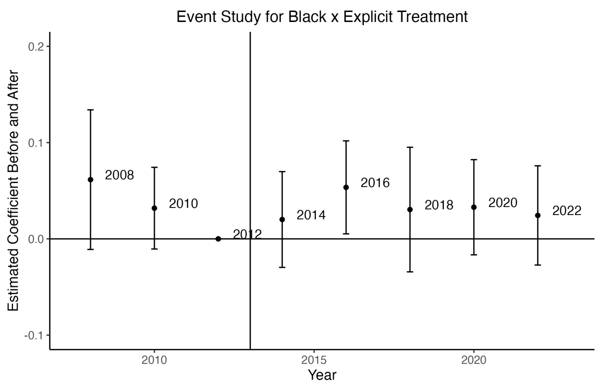


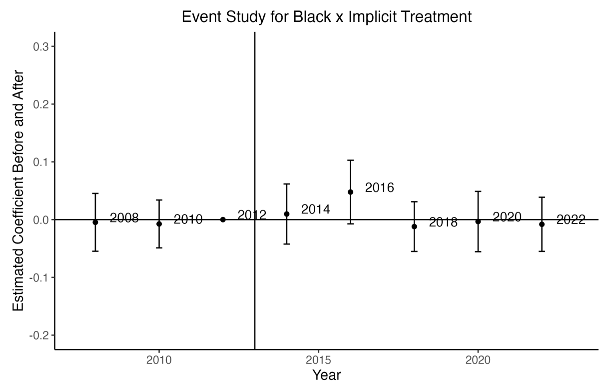
Figure 1: Proportion by Party and Treatment

Shares of self-identified party among CES respondents by treatment status over time. The unit of observation is individual-by-year and is aggregated by treatment and year. The data come from the Cooperative Election Study. The figure uses all races.

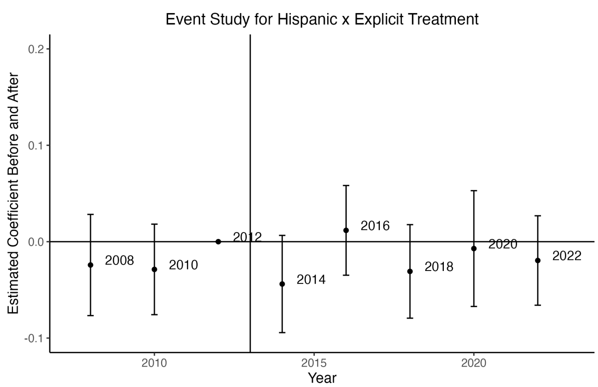
(a) Black × Post-Shelby × Explicit



(b) Black × Post-Shelby × Implicit



(c) Hispanic × Post-Shelby × Explicit



(d) Hispanic × Post-Shelby × Implicit

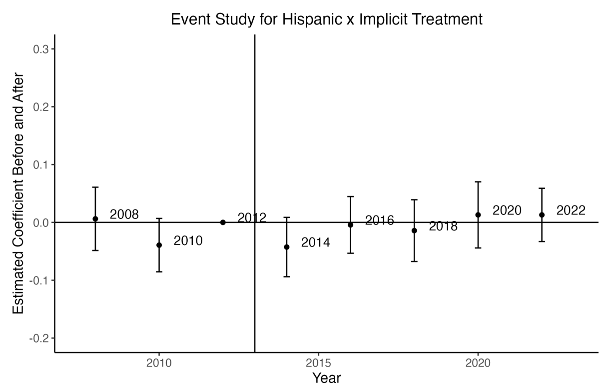
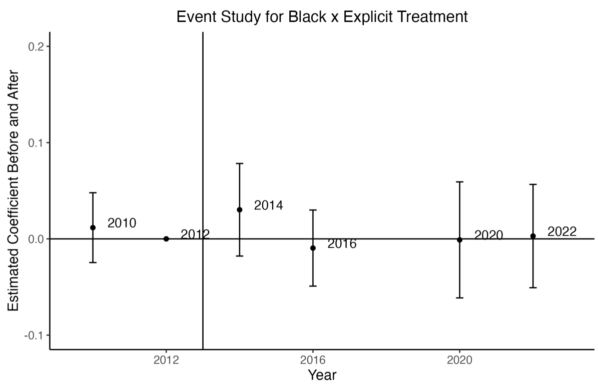


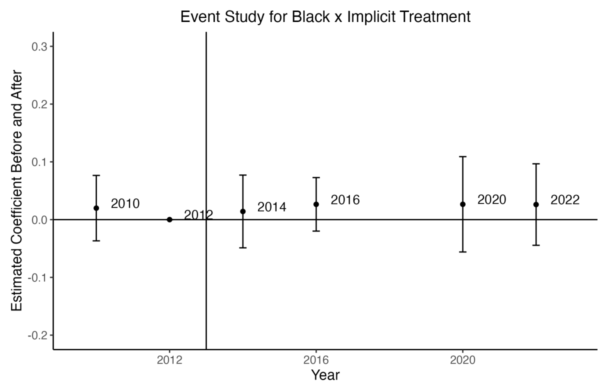
Figure 2: Verified Vote Event-Study Graphs by Preclearance Coverage Type

Event-study coefficients from triple-difference specifications of CES verified vote (respondent matched to Catalist). Models include race×year, race×state, state×year, and party×year fixed effects; demographic controls are gender, age, and age squared (following Raze (2022)). Standard errors are clustered at the state level. The bars are 95% confidence intervals. The data come from the Cooperative Election Study and include all races in the regression model.

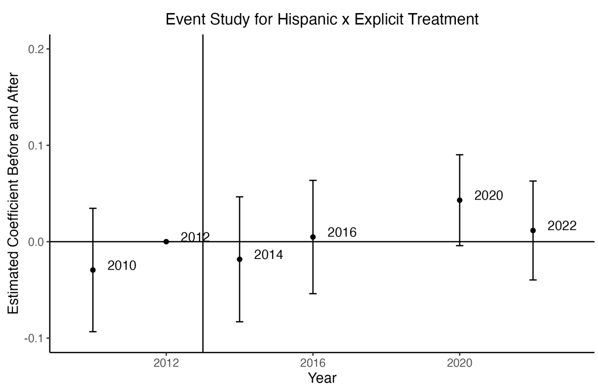
(a) Black × Post-Shelby × Explicit



(b) Black × Post-Shelby × Implicit



(c) Hispanic × Post-Shelby × Explicit



(d) Hispanic × Post-Shelby × Implicit

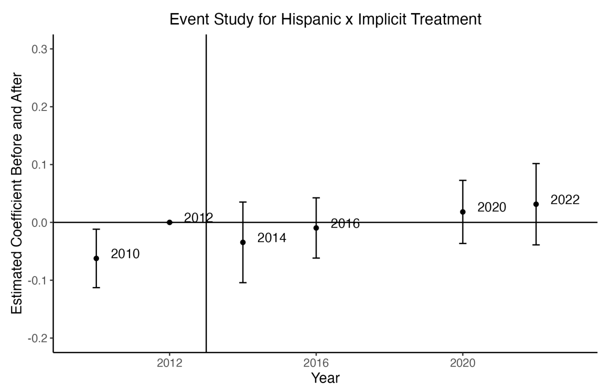
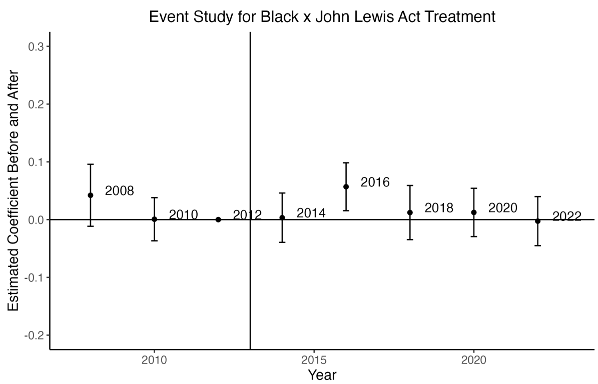


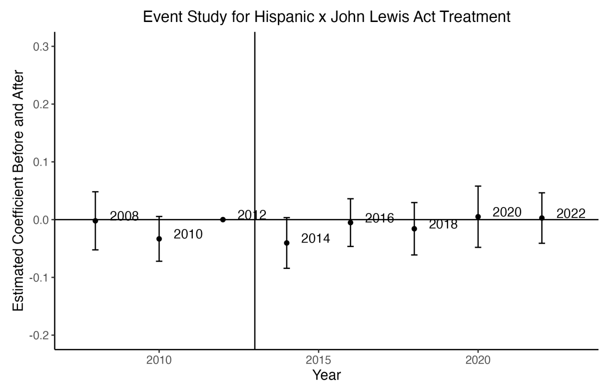
Figure 3: Campaign Contact Event-Study Graphs by Preclearance Coverage Type

Event-study coefficients from triple-difference specifications of CES campaign contact (binary: contacted during the campaign). Models include race×year, race×state, state×year, and party×year fixed effects; demographic controls are gender, age, and age squared (following Raze (2022)). Standard errors are clustered at the state level. The bars are 95% confidence intervals. The data come from the Cooperative Election Study and include all races in the regression model.

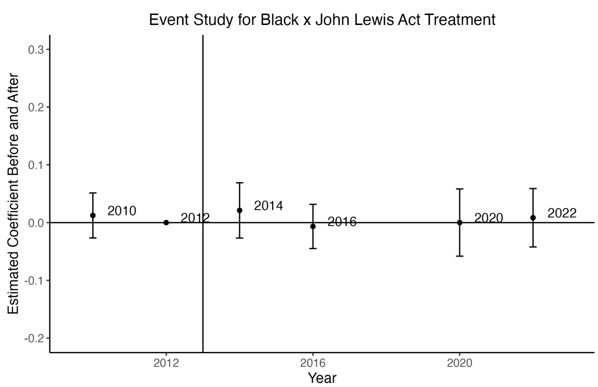
(a) Vote: Black × Post-Shelby × JLA



(b) Vote: Hispanic × Post-Shelby × JLA



(c) Contacted: Black × Post-Shelby × JLA



(d) Contacted: Hispanic × Post-Shelby × JLA

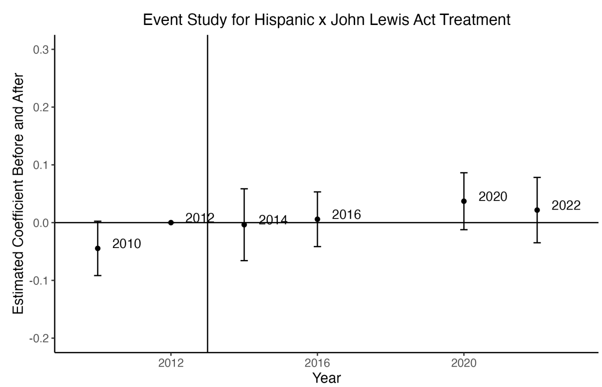


Figure 4: Vote and Campaign Contact Event-Study Graphs for John Lewis Act States

Event-study coefficients from triple-difference specifications using the JLA-targeted states as the treatment set. Outcomes are CES verified vote (top row) and CES campaign contact (bottom row). Models include race×year, race×state, state×year, and party×year fixed effects; demographic controls are gender, age, and age squared (following Raze (2022)). Standard errors are clustered at the state level. The bars are 95% confidence intervals. The data come from the Cooperative Election Study and include all races in the regression model.

7 Main Body Tables

Table 1: Verified Vote Whole State Regression Progression

	(1)	(2)	(3)	(4)
Panel A: Shelby County v. Holder States				
Any Treatment State x Post-Shelby	-0.0382***	0.0022	0.0027	0.0034
	(0.0082)	(0.0094)	(0.0083)	(0.0090)
Observations	371552	371552	371246	371156
Panel B: John Lewis Act States				
John Lewis State x Post-Shelby	-0.0409***	0.0022	0.0043	0.0049
	(0.0084)	(0.0100)	(0.0090)	(0.0100)
Observations	371552	371552	371246	371156
Race-Year and Race-State FE		✓	✓	✓
Demographic Controls FE			✓	✓
Party-Year Fixed Effects				✓

Outcome is the CES verified vote indicator. Treatment is an indicator for coverage, either explicit or implicit, by Section 4 of the VRA or JLA, and post-*Shelby*. Columns progressively add fixed effects: race-year, race-state, demographic controls (gender, age, age squared, which follows Raze (2022)), and party-year. All models use CES survey weights. Standard errors clustered at the state level. The sample includes black and white respondents only.

*, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

Table 2: Verified Vote Regression Progression

	(1)	(2)	(3)	(4)
Panel A: Race and Pooled Treatment				
Black x Any Treatment State x Post-Shelby	-0.0288**	0.0305**	0.0218	0.0194
	(0.0131)	(0.0146)	(0.0154)	(0.0157)
Observations	371552	371552	371246	371156
Panel B: Race and Explicit and Implicit Treatment				
Black x Explicit State x Post-Shelby	-0.0123	0.0419**	0.0290	0.0211
	(0.0195)	(0.0202)	(0.0194)	(0.0202)
Black x Implicit State x Post-Shelby	-0.0454***	0.0203	0.0153	0.0179
	(0.0124)	(0.0137)	(0.0171)	(0.0175)
Observations	371552	371552	371246	371156
State-Year Fixed Effects	✓	✓	✓	✓
Race-Year and Race-State FE		✓	✓	✓
Demographic Controls FE			✓	✓
Party-Year Fixed Effects				✓

Outcome is the CES verified vote indicator. Panels A and B estimate triple-difference specifications with race interacted with any Section 4 coverage (implicit or explicit) in Panel A or explicit/implicit coverage in Panel B. Columns progressively add fixed effects: state-year, race-year, race-state, demographic controls (gender, age, age squared, which follows Raze (2022)), and party-year. All models use CES survey weights. Standard errors clustered at the state level. The sample includes black and white respondents only.

*, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

Table 3: Verified Vote Main Specification

	(1)	(2)	(3)	(4)
Panel A: Black and White Respondents Only				
Black x Any Treatment State x Post-Shelby	0.0194		0.0110	
	(0.0157)		(0.0169)	
Black x Explicit State x Post-Shelby		0.0211		0.0051
		(0.0202)		(0.0196)
Black x Implicit State x Post-Shelby		0.0179		0.0165
		(0.0175)		(0.0193)
Observations	371156	371156	290074	290074
Panel B: All Races Included				
Black x Any Treatment State x Post-Shelby	0.0206		0.0121	
	(0.0158)		(0.0169)	
Black x Explicit State x Post-Shelby		0.0229		0.0066
		(0.0204)		(0.0195)
Black x Implicit State x Post-Shelby		0.0191		0.0178
		(0.0174)		(0.0191)
Hispanic x Any Treatment State x Post-Shelby	-0.0069		-0.0420	
	(0.0247)		(0.0278)	
Hispanic x Explicit State x Post-Shelby		-0.0120		-0.0398
		(0.0239)		(0.0274)
Hispanic x Implicit State x Post-Shelby		-0.0045		-0.0435
		(0.0266)		(0.0325)
Observations	433337	433337	331069	331069
CES Matched Respondents Only			✓	✓

Outcome is the CES verified vote indicator. Each panel displays the triple-difference estimate, with race interacted with any Section 4 coverage or a breakdown between implicit and explicit coverage. Panel A restricts the sample to black and white respondents; Panel B includes all races. Columns (3)–(4) limit to matched CES respondents only. All models include race-year, race-state, state-year, and party-year fixed effects, as well as demographic controls (gender, age, age squared, which follows Raze (2022)). All models use CES survey weights. Standard errors clustered at the state level.

*, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

Table 4: Verified Vote Raze (2022) Apples-to-Applies

	(1)	(2)
Panel A: Raze (2022) Base		
Black x Any Treatment State x Post-Shelby	0.0335**	
	(0.0144)	
Black x Explicit State x Post-Shelby		0.0375*
		(0.0215)
Black x Implicit State x Post-Shelby		0.0300**
		(0.0124)
Observations	274919	274919
Panel B: Raze (2022) Citizens Only		
Black x Any Treatment State x Post-Shelby	0.0339**	
	(0.0150)	
Black x Explicit State x Post-Shelby		0.0391*
		(0.0208)
Black x Implicit State x Post-Shelby		0.0292**
		(0.0145)
Observations	271925	271925
Panel C: Raze (2022) Base Plus Party-by-Year FE		
Black x Any Treatment State x Post-Shelby	0.0307**	
	(0.0149)	
Black x Explicit State x Post-Shelby		0.0317
		(0.0223)
Black x Implicit State x Post-Shelby		0.0298**
		(0.0128)
Observations	274822	274822
Panel D: Raze (2022) with My Main Specification		
Black x Any Treatment State x Post-Shelby	0.0305**	
	(0.0154)	
Black x Explicit State x Post-Shelby		0.0323
		(0.0216)
Black x Implicit State x Post-Shelby		0.0289*
		(0.0148)
Observations	271835	271835

Outcome is the CES verified vote indicator. Panel A replicates the Raze (2022) baseline specification; Panel B restricts to citizens only; Panel C adds party-year fixed effects but includes non-citizens; Panel D applies the paper's main triple-difference specification, which removes non-citizens and adds party-year fixed effects. Demographic controls include gender, age, and age squared. All models use CES survey weights. Standard errors clustered at the state level. The sample includes black and white respondents only and from the years 2008-2018 to match Raze (2022).

*, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

Table 5: John Lewis Act, Verified Vote, Main Specification

	(1)	(2)
Panel A: Black and White Respondents Only		
Black x John Lewis State x Post-Shelby	0.0086	-0.0067
	(0.0158)	(0.0166)
Observations	371156	290074
Panel B: All Races Included		
Black x John Lewis State x Post-Shelby	0.0092	-0.0062
	(0.0159)	(0.0166)
Hispanic x John Lewis State x Post-Shelby	-0.0108	-0.0497*
	(0.0218)	(0.0253)
Observations	433337	331069
CES Matched Respondents Only		✓

Outcome is the CES verified vote indicator. Panel A restricts the sample to black and white respondents; Panel B includes all races. Panels A and B estimate triple-difference specifications with race interacted with JLA treatment after *Shelby*. Column (2) limits to matched CES respondents only. All models include race-year, race-state, state-year, and party-year fixed effects, as well as demographic controls (gender, age, age squared, which follows Raze (2022)). All models use CES survey weights. Standard errors clustered at the state level.
*, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

Table 6: Campaign Contact: Main Specification

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Black and White Respondents Only								
Black x Any Treatment State x Post-Shelby	0.0164		0.0148		-0.0013		-0.0031	
	(0.0257)		(0.0265)		(0.0351)		(0.0345)	
Black x Explicit State x Post-Shelby		-0.0084		-0.0220		-0.0564		-0.0584
		(0.0371)		(0.0370)		(0.0391)		(0.0378)
Black x Implicit State x Post-Shelby		0.0391		0.0497*		0.0488		0.0472
		(0.0259)		(0.0274)		(0.0380)		(0.0370)
Observations	250347	250347	198951	198951	162436	162436	160944	160944
Panel B: All Races Included								
Black x Any Treatment State x Post-Shelby	0.0153		0.0141		-0.0016		-0.0032	
	(0.0257)		(0.0266)		(0.0353)		(0.0348)	
Black x Explicit State x Post-Shelby		-0.0085		-0.0223		-0.0559		-0.0576
		(0.0370)		(0.0371)		(0.0392)		(0.0381)
Black x Implicit State x Post-Shelby		0.0373		0.0486*		0.0484		0.0467
		(0.0259)		(0.0273)		(0.0383)		(0.0373)
Hispanic x Any Treatment State x Post-Shelby	0.0223		0.0439		0.0493		0.0476	
	(0.0236)		(0.0330)		(0.0416)		(0.0419)	
Hispanic x Explicit State x Post-Shelby		0.0023		0.0121		0.0609		0.0627
		(0.0246)		(0.0383)		(0.0535)		(0.0542)
Hispanic x Implicit State x Post-Shelby		0.0338		0.0627*		0.0448		0.0413
		(0.0264)		(0.0340)		(0.0469)		(0.0479)
Observations	289213	289213	227236	227236	182617	182617	180797	180797
`Truthtellers`			✓	✓			✓	✓
CES Verified Voters Only					✓	✓	✓	✓

Outcome is the CES campaign contact indicator, coded as one if the respondent reported being contacted during the last election cycle. Each panel displays the triple-difference estimate, with race interacted with any Section 4 coverage or a breakdown between implicit and explicit coverage. Panel A restricts the sample to black and white respondents; Panel B includes all races. Columns vary by sample restrictions: "Truthtellers" excludes inconsistent vote reporters; "CES verified voters only" restricts to validated voters. All models include race-year, race-state, state-year, and party-year fixed effects, as well as demographic controls (gender, age, age squared, which follows Raze (2022)). All models use CES survey weights. Standard errors clustered at the state level.
*, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

Table 7: John Lewis Act, Campaign Contact: Main Specification

	(1)	(2)	(3)	(4)
Panel A: Black and White Respondents Only				
Black x John Lewis State x Post-Shelby	0.0202	0.0052	-0.0054	-0.0079
	(0.0249)	(0.0270)	(0.0351)	(0.0345)
Observations	250347	198951	162436	160944
Panel B: All Races Included				
Black x John Lewis State x Post-Shelby	0.0194	0.0045	-0.0064	-0.0088
	(0.0249)	(0.0270)	(0.0353)	(0.0348)
Hispanic x John Lewis State x Post-Shelby	0.0086	0.0193	0.0088	0.0052
	(0.0223)	(0.0342)	(0.0466)	(0.0481)
Observations	289213	227236	182617	180797
`Truthtellers`		✓		✓
CES Verified Voters Only			✓	✓

Outcome is the CES campaign contact indicator, coded as one if the respondent reported being contacted during the campaign. Each panel displays the triple-difference estimate, with race interacted with JLA-suggested coverage. Panel A restricts the sample to black and white respondents; Panel B includes all races. Columns vary by sample restrictions: "Truthtellers" excludes inconsistent vote reporters; "CES verified voters only" restricts to validated voters. All models include race-year, race-state, state-year, and party-year fixed effects, as well as demographic controls (gender, age, age squared, which follows Raze (2022)). All models use CES survey weights. Standard errors clustered at the state level.

*, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.