Disparate Racial Impacts of *Shelby County v. Holder* on Voter Turnout*

Stephen B. Billings, University of Colorado
Noah Braun, University of Pittsburgh
Daniel B. Jones, University of Pittsburgh
Ying Shi, Syracuse University

December 2023

Abstract

In *Shelby County v. Holder* (2013), the Supreme Court struck down a core provision of the Voting Rights Act (VRA) that enabled federal electoral oversight in select jurisdictions. We study whether this decision disproportionately impacted ballot access for Black and Hispanic registered voters. We use a rich dataset on voter behavior for the universe of registered voters combined with Census block-level sociodemographic attributes to document a decrease in turnout for Black, relative to white, individuals. We observe suggestive but less robust evidence of decreases in Hispanic turnout. These effects are concentrated in counties with larger Black and Hispanic populations, consistent with strategic targeting of voter suppression.

Keywords: Voting Rights Act, political participation

*Billings: stephen.billings@Colorado.edu; Braun: nmb93@pitt.edu; Jones: dbj10@pitt.edu; Shi: yshi78@syr.edu

1 Introduction

The landmark Voting Rights Act of 1965 (VRA) prohibited practices used to disenfranchise voters of color, namely poll taxes and literacy tests, and drastically expanded federal oversight of electoral processes. The implementation of the VRA not only led to higher turnout and representation in local and federally elected offices of minoritized groups (Ang, 2019; Filer, Kenny, & Morton, 1991; Fresh, 2018; Schuit & Rogowski, 2017), but also improved the economic well-being of Black Americans and reduced Black-White inequality in longer-run outcomes (Aneja & Avenancio-Leon, 2019; Cascio & Washington, 2014; Jones & Shi, 2022).

A crucial component of the VRA is the "preclearance" provision, which required select jurisdictions to obtain approval from the federal government before changing any voting practices. VRA "coverage" was determined at the state or county level based on the history of disenfranchising practices and significant racial disparities in ballot access. The U.S. Supreme Court dealt a major blow to the legislation by invalidating the coverage formula for preclearance in the 2013 *Shelby County v. Holder* case, which effectively ended this form of federal oversight.[1]

This paper examines the consequences of the Supreme Court decision to end federal elections oversight, with a focus on whether *Shelby* led to differential access to the ballot for minoritized racial and ethnic groups. We combine a rich dataset on individual registered voters, aggregated to Census block-level, with block demographics and counties' coverage status. The triple-difference design compares changes in political participation in Census blocks with varying shares of Black and Hispanic individuals in covered vs. uncovered counties, before and after *Shelby*. Results show that Black, relative to white, turnout among registered voters decreased by about 1 percentage point, with larger effects in counties with greater Black and Hispanic populations. We observe suggestive but less robust evidence of decreases in Hispanic turnout.

Our outcome variable of turnout amongst registered voters is informed by a large literature on the increased costs of voting on turnout (e.g. Cantoni (2020a), Blais, Daoust, Dassonneville, and Péloquin-Skulski (2019), Cancela and Geys (2016)) showing that modern election outcomes are often influenced by which party's voters show up to vote in a given election. By focusing on this aspect of election turnout, we provide the first evidence of differential negative effects from *Shelby* on Black and Hispanic vs. white turnout. This result contributes to a rather nascent literature on the impacts of *Shelby*, which largely find null effects or even higher turnout among Black and Hispanic individuals (Komisarchik & White, 2022; Raze, 2022).[2] Komisarchik and White (2022) use administrative voter files aggregated to the county level and find no reductions in aggregate Black or Hispanic registration or turnout in formerly covered counties, with some specifications suggesting increased voter participation. Raze (2022) relies on survey data from the Cooperative Congressional Election Study (CCES) and finds that the *Shelby* decision did not widen the Black-white turnout gap, in contrast to our findings.

One facet that distinguishes our work is a rich administrative voter dataset that enables analyses at the

[1] The *Shelby* decision only invalidated the coverage formula in Section 4(b) and not the preclearance provision in Section 5. Legislative action to institute a new coverage formula can yet restore preclearance as a legal basis for defending voting rights.

[2] Another study on the effects of *Shelby* uses a regression discontinuity design based on historical turnout and the VRA coverage formula in North Carolina, which was only partially covered under preclearance. The results show no evidence of reduced turnout among minority populations (Gibson, 2020). Ricca and Trebbi (2022) document declines in Black voter registration following *Shelby*.

Census block-by-year and individual level.[3] The granularity of our data accommodates a demanding set of control variables (e.g., block, county-year, and even - in some specifications - individual fixed effects). As we discuss later, we view the added precision afforded by these controls as important in light of the fact that there is likely substantial heterogeneity in the effects of *Shelby* across states and counties, which may make detecting any impact difficult across the entire range of previously covered jurisdictions. Note that our main specification is at the block-by-year level, since individual-level administrative election data from most states does not contain race and ethnicity. Aggregating individual voter files to the block level and merging with block-level demographics from the 2010 Census allows us to assess how *Shelby* differentially impacts turnout in blocks with large Black and Hispanic populations.

It is also worth noting that several of the methods of potential voter suppression that arose after *Shelby* have a distinct *spatial* element, and so it may be *neighborhoods* of color that are targets of voter suppression as opposed to (or in addition to) individuals. Thus, considering changes at the block level is especially relevant. A particularly prominent example is the closure or relocation of polling places in previously covered jurisdictions, especially in neighborhoods with a higher share of Black or Latino voters (Simpson, 2016; Squires, 2021). Another aspect of election administration that had been the subject of many Department of Justice reviews and rejections under the preclearance prevision dealt with the redrawing of electoral districts at the local, state, or federal levels in a way that might dilute minority electoral power.[4] Such changes also target geographic areas and have been documented to impact turnout (Jones, Silveus, & Urban, 2023). In spite of these reasons for examining turnout at the block level, we also include some analyses at the individual level using the data provider's imputed race and ethnicity variable and find similar results.

Our findings of decreased relative turnout for minoritized registered voters reflect the net effect of two opposing mechanisms. On the one hand, the *Shelby* decision may allow for new electoral practices that increase the cost of voting. For example, Komisarchik and White (2022) provide evidence of increased likelihood of voter ID laws and voter roll purges in previously covered jurisdictions, both of which are pointed to as tactics to suppress participation of voters of color (Anderson, 2018). On the other hand, countermobilization may attenuate effects of heightened voting costs (Biggers, 2021; Biggers & Smith, 2020; Valentino & Neuner, 2017).

Other recent work provides evidence of expansion in Black-white differences in *economic* outcomes after 2013 in previously covered jurisdictions (Aneja & Avenancio-León, 2019; Park, Sarkar, & Vats, 2021), which comports with our finding of increased inequality in access to the ballot and also underscores the urgency of further research on longer-term effects of *Shelby*.

[3] Our data is a 2020 snapshot of individual registered voters with detailed voter history and registrations that were collected by the data vendor L2 (https://l2-data.com/)

[4] https://www.justice.gov/crt/section-5-changes-type-and-year

2 The Voting Rights Act and *Shelby* Decision

The Voting Rights Act of 1965 outlawed discriminatory practices that aimed to prevent minoritized racial/ethnic groups from exercising their right to vote. This landmark piece of legislation marked a turning point for voting rights, which Martin Luther King Jr. positioned as central to the wider struggle for civil rights (King, 1957). The statute removed widely instituted barriers to the vote, such as poll taxes and literacy tests, and significantly expanded federal oversight of the electoral process.

Of the many provisions in the VRA, Sections 4 and 5 were among the most consequential (De Rienzo, 2022). Section 5 set forth the "preclearance" special provision, which required select jurisdictions to obtain federal approval before implementing any changes to electoral or voting procedures. Jurisdictions covered by preclearance must demonstrate that the planned change "does not have the purpose and will not have the effect of denying or abridging the right to vote on account of race or color."

Section 5 came into force in combination with Section 4(b), which established a formula for identifying "covered" jurisdictions. The coverage formula targeted areas with the most pervasive and egregious discriminatory voting practices. The formula required preclearance if the jurisdiction 1) maintained a "test or device" that restricts vote access, such as literacy tests or poll taxes, or 2) less than half of individuals of voting age were registered by or voted in the November 1964 presidential election. These definitions led to coverage for the entire states of Alabama, Alaska, Georgia, Louisiana, Mississippi, South Carolina, and Virginia, as well as coverage of some counties in states such as North Carolina and Arizona.

In 2013, the U.S. Supreme Court ruled Section 4(b) as unconstitutional in the *Shelby County v. Holder* case, effectively invalidating the preclearance provision of the VRA and removed federal oversight from previously covered jurisdictions. The decision ended nearly half a century of federal oversight, and paved the way for previously-covered jurisdictions to implement new electoral practices without federal approval. There was wide coverage of electoral changes in the aftermath of *Shelby*, including the enforcement of photo ID laws in Mississippi and Alabama (Brenner Center for Justice, 2018). Select electoral changes implemented by North Carolina in 2013 were later struck down by a federal appeals court for targeting Black voters with "almost surgical precision" and a subsequent NC voter ID mandate was described by judges as "motivated at least in part by an unconstitutional intent to target African American voters."[5] A 2018 U.S. Commission on Civil Rights report described changes to voting procedures post-*Shelby* as operating "to denigrate minority voting access in ways that would have violated preclearance requirements if they were still in effect" (Lhamon, 2018). While there is existing evidence on several of these individual potential voter suppression tactics – generating mixed findings[6] – our analysis can be considered an assessment of the *accumulation* of these state and local actions.

[5] https://www.carolinapoliticalreview.org/editorial-content/2021/10/10/voter-id-in-north-carolina-what-you-need-to-know

[6] Studies on the implementation of voter ID laws find decreased registration and turnout using Rhode Island administrative data (Esposito, Focanti, & Hastings, 2019), but null effects on participation using a large administrative nationwide panel (Cantoni & Pons, 2021). Local electoral decisions such as polling place assignment are consequential, as increased distance reduces the number of ballots cast (Cantoni, 2020b). Covered jurisdictions were more likely to purge minority voters from registration rolls after *Shelby*, which may widen racial gaps in the number of eligible voters (Feder & Miller, 2020a). While some document that previously covered jurisdictions were more likely to close nearby polling stations after *Shelby*, this was not the case in North Carolina (Shepherd, Fresh, Eubank, & Clinton, 2021). Kaplan and Yuan (2020) use cross-county variation in early voting policies in Ohio to document higher turnout among jurisdictions that expanded early voting, suggesting a contraction would reduce turnout.

At the same time, any reductions in political participation resulting from the increased burden of voting may be tempered by countermobilization. A growing literature suggests emotions can be a powerful motivator in political engagement, and framing messages around efforts to restrict voting access can mobilize voters (Biggers, 2021; Biggers & Smith, 2020; Valentino & Neuner, 2017). Komisarchik and White (2022) use the CCES to document greater mobilization of nonwhite voters in previously covered jurisdictions, which may offset any negative participation effects accompanying the higher prevalence of strict and photo ID laws and voter removal from registration rolls. Cantoni and Pons (2021) describe similar mobilization effects using CCES data. Even so, the enfranchising effects of emotionally-driven countermobilization may be short-lived (Valentino & Neuner, 2017). One may therefore expect post-*Shelby* electoral changes to have increasingly negative effects as time passes, which we explore.

Finally, implementation of voter suppression policy is not without potential cost to the implementing party (Epperly, Witko, Strickler, & White, 2020). It may be expected to be employed strategically– when the size of the targeted partisan or racial/ethnic group is large enough to impact election outcomes (Blalock, 1967). Indeed, some work documents that Voter ID laws are most likely to be passed specifically in places with Republican control *and* more diverse populations (Bentele & O'brien, 2013; Biggers & Hanmer, 2017). We explore heterogeneity on this front in our setting, expecting larger effects in counties with larger Black and Hispanic populations.

3 Data

We obtain administrative voter records from the vendor L2, which gathers voter files from state elections offices. Our data access in late 2020 gives us a snapshot of the universe of registered voters in each state in the contiguous United States that was previously covered by the VRA, along with their neighboring states, as shown in Figure 1. For every individual in the sample, we observe the date of registration and voting history. The voting history reports whether an individual voted or not for elections dating back to the mid-2000s. We draw on turnout data from voter histories for elections from 2006-2018.

We observe exact addresses and basic demographic characteristics such as age and gender. The data report race/ethnicity, but there is variability in sourcing this information. Race and ethnicity is directly supplied by a small number of statewide voter registration files (e.g., North Carolina and South Carolina). Most states do not collect the race and ethnicity of registered voters, so L2 imputes it using an augmented set of sociodemographic and local characteristics.

Given variability in the source and reliability of the individual-level race and ethnicity variable, we instead use voters' addresses to aggregate voter records to the smallest geographic unit, the Census block. This aggregation yields the total number of votes and registered voters for each block and election year in our sample. We then define turnout percentage as the number of votes divided by number of registered voters.

We merge in block-level demographics on overall population and breakdown by race from the 2010 Census. In our main analysis, rather than testing how turnout varies across L2-reported race and ethnicity groups, we test how it varies across Census blocks of varying racial compositions. In doing so, our

analysis focuses on comparisons of mostly-white Census blocks to Census blocks with high shares of Black or Hispanic residents.[7] In supplementary analyses, we verify findings from our main models using individual-level data, where the dependent variable is whether the registered individual turns out to vote.

We make a small number of additional data restrictions to account for anomalous observations. In particular, some Census blocks have abnormally high population counts. Given that block groups typically have at most 3,000 residents, we drop blocks with population or registered voter count exceeding 3,000. This excludes less than 200 observations out of nearly 18 million. We then drop blocks where the count of registered voters is more than five times greater than reported block population. This excludes approximately 102,000 observations.

Our treatment variable is defined at the county-level. A county is treated if it was subject to additional federal oversight under the VRA's Section 4 as of 2013, using data on covered jurisdictions from the U.S. Department of Justice's Civil Rights Division. We restrict our sample to states fully-covered by the VRA and their adjacent states to ensure maximum comparability along hard to observe political, economic, and cultural dimensions adjacent to fully covered states (Figure 1).

A notable limitation of our data is that it is a snapshot of all registered voters at a point in time (2020), and as such we cannot accurately observe changes in registration rolls. However, we *do* observe the year each voter registered and use it to construct a count of registered voters in our sample by Census block and year. What this count of registered voters excludes is anyone who might have been registered during our sample period but was either unregistered or moved prior to 2020. Put differently, we can observe voters moving into registration status during our time period, but not *out of* registration status – and moving out of registration status is an important consideration in light of heightened voter roll purges that occurred in previously covered areas after 2013 (Feder & Miller, 2020b). This feature of the data render the sample ill-suited for examining the effects of *Shelby* on registration rates. It was with that in mind that we emphasize that the contribution of our paper is to consider the impacts of *Shelby* on turnout *conditional on registration*. Many of the changes to voting practices that appeared in previously covered areas after 2013 would impact this form of political participation (voter ID laws, polling place locations, closure of polling places, reduction in early voting availability). Empirically, because our main measure of turnout is number of votes divided by observed number of registered voters in a block, we address this data limitation by restricting our sample to individuals registered prior to 2013 such that the denominator of this fraction is not impacted by treatment.[8] Results are very similar without this restriction.

Because we base our measure of voters' location on one point in time, we may be also concerned about individual sorting from neighborhoods that are differentially impacted by *Shelby* prior to 2020. However, later analyses aim to address this concern by showing no clear patterns of changes in neighborhood residential population and racial composition from 2010 to 2020. Additionally, the segregated nature of Census blocks and typical mobility patterns of individuals suggests short distance moves across similar

[7] We focus on Black and Hispanic populations instead of other racial and ethnic subgroups such as Native Americans and Asian Americans due to sample size issues in the latter, even though all have been historically subject to political disenfranchisement.

[8] Validation exercises comparing our outcome measure with those constructed using administrative North Carolina voter data show a close correspondence. Turnout rates among registered voters do not differ by more than 3 p.p. for the 2020, 2018, 2016, and 2014 general elections.

types of neighborhoods which would limit any effects of sorting on empirical results.

The final sample comprises block-level observations of turnout for all general elections from 2006-2018 for the states indicated in Figure 1.

4 Empirical Approach

4.1 Triple-difference specification

In assessing the impact of the *Shelby* decision, we focus on its relative effects for Black and Hispanic voters. Preclearance protections directly addressed a history of racial discrimination in the ballot box in covered jurisdictions and aimed to close racial gaps in participation rates. As such, our triple-difference approach estimates *differential* rather than overall effects, by comparing changes in participation in Census blocks with varying shares of Black and Hispanic residents in covered vs. uncovered jurisdictions, before and after the *Shelby* case.[9]

We examine causal effects on voter turnout share ("turnout share"), computed as raw vote counts aggregated up to the block b in election year t level divided by counts of voters in the same block registered as of the relevant election year. Our specifications at the block-year level fully interact standard DiD variables with dummies indicating blocks with higher shares of Black or Hispanic voters, drawn from 2010 Census data:

$$\begin{aligned}(\text{turnout share})_{bct} = {} & \beta_1 * Post_t \times Cov_c \times Mid.Black_b + \beta_2 * Post_t \times Cov_c \times HighBlack_b + \\ & \beta_3 * Post_t \times Cov_c \times Mid.Hisp._b + \beta_4 * Post_t \times Cov_c \times HighHisp._b + \\ & \beta_5 * Post_t \times Cov_c \times Mid.Oth._b + \beta_6 * Post_t \times Cov_c \times HighOth._b + \\ & \beta_7 * Post_t \times Mid.Black_b + \beta_8 * Post_t \times HighBlack_b + \\ & \beta_9 * Post_t \times Mid.Hisp._b + \beta_{10} * Post_t \times HighHisp._b + \\ & \beta_{11} * Post_t \times Mid.Oth._b + \beta_{12} * Post_t \times HighOth._b + \\ & \gamma_b \times \mathbb{I}[Midterm_t] + \delta_{ct} + \epsilon_{bct}\end{aligned} \tag{1}$$

where $Post_t$ takes on a value of 1 for the years after the *Shelby* decision (2014 or later). Cov_c defines whether a county c was covered under the preclearance provision of the VRA's coverage formula. The main coefficients of interest are β_1 through β_6, which capture differential effects of Shelby on block groups with a higher share of Black and Hispanic residents.[10]

Approximately 56% of Census blocks have a Black share of 0% and 46% have a Hispanic share of 0%. We thus define the $Mid.Black_b$ and $Mid.Hisp._b$ as blocks with greater than 0% but less than the 75th percentile of Black and Hispanic share, respectively. $HighBlack_b$ and $HighHisp._b$ indicate blocks at

[9] Raze (2022) also estimates the effects of *Shelby* on relative turnout, in contrast to other studies that focus on impacts for absolute turnout (Ang (2019) and Komisarchik and White (2022)). Note that we also examine differential effects for the Hispanic population in addition to Black-White gaps.

[10] Note that remaining terms of resulting triple-interactions are absorbed into fixed effects, more fully described below.

greater than the 75th percentile. The omitted "low" categories consist entirely of blocks with zero Black or Hispanic residents as of 2010. For completeness, we include dummies for medium and high shares of all other racial and ethnic groups (combined into a single category), but we do not report their coefficients, as even the 75th percentile of "other" racial and ethnic group share is very small. The "mid. Black" share Census blocks include those with percent Black from greater than 0 to 11%, while the "high Black" share Census blocks include those with percent Black from 11 to 100%. The threshold separating "mid. Hisp." from "high Hisp." share Census blocks is 20%. Average shares are reported in Table 1. We include a variety of robustness checks in the Appendix, varying these cutoffs and the functional form more generally.

The model includes county-by-year fixed effects to absorb election-specific attributes common to a given county, such as the impact of a particular local election. A consequence of their inclusion is that we only identify relative, not overall, effects on participation.[11] Block fixed effects γ_b account for time-invariant characteristics at the block-level that may contribute to political participation. Meanwhile, block-by-midterm year fixed effects absorb differences in political participation behavior specific to midterm years for a given block, such as lower turnout. In some specifications, we furthermore include year-by-race share dummy fixed effects (medium or high Black/Hispanic/Other shares) at the block level to account for election-specific factors that similarly influence participation for these racial groups across states. Taken together, these fixed effects absorb lower-order interaction terms in the main treatment variable.

The β parameters capture the extent of differential participation consequences of the *Shelby* decision for those in blocks with higher shares of Black and Hispanic residents, relative to blocks that include zero Black or Hispanic residents as of the 2010 Census. An implicit assumption for inference is that differences in registration and turnout rates across blocks would have evolved in parallel among covered counties in the absence of the *Shelby* decision as uncovered counties. An event study design, detailed below, examines the plausibility of common trends. We cluster standard errors at the county level and, in some specifications, weight the regressions by Census block population to account for different population levels by neighborhood.[12]

Table 1 reports averages of turnout, registration per capita, and racial/ethnic share at the block level, split by blocks that are in both the low Black and low Hispanic category, blocks in the high Black category, and blocks in the high Hispanic category. One immediate observation is that turnout is higher in low Black or low Hispanic blocks (66%) than in high Black (59%) or high Hispanic blocks (55%).

4.2 Event Study Specification

Our event study model is identical to our triple-difference specification, except that we replace "post" with a set of dummies indicating years to and from 2013. Just as "post" was interacted with coverage status and dummies capturing the race/ethnic composition of the block group, so too are the individual year dummies. As in the main specification, we include dummies for high, medium, and low shares of Black,

[11] We sometimes leave out county-by-year fixed effects, but we always include at least year fixed effects.

[12] We have alternatively weighted specifications by counts of registered voters to better mimic our later individual analysis and results are similar.

Hispanic, and other groups. The figures reporting our event study estimates will focus, however, on the "high" coefficient (relative to "low").

The inclusion of block and midterm year fixed effects implies that we must omit two pre-periods, 2010 and 2012. This is ultimately desirable, as turnout varies substantially across midterm and presidential years, and further varies across these types of elections by racial and ethnic group (Einstein & Palmer, 2023). Other estimates are therefore compared to the average of the last midterm and presidential election prior to Shelby. Estimates of β_{2006} and β_{2008} that do not deviate significantly from zero are consistent with evidence of parallel trends. Coefficients on the treatment interactions for 2014 and later capture the dynamic differential effects of *Shelby* for High Black (or Hispanic) blocks.

4.3 Model Assumptions

An assumption adopted throughout our approach is that the 2010 Census provides a meaningful measure of racial/ethnic composition throughout our sample period. One particular concern is that *Shelby* itself may have led to migration out of previously covered areas, altering their composition. In Table A.1, we use the Longitudinal Tract Database to estimate a difference-in-differences specification that tests whether the racial/ethnic composition of tracts in previously covered counties changes post-*Shelby*. We found no such shifts and also observe no systematic relative changes in overall population in these tracts.

Next, we note that in studying the differential impact of *Shelby* on the basis of Census block composition, rather than the race/ethnicity of individual voters, one must to be cautious to interpret our main results as speaking to changes in individual voter behavior in light of the ecological fallacy. We discuss this further in the Appendix and later provide results using individual-level analysis which generate similar results. Moreover, we argue that block-level analyses themselves carry interest as some key voter suppression tactics are inherently spatial (e.g., polling location closure), making it worth considering the impacts on *neighborhoods* with a higher share of nonwhite voters.

5 Results

5.1 Main Results

We begin our discussion of results with the event study estimates reported in Figure 2 on turnout in blocks with high versus zero Black shares (Panel a) and turnout in blocks with high versus zero Hispanic shares (Panel b). Turnout in previously-covered Census blocks with a higher share of Black residents is significantly lower after *Shelby* under both the unweighted and population-weighted specifications. The magnitude of the effect grows each election year. We do not observe significant changes in turnout in Census blocks with a high share of Hispanic residents in the post-period. Across both figures, pre-trends in differential turnout by race prior to Shelby are relatively stable and not significantly different than 2010-2012, albeit with a large but imprecise positive estimate for Hispanic-block turnout in 2006.

Table 2 reports our main triple-difference results. Columns 1-3 represent unweighted estimates while Columns 4-6 are weighted by population. We begin with a specification that omits county-by-year fixed

effects. Columns 1 and 4 show negative effects on turnout in high Black-share blocks, relative to those with zero Black residents. Columns 2 and 5 show a significant decline in turnout in the highest Black-share blocks of between 0.8 and 1.0 percentage points, estimated with greater precision than Columns 1 and 4. Results remain similar when including year fixed effects interacted with race-share dummies in Columns 3 and 6.

The results on Hispanic turnout are somewhat more sensitive to model specification. The positive impacts on turnout reported in Columns 1 and 4 become negative when including county-by-year fixed effects in the remaining models. However, given the results in the event study, we interpret these findings of lower Hispanic turnout as less robust and at best suggestive.

If results are driven by disenfranchisement efforts differentially targeting voters of color, then – in a world where voter suppression is costly but employed strategically – we would expect our results to be strongest in areas where voters of color make up a larger share of the electorate. We investigate this possibility by splitting the sample into counties whose share of Black and Hispanic residents are above or below the median. The correlation between race and partisanship motivates further segmenting the sample by county-level Democratic share. Doing so helps us assess whether voter suppression efforts are targeted by race or partisanship. Counties are a useful geographic unit for conducting heterogeneity analysis for several reasons: counties are the smallest unit of treatment, election administration is generally conducted at the county level[13], and finally counties allow us to assess differences by broader geographic units but that remain stable over time - unlike, for instance, Congressional districts. Table 3 shows that the largest declines in turnout are in counties with above median shares of Black and Hispanic residents; that is true regardless of county partisan composition. These results are suggestive of racial, rather than partisan, targeting of voter suppression efforts.

Note that estimated impact on high Black census blocks in High Black and Hispanic counties is 30 to 60 percent larger than our full sample estimate. Not only have we diverged from the existing literature in documenting negative differential impacts of the *Shelby* ruling on Black turnout, we also show substantial heterogeneity with the effect driven by counties with larger shares of Black and Hispanic voters. These heterogeneous impacts would be difficult to detect without the precision afforded by our rich data and specifications; when focusing on areas where *Shelby* has the largest effect, simpler specifications can detect the effect.

5.2 Robustness

This section reports robustness tests.

Appendix Table A.2 re-estimates our main specification using other subsamples. Our preferred sample includes states in the southeast and southwest regions, which differ from each other in demographics and history. Columns 1 and 2 therefore restrict our sample to South and Border states (Column 1) and Southern

[13] Notably, for a number of states in our sample, election administrators are appointed by state government or match the party of the governor, so Democrat-leaning counties need not imply Democrat-leaning election administrators. Moreover, the state may find ways to locally target votes, as - for instance - when Alabama proposed closing DMV offices primarily in counties with a large Black population after first introducing a voter ID law.

states only (Column 2). Findings in both columns echo our main results, but with more precisely estimated negative impacts on Hispanic turnout. While our main specifications restrict the sample to individuals registered prior to 2013, we remove that restriction in Column 3. These results are similar to our main specifications.

Tables A.3 and A.4 repeat our main specification 9 times, dropping one fully covered state in each. Estimates are quite stable across these specifications, indicating that our results are not driven by a single anomalous state.

We test the robustness of our results to other cutoffs for the racial/ethnic composition of Census blocks. The "mid." and "High" Black and Hispanic-share dummies used in the main analysis are based on the 75th percentile of each share. Table A.5 instead sets the cutoff between "mid." and "high" blocks at 20% for both Black and Hispanic while Table A.6 sets the cutoff for "high" blocks at 50%. In both tables, results are similar to our main results for "mid." and "high" Black-share blocks, with significant decreases in turnout in those blocks. Coefficients for "mid." and "high" Hispanic blocks are generally negative, but not consistently significant.

Table A.7 adopts a different functional form. Whereas main specifications flexibly identify "High" Black and Hispanic blocks via dummy variables, this table simply interacts "Post X Cov." with continuous Black and Hispanic shares at the block level. Results generally provide the same conclusions as our main analysis, but with a loss of precision in some of the estimates.

Table A.8 adopts a propensity score weighted approach to identify more comparable control counties. Specifically, we estimate a propensity score at the county-level based on predicting Cov_c using county race shares, urban share, share of pop. over 18, share of vacant vs. owner vs. renter-occupied housing units, and 2008 Dem. presidential vote share. For common support, we exclude counties above the 95th percentile of treated-county propensity scores or below the 5th percentile of control-unit propensity scores. We then reestimate our main specifications with inverse probability weighting and using the common support sample. Results are again similar to our main findings.

5.3 Individual-level data

While our main specifications aggregate individual-level L2 data to the block level, we also verified our results using individual-level data. Table 4 shows results using a 10% sample of all registered voters to reduce the number of observations and facilitate estimation.[14] The outcome variable is whether the individual voted, in contrast to block-level data examining effects on turnout as a share of registered voters. We include the same fixed effects in these specifications as in our main specifications (e.g., county-by-year), *except* that we replace block-by-midterm fixed effects with individual voter-by-midterm fixed effects to estimate *within*-voter changes in turnout. Like the main event study, standard errors are clustered at the county level.

We find that treated voters in High Hispanic and High Black-share blocks are less likely to have voted (Column 1). While we prefer to identify race/ethnicity effects based on Block demographics, our

[14] In assembling the individual-level data, we took a randomly selected 10% sample within each state-race/ethnicity grouping.

individual-level analysis separately tests for differences in turnout based on individuals' L2-identified race. We find that voters identified as Black in L2 data are nearly 1 percentage point less likely to turn out (Column 2), which remains true with year-by-gender, age, and party fixed effects (Column 3). We do not observe a significant change in turnout for voters identified as Hispanic, Asian, or Other. Column 4 restricts the sample to people registered prior to 2013 to match the sample of the block-level analysis. Appendix Figure A.1 depicts events studies drawing on individual-level data and L2-imputed race/ethnicity identifiers.[15]

6 Conclusion

This paper examines the differential impacts of the *Shelby v. Holder* decision, which invalidated the preclearance provision in the Voting Rights Act, on turnout across racial and ethnic groups. We aim to complement recent work documenting mechanisms which might impact turnout (e.g., Komisarchik and White (2022)) by drawing on rich data that are well suited to identifying these differential changes. Our estimates suggest that turnout among registered voters declines by roughly one percentage point in Census blocks with a high share of Black residents relative to blocks with zero Black population. There is significant heterogeneity in our results, with a decline in turnout of two percentage points in Census blocks with a larger Black population in counties with larger Black and Hispanic populations. Such heterogeneity is consistent with the notion that voter suppression is costly to implement and more likely to be employed where the targeted group is larger in number and more politically powerful (Epperly et al., 2020). Taken together, we interpret our results as an *underestimate* of the effects of exclusionary voting practices deriving from Shelby. Our estimates cannot disentangle the moderating effects of countermobilization which may be more pronounced for voters of color, do not consider Shelby's larger disenfranchising effects on registration for Black and Hispanic individuals (Feder & Miller, 2020a), and furthermore finds similarly depressed turnout for continually registered voters for whom we may expect to see the smallest impact due to habitual voting.

What specific policy changes lead to the changes in turnout that we observe? As already noted, there is existing evidence of increased implementation of Voter ID laws and voter roll purges in previously covered jurisdictions (Komisarchik & White, 2022). A report from the NAACP Legal Defense Fund highlights the importance of changes at the local level: "Voting changes at the local level, such as moving a polling place or switching from district-based to at-large voting, have garnered less attention, but are no less problematic. In fact, more than 85% of preclearance work previously done under Section 5 was at the local level."[16] In short, we expect that the turnout changes we observe are the result of the accumulation of a variety of state and local actions and that there is unlikely to be a single primary driver. Thus, while we cannot speak to specific mechanisms underlying changes in turnout, we view our paper as providing evidence on the impacts of the accumulation of suppression tactics that have occurred in the absence of

[15] The individual-level event study largely matches the structure of the block-level one. It includes county-year fixed effects, individual-by-midterm fixed effects (in place of block-by-midterm), and year-by-race fixed effects. Like the main event study, standard errors are clustered at the county level.

[16] https://www.naacpldf.org/wp-content/uploads/State-local-responses-post-Shelby-11.12.20-final.pdf

federal oversight.

References

Anderson, C. (2018). *One person, no vote: How voter suppression is destroying our democracy*. Bloomsbury publishing USA.

Aneja, A. P., & Avenancio-León, C. F. (2019). Disenfranchisement and Economic Inequality: Downstream Effects of Shelby County v. Holder. *AEA Papers and Proceedings*, *109*, 161–165.

Aneja, A. P., & Avenancio-Leon, C. F. (2019). The Effect of Political Power on Labor Market Inequality: Evidence from the 1965 Voting Rights Act.

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

Bentele, K. G., & O'brien, E. E. (2013). Jim crow 2.0? why states consider and adopt restrictive voter access policies. *Perspectives on Politics*, *11*(4), 1088–1116.

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

Biggers, D. R., & Hanmer, M. J. (2017). Understanding the adoption of voter identification laws in the american states. *American Politics Research*, *45*(4), 560–588.

Biggers, D. R., & Smith, D. A. (2020). Does threatening their franchise make registered voters more likely to participate? Evidence from an aborted voter purge. *British Journal of Political Science*, *50*(3), 933–954.

Blais, A., Daoust, J.-F., Dassonneville, R., & Péloquin-Skulski, G. (2019). What is the cost of voting? *Electoral Studies*, *59*, 145–157.

Blalock, H. M. (1967). *Toward a theory of minority-group relations* (Vol. 325). New York: Wiley.

Brenner Center for Justice. (2018). *The Effects of Shelby County v. Holder.*

Cancela, J., & Geys, B. (2016). Explaining voter turnout: A meta-analysis of national and subnational elections. *Electoral Studies*, *42*, 264–275.

Cantoni, E. (2020a). A precinct too far: Turnout and voting costs. *American Economic Journal: Applied Economics*, *12*(1), 61–85.

Cantoni, E. (2020b). A Precinct Too Far: Turnout and Voting Costs. *American Economic Journal: Applied Economics*, *12*(1), 61–85.

Cantoni, E., & Pons, V. (2021). Strict Id Laws Don't Stop Voters: Evidence from a U.S. Nationwide Panel, 2008–2018*. *The Quarterly Journal of Economics*, *136*(4), 2615–2660.

Cascio, E. U., & Washington, E. (2014). Valuing the Vote: The Redistribution of Voting Rights and State Funds following the Voting Rights Act of 1965. *The Quarterly Journal of Economics*, *129*(1), 379–433.

De Rienzo, S. M. (2022). Shelby County v. Holder and Changes in Voting Behavior. *The American Economist*, 1–16.

Einstein, K. L., & Palmer, M. (2023). Racial disparities in local elections.

Epperly, B., Witko, C., Strickler, R., & White, P. (2020). Rule by violence, rule by law: Lynching, jim crow, and the continuing evolution of voter suppression in the us. *Perspectives on Politics*, *18*(3), 756–769.

Esposito, F. M., Focanti, D., & Hastings, J. S. (2019). *Effects of Photo ID Laws on Registration and Turnout: Evidence from Rhode Island* [Working Paper]. National Bureau of Economic Research.

Feder, C., & Miller, M. G. (2020a). Voter Purges After Shelby: Part of Special Symposium on Election Sciences. *American Politics Research*, *48*(6), 687–692.

Feder, C., & Miller, M. G. (2020b). Voter purges after shelby: Part of special symposium on election sciences. *American Politics Research*, *48*(6), 687–692.

Filer, J. E., Kenny, L. W., & Morton, R. B. (1991). Voting Laws, Educational Policies, and Minority Turnout. *The Journal of Law & Economics*, *34*(2), 371–393.

Fresh, A. (2018). The Effect of the Voting Rights Act on Enfranchisement: Evidence from North Carolina. *The Journal of Politics*, *80*(2), 713–718.

Gibson, N. S. (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.

Jones, D., & Shi, Y. (2022). Reducing racial inequality in access to the ballot reduces racial inequality in children's later-life outcomes. *Available at SSRN*.

Jones, D., Silveus, N., & Urban, C. (2023). Partisan gerrymandering and turnout. *The Journal of Law & Economics*.

Kaplan, E., & Yuan, H. (2020, January). Early Voting Laws, Voter Turnout, and Partisan Vote Composition: Evidence from Ohio. *American Economic Journal: Applied Economics*, *12*(1), 32–60.

King, M. L., Jr. (1957). *Give Us the Ballot.*

Komisarchik, M., & White, A. (2022). Throwing Away the Umbrella: Minority Voting after the Supreme Court's Shelby Decision. , 71.

Lhamon, C. E. (2018). *Assessment of Minority Voting Rights Access* (Tech. Rep.). U.S. Commission on Civil Rights.

Park, S., Sarkar, A., & Vats, N. (2021). Political voice and (mortgage) market participation: Evidence from minority disenfranchisement. *Available at SSRN 3891961*.

Raze, K. (2022). Voting rights and the resilience of Black turnout. *Economic Inquiry*, *60*(3), 1127–1141.

Ricca, F., & Trebbi, F. (2022). *Minority underrepresentation in us cities* (Tech. Rep.). National Bureau of Economic Research.

Schuit, S., & Rogowski, J. C. (2017). Race, Representation, and the Voting Rights Act. *American Journal of Political Science*, *61*(3), 513–526.

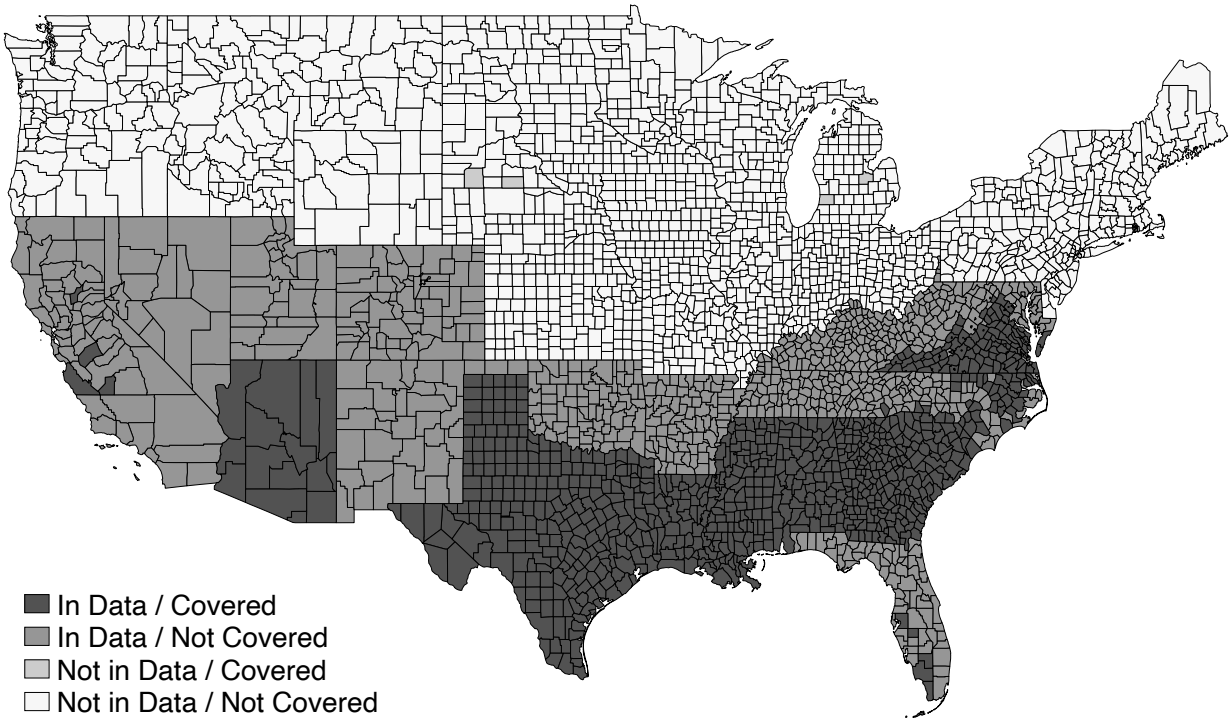
Shepherd, M. E., Fresh, A., Eubank, N., & Clinton, J. D. (2021). The Politics of Locating Polling Places: Race and Partisanship in North Carolina Election Administration, 2008–2016. *Election Law Journal: Rules, Politics, and Policy*, *20*(2), 155–177.

Simpson, S. (2016). *The Great Poll Closure* (Tech. Rep.). The Leadership Conference Education Fund. Retrieved from https://civilrights.org/edfund/resource/the-great-poll-closure/

Squires, J. M. (2021). *Shutting the door on voting: The effects of" the great poll closure"*. West Virginia University.

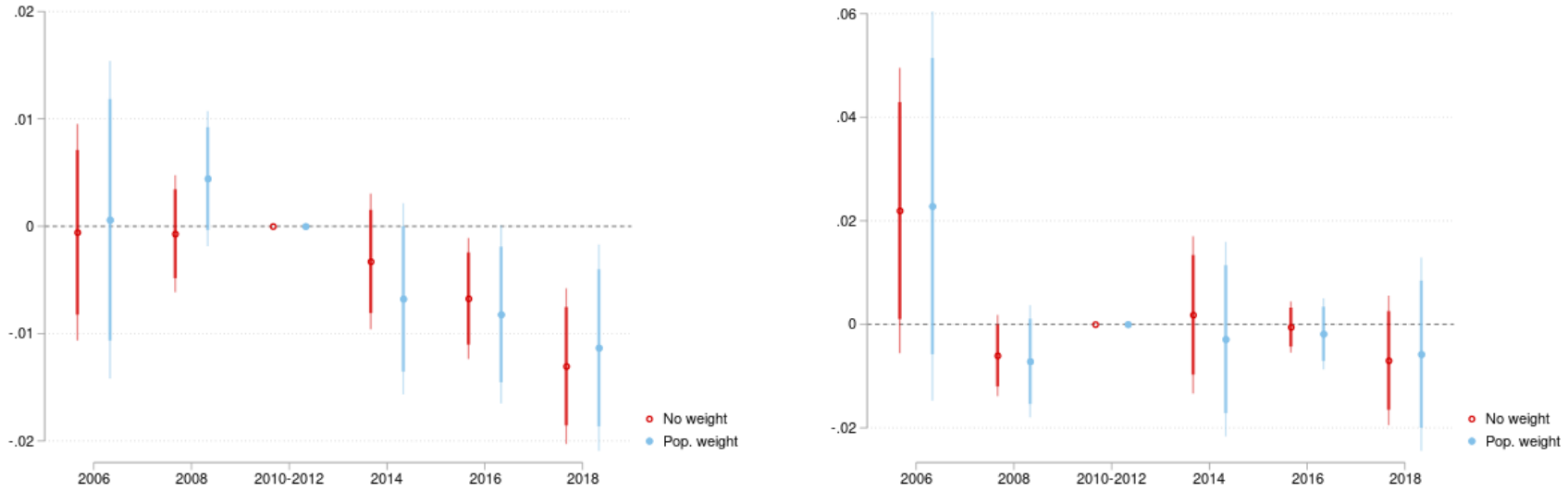
Valentino, N. A., & Neuner, F. G. (2017). Why the Sky Didn't Fall: Mobilizing Anger in Reaction to Voter ID Laws. *Political Psychology*, *38*(2), 331–350.

Figure 1: Covered jurisdictions



Notes: Alaska and Hawaii not pictured, but also not included in our sample.

Figure 2: Event Studies



(a) Turnout in High Black v. Zero Black Share Census Blocks

(b) Turnout in High Hisp. v. Zero Hisp. Share Census Blocks

Notes: In each panel, estimates depicted are from two separate specifications: one without weighting (red dots, on the left of each pair), one weighted by Census block population (blue dots, on the right of each pair). Reported estimates are the *High [Black OR Hisp.] Block*Covered*[Year]* coefficients, estimated relative to the Census blocks in the "Low [Black OR Hisp.]" category. "High" is top quartile. "Low" is bottom quartile, which is equal to zero in all cases. Dummies for "Mid." group shares, for both of these groups and the "other" category are included but not reported here. Both specifications include county-by-year fixed effects, Census block race dummies-by-year fixed effects, and Census block-by-midterm fixed effects. Thicker lines depict 95% confidence intervals; thinner lines depict 99% confidence intervals.

Table 1: Summary Statistics by Census Block Composition

	(1) Full Sample	(2) Low Black + Low Hisp.	(3) High Black	(4) High Hisp.
Turnout	0.63	0.67	0.61	0.56
Registered per cap.	0.50	0.63	0.48	0.36
Block Pct. White	0.66	0.95	0.37	0.34
Block Pct. Black	0.13	0.00	0.55	0.07
Block Pct. Hisp.	0.16	0.00	0.03	0.53
Block Pct. Other	0.06	0.05	0.04	0.06
Observations	17818423	5418699	3552898	4388678

Notes: Column 1 reports average turnout and block characteristics for our full analysis sample. Columns 2-4 report the same for non-exhaustive subsets of the data. "Turnout" (total votes divided by count registered) and "Registered per capita" are defined from our L2 data aggregated to the block level. Remaining variables are from the 2010 Census. "Low Black + Low Hisp." blocks are, as defined in the text, the bottom 25th percentile of blocks with respect to percent Black and percent Hispanic. In practice, these blocks include zero Black or Hispanic residents as of 2010. "High Black" and "High Hispanic" blocks are those in the upper 75th percentile of their respective race shares.

Table 2: Differential impacts of *Shelby* decision on turnout by Census block race/ethnic composition, difference-in-differences

VARIABLES	(1) Turnout	(2) Turnout	(3) Turnout	(4) Turnout	(5) Turnout	(6) Turnout
Post X Cov. X Mid. Hisp.	0.009*	-0.000	-0.001	0.009*	-0.000	-0.000
	(0.005)	(0.001)	(0.001)	(0.005)	(0.001)	(0.001)
Post X Cov. X High Hisp.	0.042***	-0.007**	-0.007***	0.052***	-0.008**	-0.009**
	(0.013)	(0.003)	(0.003)	(0.017)	(0.004)	(0.003)
Post X Cov. X Mid. Black	-0.001	-0.000	0.000	-0.002	-0.001	-0.001
	(0.004)	(0.001)	(0.001)	(0.004)	(0.001)	(0.001)
Post X Cov. X High Black	-0.011*	-0.008***	-0.006***	-0.013	-0.010***	-0.009***
	(0.007)	(0.002)	(0.002)	(0.010)	(0.003)	(0.004)
Observations	17,811,228	17,811,228	17,811,228	17,811,228	17,811,228	17,811,228
R-squared	0.686	0.748	0.748	0.745	0.855	0.855
Race-Year Control	No	No	Yes	No	No	Yes
FIPS X Year FE	No	Yes	Yes	No	Yes	Yes
Weighting	No	No	No	Pop.	Pop.	Pop.

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: All specifications are at the block-by-election year level and take turnout (total votes divided by count of registered voters from our L2 data) as the outcome. All include year fixed effects and block-by-midterm fixed effects. The columns vary in additional controls included and weight. "Race-year controls" are interactions of year fixed effects with dummies indicating "mid" (25th-75th pctl.) and "high" (75th-100th pctl.) Black, Hispanic, and Other Race/Eth. shares.

Table 3: Heterogeneity by County Partisanship and Racial/Ethnic Composition

	(1)	(2)	(3)	(4)
	Low Black and Hispanic		High Black and Hispanic	
VARIABLES	Low Dem.	High Dem.	Low Dem.	High Dem.
Post X Cov. X Mid. Hisp.	0.001	-0.004	0.003	-0.003
	(0.001)	(0.003)	(0.003)	(0.002)
Post X Cov. X High Hisp.	-0.008***	-0.001	0.007*	-0.014**
	(0.003)	(0.005)	(0.004)	(0.006)
Post X Cov. X Mid. Black	0.002**	-0.003	-0.001	-0.003
	(0.001)	(0.004)	(0.002)	(0.002)
Post X Cov. X High Black	-0.003	0.002	-0.013***	-0.016***
	(0.003)	(0.003)	(0.003)	(0.006)
Observations	6,580,669	2,329,542	2,060,740	6,840,277
R-squared	0.811	0.857	0.818	0.880

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: All specifications are at the block-by-election year level and use turnout among registered voters (total votes divided by count of registered voters from our L2 data) as the outcome. All include block-by-midterm fixed effects. The columns show four subgroups that vary in county racial/ethnic composition and partisan composition. They split the sample at the median of the combined county-level share of Black and Hispanic residents (31%), and at the median of the county Democratic share (45%).

Table 4: Difference-in-Differences Estimates, Individual-Level Data

VARIABLES	(1) Voted	(2) Voted	(3) Voted	(4) Voted
Post X Cov. X Mid. Hisp.	-0.001			
	(0.001)			
Post X Cov. X High Hisp.	-0.010***			
	(0.003)			
Post X Cov. X Mid. Black	0.001			
	(0.001)			
Post X Cov. X High Black	-0.006**			
	(0.003)			
Post X Cov. X Black		-0.009**	-0.009**	-0.009**
		(0.004)	(0.004)	(0.004)
Post X Cov. X Hispanic		-0.002	-0.004	-0.004
		(0.004)	(0.004)	(0.004)
Post X Cov. X Asian		-0.005	-0.005	-0.005
		(0.006)	(0.006)	(0.006)
Post X Cov. X Other		0.005	0.004	0.004
		(0.004)	(0.004)	(0.004)
Observations	41,944,478	43,629,053	43,349,815	42,464,481
R-squared	0.648	0.648	0.648	0.645
Race ID	Block Demos	L2	L2	L2
Added Controls	No	No	Yes	Yes
Rest. Reg Yrs.				Yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: All specifications are at the individual registered voter-by-election year level and take turnout (binary variable indicating voting) as the outcome. All include individual voter-by-midterm fixed effects. In Column 1, we interact treatment with the racial/ethnic composition of the block the voter lives in. That specification includes full triple interaction of block group race, post-2013 dummy, and coverage status. In Columns 2-3, we use the individual-level race variable provided in the L2 data (often imputed) and interact treatment with that. Again, we include all terms from full triple interaction of L2-provided race, post-2013 dummy, and coverage status. Column 3 adds year-by-gender, year-by-age (above/below median age), and year-by-party fixed effects.

A Appendix

A.1 Discussion on Ecological Inference

We note that, in studying the differential impact of Shelby on the basis of Census block composition, rather than the race/ethnicity of individual voters, one must to be cautious to interpret our results in light of the ecological fallacy. While our results can provide causal evidence on changes in turnout in blocks with a higher share of Black or Hispanic residents, we must stop short of claiming that our estimates definitively identify differential shifts in the probability of turnout amongst Black or Hispanic *individuals*. One particularly prominent threat to doing so is that a change in turnout in high-Black share Census blocks may reflect shifts in behavior of *non*-Black voters residing within the block.

That being said, our results still speak to the differential impacts of Shelby on minority neighborhoods, which has important policy implications. First, one could argue that the racial composition of the block is just as relevant as a unit of observation as the individual voter. In particular, many mechanisms of voter suppression are inherently spatial (e.g., relocation of polling stations) and would therefore target neighborhoods rather than individuals. Second, while an alternative explanation like that posed above is possible, it would require a particular set of circumstances. Namely, as our main result is that turnout declines in blocks with a higher share of Black residents, to provide an alternative explanation, it would have to be that non-Black voters specifically in those blocks are also less likely to turn out. If that happens because of some geographically targeted voter suppression tactic, that is not an alternative explanation at all and indeed provides evidence that more diverse neighborhoods are targeted in voter suppression efforts. If, on the other hand, there is a behavioral response from non-Black voters in high Black-share blocks, where - for instance - they feel that turnout is less necessary given that voters from another group have been suppressed, it is not clear that this would occur *specifically* in high Black-share blocks. Instead, this phenomenon - if it occurred - would seem most likely to occur in the largely white block groups of broader jurisdictions with a high (closer to pivotal) share of voters of color. But in our results, we find the largest differential effect on high Black-share blocks (relative to zero-Black blocks) in counties with a higher share of Black and Hispanic residents, a pattern that runs in the opposite direction of the alternative explanation outlined here. And third, when we do analyze data at the individual-level using L2's imputed race/ethnic categories, we observe similar findings to our main results.

Thus, in short, while we cannot definitively make claims about changes in probability of turnout for individual voters, we remain confident that our results speak to differential consequences of Shelby for Black and Hispanic voters. Moreover, we feel that there are substantial benefits to this block-aggregated approach, both because we do not rely on imputed individual-level race identifiers included in the L2 data and also because our approach allows us to assess how *neighborhoods* may be targeted with voter suppression.

A.2 Additional Tables and Figures

Table A.1: Assessing Demographic Change from 2010 to 2020

VARIABLES	(1) ln(Pop.)	(2) Pct. White	(3) Pct. Black	(4) Pct. Hisp.	(5) Pct. Oth.
Post X Cov.	0.005	0.006	0.003	-0.003	-0.000
	(0.009)	(0.004)	(0.002)	(0.003)	(0.000)
Observations	76,424	76,424	76,424	76,424	76,424
R-squared	0.969	0.989	0.989	0.991	0.505

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: Difference-in-differences specification estimated at the Census tract level using the Longitudinal Tract Database.

Table A.2: Main DiD specification in other samples

VARIABLES	(1) Turnout	(2) Turnout	(3) Turnout
Post X Cov. X Mid. Hisp.	0.000	0.000	0.002*
	(0.001)	(0.001)	(0.001)
Post X Cov. X High Hisp.	-0.013***	-0.015***	-0.006
	(0.004)	(0.004)	(0.004)
Post X Cov. X Mid. Black	-0.000	-0.001	-0.003**
	(0.001)	(0.001)	(0.002)
Post X Cov. X High Black	-0.008**	-0.010**	-0.013***
	(0.004)	(0.004)	(0.004)
Observations	13,448,681	11,472,361	17,815,291
R-squared	0.853	0.858	0.856
Sample	South+Border	South	Drop reg. restriction

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: The South sample includes the former Confederate states: Texas, Arkansas, Louisiana, Tennessee, Mississippi, Alabama, Georgia, Florida, South Carolina, North Carolina, and Virginia. South+Border further includes Kentucky, Maryland, Oklahoma, and West Virginia. Column 3 restricts to individuals who registered prior to 2013.

Table A.3: Main DiD specification, dropping one treated state at a time (1)

VARIABLES	(1) Turnout	(2) Turnout	(3) Turnout	(4) Turnout	(5) Turnout
Post X Cov. X Mid. Hisp.	-0.000	-0.000	-0.001	-0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Post X Cov. X High Hisp.	-0.008**	-0.007**	-0.008**	-0.008**	-0.008**
	(0.004)	(0.003)	(0.004)	(0.004)	(0.004)
Post X Cov. X Mid. Black	-0.001	-0.000	-0.001	-0.001	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Post X Cov. X High Black	-0.011***	-0.010***	-0.010***	-0.011***	-0.009***
	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)
Observations	17,008,064	17,205,883	16,797,907	17,189,391	17,331,037
R-squared	0.856	0.856	0.856	0.855	0.855
Dropped State	AL	AZ	GA	LA	MS

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: Specifications match those presented in the final column of Table 2 of the main text, but dropping one state at a time – with dropped state noted in the bottom row of the table.

Table A.4: Main DiD specification, dropping one treated state at a time (2)

VARIABLES	(1) Turnout	(2) Turnout	(3) Turnout	(4) Turnout
Post X Cov. X Mid. Hisp.	-0.001	-0.001	-0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)
Post X Cov. X High Hisp.	-0.009**	-0.009**	-0.005	-0.009**
	(0.004)	(0.004)	(0.005)	(0.004)
Post X Cov. X Mid. Black	-0.001	-0.001	-0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)
Post X Cov. X High Black	-0.011***	-0.010***	-0.006**	-0.011***
	(0.003)	(0.003)	(0.002)	(0.003)
Observations	16,661,842	17,234,608	15,167,322	16,936,805
R-squared	0.854	0.855	0.844	0.854
Dropped State	NC	SC	TX	VA

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: Specifications match those presented in the final column of Table 2 of the main text, but dropping one state at a time – with dropped state noted in the bottom row of the table.

Table A.5: Difference-in-Differences Estimates, with "High" Black and Hispanic-share blocks defined as greater than 20%

VARIABLES	(1) Turnout	(2) Turnout	(3) Turnout	(4) Turnout
Post X Cov. X Mid. Hisp.	-0.000	0.001	0.000	0.002*
	(0.001)	(0.001)	(0.001)	(0.001)
Post X Cov. X High Hisp.	-0.007**	-0.008**	-0.005	-0.006
	(0.003)	(0.004)	(0.005)	(0.007)
Post X Cov. X Mid. Black	-0.002**	-0.005***	-0.003**	-0.005**
	(0.001)	(0.002)	(0.001)	(0.002)
Post X Cov. X High Black	-0.007**	-0.013**	-0.008***	-0.011***
	(0.003)	(0.005)	(0.003)	(0.003)
Observations	17,815,291	17,815,291	5,939,018	5,939,018
R-squared	0.746	0.856	0.750	0.855
Sample	Main	Main	High Black+Hisp.	High Black+Hisp.
Weighting	No	Yes	No	Pop.

Clustered standard errors at the county level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Notes: Specifications match those presented in the third (no weighting) or sixth (weighting) column of Table 2 of the main text, but adopting a different definition of "mid" and "high" race/ethnic group Census blocks. In this table, "mid" is 0-20% and "high" is 20-100% for both groups.

Table A.6: Difference-in-Differences Estimates, with "High" Black and Hispanic-share blocks defined as greater than 50%

VARIABLES	(1) Turnout	(2) Turnout	(3) Turnout	(4) Turnout
Post X Cov. X Mid. Hisp.	-0.002***	-0.002**	-0.002	-0.000
	(0.001)	(0.001)	(0.001)	(0.002)
Post X Cov. X High Hisp.	-0.009**	-0.011**	-0.004	-0.007
	(0.004)	(0.005)	(0.006)	(0.007)
Post X Cov. X Mid. Black	-0.004***	-0.005***	-0.003**	-0.004**
	(0.001)	(0.002)	(0.001)	(0.002)
Post X Cov. X High Black	-0.006	-0.011*	-0.012***	-0.015***
	(0.004)	(0.006)	(0.003)	(0.004)
Observations	17,811,228	17,811,228	5,937,729	5,937,729
R-squared	0.748	0.855	0.751	0.853
Sample	Main	Main	High Black+Hisp.	High Black+Hisp.
Weighting	No	Yes	No	Pop.

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: Specifications match those presented in the third (no weighting) or sixth (weighting) column of Table 2 of the main text, but adopting a different definition of "mid" and "high" race/ethnic group Census blocks. In this table, "mid" is 0-50% and "high" is 50-100% for both groups.

Table A.7: Difference-in-Differences Estimates, interacting Continuous Black and Hispanic shares with treatment indicators

VARIABLES	(1) Turnout	(2) Turnout	(3) Turnout	(4) Turnout
Post X Cov. X Pct. Black	-0.005	-0.011	-0.014***	-0.020***
	(0.005)	(0.008)	(0.005)	(0.006)
Post X Cov. X Pct. Hisp	-0.013**	-0.021**	-0.008	-0.015
	(0.006)	(0.008)	(0.009)	(0.012)
Observations	17,811,228	17,811,228	5,937,729	5,937,729
R-squared	0.748	0.855	0.751	0.853
Sample	Main	Main	High Black+Hisp.	High Black+Hisp.
Weighting	No	Pop.	No	Pop.

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: Specifications match those presented in the third (no weighting) or sixth (weighting) column of Table 2 of the main text, but interacting treatment with continuous measures of race/ethnic shares instead of dummies. Specifically, we include all terms from the full triple interaction of continuous race share, year, and coverage status. We also include county-year fixed effects, block group-by-midterm fixed effects, and year-by-broad race grouping (high/medium/low) fixed effects.

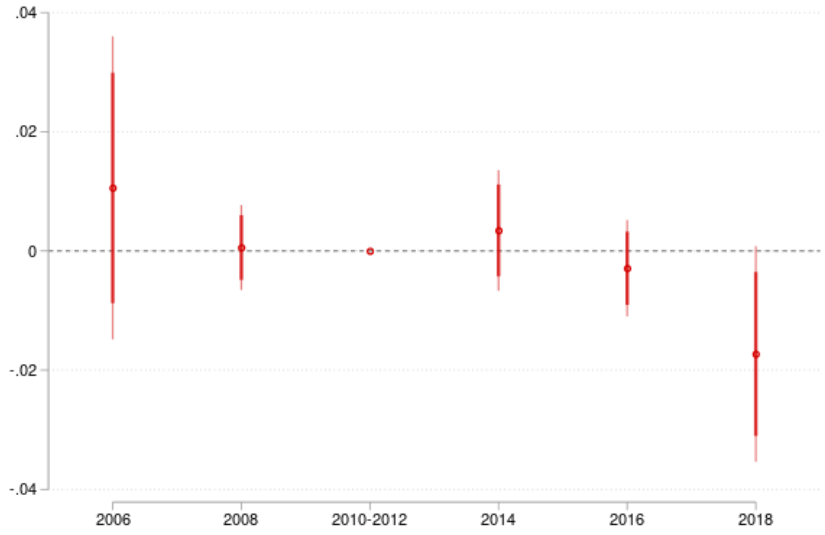
Table A.8: Difference-in-Differences Estimates, with Inverse Propensity Score Weighting and Restricted to Common Support

VARIABLES	(1) Turnout	(2) Turnout	(3) Turnout
Post X Cov. X Mid. Hisp.	0.017**	0.002	0.002
	(0.007)	(0.001)	(0.001)
Post X Cov. X High Hisp.	0.055***	-0.007**	-0.007**
	(0.015)	(0.003)	(0.003)
Post X Cov. X Mid. Black	0.001	-0.000	0.000
	(0.003)	(0.001)	(0.001)
Post X Cov. X High Black	-0.019***	-0.010***	-0.009***
	(0.006)	(0.003)	(0.003)
Observations	10,856,330	10,856,330	10,856,330
R-squared	0.685	0.748	0.749
Race-Year Control	No	No	Yes
FIPS X Year FE	No	Yes	Yes
Weighting	IPW	IPW	IPW

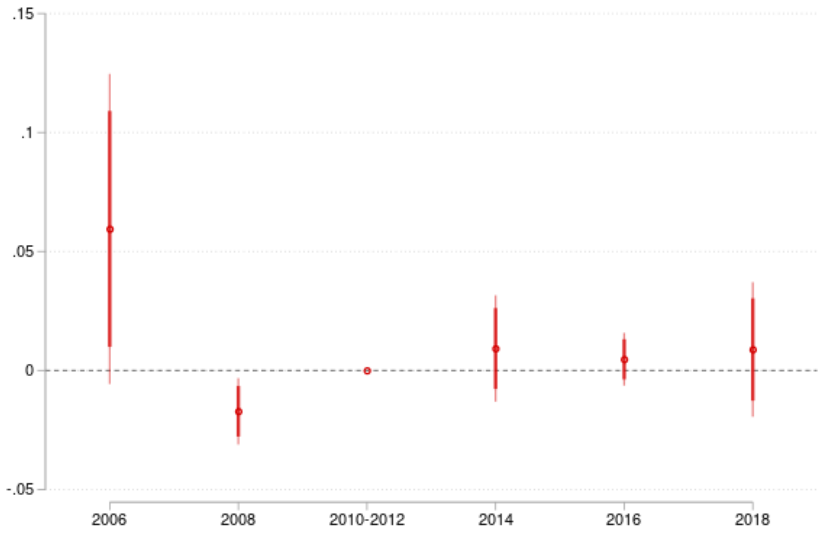
Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: All specifications are at the block-by-election year level and use turnout among registered voters (total votes divided by count of registered voters from our L2 data) as the outcome. All include block-by-midterm fixed effects.

Figure A.1: Event Studies, Individual-Level Data



(a) Black (v. White) Turnout



(b) Hisp. (v. White) Turnout