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ABSTRACT

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

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. These effects are concentrated in counties with larger Black and Hispanic populations, consistent with strategic targeting of voter suppression.

JEL Classification: D72, J15, K16

Keywords: Voting Rights Act, political participation

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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. "Covered" counties were chosen for their history of disenfranchising practices and significant racial disparities in ballot access and lie primarily in the South and Southwest. Despite the demonstrable progress made in equitable voting access from the VRA, 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, thereby ending this form of federal oversight. Within one day of the *Shelby* decision, Texas announced implementation of a Voter ID law that had previously been blocked under the VRA on the grounds that it "imposed strict unforgiving burdens on the poor, and racial minorities in Texas". Alabama and Mississippi shortly followed. Throughout previously covered jurisdictions, large numbers of polling locations were closed, especially in areas with large Black populations (Squires, 2021).

This paper examines the consequences of the Supreme Court decision to remove 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. Our main results using a difference-in-differences specification suggest that Black, relative to white, turnout among registered voters significantly decreased by about 1 percentage point. This decrease in turnout is especially pronounced in counties with larger Black and Hispanic populations. We observe suggestive but less robust evidence of decreases in Hispanic turnout.

Our focus is on turnout amongst registered voters. As highlighted by the substantial effort of political campaigns to get voters to the polls on election day and a larger academic literature on the increased costs of voting on turnout (e.g. Cantoni (2020a), Blais, Daoust, Dassonneville, and Péloquin-Skulski (2019), Cancela and Geys (2016), Rolfe (2012)), 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*. Specifically, Komisarchik and White (2021) 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). Despite that, and somewhat surprisingly, the small number of existing studies largely find null effects or even higher turnout among Black and Hispanic individuals in the aftermath of *Shelby*

(Komisarchik & White, 2021; Raze, 2022).[1]

One other facet that distinguishes our work is a rich national administrative voter dataset that enables analyses at the Census block-by-year and individual level.[2] A challenge with administrative voter files is that most states do not collect race/ethnicity. Starting from the universe of individual registered voters, but aggregating to the block level allows us to assess how Shelby differentially impacts turnout in blocks with large Black and Hispanic populations. That is our main approach.[3] The granularity of our data also 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 important in light of the fact that there is likely substantial heterogeneity in the impacts of *Shelby* across states and counties, which may make detecting any impact difficult across the entire range of previously covered jurisdictions.

As such, this study complements existing work on the consequences of the Shelby decision. Ricca and Trebbi (2022) document declines in Black voter registration following Shelby. The small number of papers focusing on turnout (as we do in this paper), however, surprisingly do not detect a negative effect. Komisarchik and White (2021) use a series of county-level snapshots based on administrative voter data 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 similarly document no relative decreases in Black registration or turnout.

In addition to greater voter participation, and as noted above, the original passage of the VRA also measurably improved the well-being of under-represented populations along a number of dimensions by increasing their political representation. While the *Shelby* decision only materialized within the decade, emerging evidence already suggests greater Black-White wage differentials after 2013 in previously covered jurisdictions (Aneja & Avenancio-León, 2019). Our findings of greater Black disenfranchisement suggests a possible link between weakened political influence and economic outcomes, and underscores the urgency of further research on the longer-term effects of *Shelby*.

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.

[1] 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).

[2] Our data is a 2020 snapshot of individual registered voters with detailed voter history and registrations that were collected from state voter registration rolls and augmented with marketing data by the data company L2 (https://l2-data.com/)

[3] Vendor-provided compilations of voter registration files, like the L2 files we use, include *imputed* race/ethnicity. We use that variable in some analyses under an alternative approach and find similar results.

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 approval from the U.S. Attorney General or a declaratory judgment from the U.S. District Court for D.C. 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, most commonly counties, 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 partial coverage for states such as North Carolina and Arizona.

Reauthorizations of the VRA in 1970 and 1975 led to an expansion of preclearance coverage. The 1975 VRA amended the original formula to include members of "language minority groups," or individuals of American Indian, Asian American, Alaskan Natives or Spanish heritage. This resulted in Texas and Arizona becoming fully covered and partial coverage (at least one covered county) in several additional states.

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 majority opinion of the Court argued that the state of contemporary voting practices does not justify the expanded federal power granted under Section 5. The decision ended nearly half a century of federal oversight, and paved the way for jurisdictions to implement new electoral practices without submitting the proposed changes for 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). The U.S. Commission on Civil Rights issued a report in 2018 on changes to voting procedures post-*Shelby*. Jurisdictions "required strict forms of voter ID, purged voter rolls, reduced polling locations, required documentary proof of citizenship to register to vote, and cut early voting, among other contested voting changes that, on the specific facts in those states, operate 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[4] – our analysis can be considered an assessment of the *accumulation* of these state and local actions.

[4] Studies on the implementation of voter ID laws find disproportionately negative turnout effects for minority voters using nationwide survey data (Hajnal, Lajevardi, & Nielson, 2017) and 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). 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). 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, 2020).

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). Since countermobilization balances out the disenfranchising effects of increased voting costs, existing studies on the null or even positive participation effects of *Shelby* point to countermobilization as a key mechanism. Komisarchik and White (2021) 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. That being said, the enfranchising effects of emotionally-driven countermobilization may be short-lived (Valentino & Neuner, 2017). As such, one may expect post-*Shelby* electoral changes to have increasingly negative effects as time passes. We explore this in our setting.

Finally, some work considers the conditions under which voter suppression tactics have been employed in the modern context. As Epperly, Witko, Strickler, and White (2020) note, and as the discussion on countermobilization highlights, implementation of voter suppression policy is not without potential cost to the implementing party. As such, 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 explores determinants of passage of Voter ID laws and other restrictive access legislation; they find that laws do not occur systematically with Republican control, but instead are most likely to occur 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 and modifies them into a national voter database containing over 190 million records. This data represents the universe of registered voters in early 2020 based on our data access in late 2020. For each 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 also have exact addresses and basic demographic characteristics such as age and gender. The data report race and 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 socio-demographic and local characteristics.

Given this 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.[5] In supplementary analyses, we verify findings from our main models using individual-level data, where the dependent variable is whether the registered voter 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 (which are constructed from multiple blocks) 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 (of the nearly 18 million in our base dataset). We then further drop blocks where the count of registered voters is more than five times greater than reported block population. (The 99th percentile block features count of registered voters that is 3.5 times greater than population.) This excludes roughly 102,000 observations. Relative to the full sample, this excludes only 0.06% of 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. To ensure maximum comparability, we restrict our sample to all states in the contiguous United States that were fully-covered as of the *Shelby* decision and all states adjacent to fully covered states (Figure 1). The resulting sample includes the vast majority of covered counties. A small number of jurisdictions in South Dakota, Michigan, New York, New Hampshire were also covered, but are excluded from our sample, as they are not in or adjacent to a fully covered state. A handful of counties in Virginia, Colorado, and Oklahoma were covered under the 1965 Voting Rights Act or a later amendment, but were later "bailed out"; we consider these counties to be in the control group, so long as they were bailed out prior to the beginning of our sample period.

Several considerations are worth noting in regards to our analytic sample. Since our data is a snapshot of the universe of registered voters at a point in time (2020), we do not observe changes in registration rolls. This feature of the data render the sample ill-suited for examining the effects of *Shelby* on registration rates. We can, however, observe each voters date of registration. With that information, our main dependent variable is turnout conditional on registration by an election date. We may be 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 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

[5] 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.

for the universe of blocks in the states indicated in Fig. 1.

4 Empirical Approach

4.1 Difference-in-differences 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 difference-in-differences 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.[6]

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 difference-in-differences specifications at the block-year level interact standard DiD variables with dummies indicating blocks with higher shares of Black or Hispanic voters, drawn from 2010 Census data:

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

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.

Notably, roughly 56% of Census blocks have a Black share of 0% and roughly 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 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.

In our sample, the "mid. Black" share Census blocks include those with percent Black from greater than 0 to 11%. The "high Black" share Census blocks include those with percent Black from 11 to 100%. The "mid. Hisp." share Census blocks include those with percent Hispanic from greater than 0 to 20%. The "high Hisp." share Census blocks include those with percent Hispanic from 20 to 100%. Average

[6] 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 (2021)). Note that we also examine differential effects for the Hispanic population in addition to Black-White gaps.

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. 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/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 jurisdictions in the absence of the *Shelby* decision as uncovered jurisdictions. 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.[7]

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 specification retains all of the same fixed effects and their interactions as the original difference-in-differences equation. One modification is that we now compare participation rates in high Black (or Hispanic) blocks to low Black (or Hispanic) blocks, omitting the middle of the distribution in block racial composition. The following is the specification used to estimate the relative effects of *Shelby* for blocks with high vs. low shares of Black residents (we estimate an analogous specification for Hispanic

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

composition):

$$\begin{aligned}(\text{turnout share})_{bct} = \beta_{2006} * \mathbb{I}[Year_t = 2006] \times Cov_c \times HighBlack_b + \\ \beta_{2008} * \mathbb{I}[Year_t = 2008] \times Cov_c \times HighBlack_b + \\ \beta_{2014} * \mathbb{I}[Year_t = 2014] \times Cov_c \times HighBlack_b + \\ \beta_{2016} * \mathbb{I}[Year_t = 2016] \times Cov_c \times HighBlack_b + \\ \beta_{2018} * \mathbb{I}[Year_t = 2018] \times Cov_c \times HighBlack_b + \\ \gamma_b \times \mathbb{I}[Midterm_t] + \delta_{ct} + \epsilon_{bct}\end{aligned} \tag{2}$$

The event study model replaces the post-treatment indicator with a series of election year-specific dummy variables from 2006 through 2018. 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, n.d.). 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 A.1, we estimate our difference-in-differences specification at the tract-level – drawing on the Longitudinal Tract Database–, testing whether the racial/ethnic composition of tracts in previously covered counties changes post-Shelby; we found no such shifts. Nor do we observe 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, but here note that we probe the robustness of our results by including individual-level analysis and find 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, Panel (a) (turnout in blocks with high versus zero Black share) and Panel (b) (turnout in blocks with high versus zero Hispanic share). 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 negative coefficient grows from less than 1 percentage point in 2014 to a nearly two percentage point decline in turnout by 2018. 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 difference-in-differences results, varying included controls and weighting. Recall that these estimates include a broader sample than the event studies, because we include blocks with Black or Hispanic share above zero but below the 75th percentile ("mid. Hisp." and "mid. Black"). Columns 1-3 represent unweighted estimates; Columns 4-6 are weighted. We begin with a specification that omits county-by-year fixed effects, before including them in Columns 2 and 5. Columns 3 and 6 augment the specifications further with election year fixed effects interacted with race-share dummies.

We begin by focusing on Black turnout. Columns 1 and 4 show negative but insignificant effects on turnout in high Black-share blocks. While insignificant, the magnitude is similar to our estimates in the remainder of the table, highlighting the role that county-year fixed effects play in increasing the precision of our estimates. In Columns 2 and 5, we observe a significant decline in turnout in the highest Black-share blocks of between 0.9 and 1.3 percentage points and smaller declines of 0.2 to 0.3 p.p. in mid-Black-share blocks. Results remain similar when including year fixed effects interacted with race-share dummies.

Turning to Hispanic turnout: the inclusion of county-by-year fixed effects changes the conclusion, with positive impacts on turnout reported in Columns 1 and 4 and negative (and not consistently significant) impacts reported in remaining models. However, given the results in the event study, we hesitate to conclude from this table that there is any change in Hispanic turnout.

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. Table 3 tests this, splitting the sample at the median by counties with a high versus low share of Black and Hispanic residents (Columns 1 and 2). Counties are a relevant unit for conducting such heterogeneity analysis, both because election administration happens at the county level and counties are the treated units in our setting. Indeed, results are almost entirely driven by "High Black/Hisp." counties. Columns 3 and 4 then split the sample on the basis of partisan composition, revealing that results are strongest in Democratic-leaning counties.[8] Given the correlation between these two attributes, Appendix Table A.2 shows analogous results

[8] 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

by all four combinations of high and low Black/Hispanic share and high and low Democratic share. The results appear to be associated with the racial composition of counties rather than partisan composition. While declines in turnout are largest in counties that are both "High Black/Hisp." and "High Dem.", there is no impact on Black turnout in counties with a low share of Black and Hispanic residents and high share of Democrats.

With these results in mind, we re-estimated all specifications reported in Table 2, restricting to counties with an above median share of Black and Hispanic residents. Results are reported in Table A.3. That table shows a significant decline in turnout in higher Black-share census blocks even without county-year fixed effects. Thus, while helpful in adding precision to our estimates – especially when combining counties where there are and are not effects–, the main conclusion from our work does not rely on the presence of county-year fixed effects.

This is where our results diverge from existing estimates on the differential impact of Shelby on turnout by race. As noted, existing papers largely document null or positive impacts on turnout. We have documented here that there are negative differential impacts of the ruling on turnout, but that there is substantial heterogeneity in said effects. Intuitively, the effect is driven by counties with larger shares of Black and Hispanic voters. Given the heterogeneity, the impacts of Shelby 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 additional tests probing the robustness of our main findings.

Appendix Table A.4 reestimates our main difference-in-differences approach within other subsamples. While our main analysis includes states ranging from east coast to west coast, states in the Southeast region differ from those in the Southwest, both in terms of demographics and history. With that in mind, Columns 1 and 2 restrict our sample to South and Border states (Column 1) and Southern states only (Column 2). Both columns reveal results similar to our main results, but with clearer and more precisely estimated negative impacts on Hispanic turnout.

Our primary outcome variable is defined as turnout count divided by count of registered voters within each block-year. In Column 4, we restrict the sample to individuals who had registered prior to 2013 thus fixing our pool of registered voters and eliminating *Shelby*-initiated changes in registration counts. These results are similar to our main specifications.

Tables A.5 and A.6 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.7 instead sets the cutoff between "mid." and "high" blocks at 20%

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.

for both Black and Hispanic while Table A.8 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, on the other hand, are generally negative, but not consistently significant.

Table A.9 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. When weighted by population and/or when focusing on counties with a high Black and Hispanic share, we find that there is a decreasing relationship between "Post X Cov." and turnout as Black share increases, consistent with our main specification. Results are less precisely estimated for Hispanic share, but reveal a negative effect as well.

5.3 Individual-level data

Finally, while our main specifications aggregate individual-level L2 data to the block level and take turnout percentage as the outcome, we have also estimated specifications directly drawing on the individual-level data. We take a 10% sample of the universe of registered voters to facilitate estimation. Results are reported in Appendix Table A.10. The outcome variable is a dummy indicating whether the voter voted. 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. That is, in the table we estimate *within*-voter changes in turnout. 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 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.

6 Conclusion

This paper examines the differential impacts of the 2013 *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 (e.g., Komisarchik and White (2021)) – which documents mechanisms which might impact turnout – by drawing on a rich dataset that is well suited to identifying these differential changes. Our main analysis measures changes in turnout amongst registered voters at the Census Block level. Our estimates suggest that turnout 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).

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, 2021). A report from the NAACP Legal Defense Fund [9] 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." In short, we expect that the turnout changes we observe are the result of the accumulation of a variety of state and local actions, many of which are difficult to observe in data, and that there is unlikely to be single primary driver. Thus, while we cannot speak to specific drivers of 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 Federal oversight.

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

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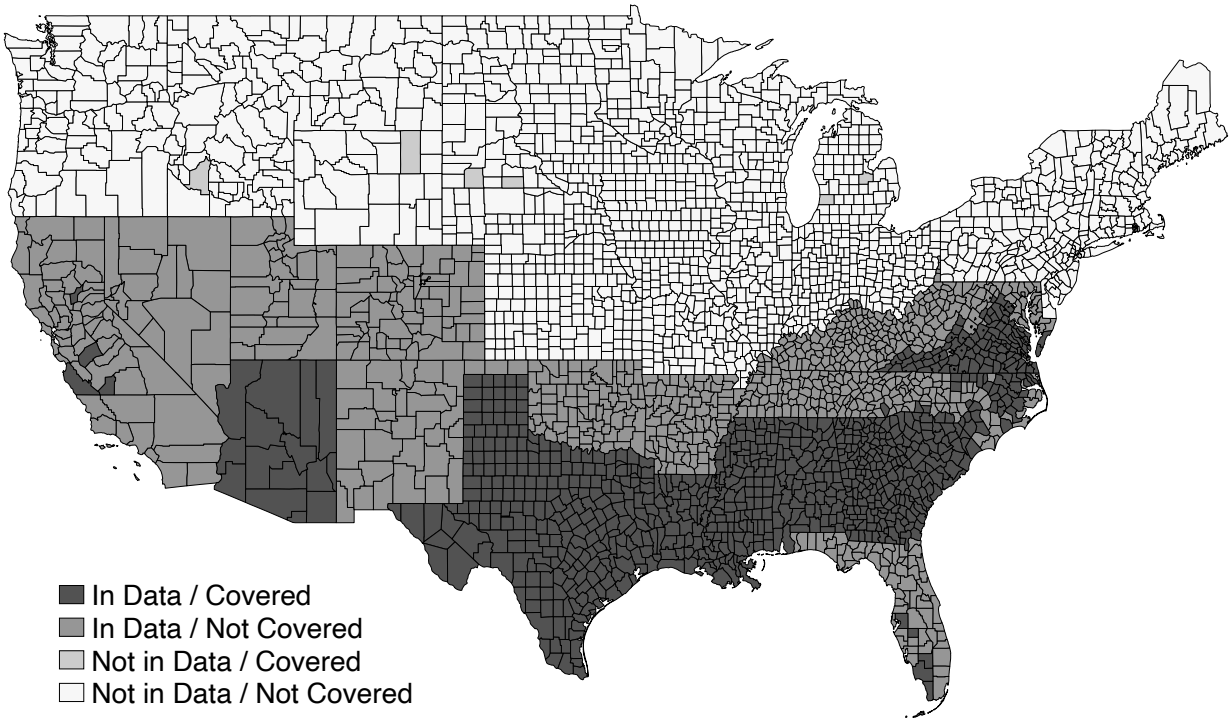
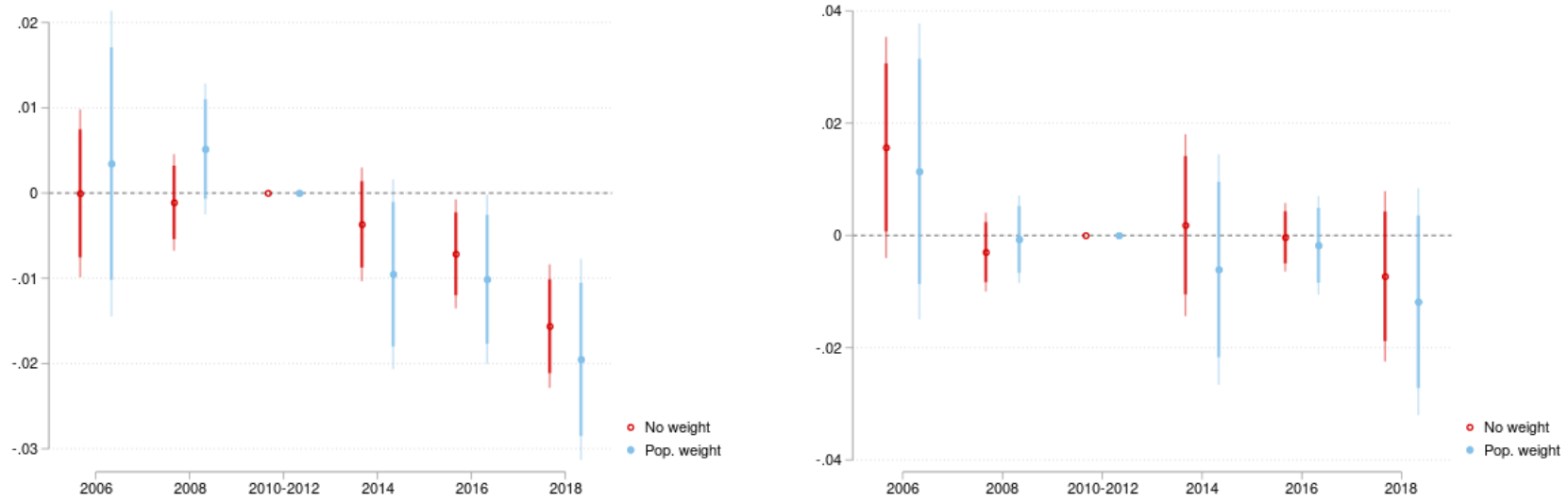
Figure 1: Covered jurisdictions



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

Figure 2: Event Studies



(a) Turnout in High Black v. Low Black Census Blocks

(b) Turnout in High Hisp. v. Low Hisp. 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. Both 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	0.62	0.66	0.59	0.55
Registered per cap.	0.53	0.67	0.52	0.40
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	17893339	5460838	3563005	4406771

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.	0.004	0.001	0.000	0.004	0.002*	0.002
	(0.005)	(0.001)	(0.001)	(0.005)	(0.001)	(0.001)
Post X Cov. X High Hisp.	0.031**	-0.005*	-0.006**	0.037**	-0.006	-0.007*
	(0.013)	(0.003)	(0.003)	(0.017)	(0.004)	(0.004)
Post X Cov. X Mid. Black	-0.003	-0.002**	-0.001	-0.004	-0.003**	-0.003**
	(0.004)	(0.001)	(0.001)	(0.004)	(0.002)	(0.002)
Post X Cov. X High Black	-0.010	-0.009***	-0.007***	-0.012	-0.013***	-0.012***
	(0.007)	(0.002)	(0.003)	(0.010)	(0.004)	(0.004)
Observations	17,815,291	17,815,291	17,815,291	17,815,291	17,815,291	17,815,291
R-squared	0.693	0.746	0.746	0.763	0.856	0.856
Race-Year Control	No	No	Yes	No	No	Yes
County X Year FE	No	Yes	Yes	No	Yes	Yes
Weighting	No	No	No	Pop.	Pop.	Pop.

Clustered standard errors at the county level 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 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 in DiD estimates by county composition

VARIABLES	(1) Low Black/Hisp	(2) High Black/Hisp.	(3) Low Dem.	(4) High Dem.
Post X Cov. X Mid. Hisp.	0.001	0.001	0.002*	0.000
	(0.001)	(0.002)	(0.001)	(0.002)
Post X Cov. X High Hisp.	-0.003	-0.009*	0.001	-0.012**
	(0.003)	(0.005)	(0.004)	(0.006)
Post X Cov. X Mid. Black	0.000	-0.007***	0.002*	-0.007***
	(0.001)	(0.002)	(0.001)	(0.003)
Post X Cov. X High Black	-0.002	-0.017***	-0.002	-0.018***
	(0.002)	(0.006)	(0.003)	(0.007)
Observations	8,905,891	8,909,400	8,649,727	9,165,564
R-squared	0.828	0.872	0.816	0.880

Clustered standard errors at the county level 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 block-by-midterm fixed effects. The columns vary in county racial/ethnic composition (Columns 1-2) or partisan composition (Columns 3-4), in both cases splitting the sample at the sample median of the relevant measure.

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: Heterogeneity by County Party and Racial/Ethnic Composition

VARIABLES	(1) Low Black/Hisp. Low Dem.	(2) Low Black/Hisp. High Dem.	(3) High Black/Hisp. Low Dem.	(4) High Black/Hisp. High Dem.
Post X Cov. X Mid. Hisp.	0.001	-0.000	0.010***	-0.000
	(0.001)	(0.002)	(0.002)	(0.003)
Post X Cov. X High Hisp.	-0.003	0.008*	0.019**	-0.014**
	(0.004)	(0.004)	(0.009)	(0.006)
Post X Cov. X Mid. Black	0.002**	-0.001	-0.003*	-0.008***
	(0.001)	(0.003)	(0.002)	(0.003)
Post X Cov. X High Black	-0.000	0.003	-0.007*	-0.020***
	(0.003)	(0.003)	(0.004)	(0.008)
Observations	6,580,794	2,325,097	2,068,933	6,840,467
R-squared	0.812	0.859	0.818	0.882

Clustered standard errors at the county level 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 block-by-midterm fixed effects. The columns vary in county racial/ethnic composition and partisan composition, in both cases splitting the sample at the sample median of the relevant measure.

Table A.3: Main DiD specification in high Black/Hisp. counties only

VARIABLES	(1) Turnout	(2) Turnout	(3) Turnout	(4) Turnout	(5) Turnout	(6) Turnout
Post X Cov. X Mid. Hisp.	0.007	-0.000	-0.001	-0.001	0.000	0.000
	(0.011)	(0.002)	(0.002)	(0.012)	(0.002)	(0.003)
Post X Cov. X High Hisp.	0.031*	-0.005	-0.006	0.027	-0.009	-0.009*
	(0.016)	(0.004)	(0.004)	(0.021)	(0.006)	(0.005)
Post X Cov. X Mid. Black	-0.003	-0.004**	-0.003*	-0.010	-0.008***	-0.007***
	(0.008)	(0.001)	(0.002)	(0.007)	(0.002)	(0.003)
Post X Cov. X High Black	-0.015	-0.012***	-0.008**	-0.022*	-0.021***	-0.018***
	(0.010)	(0.004)	(0.004)	(0.013)	(0.005)	(0.006)
Observations	8,909,400	8,909,400	8,909,400	8,909,400	8,909,400	8,909,400
R-squared	0.701	0.772	0.773	0.759	0.870	0.872
Race-Year Control	None	None	Yes	None	None	Yes
County X Year FE	No	Yes	Yes	No	Yes	Yes
Weighting	None	None	None	Pop.	Pop.	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 Table 2 of the main text, but restricted to counties above the median of Black/Hispanic county share.

Table A.4: Main DiD specification in other samples

VARIABLES	(1) Turout	(2) Turnout	(3) Turnout
Post X Cov. X Mid. Hisp.	0.002*	0.002*	-0.000
	(0.001)	(0.001)	(0.001)
Post X Cov. X High Hisp.	-0.008**	-0.010**	-0.008**
	(0.004)	(0.004)	(0.003)
Post X Cov. X Mid. Black	-0.001	-0.002	-0.001
	(0.002)	(0.002)	(0.001)
Post X Cov. X High Black	-0.009**	-0.010**	-0.009***
	(0.004)	(0.005)	(0.003)
Observations	13,456,399	11,477,570	17,811,228
R-squared	0.855	0.860	0.855
Sample	South+Border	South	Reg. before '13

Clustered standard errors at the county level 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.5: 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.	0.002*	0.002*	0.001	0.002*	0.002*
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Post X Cov. X High Hisp.	-0.006	-0.004	-0.006	-0.007*	-0.007*
	(0.004)	(0.003)	(0.004)	(0.004)	(0.004)
Post X Cov. X Mid. Black	-0.004**	-0.003*	-0.004**	-0.004**	-0.003*
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Post X Cov. X High Black	-0.014***	-0.013***	-0.012***	-0.014***	-0.012***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Observations	17,012,267	17,209,769	16,801,796	17,193,585	17,334,983
R-squared	0.858	0.857	0.858	0.856	0.856
Dropped State:	AL	AZ	GA	LA	MS

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 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.6: 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	0.002	0.002*	0.002
	(0.001)	(0.001)	(0.001)	(0.001)
Post X Cov. X High Hisp.	-0.008*	-0.007*	-0.007	-0.008*
	(0.004)	(0.004)	(0.006)	(0.004)
Post X Cov. X Mid. Black	-0.004**	-0.004**	-0.003	-0.003*
	(0.002)	(0.002)	(0.002)	(0.002)
Post X Cov. X High Black	-0.015***	-0.013***	-0.008***	-0.015***
	(0.004)	(0.004)	(0.003)	(0.004)
Observations	16,665,647	17,238,568	15,169,918	16,940,637
R-squared	0.856	0.857	0.845	0.855
Dropped State:	NC	SC	TX	VA

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 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.7: 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
postXpcXmid_hisp	-0.000	0.001	0.000	0.002*
	(0.001)	(0.001)	(0.001)	(0.001)
postXpcXhi_hisp	-0.007**	-0.008**	-0.005	-0.006
	(0.003)	(0.004)	(0.005)	(0.007)
postXpcXmid_blk	-0.002**	-0.005***	-0.003**	-0.005**
	(0.001)	(0.002)	(0.001)	(0.002)
postXpcXhi_blk	-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.8: 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
postXpcXmid_hisp	-0.002**	-0.000	-0.001	0.001
	(0.001)	(0.001)	(0.002)	(0.002)
postXpcXhi_hisp	-0.005	-0.006	-0.004	-0.008
	(0.004)	(0.004)	(0.007)	(0.008)
postXpcXmid_blk	-0.005***	-0.009***	-0.005***	-0.007***
	(0.001)	(0.003)	(0.002)	(0.002)
postXpcXhi_blk	-0.005	-0.012*	-0.009***	-0.015***
	(0.004)	(0.007)	(0.003)	(0.004)
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 0-50% and "high" is 50-100% for both groups.

Table A.9: 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	-0.015*	-0.012***	-0.020***
	(0.005)	(0.009)	(0.004)	(0.006)
Post X Cov. X Pct. Hisp	-0.009	-0.015*	-0.012	-0.022
	(0.006)	(0.008)	(0.011)	(0.015)
Observations	17,815,291	17,815,291	5,939,018	5,939,018
R-squared	0.746	0.857	0.750	0.856
Sample	Main	Main	High Black+Hisp.	High Black+Hisp.
Weighting	No	Pop.	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 interacting treatment with continuous measures of race/ethnic shares instead of dummies.

Table A.10: Difference-in-Differences Estimates, Individual-Level Data

VARIABLES	(1) Voted	(2) Voted	(3) 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**
		(0.004)	(0.004)
Post X Cov. X Hispanic		-0.002	-0.004
		(0.004)	(0.004)
Post X Cov. X Asian		-0.005	-0.005
		(0.006)	(0.006)
Post X Cov. X Other		0.005	0.004
		(0.004)	(0.004)
Observations	41,944,478	43,629,053	43,349,815
R-squared	0.648	0.648	0.648
Race ID	Block Demos	L2	L2
Added Controls	No	No	Yes

Clustered standard errors at the county level 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. In Columns 2-3, we use the individual-level race variable provided in the L2 data (often imputed) and interact treatment with that. Column 3 adds year-by-gender, year-by-age (above/below median age), and year-by-party fixed effects.