Throwing Away the Umbrella: Minority Voting after the Supreme Court's *Shelby* Decision*

Mayya Komisarchik† Ariel White‡

September 21, 2022

Abstract

The Supreme Court's 2013 decision in *Shelby County v. Holder* dramatically changed the Voting Rights Act, ending the "preclearance" process that had required federal approval before places with a history of discrimination changed their voting procedures. Dissenting justices and voting-rights advocates feared the decision could allow changes to election administration that would suppress minority voter participation. This paper evaluates the decision's impact on election practices and on Black and Hispanic voter registration and turnout. Using administrative data and a difference-in-differences design comparing places affected and unaffected by the court's decision, we find minimal changes in minority registration and voting in the post-*Shelby* period. We then delve into possible mechanisms that could underlie this pattern, using a variety of data sources to examine changes in state and local voting laws and practices as well as the possibility of public backlash and countermobilization.

*Authors are listed in alphabetical order and contributed equally. For helpful comments on this project, we thank Connor Huff, Christopher Lucas, Max Palmer, Jon Rogowski, and the members of the MIT Junior Faculty Research Lunch group, Columbia American Politics Speaker Series, the Center for Effective Government's American Politics Conference at the Harris School of Public Policy, and UR brown-bag series, as well as seminar participants at UCLA, Stanford, and Cornell. We thank Diana Camilla Alarcón, Laurel Bliss, Wenyan Deng, Caitlin Fukumoto, Benjamin Muñoz Rojas, Rorisang Lekalake, Athena Sanchez, Anna Weissmann, and Brian Williams for excellent research assistance. Ariel White gratefully acknowledges the support of the Russell Sage Foundation's visiting scholars program.

†mayya.komisarchik@rochester.edu

‡arwhi@mit.edu

1 Introduction

The Supreme Court's decision in *Shelby County v. Holder* sent shockwaves through the voting-rights world. The court invalidated Section 4 of the Voting Rights Act ("VRA"), effectively ending the "pre-clearance" process under which localities with a history of discrimination were required to seek Justice Department approval before making changes to their election procedures. This decision meant that the federal government would no longer strike potentially discriminatory changes to voting practices before they were implemented.

The VRA had been passed to combat widespread and persistent voter exclusion on the basis of race, and many advocates feared that removing pre-clearance would return the US to the pre-VRA era. Some warned that the change would "open the floodgates to voter suppression" and make it harder "to affirmatively protect [minority] communities from the spread of restrictions."[1] In the immediate aftermath of *Shelby*, states and localities began to make previously-forbidden changes to their election practices. Less than 24 hours after the court's decision, then-Texas Attorney General Greg Abbott issued a statement saying that the state's voter identification law, which had been suspended under federal review, would take effect immediately. Soon after, North Carolina passed an expansive set of restrictions on early voting, registration, and polling station hours, and instituted a strict photo ID requirement. At the same time, activists and voting-rights groups began to mobilize resources to challenge voting restrictions in court and to provide grassroots assistance to help people register and vote across the South.

In this paper, we take a preliminary look at what the *Shelby* decision meant for minority voting in previously-covered places. We draw on numerous data sources detailing a wide variety of state and county activities and their possible consequences before and after the

[1] Leigh Chapman, director of the voting rights program at the Leadership Conference on Civil and Human Rights, and John Yang, the president and executive director of Asian Americans Advancing Justice-AAJC, quoted in Vox.

Shelby decision. First, we look to voter file and census data to assess *Shelby*'s impact on minority registration and turnout. Our main analysis, using a difference-in-differences approach to compare places that were and were not affected by the *Shelby* decision, finds that the decision did not reduce aggregate Black or Hispanic voter registration or turnout. If anything, some specifications suggest that these groups have increased their participation since 2013 in places no longer covered by pre-clearance.

In the second half of the paper, we turn to possible explanations for this pattern. Did jurisdictions not change voting practices? Or did individuals or organizations work harder to mobilize potential voters in the wake of the decision, fearing representation losses otherwise? We find clear evidence that some voting practices changed in the wake of the court's decision in *Shelby*, notably that previously-pre-clearance states adopted stricter voter identification laws and previously-covered places purged more registrations from their voter rolls.[2] But we also see evidence of grassroots countermobilization efforts: survey responses suggest that minority voters were more likely to be *asked* to vote in the post-2013 period in previously-covered places.

Changes to voting rights law can have cross-cutting effects, with potentially-suppressive changes to voting practices being met by grassroots efforts to mobilize voters and ensure they are able to register and vote. In the case of the *Shelby* decision, the most important voting-rights case of a generation, we have attempted to measure these various reactions as well as the case's net effect on voter participation. Our approach allows observers to consider not only the overall effect of the *Shelby* decision on the voters the VRA sought to protect, but also the complicated story underlying it. We hope that it will contribute both to public discussion of the VRA and to political science discussions about mobilization's importance for voter participation.

[2] As we discuss in Section 4, previous research on the direct effects of these specific changes on minority voter participation have found limited effects.

2 Voting Rights Law and Political Participation

2.1 The Voting Rights Act

The Voting Rights Act of 1965 was designed to stop the egregious and widespread exclusion of minority voters, especially Black voters in the South, that persisted well into the 1960's. The original law had multiple components, and has since been renewed and amended several times. We focus here on Sections 4 and 5 because these were the sections most affected by the *Shelby* decision.

Section 4 of the VRA was intended to identify places with a particularly extreme history of racist voter exclusion.[3] In its original form, Section 4 identified jurisdictions that had used literacy tests or similar exclusionary devices in the past, or whose rates of turnout and registration were below 50% as of November 1964. Section 5 of the VRA then laid out the "pre-clearance" process: these jurisdictions, concentrated in the South, had to submit any proposed changes to their voting laws for approval by the federal government. Anticipating resistance and circumvention throughout the South, the federal government also sent federal examiners to covered jurisdictions to ensure compliance. Other portions of the Act applied nationwide and offered remedies that could be applied by courts after discriminatory changes had already taken place. But only these jurisdictions covered under the Section 4 formula were required to submit proposed changes in advance, allowing the either the Department of Justice or the United States District Court for the District of Columbia to pre-empt potentially discriminatory changes before they even went into effect. In 1975, Congress expanded the pre-clearance formula, bringing several more states and a number of additional counties into coverage (Ang, 2019).

This process of "pre-clearance" continued to operate robustly up until the court's 2013

[3]The use of exclusionary measures such as white-only primaries, literacy tests, tests of good character, separate ballot boxes, and others ensured extremely low voting rates among Black Americans in the areas targeted by Section 4 (Rosenberg, 1991).

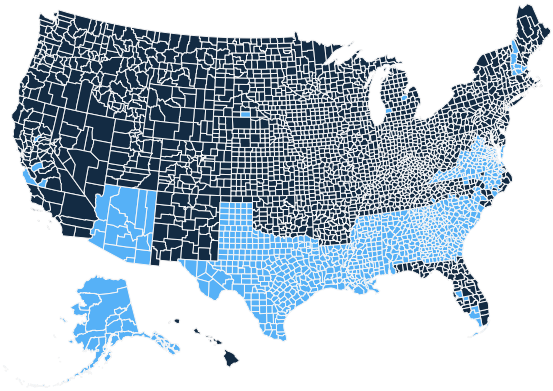


Figure 1: Counties covered by "pre-clearance" as of the *Shelby* decision (highlighted in light blue)

decision in *Shelby*: covered jurisdictions submitted almost 400,000 proposed changes to voting laws and procedures between 1982 and 2005 alone (Fraga and Ocampo, 2006). The DOJ outright objected to almost 2,300 of these, and issued requests for more information in almost 14,000 cases (Fraga and Ocampo, 2006). While the volume of both objections and requests for additional information dropped over time, Fraga and Ocampo (2006) show that even requests for additional information appeared to have a deterrent effect on jurisdictions considering the adoption of potentially discriminatory changes to voting laws.

The scholarly consensus surrounding the immediate impact of the VRA is that the Act made a massive difference for Black registration and turnout throughout the South. Average Black-white registration gaps in Alabama, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, and Virginia were as high as 40% in favor of white voters before the VRA passed (Grofman, Handley and Niemi, 1992). Black registration rates rose by nearly 70%, on average, within three years of the VRA's passage (Cascio and Washington, 2014). More recent work has leveraged differences within North Carolina, which has only partially been subject to preclearance since 1965, to show that the VRA increased Black

voter registration in the state by as much as 14% (Fresh, 2018). Researchers have similarly found evidence that aspects of the VRA increased Black turnout (Ang, 2019), the numbers of Black legislators elected to public office (Grofman and Handley, 1991), transfers of financial resources from states to preclearance counties (Cascio and Washington, 2014), and public spending on education (Kousser, 1973).Members of Congress elected from covered jurisdictions showed more support for civil rights laws than legislators from outside covered areas (Schuit and Rogowski, 2017).

Despite the robust literature devoted to analyzing the initial impact of the VRA, little published research has concentrated on what has happened in the Act's target jurisdictions after the *Shelby* decision removed the preclearance constraints on those governments. One study provided evidence that formerly preclearance counties have purged voters from their rolls at higher rates than counties never subject to preclearance in the wake of *Shelby* (Feder and Miller, 2020), but did not explore the impact purges or other such changes to voting procedures might have had on voters. In this project, we consider a broad range of possible changes to voting laws and procedures, including those specifically identified by legal experts and voting rights advocates. We also make the first attempt, to our knowledge, to combine a variety of data sources to shed light on both the institutional and behavioral consequences of the *Shelby* decision.

2.2 The *Shelby* Decision

In *Shelby v. Holder*, the Supreme Court took issue with the section of the VRA that identified jurisdictions subject to preclearance. A 5-4 majority ruled that applying the original coverage formula exceeded Congress' authority under the Fourteenth and Fifteenth Amendments. The issue, according to the Court, was that Congress was applying a coverage formula developed in the 1960s and 1970s to a set of places that had since changed dramatically. Justices Roberts and Thomas argued that the racially discriminatory practices that had provided the original mandate for the VRA had all but evaporated, and gaps

in participation between white and nonwhite citizens had essentially disappeared. Thus, forcing jurisdictions to submit all proposed changes to voting laws subjected them to an undue burden. Chief Justice John Roberts' majority opinion held that the Court had "no choice but to declare Section 4(b) unconstitutional. The formula in that section can no longer be used as a basis for subjecting jurisdictions to preclearance" (*Shelby v. Holder* 570 U.S. 529 (2013) (Roberts, J. majority opinion)). The Court held that Congress could review and update the coverage formula to reflect contemporary circumstances. Congress has not issued an updated coverage formula to date, so the preclearance process has effectively disappeared for places previously covered under Section 4.

In her dissent, the late Justice Ruth Bader Ginsburg argued that *Shelby* effectively made it impossible to supervise the jurisdictions with the deepest and most pervasive histories of vote suppression. "Volumes of evidence," Ginsburg wrote,"supported Congress' determination that the prospect of retrogression was real. Throwing out preclearance when it has worked and is continuing to work to stop discriminatory changes is like throwing away your umbrella in a rainstorm because you are not getting wet" (*Shelby v. Holder* 570 U.S. 529 (2013) (Bader Ginsburg, R. dissenting opinion)). At issue in *Shelby*, and at the heart of this research project, is the question of whether jurisdictions newly freed from federal oversight *did* effectively make it more difficult for minority citizens to register and cast ballots, and whether *Shelby* ultimately led to reduced participation by voters from minority groups.

2.3 Possible Changes

We see at least two forces that could change minority voting rates in the wake of the *Shelby* decision, and they push in opposite directions. First and most obvious are changes in local and state election practices. The preclearance provision of the Voting Rights Act was originally constructed to prevent state and local election officials from using discriminatory practices to limit voting and representation. Advocates feared that without preclearance,

officials would rush to implement laws and policies that would make it more difficult to vote, and the only way to prevent those changes would be costly and long-running litigation. They pointed to past cases in which jurisdictions had tried to implement changes like voter identification laws, reductions in the number of polling places, voter list purges, or even election cancellations, and had been constrained by preclearance (Perez and Agraharkar, 2013). So one possible outcome of the decision, feared by many voting-rights advocates, was that newly-allowed changes in election administration would impose burdens that would ultimately prevent many eligible minority voters from casting ballots.

We should note that the evidence that these particular election changes affect voter participation is mixed, and that effects could depend on how changes were implemented. For example, "purging" voters from registration lists can be a routine part of list maintenance, useful for ensuring the voting rolls are not clogged with people who are deceased or have moved away (Ansolabehere and Hersh, 2014; Shaw, Ansolabehere and Stewart III, 2015; Huber et al., 2021). But list purges can also be misused to remove people who actually belong on the list, and to disproportionately remove voters of color. Memorably, an "overzealous" 2000 effort to remove people with past felony convictions from the voter rolls mistakenly removed many eligible Black voters (United States Commission on Civil Rights, 2001; Tokaji, 2005). Rather than tracking down every local story and examining the motives behind it, we take a high-level look at whether these types of election changes occurred more in previously-covered places than elsewhere in the wake of the Court's decision.

The second way that *Shelby* could affect minority voter turnout is through backlash or countermobilization. Many advocates feared that the decision would usher in a new era of vote suppression, so a natural reaction would be to try to counteract those changes through mobilization. Indeed, some prominent organizations have explicitly framed their mobilization efforts in southern states as a response to the *Shelby* decision. In announcing its $30 million "Vote Your Voice" voter outreach campaign in fall 2020, for example, the Southern Poverty Law Center highlighted *Shelby*: "since the Supreme Court gutted the

Voting Rights Act in 2013, there has been a blatant effort to deny voting rights through state efforts."

Such countermobilization efforts by advocacy groups could work even if voters were unaware of the *Shelby* decision or the election law changes that followed. Generic voter registration drives and get-out-the-vote activities can increase participation in targeted communities (Bedolla and Michelson, 2012; Green and Gerber, 2019). But messages highlighting threats to voting rights could be an especially effective mobilization tool: work from political psychology suggests that telling people about efforts to restrict voting can be a powerful motivator (Biggers and Smith, 2018; Biggers, 2019; Valentino and Neuner, 2017).

We are interested both in the net effects of the court's decision on minority voting, and in the mechanisms that underlie those effects. The goal of the Voting Rights Act was ensuring that historically-disenfranchised groups were able to register and vote, so our main analysis focuses on registration and voting as outcomes. But it is important to understand the forces that underpin the effects we observe, because they yield differrent understandings of the present and disparate predictions about the future. A finding that minority registration and voting went unchanged, as did local election practices and state election laws, might be in line with Justice Roberts' belief that preclearance was no longer needed to constrain discriminatory behavior by election officials. But what if we instead found no decrease in voting, but many electoral changes combined with (possibly-offsetting) mobilization of minority voters? We might find such a pattern more troubling. First, such electoral changes might impose unreasonable burdens on voters even if they did not reduce aggregate turnout. And if jurisdictions reacted to the court's decision by immediately changing some of their voting practices, we might imagine that in the long run, they might make more extreme changes. These longer-term changes could pose larger hurdles to minority voting and representation, particularly as countermobilization efforts waned and short-term public outrage wore off.

3 Voter Registration and Turnout

We begin with a look at whether the *Shelby* decision had a measurable effect on Black or Hispanic registration and voting rates.[4] For this analysis, we need a dataset with several characteristics. First, we need to go beyond aggregate data on overall turnout and registration: we need information about how voters of different racial groups fared, since most concerns about the *Shelby* decision were specifically about minority voting rights. And second, we need a dataset that allows us to precisely estimate participation rates for groups that represent a small share of the population in some places. Surveys of voter participation are prone to overstating turnout (Ansolabehere and Hersh, 2012; Burden, 2000) and to yielding very noisy estimates of minority turnout, so we avoid them. Instead, we rely on voter-file data drawn from state elections records, combined with estimates of voter identity.

For this project, we use a dataset constructed from the voter database maintained by Catalist, LLC, a voter-list vendor that collects and cleans voter-file data from state elections offices. Catalist's database includes individual observations for people registered in each state, as well as estimates of each registered voter's racial identity.[5] We contracted with

[4]We focus on Black and Hispanic voters as two large and geographically-dispersed groups of voters that have historically faced vote suppression efforts. Other groups could also be affected by the *Shelby* decision, but we are less sanguine about our ability to identify effects on their behavior using the county-level design of this paper.

[5]In states (mainly in the South) where the voter file contains voter race, Catalist relies heavily on these self-identifications. In other states, Catalist estimates race using voters' names as well as other available demographic information about them and their neighborhood.(Fraga, 2016). For a discussion of the accuracy of Catalist's race predictions, see Fraga (2018) Appendix A.3. Note that they applied the same classification model across years, so any changes we observe should not be driven by variation in classification accuracy.

Catalist to produce an aggregated dataset with county-level estimates of the number of registered voters from each racial group in each year from 2008-2018, as well as the number of people from each group that turned out to vote in each of those years. This dataset was constructed using a series of voter-file snapshots from previous years, and does not rely on a given voter's being registered as of 2018. This approach yields a dataset at the county-year level, with estimates of (for example) how many Black voters were registered as of 2008 in a given county, and how many Black voters turned out to vote.

The Catalist data yields raw counts of registrants and voters, but as local population could change over the ten-year period spanned by our data, we want to calculate the *share* of eligible voters who registered or voted in an area. To do this, we divide Catalist's counts by Census Bureau estimates of the citizen voting-age population (CVAP) for each corresponding racial category in each county.[6] For instance, the registration rate for Black voters in Autauga County, Alabama in 2010 would be 6,093 registered voters divided by an estimated 6,480 Black citizens aged 18 or older living in the county, or 0.94. To calculate voter turnout rates, we divided Autauga's 2,754 votes cast by Black voters by the same 6,480 eligible Black voters. We constructed these rates for each county in each federal election year from 2008-2018.[7]

Using this dataset, how can we tell whether the court's decision mattered? One possible approach would be to simply look at the set of places affected by it and ask whether minority voter turnout in these places looked different after the 2013 decision than before. But such an approach would not account for many other changes that could be happening in the background over this time period, like national trends in turnout. Instead, we use a difference-in-differences approach: we compare the over-time changes in affected places to

[6]We rely on the 2009 American Community Survey CVAP estimates to estimate 2008 CVAP because the five-year estimates we use only became available in 2009.

[7]Section B.2 in the Supporting Information compares county- and state-level estimates from this dataset to several other data sources.

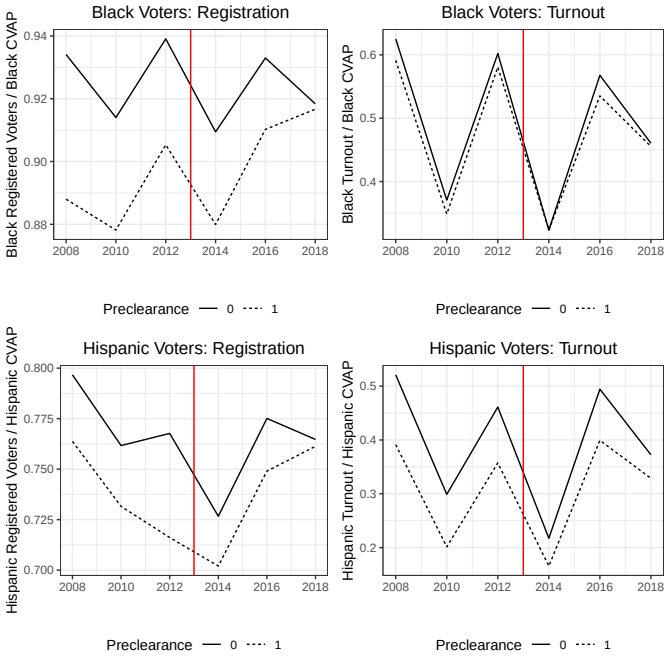


Figure 2: Time trends in Black and Hispanic registration (left two panels) and voter turnout (right two panels) rates. In all panels, the dotted line represents mean values in preclearance or formerly-preclearance counties, while the solid line represents non-covered places.

the same time trends in places that were unaffected by the decision. This approach allows us to capture trends that are not specific to affected places, and to pin down the causal effect of the court decision itself.

This difference-in-differences approach relies on a "parallel trends" assumption. We assume that although affected and unaffected places might have different baseline rates of minority voter participation, their trends over time would have been similar were it not for the court's decision. This assumption cannot be explicitly tested for the period of our analysis, but Figure 2 displays trends from earlier periods as a first pass at evaluating the assumption's plausibility.[8] Preclearance and non-preclearance counties show very similar

[8] Alert readers will notice that registration rates in this dataset appear higher than many

turnout trends before 2013. The trends in Hispanic registration look slightly less-well-matched, due to the fact that ACS estimates of CVAP grow faster in 2010-2012 than Catalist's estimates of registered voters in counties with the largest Hispanic populations. We continue with the simplest difference-in-differences specification here, but in Section B.6 of the Supporting Information, we discuss a variety of alternative specifications that address concerns about specific violations of the parallel trends assumption.

We implement this difference-in-differences approach by estimating the specification:

$$Y_{it} = \alpha + \beta \text{Preclearance}_i + \delta \text{Shelby}_t + \tau \text{Preclearance} \cdot \text{Shelby}_{it} + \text{County} + \text{Year} + \epsilon_{it}$$

Here, Y_{it} represents registration or turnout. Preclearance is an indicator variable for whether county i was subject to preclearance before 2013. Shelby is an indicator for whether or not the year post-dates the *Shelby v. Holder* decision: this variable takes on a value of 0 for the years 2008-2012, and a value of 1 for the years 2014-2018. We include two-way fixed effects in the form of a fixed effect for each county and a fixed effect for each year. Our treatment effect of interest, τ, can be interpreted as the average difference in group turnout or registration between preclearance and non-preclearance counties in the period after *Shelby* relative to the period before. Throughout the paper, we present block-bootstrapped standard errors (Bertrand, Duflo and Mullainathan, 2004).

Our main specification for estimating τ uses estimates of Black and Hispanic voter registration and turnout rates constructed from Catalist and Census data as described

other sources would indicate, potentially due to outdated or "deadwood" registrations for people no longer living in the county. This overestimate should not pose a problem for the diff-in-diff setup unless there are specific time-varying geographic differences in registration purge patterns, which are unlikely to occur in a way that would produce positive (as opposed to negative) bias in the estimates. But we would not directly interpret the levels of registration shown here as true registration rates among current residents.

above. We weight these models by the estimated size of each racial group in each county. This approach limits the impact that measurement error in small counties can have on our estimates.[9] And substantively, we are interested in turnout among voters, not among counties, so it makes sense to upweight the counties with more people in them.

The specification above is equivalent to a canonical two-group difference-in-differences estimator. Our data consists of between 3130 and 3142 counties each election year from 2008 to 2018. Approximately 900 of these counties were subject to preclearance until the *Shelby* decision in 2013, and are therefore all treated in 2014, 2016, and 2018 (and untreated in 2012 and all prior years). The remaining 2238 counties are untreated for the entire period. Small variations in the number of counties included each election year result from differences in Census data availability at the county level in different years. The nature of the *Shelby* decision implies no variation in treatment timing: all preclearance counties under Sections 4(b) and 5 were simultaneously allowed to implement changes to voting law without direct federal supervision. Figure 20 in the SI provides a detailed distribution of treatment history for each county. Given the structure of treatment, τ corresponds to the average treatment effect on the treated (ATT) (Bertrand, Duflo and Mullainathan, 2004). While a wealth of recent literature has addressed difference-in-differences assumptions and estimation strategies when researchers do encounter variation in treatment timing (Goodman-Bacon, 2018; Callaway and Sant'Anna, 2020), these corrections specifically target cases more complex than the two-group case we present in this paper. To address

[9]Combining distinct datasets from Catalist and the Census occasionally yields strange patterns, as in counties with small Black populations where Catalist's estimated number of Black voters exceeds the Census' estimate of Black eligible voters in the county. Rather than censoring the estimates at 100% turnout (and potentially introducing other biases), we keep all estimates for counties with group populations above 100 people, but upweight larger and thus better-estimated counties. Unweighted estimates are shown in the SI and yield similar conclusions.

concerns that this two-way fixed effects estimator may rely too heavily on modeling assumptions (Imai and Kim, 2020; Ho et al., 2007), Appendix B.9 presents estimates from matching methods proposed by Kim, Wang and Imai (2018).

3.1 Estimates

Figure 3 presents estimates of the effect of the *Shelby* decision on Black and Hispanic voter registration and turnout rates in affected counties. A point estimate of .02, as we see for Black turnout, indicates that Black turnout was two percentage points *higher* in previously-preclearance counties than we would have expected without the *Shelby* decision. Across the groups and outcomes examined, we see null effects or small increases in participation after the decision. Figure 10 in the SI presents estimates in terms of absolute numbers of voters rather than turnout rates, which show a similar pattern.

In the wake of *Shelby*, it appears minority registration and turnout in formerly preclearance counties have been flat or increasing relative to counties that were not covered. We note that this approach examines net effects; they do not imply that all voters in all counties became more likely to vote. These estimates do not contradict specific examples of voter suppression or vote reductions in specific counties explored in media coverage. But the aggregate effect appears to be small increases in registration and voting among Black and Hispanic voters. In Section 4, we explore some specific mechanisms that could underlie this overall pattern.

3.2 Robustness

These findings may be surprising, but we do not think they are an artifact of our data or analytic choices. Section B.6 of the Supporting Information discusses robustness of these patterns to a range of alternate specifications. These include restricting analyses only to the South as well as to only presidential or only midterm years, including covariates, and sequentially dropping specific years or states from the dataset. We also present dynamic

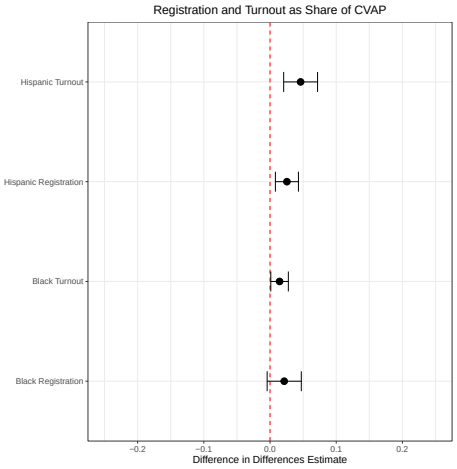


Figure 3: Difference-in-Differences for Black and Hispanic Turnout and Registration

estimates following the approach of Callaway and Sant'Anna (2020). The SI also presents a triple-differences analysis that compares changes in minority turnout to changes in white turnout to see whether these effects are concentrated among voters from minority groups; the estimates are directionally consistent with such a pattern but extremely noisy.

Finally, we note that these findings are consistent with patterns seen in several other data sources. In the online appendix (Section C), we present data on overall registration and vote counts from two sources: the Catalist data described above, and David Leip's election atlas. Though this approach does not include breakdowns of registrants or voters by race, it does allow for a comparison of overall registrant and voter counts between previously-covered places and other places before and after 2013. A difference-in-differences analysis like the one above finds similar patterns: if anything, registration and turnout appear to have increased in previously-covered places since 2013, relative to non-covered places. And in a paper similar in focus to this one, Raze (2021) analyzes survey estimates of minority voter participation from the CCES and finds "resilience" in that *Shelby* did not reduce (and may have increased) Black voters' relative share of the electorate in previously preclearance states. In short, a variety of data sources and model specifications point to

unchanged or increased turnout in previously-preclearance jurisdictions after *Shelby*.

4 Mechanisms

In the previous section, we found that after the *Shelby* decision, minority registration and voting did not decrease in previously-covered places: if anything, they increased slightly. But this finding raises more questions than it answers. Once freed from federal oversight, did previously-covered jurisdictions choose not to make any previously-forbidden changes to their elections? Or did they make changes that were either ineffective at suppressing voting, or were potentially countered by grassroots mobilization efforts? In this section, we examine those details.

We note the limitations of this look at mechanisms: it is difficult to test for specific causal mechanisms that yield a given effect or non-effect (Bullock, Green and Ha, 2010). Here, we look for suggestive evidence that various possible outcomes of the *Shelby* decision occurred, not for a conclusive test of their causal impact on turnout or registration. We look in particular at two possible responses to the Supreme Court's decision: changes in election administration at the state or local level, and changes in mobilization efforts by community organizations and voting-rights advocates. These responses could plausibly have opposing effects on minority voter participation.

4.1 Election Changes

Under preclearance, covered places had to submit any proposed changes in their election practices to the federal government. With that requirement removed, one possible outcome was that states and municipalities would make dramatic changes to their election laws or practices that would not previously have been allowed. States might pass voter identification laws that would not have passed muster under preclearance, or counties or cities might take the opportunity to remove voters from the rolls or make it less convenient to vote. Indeed, advocates have highlighted some high-profile changes that took place shortly

after the decision. A 2014 Brennan Center report pointed out nearly-immediate changes in voter identification statutes, as well as reductions in early voting periods (Lopez, 2014).

We examine several measures of state and local election changes. First, we use data from the National Conference of State Legislatures (NCSL) to observe whether previously-covered states became more likely to implement voter ID laws in the wake of the *Shelby* decision. Then, we use data from the Election Administration and Voting Survey (EAVS) of local elections offices to see whether previously-covered municipalities became more likely to purge registrants from the voter rolls or to reduce polling-place resources after 2013. In each case, we use a difference-in-differences approach similar to our main analyses above: we compare time trends from before to after the 2013 decision, between places that were and were not affected by the decision.

These outcome measures are far from a complete picture of potential changes to state and local election practices. Nor do they all represent practices that have been consistently linked to changes in minority voter participation. Voter identification laws, for example, impose disproportionate burdens on voters of color, but do not appear to dramatically reduce their overall voter turnout (White, Nathan and Faller, 2015; Barreto et al., 2019; Grimmer and Yoder, 2019; Cantoni and Pons, 2019; Zhang, 2022). However, these are changes that can be observed using extant data, and we intend them as a test of the idea that jurisdictions changed their election practices when given the opportunity. We anticipate that a variety of other harder-to-observe changes could also have taken place; though our evidence cannot directly test for those other changes, these highly-visible measures are a natural place to start looking.

Voter ID laws We begin by examining states' implementation of voter identification laws, relying on the National Conference of State Legislatures' detailed history of voter

ID.[10] For this analysis, we follow the NCSL in recording whether a state had any voter identification requirement (beyond the requirements of the Help America Vote Act) in place in a given year, as well as whether the state had a photo-ID requirement and whether the state had a "strict" requirement that actually required (rather than requesting) an ID in order to cast a regular ballot. For each of these three measures, we focus on whether the state actually had an active ID law in place in a given year (not a law that passed but would be implemented in future years or was delayed by litigation).

Figure 4 shows the time trends in voter ID laws in previously covered and non-covered places between 2001 and 2018.[11] Preclearance states were more likely to have any ID law in place than non-preclearance states, even before the *Shelby* decision. But the two groups appear to follow broadly common trends both before and after the decision: it doesn't seem that preclearance states began implementing substantially more voter ID laws in the wake of the decision, perhaps due to ceiling effects (nearly all of these states already had

[10] We collected the NCSL data from its website: https://www.ncsl.org/research/elections-and-campaigns/voter-id-history.aspx. For a handful of places with unclear legal status, and for 2016-2018, we supplement the NCSL data with information from Ballotpedia.

[11] We diverge from the previous section's focus on counties and use states as the unit for this analysis, because voter ID laws are passed at the state level. We consider Alabama, Alaska, Arizona, Georgia, Louisiana, Mississippi, South Carolina, Texas, and Virginia to be covered. The estimates are robust to including partially-covered North Carolina as a covered state; including all 15 states with any covered jurisdictions (such as New York and Michigan) as covered yields estimates that point in the same direction but are smaller and noisier. Omitting partially-covered states from the analysis in light of their differences from fully-covered states yields similarly-sized but slightly noisier estimates to those shown in Table 1.

some sort of voter ID law on the books by 2013).

However, the *content* of state laws changed dramatically after the decision. The central panel of Figure 4 demonstrates that both groups of states followed similar trends in the implementation of photo ID laws prior to *Shelby*, but that previously-covered states rapidly implemented photo ID laws after the decision took effect. This pattern is consistent with high-profile cases of photo ID laws that had previously been blocked via the preclearance process but were then implemented after the court's 2013 decision, as happened in Texas. In the rightmost panel (looking at "strict" ID laws), we also see a sudden increase after 2013, though the pre-trends are slightly less comparable there. Further, both strict and photo ID laws have dropped since their immediate post-*Shelby* peaks in previously-covered places, perhaps due to litigation that has gradually led to these laws being removed or rewritten.

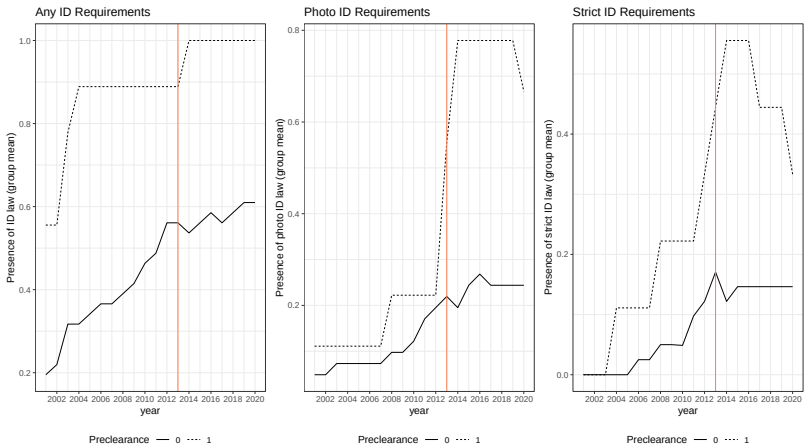


Figure 4: Time trends in types of voter ID laws as recorded by the NCSL. In all panels, the dotted line represents mean values in preclearance or formerly-preclearance states, while the solid line represents non-covered states.

Table 1 presents difference-in-difference estimates of these patterns: indeed, previously-covered states became substantially more likely to implement strict and photo ID laws after the *Shelby* decision.

Table 1: NCSL Difference-in-Differences Estimates for Preclearance After Shelby

Outcome	Diff-in-Diff	Classical SE	Bootstrapped SE	95% CI	p-val
Any ID Law	-0.04	0.04	0.10	(-0.23, 0.16)	0.71
Photo ID Law	0.44	0.04	0.14	(0.16, 0.73)	0.002
Strict ID Law	0.25	0.04	0.12	(0.01, 0.48)	0.04

EAVS data Next, we turn to data on local election administration. County-level election officials have substantial discretion in administering elections, as they are typically tasked with recruiting and training pollworkers, maintaining voter registration lists, siting polling places, and directing ballot counting. Voters of color have worse voting experiences than white voters on average (Chen et al., 2019), and election officials' decisionmaking could potentially contribute to racial disparities. There is evidence that local clerks and pollworkers discriminate in implementing and providing information about election laws, particularly in jurisdictions not subject to pre-clearance (Cobb, Greiner and Quinn, 2010; White, Nathan and Faller, 2015). It is plausible that local election officials freed from VRA oversight might make decisions about election administration that would disadvantage voters of color, though we note other work that has found election officials do not use their decisionmaking power to attempt to advantage their preferred group (along partisan lines: see Ferrer, Geyn and Thompson (2021)).

For measures of local election administration, we use the Election Administration and Voting Survey (EAVS), conducted during election years by the US Election Assistance Commission. Since 2004, the EAC has sent surveys to election officials across the country, asking questions about their election practices and about registration and voting in their jurisdictions. We reviewed the survey for any questions that might indicate changes in local election administration that could potentially make it easier or more difficult for minority voters to participate. Section D of the SI discusses the process of cleaning this dataset.

We examine three measures of election administration, all displayed in Figure 5. We follow previous work in examining the removal or "purging" of registrants from the voter

file (Feder and Miller, 2020). We follow the Pew Elections Performance Index in constructing a measure of the provisional ballot rejection rate (the number of provisional ballots cast but not counted divided by the total votes cast). Given public attention to poll closures (The Leadership Conference Education Fund, 2019), we also examine the number of poll-workers per registered voter as a measure of election-day capacity.[12] The EAVS measures are suggestive of some post-*Shelby* electoral changes, but there is substantial uncertainty around these estimates.

The top panel of Figure 5 shows trends in the registration removal rate, based on an EAVS question that asks officials to report the total number of voters removed from the voter registration rolls between the close of registration for the previous general election and the close of registration for the current year's general election. We follow Feder and Miller (2020) in calculating a registration removal rate, dividing the number of removals by the overall number of registered voters in that jurisdiction in that year.[13] It appears that previously-covered places moved from removing similar shares of voters from the rolls (or even fewer) to removing substantially more voters than non-covered places, beginning in 2014. The first row of Table 2 reports difference-in-differences estimates of this relationship. The positive coefficient is consistent with previously-covered places starting to purge more voters after the *Shelby* decision, in line with the conclusions of previous work by Feder and Miller (2020).

The second panel of Figure 5 shows a measure of the provisional ballot rejection rate

[12]We include these measures given high-profile cases in which advocates asserted that polling place closures were designed to disproportionately inconvenience minority voters. But we acknowledge that this measure may not make as much sense in jurisdictions that are moving to vote-by-mail systems, and that overall polling place counts could obscure racialized patterns of poll closures in specific neighborhoods.

[13]An approach that instead benchmarks each year's removals to the jurisdiction's 2008 (pre-treatment) registration counts yields equivalent conclusions.

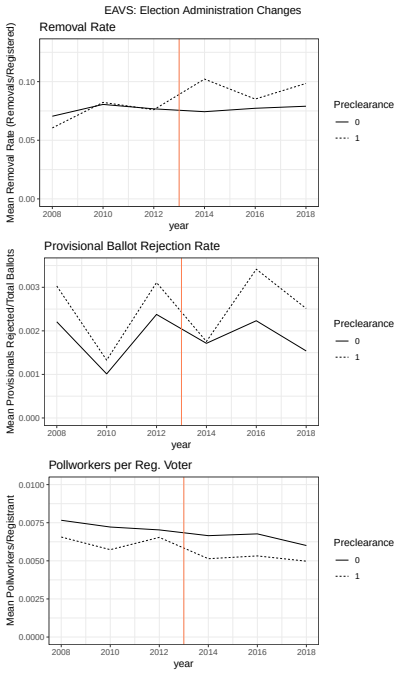


Figure 5: Time trends in election administration as reported in EAVS survey of jurisdictions. In all panels, the dotted line represents mean values in preclearance or formerly-preclearance counties, while the solid line represents non-covered places.

Table 2: EAVS Difference-in-Differences Estimates for Preclearance After Shelby

Outcome	Diff-in-Diff	Classical SE	Bootstrapped SE	95% CI	p-val
Registration Purge Rate	0.0294	0.0016	0.0160	(0, 0.06)	0.0660
Provisional Reject Rate	0.0001	0.0001	0.0006	(0, 0)	0.8600
Pollworkers / Reg. Voter	0.0001	0.0001	0.0002	(0, 0)	0.7642

over time in affected and unaffected jurisdictions.[14] Having many provisional ballots cast and ultimately rejected could indicate issues with the voting process: inaccurate registra-

[14]We follow the Pew Elections Performance Index in calculating the provisional rejection rate as a share of all ballots cast rather than as a share of provisionals cast: states use provisional ballots at different rates for many reasons, and we are particularly interested in the influence that the rejection of provisional ballots has on the overall vote count, not just on the count of provisional ballots.

tion data, confusing voting instructions that make it hard for people to find their polling place, or poorly-trained pollworkers. Jurisdictions affected by the *Shelby* decision had somewhat higher provisional-rejection rates than other jurisdictions even before 2013, but covered and non-covered places follow similar trends in the pre-2013 period. After 2014, the trends appear to diverge, with previously-covered places increasing their provisional-ballot rejections more steeply than unaffected places; this pattern would be consistent with it becoming harder to vote in these affected places post-*Shelby*. But this increase is small enough in magnitude that we cannot statistically distinguish it from zero (see row 2 of Table 3), so we present these estimates with caution.

The final panel of Figure 5 shows trends in the number of pollworkers per registered voter. Affected places consistently use fewer pollworkers than unaffected places. But that difference does not appear to increase substantially after the Shelby decision, as seen both in the figure and in the third row of Table 3.

Two of the three election-administration measures we examined showed noisy but suggestive evidence of growing registration and voting difficulties in previously-covered places after the *Shelby* decision, while the third measure (pollworker density) showed essentially no change. Combined with the NCSL data on voter identification laws, we think it is plausible that some aspects of election administration in previously-covered places changed in the wake of the *Shelby* decision, while other policies remained unchanged on average.

4.2 Countermobilization

As noted in Section 2.3, we observe overall patterns of turnout and registration that could result from a mix of negative and positive forces operating on turnout. In the previous section, we looked at election changes that could have made it harder to vote. Here, we look for evidence that efforts to register and mobilize Black and Hispanic voters increased after the *Shelby* decision, or that voters saw more reason to turn out.

We begin by noting that this paper's main estimates hint at the presence of countermo-

bilization, since we see increases in Black and Hispanic participation rates in previously-preclearance places after the *Shelby* decision. And there are prominent examples of GOTV efforts explicitly targeted to counter potential voter suppression in the wake of the decision: earlier in the paper, we noted the SPLC's "Vote Your Voice" campaign and its references to *Shelby*. Similarly, major philanthropic donors gave to the Shelby Response Fund, set up to "support legal, organizing, and public education work focused on protecting voting rights in states formerly covered under Section 5 of the Voting Rights Act."[15] Though it is difficult to quantify all of the get-out-the-vote efforts of many disparate organizations, it is plausible that they ramped up in the wake of the *Shelby* decision.

We turn to survey data to look for evidence of such efforts. The Cooperative Election Study (CES, formerly CCES) is run every two years. From 2010 through 2016, the survey asked whether people had been contacted during the election cycle by a campaign organization or candidate. We use this question, combined with information about respondents' county of residence and self-reported racial identity, to see whether campaigns' GOTV efforts targeted at voters of color increased in previously-covered places after the *Shelby* decision. This question does not capture all possible mobilizing activity, since it is focused on campaigns and not other groups' efforts, but it gives a consistent view of mobilization efforts across time and geography.

We present these results with caution, as the CCES is designed to be a nationally-representative survey, not to yield precise estimates within small geographic areas or for segments of the population (Grimmer et al., 2018). It is also difficult to judge whether covered and non-covered places had similar pre-*Shelby* trends, since these questions were asked in only a handful of years before the decision.[16] Still, we present these analyses as a preliminary look at the phenomenon of countermobilization. We approximately follow the specification of Cantoni and Pons (2019), though we focus on a *"Shelby v. Holder"*

[15] https://www.macfound.org/grantee/neo-philanthropy-39197/

[16] Appendix Figure 23 plots this measure over time.

Table 3: Self-Reported Mobilization (CCES)

	Dependent variable:			
	Mobilization			
	(1)	(2)	(3)	(4)
Preclearance	−0.063*	−0.070*	−0.027*	−0.034*
	(0.024)	(0.027)	(0.012)	(0.010)
Post-Shelby	−0.125*	−0.110*	−0.120*	−0.096*
	(0.013)	(0.015)	(0.012)	(0.013)
Non-white			0.013	0.022
			(0.029)	(0.028)
Preclearance * Post-Shelby	0.034	0.032*	0.027*	0.023
	(0.010)	(0.015)	(0.010)	(0.017)
Preclearance * Non-white			−0.036	−0.033
			(0.024)	(0.022)
Post-Shelby * Non-white			−0.006	−0.052*
			(0.021)	(0.020)
Preclearance * Post-Shelby * Non-white			0.022	0.033
			(0.013)	(0.021)
Constant	0.716*	0.686*	0.688*	0.655*
	(0.026)	(0.027)	(0.013)	(0.010)
Race-by-state FE's	X	X	X	X
Race-by-year FE's	X	X	X	X
Survey Weights		X		X
Observations	273,407	273,407	273,407	273,407
R^2	0.044	0.028	0.054	0.038
Adjusted R^2	0.044	0.028	0.054	0.037
Note:				*p<0.05

treatment rather than the voter ID laws they considered. Standard errors are clustered at the county level.

We begin by asking whether voters experience different mobilization trends in places that were and were not affected by the *Shelby* decision.Columns 1 and 2 of Table 3 indicates that voters in previously-covered places reported extra campaign mobilization after the *Shelby* decision. These voters saw about three percentage points' higher rates of campaign contact after the decision than would otherwise have been expected (shown by the interaction between "preclearance" and "post-*Shelby*" in the table).

In the second half of Table 3, we ask whether that additional mobilization was focused on minority voters, as we would expect if it were driven by efforts from groups worried about voting rights. Here, the coefficient of interest is the interaction between "Preclearance" (whether a jurisdiction was covered by preclearance before 2013), "Post-Shelby" (whether the observation is from before or after the 2013 *Shelby* decision), and "Non-white." In both specifications, this coefficient is positive, suggesting more mobilization of nonwhite voters in previously-covered places after the *Shelby* decision. This pattern is consistent with a story about countermobilization, though the estimates are noisy and not statistically distinguishable from 0.

Next, we turn to another survey dataset to look at voters' perceptions of the electoral system. As noted above, some research in political psychology finds that voters can react strongly to perceived attempts to disenfranchise them. Voters could potentially react to the *Shelby* decision or the electoral changes that followed with backlash, perhaps becoming more likely to vote in the wake of those changes.[17] We are not aware of any panel survey

[17]Unlike the analysis of countermobilization above, which asked whether someone had been explicitly asked to vote, this mechanism could occur even without organizations or campaigns communicating with voters about the threat (if voters simply saw news stories, for example). However, we think it is possible that political organizations help spread the word about possible threats to the franchise.

that asks voters directly whether they think voting rights are under threat. However, the Survey on the Performance of American Elections (SPAE) asks voters whether they believe their vote was counted as intended in the most recent election. Although this is not a question explicitly about voting rights, we expect it to capture respondents' views about the integrity of the electoral system in their area, which should give an idea of whether they are concerned about voting access for people like them.

The SPAE surveys registered voters in a sample of states about their experiences in each federal election (Stewart, 2017). We rely on responses to the question "How confident are you that your vote in the General Election was counted as you intended?" from 2008, 2012, 2014, and 2016.[18] We focus on the share of voters reporting that they were "not too confident" or "not at all confident" that their votes were counted as intended; Figure 6 plots this measure over time for all respondents and for white and non-white voters separately. In these simple unweighted plots, it appears respondents in places previously covered by pre-clearance follow similar trends to those in other places before the *Shelby* decision, but then show much higher rates of skepticism about the electoral process after the decision; this pattern is particularly striking among non-white voters.

Table 4 again presents difference-in-differences estimates for all voters and then considers nonwhite voters specifically. Columns 1 and 2 show that after the 2013 *Shelby* decision, voters in previously-covered places became several percentage points more likely to say they doubted their vote was counted as intended. In exactly the places where we might think voters would be turning out due to a sense of foreboding about voting rights, we see more voters expressing a lack of confidence in elections. Columns 3-4 ask whether this pattern is especially pronounced for non-white voters, as we would expect if minority vot-

[18]The SPAE was not run in 2018, and though it resumed in 2020 we omit responses from that year out of concerns that the political environment in that year would lead respondents to interpret the question differently than they had before (that is, as an opportunity to embrace or reject Donald Trump's claims of election fraud in the presidential election).

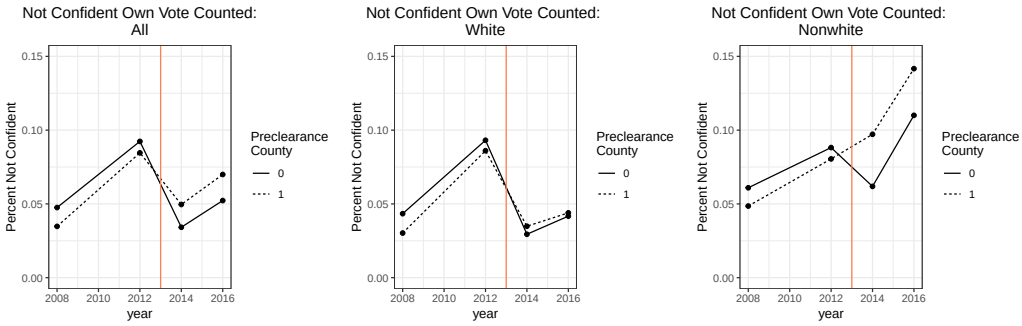


Figure 6: SPAE respondents' lack of confidence in vote counting by race and by home county's pre-clearance status.

ers were responding to perceived threats to voting rights after the *Shelby* decision. When we include an interaction between previous pre-clearance status, the post-*Shelby* period, and voter race, we see a pattern consistent with higher rates of concern among minority voters in affected places. The coefficient on the "Preclearance * Post-Shelby" effect shrinks substantially, and the "Preclearance * Post-Shelby * Non-white" coefficient suggests that nonwhite voters in affected places became several percentage points more likely to say they were not confident about vote-counting after the *Shelby* decision, though this coefficient is somewhat noisily-estimated and cannot be statistically distinguished from 0.

4.3 Discussion of Mechanisms

Section 4.1 established that previously-covered places changed some of their election practices in the wake of *Shelby*, suggesting that their behavior had previously been constrained by preclearance. Section 4.2 suggests minority voters may have become more likely to be invited to participate in politics in previously-covered places after *Shelby*, consistent with a story about countermobilization. There was also a dramatic decline in voter confidence in previously-covered places, seemingly driven by non-white voters. And the net effect of *Shelby* on Black and Hispanic participation appears to be a small increase in

Table 4: Lack of Confidence that Own Vote Counted Correctly (SPAE)

	Dependent variable:			
	Not Confident Own Vote Counted			
	(1)	(2)	(3)	(4)
Preclearance	−0.004	−0.007	−0.008	−0.011
	(0.008)	(0.010)	(0.007)	(0.007)
Post-Shelby	−0.008	−0.009	−0.009	−0.013
	(0.006)	(0.009)	(0.005)	(0.007)
Non-white			−0.005	0.007
			(0.017)	(0.029)
Preclearance * Post-Shelby	0.023*	0.031*	0.010	0.016
	(0.009)	(0.010)	(0.010)	(0.012)
Preclearance * Non-white			0.025	0.022
			(0.019)	(0.032)
Post-Shelby * Non-white			0.021	0.026*
			(0.011)	(0.012)
Preclearance * Post-Shelby * Non-white			0.031	0.034
			(0.020)	(0.027)
Constant	0.058*	0.071*	0.059*	0.071*
	(0.007)	(0.012)	(0.007)	(0.009)
State FE's	X	X	X	X
Year FE's	X	X	X	X
Survey Weights		X		X
Observations	28,678	28,678	28,678	28,678
R^2	0.014	0.014	0.022	0.024
Adjusted R^2	0.012	0.012	0.018	0.020
Note:				*p<0.05

registration and voting in affected places. How do we square these patterns?

We note, first, that the positive turnout effects shown in Section 3 indicate a key role for voter activation. Someone or something is inspiring Black and Hispanic voters to register and turn out in previously-covered places, and those forces are enough to yield a visible positive change in participation in recent years. Both current events and academic research present possible descriptions of this mobilizing force. For one thing, individual voters may react to perceived threats to voting rights by turning out, consistent with the patterns seen in the SPAE survey data (Biggers and Smith, 2018; Biggers, 2019; Valentino and

Neuner, 2017). Further, voters may be invited to participate by campaigns or by grassroots organizations seeking to counter vote suppression; a high-profile recent example of this kind of effort is Stacey Abrams' Georgia-based organization Fair Fight. Many individual activists and grassroots organizations small and large work to encourage members of their communities to register and vote each election cycle, and we think it is likely that these efforts gained both urgency and resources in the post-*Shelby* era. But the work of grassroots organizations is notoriously difficult to observe at a national level, so we do not have data that allows us to systematically characterize this kind of mobilization.

Readers may also wonder how much vote suppression resulted from the *Shelby* decision, even if it was numerically offset by mobilization efforts. This is a difficult question to answer. There are certainly high-profile and egregious examples of vote suppression efforts in previously-covered places.[19] But it is harder to systematically measure changes in election practices across many jurisdictions, and harder still to link those changes to reduced voter participation. As we note above, the particular policy changes we have identified in the wake of *Shelby* have shown limited effects on participation. Voter identification laws appear to directly reduce turnout among the small number of people who do not have the required forms of identification; Grimmer and Yoder (2019) puts the size of this effect at several thousand voters in North Carolina's 2016 elections, a small fraction of the state's electorate. But it is harder to know whether there are other deterrent effects among people who have identification but might be confused about the law or otherwise prevented from voting. Studies focused on aggregate voter turnout (as opposed to people without ID) have found limited turnout effects. Cantoni and Pons (2019), relying on a decade-long national panel of voter file data, report null effects of voter ID laws on overall turnout,[20] though

[19] A rural county in Georgia, for example, faced a federal lawsuit and ultimately agreed to external monitoring after it attempted to purge nearly one-fifth of the county's voters, nearly all of them Black, from the voter rolls (McLaughlin, 2021).

[20] They report point estimates of up to one percentage point reduction in overall voter

like us they note that these null effects could be due to a combination of vote suppression and active countermobilization.

Of course, voter identification laws may have only been the most easily-observed part of a broader suite of election-administration changes undertaken after *Shelby*, making any discussion of the voter-identification literature incomplete for this purpose. In sum, our evidence (and the broader literature) do not allow us to guess at how many individual voters may have been prevented from voting in the wake of *Shelby*, even as others were being mobilized. Our overall estimates (of increases in minority voter participation in previously-covered places) should not be interpreted as evidence that vote suppression is non-existent.

5 Conclusion

We have used a wide variety of data sources to examine the effect of the Supreme Court's 2013 decision in *Shelby v. Holder* on the voting landscape for members of historically-excluded groups. It does not appear that Black or Hispanic registration or voter turnout have dropped in previously-covered places since that decision; if anything, it seems participation has increased. These increases have occurred despite real changes in election practices in jurisdictions previously covered by preclearance. We see clear changes in voter identification laws, and suggestive evidence of changes in local practices such as registration purges and provisional ballot rejections. And we observe survey responses consistent with increased mobilization efforts in previously-covered places. These disparate results suggest opposing forces: localities have indeed taken advantage of the *Shelby* decision to implement some voting changes that would not have been allowed under preclearance. But it appears that these changes have either not affected voter participation, or that any negative effects

turnout, not statistically distinguishable from zero; they note that the analyses can rule out reductions of 2.7 percentage points or more.

have been swamped by counter-mobilization efforts or public backlash against perceived threats to voting rights. Voter participation among historically-excluded groups has been resilient in the face of recent events.

Such a short-term pattern raises questions for the future. Will public outrage against election changes persist, or will the mobilizing effects of legal changes eventually wane even as the burdens they pose to voters persist? Will jurisdictions gradually impose more extreme changes that might be more effective at deterring minority voters?[21] Further, we wonder what kinds of compositional effects these two forces (of voting changes and voter counter-mobilization) might have on the electorate. It is plausible that some small number of voters are deterred by changes to election practices (Grimmer and Yoder, 2019), while a different pool of voters are mobilized by concerns about voting rights or get-out-the-vote efforts (Enos, Fowler and Vavreck, 2014). We might see stable rates of Black or Hispanic voter participation, but it is possible that the set of people casting those votes is shifting in patterns that shape local politics in important ways.

Finally, we underscore the limits of this analysis both in time and in the outcomes considered. The question of *Shelby*'s effect on voters was so pressing that we thought it important to begin preliminary investigations. But we acknowledge that some of the concerns raised by Justice Ginsburg and voting-rights advocates were about matters like vote dilution and the process of redistricting, not solely on individual voter participation. We are only now entering the first full redistricting cycle since the *Shelby* decision, and that process merits additional attention.

[21] The Court's recent decision in *Brnovich*, limiting the scope of legal cases under Section 2 of the VRA, might mean that previously-covered jurisdictions have even more leeway to change their election practices going forward.

References

Ang, Desmond. 2019. "Do 40-year-old facts still matter? Long-run effects of federal oversight under the Voting Rights Act." *American Economic Journal: Applied Economics* 11(3):1–53.

Ansolabehere, Stephen, Bernard Fraga and Brian Schaffner. 2020. "The CPS Voting and Registration Supplement Overstates Minority Turnout.".

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

Ansolabehere, Stephen and Eitan Hersh. 2014. "Voter registration: The process and quality of lists." *The measure of American elections* pp. 61–90.

Barreto, Matt, Stephen Nuño, Gabriel Sanchez and Hannah Walker. 2019. "The racial implications of voter identification laws in America." *American Politics Research* 47(2):238–249.

Bedolla, Lisa Garcia and Melissa Michelson. 2012. *Mobilizing inclusion: Transforming the electorate through get-out-the-vote campaigns.* Yale University Press.

Bertrand, Marianne, Esther Duflo and Sendhil Mullainathan. 2004. "How Much Should We Trust Difference-in-Differences Estimates?" *Quarterly Journal of Economics* 119(1):249–275.

Biggers, Daniel. 2019. "Can the Backlash Against Voter ID Laws Activate Minority Voters? Experimental Evidence Examining Voter Mobilization Through Psychological Reactance." *Political Behavior* pp. 1–19.

Biggers, Daniel and Daniel Smith. 2018. "Does threatening their franchise make registered voters more likely to participate? Evidence from an aborted voter purge." *British Journal of Political Science* pp. 1–22.

Bullock, John, Donald Green and Shang Ha. 2010. "Yes, but what's the mechanism?(don't expect an easy answer)." *Journal of personality and social psychology* 98(4):550.

Burden, Barry. 2000. "Voter Turnout and the National Election Studies." *Political Analysis* 8(4):389–398.

Callaway, Brantly and Pedro Sant'Anna. 2020. "Difference-in-differences with multiple time periods." *Journal of Econometrics* .

Cantoni, Enrico and Vincent Pons. 2019. Strict ID Laws Don't Stop Voters: Evidence from a US Nationwide Panel, 2008–2016. Technical report National Bureau of Economic Research.

Cascio, Elizabeth and Ebonya Washington. 2014. "Valuing The Vote: The Redistribution of Voting Rights and State Funds Following The Voting Rights Act of 1965." *Quarterly Journal of Economics* 129:1–55.

Chen, M Keith, Kareem Haggag, Devin G Pope and Ryne Rohla. 2019. "Racial disparities in voting wait times: evidence from smartphone data." *The Review of Economics and Statistics* pp. 1–27.

Cobb, Rachael V, D James Greiner and Kevin M Quinn. 2010. "Can voter ID laws be administered in a race-neutral manner? Evidence from the city of Boston in 2008." *Quarterly Journal of Political Science* 7(1):1–33.

Enos, Ryan, Anthony Fowler and Lynn Vavreck. 2014. "Increasing inequality: The effect of GOTV mobilization on the composition of the electorate." *The Journal of Politics* 76(1):273–288.

Feder, Catalina and Michael Miller. 2020. "Voter Purges After Shelby." *American Politics Research* p. 1532673X20916426.

Ferrer, Joshua, Igor Geyn and Daniel Thompson. 2021. How Partisan Is Local Election Administration? Technical report Working Paper.

Fraga, Bernard. 2016. "Candidates or districts? Reevaluating the role of race in voter turnout." *American Journal of Political Science* 60(1):97–122.

Fraga, Bernard. 2018. *The turnout gap: Race, ethnicity, and political inequality in a diversifying America*. Cambridge University Press.

Fraga, Luis and Maria Ocampo. 2006. "More information requests and the deterrent effect of section 5 of the voting rights act." *Voting Rights Act Reauthorization of 2006: Perspectives on Democracy, Participation, and Power* .

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

Goodman-Bacon, Andrew. 2018. Difference-in-differences with variation in treatment timing. Technical report National Bureau of Economic Research.

Green, Donald and Alan Gerber. 2019. *Get out the vote: How to increase voter turnout*. Brookings Institution Press.

Grimmer, Justin, Eitan Hersh, Marc Meredith, Jonathan Mummolo and Clayton Nall. 2018. "Obstacles to estimating voter ID laws' effect on turnout." *The Journal of Politics* 80(3):1045–1051.

Grimmer, Justin and Jesse Yoder. 2019. The Durable Deterrent Effects of Strict Photo Identification Laws. Technical report Working Paper.

Grofman, Bernard and Lisa Handley. 1991. "The Impact of the Voting Rights Act on Black Representation in Southern State Legislatures." *Legislative Studies Quarterly* 16:111–128.

Grofman, Bernard, Lisa Handley and Richard Niemi. 1992. *Minority Representation and the Quest for Voting Equality.* Cambridge University Press.

Ho, Daniel, Kosuke Imai, Gary King and Elizabeth Stuart. 2007. "Matching as Nonparametric Preprocessing for Reducing Model Dependence in Parametric Causal Inference." *Political Analysis* 15:199–236.

Huber, Gregory, Marc Meredith, Michael Morse and Katie Steele. 2021. "The racial burden of voter list maintenance errors: Evidence from Wisconsin's supplemental movers poll books." *Science Advances* 7(8):eabe4498.

Imai, Kosuke and In Song Kim. 2020. "On the Use of Two-Way Fixed Effects Regression Models for Causal Inference with Panel Data." *Political Analysis* 29:405–415.

Kim, In Song, Erik Wang and Kosuke Imai. 2018. "PanelMatch: Matching Methods for Causal Inference with Time-Series Cross-Section Data." *URL: https://imai. princeton. edu/research/tscs. html* .

Kousser, Morgan J. 1973. "Post-Reconstruction Suffrage Restrictions in Tennessee: A New Look at the V.O. Key Thesis." *Political Science Quarterly* 88:655–683.

Lee, Jay and Paul Gronke. 2020. *cpsvote: A Toolbox for Using the CPS's Voting and Registration Supplement.* R package version 0.1.0.
URL: *https://CRAN.R-project.org/package=cpsvote*

Lopez, Tomas. 2014. "'Shelby County': One Year Later." *Brennan Center for Justice* .

McLaughlin, Elliott. 2021. "In majority-Black Georgia county, voting in Senate runoffs is more about fight to vote than right to vote." *CNN* .
URL: *https://www.cnn.com/2021/01/04/politics/georgia-hancock-county-right-to-vote-senate/index.html*

Perez, Myrna and Vishal Agraharkar. 2013. "If Section 5 Falls: New Voting Implications." *Brennan Center for Justice. http://www. brennancenter. org/publication/if-section-5-falls-new-voting-implications* .

Raze, Kyle. 2021. "Voting Rights and the Resilience of Black Turnout.".

Rosenberg, Gerald. 1991. *The Hollow Hope.* The University of Chicago Press.

Schuit, Sophie and Jon Rogowski. 2017. "Race, Representation, and the Voting Rights Act." *American Journal of Political Science* pp. 513–526.

Shaw, Daron, Stephen Ansolabehere and Charles Stewart III. 2015. "A Brief Yet Practical Guide to Reforming US Voter Registration Systems." *Election Law Journal* 14(1):26–31.

Stewart, Charles. 2017. "2016 Survey of the Performance of American Elections.". **URL:** *https://doi.org/10.7910/DVN/Y38VIQ*

The Leadership Conference Education Fund. 2019. "Democracy Diverted: Polling Place Closures and the Right to Vote.".

Tokaji, Daniel. 2005. "The new vote denial: Where election reform meets the Voting Rights Act." *SCL Rev.* 57:689.

United States Commission on Civil Rights. 2001. *Voting Irregularities in Florida During the 2000 Presidential Election.* The Commission.

Valentino, Nicholas and Fabian Neuner. 2017. "Why the sky didn't fall: mobilizing anger in reaction to voter ID laws." *Political Psychology* 38(2):331–350.

White, Ariel, Noah Nathan and Julie Faller. 2015. "What do I need to vote? Bureaucratic discretion and discrimination by local election officials." *American Political Science Review* 109(1):129–142.

Zhang, Emily Rong. 2022. "Questioning Questions in the Law of Democracy: What the Debate over Voter ID Laws' Effects Teaches about Asking the Right Questions." *UCLA Law Review* .

Supporting Information

Table of Contents

A Pre-clearance Definition

Our definition of "covered" counties (those previously subject to pre-clearance under Section 4 of the VRA) is drawn largely from a list provided by the Department of Justice at https://www.justice.gov/crt/jurisdictions-previously-covered-section-5

We include all counties in fully-covered states as covered, as well as the individual counties included in the DOJ's list. There are also several townships in Michigan and South Dakota that were covered as of 2013; we conservatively include the counties containing these townships as covered in our county-level analyses, though some jurisdictions in these counties were not covered.

In the case of jurisdictions in Virginia and New Hampshire that had "bailed out" of coverage by 2013, we continue to include them as covered here if they bailed out after the year 2003. Many of these bailouts occurred in the decade immediately preceding the *Shelby* decision, meaning that in many ways officials would still need to act as if they were covered (the decade-long "recapture period" would allow them to immediately be bailed back in if they did anything that would have prevented a bailout in the first place: see https://www.justice.gov/crt/section-4-voting-rights-act).

B Catalist Validation and Robustness

B.1 Table from main paper Figure 3

Table 5: Difference-in-Differences Results for Preclearance After Shelby

Outcome	Diff-in-Diff	Classical SE	Bootstrapped SE	95 pct. CI	p-value
Black Reg. Rates	0.02	0.00	0.01	(0, 0.05)	0.10
Black Turnout Rates	0.01	0.00	0.01	(0, 0.03)	0.03
Hispanic Reg. Rates	0.03	0.00	0.01	(0.01, 0.04)	0.00
Hispanic Turnout Rates	0.05	0.00	0.01	(0.02, 0.07)	0.00

B.2 Validating Catalist data against other datasets

We validated the Catalist data we use in this project by comparing it to several other datasets, in hopes of noticing any strange patterns or major errors. We began with a comparison to Current Population Survey estimates. The CPS is often used to produce estimates of turnout by race at the state level, so we aggregated the Catalist dataset to the state level for comparison. We used state-level estimates of citizen voting age population from the ACS (for 2010-2018) to turn the raw Catalist turnout counts into turnout rates comparable to the ones calculated from CPS data. When calculating CPS turnout rates, we

rely on the "cpsvote" R package (Lee and Gronke, 2020), using its "Hur-Achen" approach to nonresponse and its provided weights to handle over-time changes in response rates.

We note that the CPS is not a perfect source of group-specific turnout estimates and should not be treated as the "ground truth," but we nevertheless think it is useful to see how the Catalist-derived estimates we produce compare to the CPS ones. The left panel of Figure 7 shows that comparison for state-specific Hispanic turnout estimates from 2010-2018. The Catalist estimates are on the horizontal axis and CPS estimates are on the vertical axis, with the black diagonal line showing the 45-degree line (along which estimates are exactly the same across the two datasets). Points are scaled by population size (states with larger Hispanic populations appear larger) and shaded by year. These datasets look similar, with points clustered along the 45-degree line. There are some points above and below it, where one source shows much higher turnout than the other, but for the most part these are states with small Hispanic populations (where we expect more measurement error, which is part of why we weight our main estimates by population size). The years cluster somewhat, as expected (turnout in 2016 was higher than in 2014 almost everywhere), but there is not a clear pattern of one year straying farther from the 45-degree line than others.

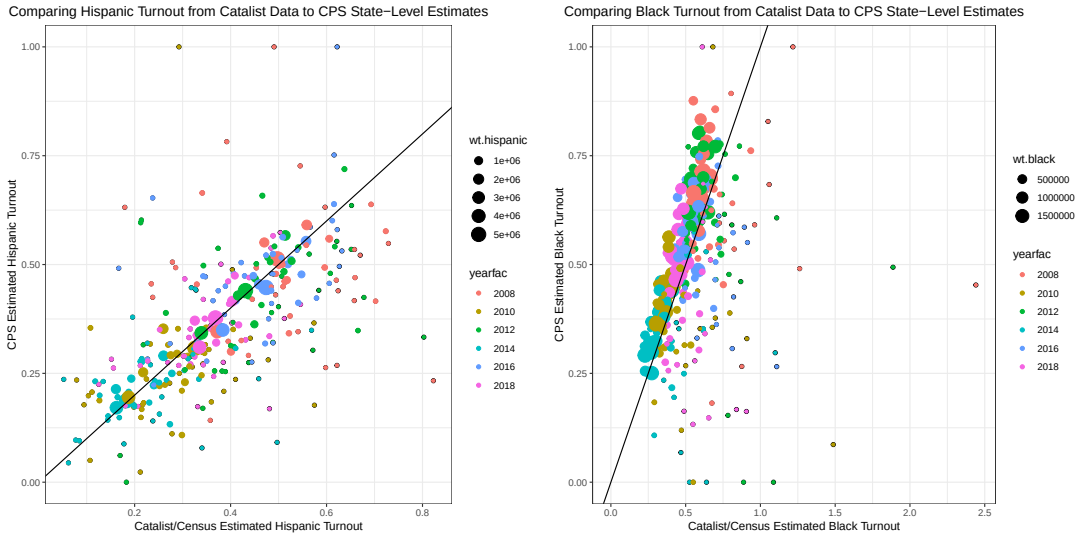


Figure 7: Comparing Catalist Hispanic and Black turnout estimates to CPS-derived estimates

The left panel of Figure 7 compares Catalist and CPS estimates of Black turnout. The diagonal line again shows equivalence between the Catalist and CPS estimates, though in this case the axis is stretched out by the presence of a few extreme outliers in the Catalist data. As noted in the main paper, there are a few places where small Black populations combined with measurement error in either the Catalist turnout estimates or the ACS

estimates yield impossible turnout estimates of over 100%. The two points on the extreme right side of the plot are estimates from North Dakota, a state with a very small number of estimated Black eligible voters and thus a lot of room for measurement error to influence estimated turnout in fairly extreme ways. Given our population-weighted approach to the main estimates, we do not think counties in ND are likely to exert a large influence over our analyses. The estimates are broadly similar across the two datasets, particularly for places with large Black populations (represented by larger points), though the CPS estimates are on average slightly higher than the Catalist ones (consistent with turnout over-reporting on the CPS, as in Ansolabehere, Fraga and Schaffner (2020)).

Next, we compared our county-level Catalist estimates to estimates from David Leip's county-level elections data (obtained for 2008-2016 through the MIT library system). Leip's data reports aggregate registration and turnout counts for each county in each year, not estimates for specific racial groups. Still, we thought it worth summing up our Catalist data to produce county-level estimates of the total number of registered voters and ballots cast for each county year and comparing those to the Leip estimate to diagnose problems.

We were able to merge over 99% of the counties in our main dataset to counties in Leip's data using FIPS codes; the main source of missed matches was a difference in how Alaskan counties/election districts were handled across datasets. The left panel of Figure 8 compares our Catalist turnout estimates to Leip's, with the diagonal line representing equivalence in the two datasets' estimates. The datasets have very similar county turnout numbers; slight differences (points off the line) do not appear systematic across years.The right panel of Figure 8 performs the same exercise for county registration numbers. Again, the estimates line up tidily on the 45-degree line for most county-years.[22]

B.3 Analysis of Raw Catalist Vote Counts

In the main paper, we analyze Black and Hispanic voter registration and turnout in terms of rates: what share of (Census-estimated) citizen voting-age population registered or voted? But constructing rates based on ACS population estimates means that we only include county-years for which we have group population estimates (omitting some counties with small numbers of people from a given group) and could be introducing errors from the combination of two different datasets. In this section, we rely only on the raw Catalist estimates of registration and vote counts by group. That is, rather than using "share of Black eligible voters who registered" as our outcome measure, we use "count of Black registrants" as the outcome measure. Figure 9 shows time trends of these measures for

[22]The cluster of five points in the middle of the plot, where Catalist estimates are lower than the Leip data, are all from Cook County, Illinois. We are not sure why the datasets diverge, though we suspect it could stem from the aggregation of Chicago with the suburban portions of the county. Knowing that the whole cluster of odd-looking points is in one state is reassuring, since it means that any problems in the analysis caused by those observations can be diagnosed by our state jackknife process in Section B.6.3.

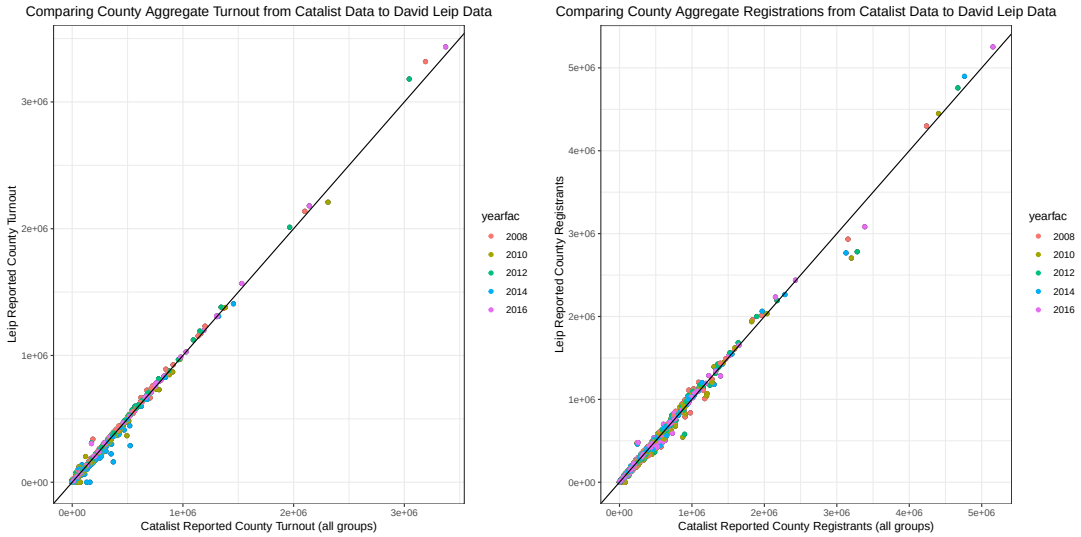


Figure 8: Comparing Catalist county turnout/registration estimates to Leip data

preclearance and non-preclearance counties.

Figure 10 reproduces Figure 3 from the main paper for reference, then presents the analogous estimates from a model using raw Catalist counts as the outcome rather than rates. These estimates are harder to interpret than the ones in the main paper, but they again suggest null or small positive effects on Black and Hispanic turnout and registration in previously-preclearance places after *Shelby*.

B.4 Triple Differences

Most predictions about the *Shelby* decision and turnout were focused on effects among voters from historically-excluded groups, though it is possible that some policy changes and mobilization efforts could also affect white voters. We next compare effects among minority voters to those among white voters in a "triple differences" framework. To do this, we conceptualize our outcome variable as the gap between white and nonwhite registration and turnout rates in each county-year. These triple-differences estimates are shown in Figure 11. If anything, these estimates suggest slightly larger registration and turnout effects among minority voters than white voters, though we note they are noisy and we urge caution in interpreting them.

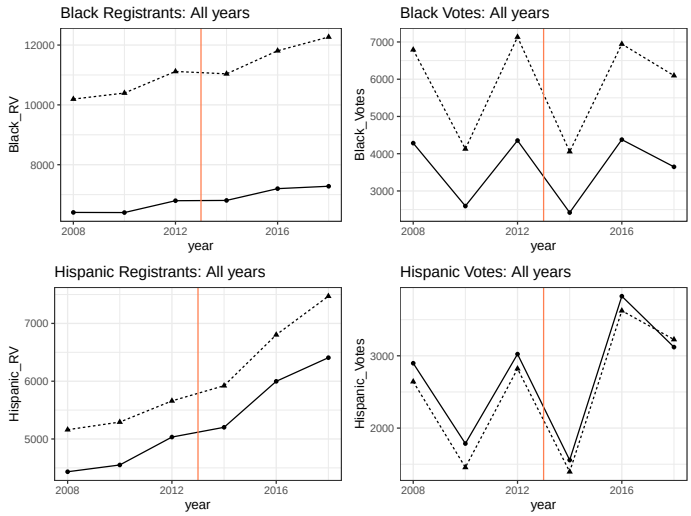


Figure 9: Time trends in Black and Hispanic registration (left two panels) and voter turnout (right two panels). In all panels, the dotted line represents mean values in preclearance or formerly-preclearance counties, while the solid line represents non-covered places.

B.5 Placebo Tests

One might wonder whether the kinds of estimates shown in the main paper could arise by chance, perhaps due to some other background "treatment" or some systematic issue with the data used. To assess this possibility, we run placebo tests where we implement the main analysis using "placebo" treatment years. We set false decision years for the *Shelby* case in 2009 and 2011 (rather than 2013, as in reality) and report the results of our estimation procedure under these assumptions. We rely on these years because they are the only pre-treatment years for which data is available; including post-treatment years would risk incorporating real effects from any real treatment period. Figure 12 presents the resulting estimates: no choice of placebo year produces statistically significant effects on Black or Hispanic turnout/registration rates (the main specification used in the paper), and the estimates vary in direction.

B.6 Robustness to Alternative Specifications

B.6.1 State-Level Analyses

Following Bertrand, Duflo and Mullainathan (2004), we further validate our estimates by aggregating to the state level. Table 6 summarizes our main difference-in-differences specifications at the state level. Here, registration and turnout levels are summed over counties within each state and year and divided by corresponding group CVAP in order to

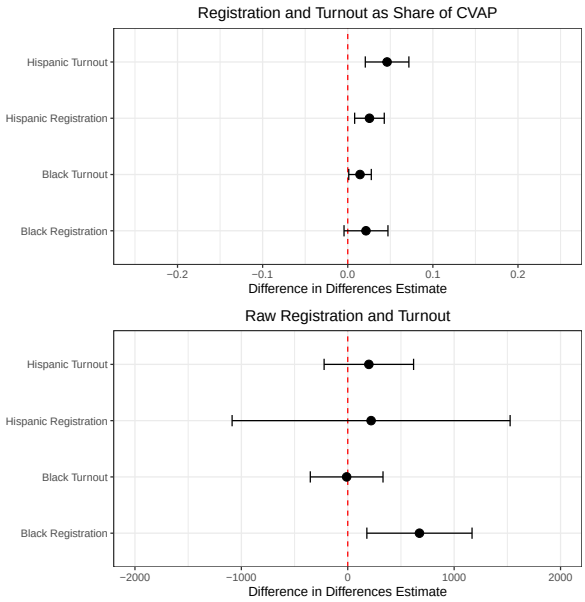


Figure 10: Difference-in-Differences for Black and Hispanic Turnout and Registration

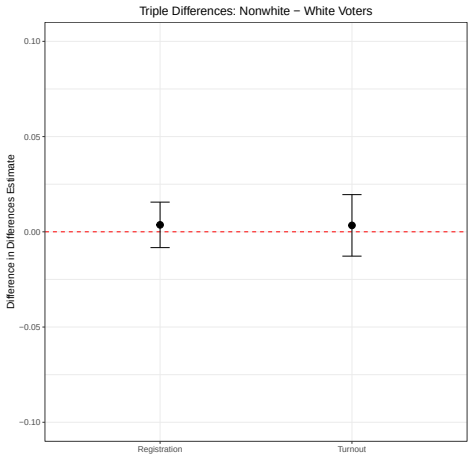


Figure 11: Triple differences

generate registration and turnout rates by state. Following our previous analysis, we weight by group population in order to upweight states with large subgroup populations.States designated as preclearance include those states previously under statewide coverage (see Footnote 14 of the main paper); states that contain several covered jurisdictions, but are

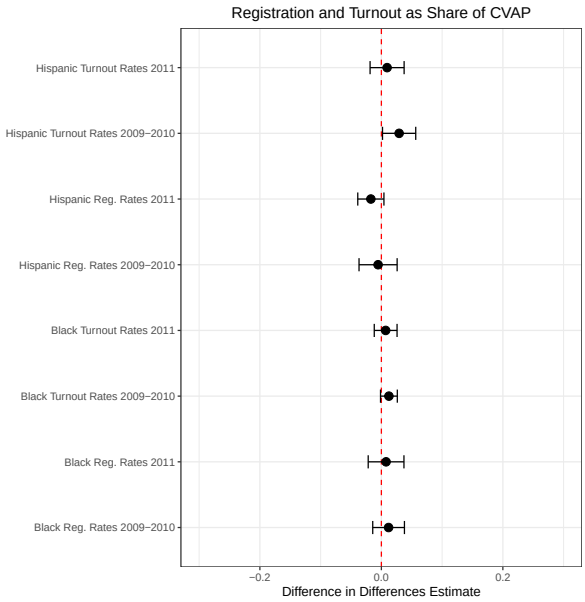


Figure 12: Difference-in-Differences Estimates for Placebo Treatment Years

not covered statewide, are designated as untreated. However, these results are robust to the inclusion of North Carolina as a preclearance state. These estimates are consistent with those we report in the main paper: point estimates across all groups suggest registration and turnout rates *increased* in formerly preclearance areas after the *Shelby* decision. They are not statistically distinguishable from 0 in the case of Black voters, unsurprising given the loss of power from aggregating up from the county to the state level.

Table 6: Difference-in-Differences Results for Registration/Turnout at the State Level

	Dependent variable:			
	Black Reg.	Hispanic Reg.	Black Turnout	Hispanic Turnout
	(1)	(2)	(3)	(4)
Preclearance x Shelby	0.014	0.020***	0.012	0.048***
	(0.010)	(0.007)	(0.008)	(0.008)
Observations	306	306	306	306
R^2	0.775	0.929	0.944	0.944
Adjusted R^2	0.724	0.913	0.931	0.932
Residual Std. Error (df = 249)	29.569	18.909	24.018	21.024

Note: *p<0.1; **p<0.05; ***p<0.01
Results based on aggregate state registration by group, weighted by state-group population in 2008.
See footnote 15 for preclearance criteria at the state level.

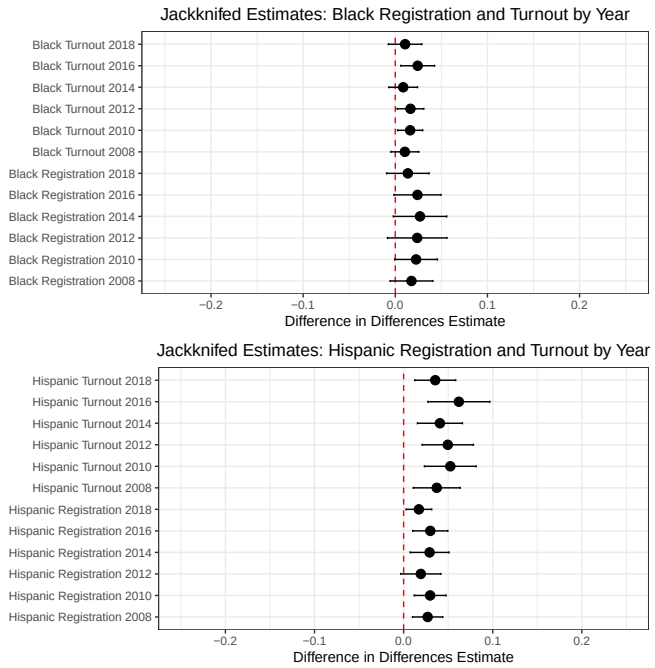


Figure 13: Difference in Differences Estimates for Dropped Years

B.6.2 Different Time Periods

In addition to artificially re-setting treatment to years other than 2013 and finding the anticipated null effects, we run additional checks to ensure robustness over space and time. Figure 13 presents the estimates from an analysis in which we iteratively drop every year in our data, to show that our estimates are not dependent on events or data issues occurring in any one year. Estimates are consistent with the main specification we present in the paper even when excluding any given year.

Further, in Figure 14 we subset the main dataset to run separate analyses focused on midterm and presidential elections. These estimates are also consistent with our main specification, if noisier as a result of using fewer observations. They suggest that positive trends in turnout and registration may be driven by midterm rather than presidential years, as the point estimates for midterm elections are more consistently positive across groups.

B.6.3 Excluding Individual States

Another robustness concern is the possibility that our estimates are driven primarily by outcome changes in a single state, or perhaps by measurement error in one state's data. To investigate this possibility, we iteratively drop states from our analysis in order

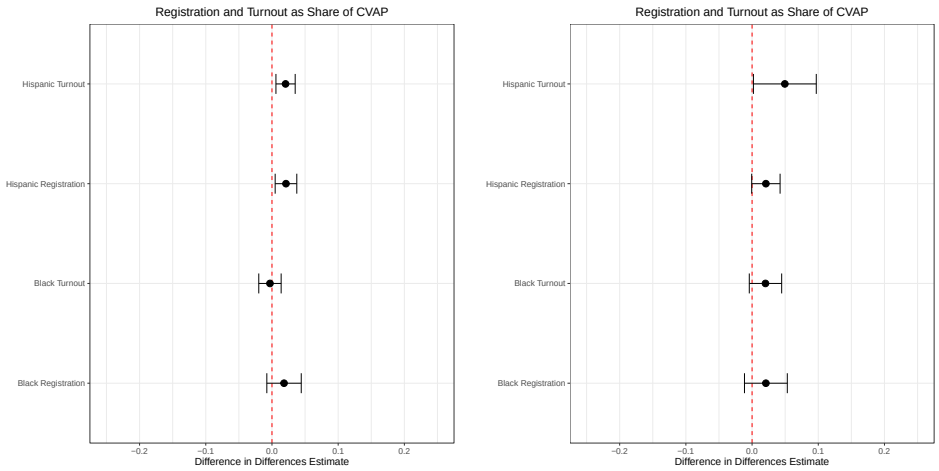


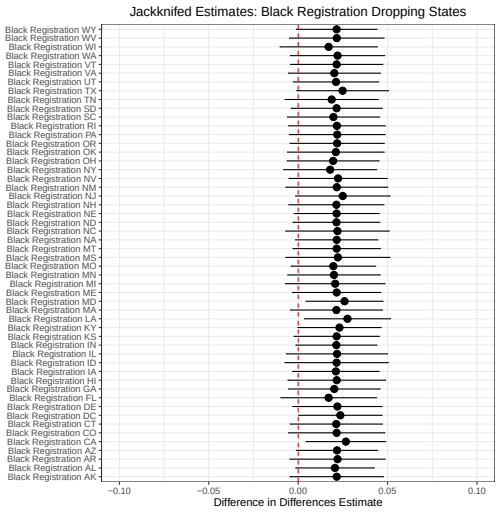
Figure 14: Difference-in-Differences Estimates: Presidential (left) and Midterm (right) Election Years

to examine whether differences in turnout and registration by group remain consistent. Figures 15 and 16 show that the results do not depend exclusively on the presence of specific states. Difference-in-differences estimates for Black and Hispanic voters remain consistently positive even if specific states are excluded. Though there is some variation in effect size and standard errors, particularly when we exclude states with substantial populations of these voters (e.g. California and Texas for Hispanic voters), there is no state which, if dropped, might change our substantive conclusions about the main estimates.

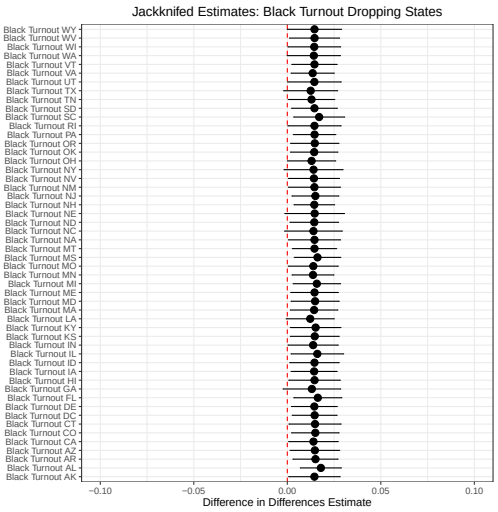
B.6.4 Weighting

As we discuss in Section 3, our main analysis weights counties by the size of the relevant minority group for which we analyze turnout and registration. Here, we verify that the results of our analysis are not strictly an artifact of these population weights. We show this, in part, by using raw registration and turnout totals from Catalist in Figure 9. In addition to this, we show the results of our main analysis of turnout and registration *rates* without weighting in Figure 17.[23] These results are also relatively consistent with our main specification; they show positive trends in minority registration and turnout in pre-clearance counties after *Shelby* relative to non-preclearance counties.

[23]These analyses continue to drop places with extremely small (<100) group population estimates and thus high chances of measurement error; the estimates presented in Figure 9 remove this restriction as well.

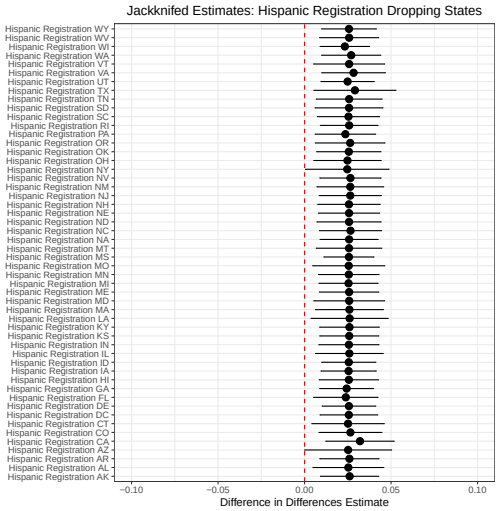


(a) Black Registration

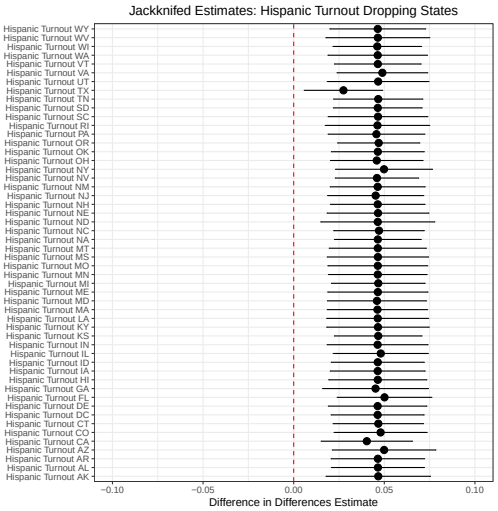


(b) Black Turnout

Figure 15: Difference-in-Differences Estimates for Black Turnout and Registration Excluding Individual States



(a) Hispanic Registration



(b) Hispanic Turnout

Figure 16: Difference-in-Differences Estimates for Hispanic Turnout and Registration Excluding Individual States

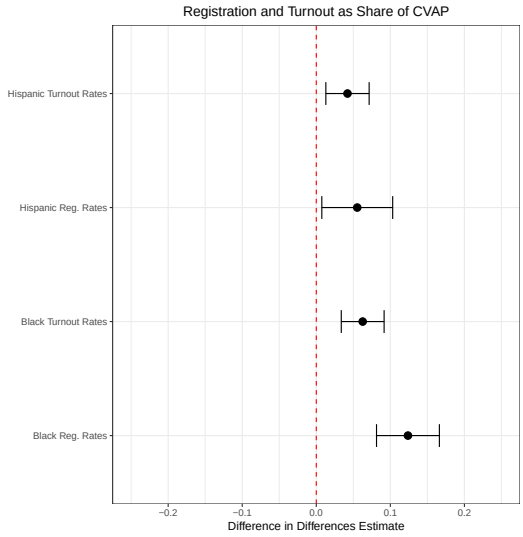


Figure 17: Difference-in-Differences Estimates with Unweighted Data

B.6.5 The South

The VRA's original target jurisdictions for pre-clearance were all states in the Deep South. While the pre-clearance formula expanded over time, the South's large Black citizen population and robust history of minority vote suppression rendered it especially subject to federal scrutiny until *Shelby*. In Figure 18, we examine trends in minority turnout and registration in the South alone. Following our pre-analysis plan, we use two different definitions. One approach narrowly defines "the South" as the 11 original Confederate states: AL, AR, FL, GA, LA, MS, NC, SC, TN, TX, and VA. Arkansas, Florida, North Carolina, and Tennessee were never pre-clearance in their entirety, so counties within these states form a comparison group within the southern region. We also use the U.S. Census Bureau's broader definition of the South, which includes the following states: AL, AR, DE, Washington DC, FL, GA, KY, LA, MD, MS, NC, OK, SC, TN, TX, VA, and WV.

The left panel of Figure 18 relies on the Confederacy definition and the right panel on the Census definition. Overall, it appears that the main paper's results, showing positive trends in minority registration and turnout rates in pre-clearance areas (relative to non-preclearance areas) after *Shelby*, generally hold when limiting the analysis to counties in the South. While these estimates are necessarily noisier because they are comprised of a smaller set of counties, they show rising rates of engagement despite clear evidence of attempts to change voting practices made in southern states. There is one exception, in that the estimated effect on Hispanic registration becomes much noisier and flips sign in the left panel; we attribute this pattern to the limited comparison group (and limited Hispanic

population) in this narrower definition of the South.

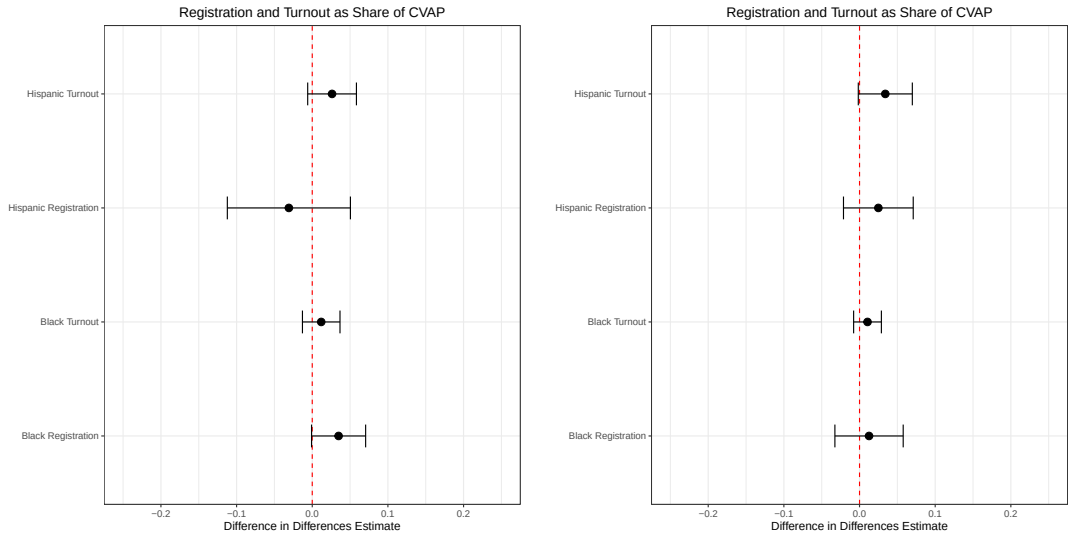


Figure 18: Difference-in-Differences Estimates for Southern Counties, Using Confederacy (Left Panel) or US Census Definition (Right Panel) of Southern States

B.7 Including Covariates

In this section, we include as covariates a set of county-level characteristics drawn from the ACS: total county population, population density, gender ratio (% male), the proportion of the population that is 65 years or older, the proportion of the population that is nonwhite, the proportion of the population that is Hispanic, the proportion who are married, the proportion of adults 25 years or older who have completed high school, the civilian unemployment rate, median household income, and proportion foreign-born. Estimates incorporating these control variables appear in Table 7; they are broadly similar to the main estimates, and consistently show positive trends in turnout for minority voters in pre-clearance (relative to non-preclearance) areas after *Shelby*.

B.8 Dynamic effects

In this section, we apply the difference-in-differences approach introduced by Callaway and Sant'Anna (2020), using their "did" package. Although the design used in this paper does not involve units receiving treatment at staggered times (one of the primary motivations for using this approach), the Callaway and Sant'Anna (2020) approach also allows for disaggregation of effects by time since treatment.

Table 7: Difference-in-Differences Results for Preclearance After Shelby with County-Level Controls

Outcome	Diff-in-Diff	Classical SE	Bootstrapped SE	95 pct. CI	p-value
Black Reg. Rates	0.02	0.00	0.01	(-0.00, 0.04)	0.17
Black Turnout Rates	0.02	0.00	0.01	(0.00, 0.03)	0.03
Hispanic Reg. Rates	0.02	0.00	0.01	(0.00, 0.04)	0.02
Hispanic Turnout Rates	0.04	0.00	0.01	(0.01, 0.06)	0.00

We began by confirming that the overall treatment estimates from this approach (using the "simple" aggregation approach from the did package) essentially reproduce the main estimates presented in the paper. Then, we plot time-period-specific treatment effect estimates for each of the four main outcome measures in Figure 19. Each panel includes two pre-treatment periods (in red) and three post-treatment periods (in blue: period 4 corresponds to 2014, 5 to 2016, and 6 to 2018). The pre-treatment estimates serve as a means of diagnosing problems with parallel trends, and generally are close to zero and non-significant, with the exception of the issue with Hispanic registration data noted in the main paper.[24] In general, the estimates are noisy but suggest positive effects across periods, with the exception of one negative point estimate for Black turnout. Like the estimates in Section B.6.2 above, they suggest the possibility of different effects across midterm and presidential elections.

B.9 PanelMatch

In order to further distance ourselves from the linearity assumptions characteristically imposed by the differences-in-differences estimator, we also apply Kim, Wang and Imai (2018)'s nonparametric PanelMatch approach to estimating Average Treatment Effects on the Treated (ATTs). Note that, given the nature of treatment via the *Shelby* decision, this is very similar to matching along observable county covariates. Figure 20 displays the distribution of treatment status by county (row) and year (column). Matching according to treatment history effectively means finding candidate matches among counties that remain red across all columns indicating treatment years. We look for counties that are similar along the covariates throughout the entire pre-treatment period (setting our lag to 3), and match to calculate treatment effects for the entire post-treatment period (setting leads from 0, which would be 2014, to 2 which would be 2018). We use the CBPS.weight refinement

[24] Because the pre-trends difference on Hispanic registration seems to stem from the construction of registration rates using census CVAP data, the raw registration counts appear to not have this concern (see Figure 9 for a look at trends for the raw counts). Running the same Callaway and Sant'Anna analysis using raw counts of Hispanic registration yields positive (but noisy) treatment effect estimates for all three treatment periods, with smaller and non-significant (less concerning) pre-period placebo estimates.

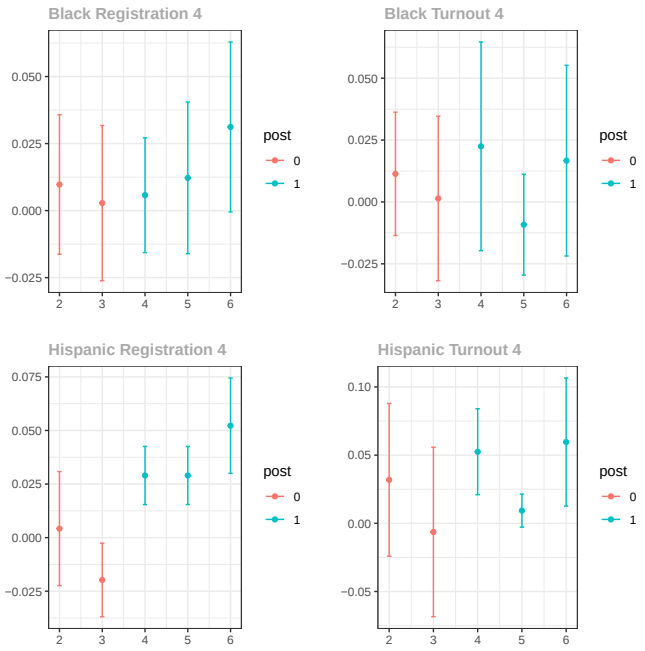


Figure 19: Dynamic estimates using Callaway & Sant'Anna approach

method to refine matched sets. Figure 21 summarizes our ATT estimates for turnout and registration across groups. Results for Black and Hispanic turnout and registration are broadly consistent with our main specification: both sets of estimates show positive effects contemporaneously and in subsequent periods after *Shelby* passes. Removing observations produces wider confidence intervals. Effects on total turnout and registration, as well as turnout and registration for white Americans are substantively and statistically close to zero across time using this approach, with small negative effects immediately following *Shelby* and essentially no effect thereafter.

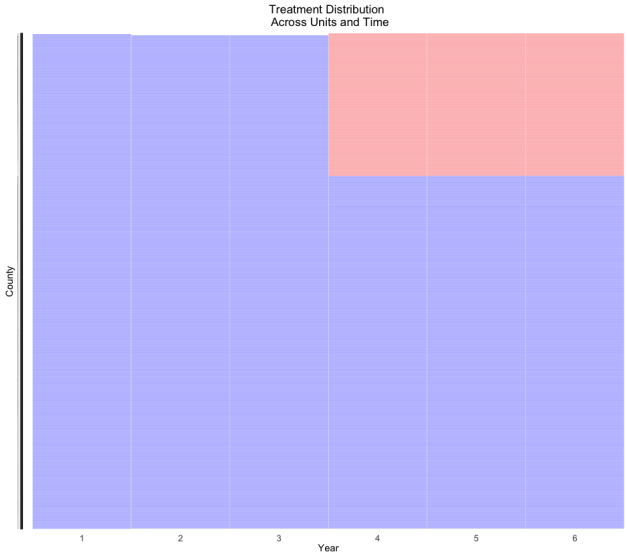


Figure 20: Treated Units over Time

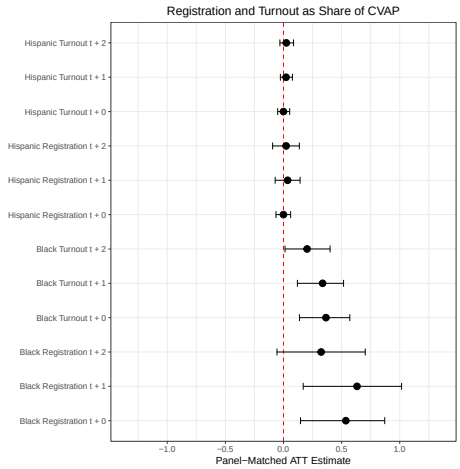


Figure 21: Treated Units over Time

B.10 New Registrations from Catalist (Countermobilization)

In this section, we look for evidence of "countermobilization," using a dataset constructed by Catalist of new voter registrations recorded in each county over each two-year election cycle from 2008 to 2018. For each election year (presidential and midterm), the

dataset uses a snapshot of the voter file taken shortly after the election to tally up the number of new voter registrations added to the voter file in each county over the previous two years since the prior election.[25] For example, a person who moved to Cobb County, Georgia and registered to vote in 2011 would be recorded in the 2012 "new registrations" data for that county, as would a person who had previously lived in the county unregistered but decided to register in summer 2012. These estimates are based not on comparing the total number of registrants in a county at different time points, but on the dates that each individual person's registration appeared on the voter file.

This dataset should allow us to see whether new registrations increased in previously-preclearance counties after the *Shelby* decision. If voting-focused organizations worked to contact and register unregistered people or to help them update outdated registrations to reflect their current addresses, this dataset should capture the results of those efforts. We note that efforts to contact and turn out already-registered people would not be captured by this dataset.

We run a similar difference-in-differences analysis to the one presented in Section 3 above, but the outcome measure is now the share of voting-eligible residents of a county who appear as newly registered in a given year. As above, we block bootstrap standard errors and weight by county population (in this case, total population rather than group-specific estimates, since we do not have new-registrations data by race). Figure 22 displays the difference-in-differences estimate from this approach: the point estimate is positive, consistent with new registrations increasing in previously-covered places after 2013, but is noisily-estimated and cannot be distinguished from zero. It is possible that on-the-ground efforts to help voters register ramped up in counties affected by the *Shelby* decision, but this analysis does not allow us to say with certainty that those efforts occurred or succeeded.

C Other Outcomes: Total Registration, Total Turnout

Our main analysis focuses on registration and turnout among voters from specific groups that have faced disenfranchisement and political exclusion. This section looks at a broader measure: what happened to overall registration and turnout in previously-preclearance places after *Shelby*? We focus on raw counts of registrants and voters from two sources: the Catalist dataset used in the main analysis, and county-level data from Leip's election atlas. Using raw counts of registrants and voters makes these estimates slightly harder to interpret, but it also means we are not relying on any additional datasets (such as Census data) to calculate rates. And looking at overall registration and turnout means that we are no longer relying on Catalist's racial classifications of voters.

Table 8 presents difference-in-differences estimates calculated from the Leip data for 2008-2016 (the years covered by the Leip dataset we have), while Table 9 presents estimates

[25] This time window means that we have election years from 2010 through 2018 in this dataset: the 2010 observation captures new registrations taking place between the 2008 and 2010 elections.

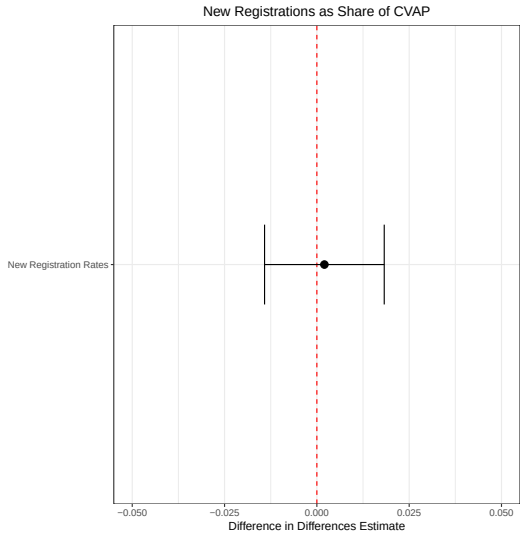


Figure 22: Difference-in-difference estimates in newly-recorded voter registrations

from the Catalist dataset for 2008-2018. The estimates vary in size and precision, with the Leip data covering fewer years and being noisier, but they generally point to increases in overall registration and turnout in previously-covered places after *Shelby*, consistent with our main findings and also with those of Raze (2021).

Table 8: Leip Difference-in-Differences Results for Preclearance After Shelby

Outcome	Diff-in-Diff	Classical SE	Bootstrapped SE	95 pct. CI	p-value
Leip Total Registration	1,093.72	417.78	1,079.25	(-1021.56, 3209.01)	0.31
Leip Total Turnout	1,118.08	1,062.98	835.91	(-520.27, 2756.43)	0.18

Table 9: Difference-in-Differences Results for Preclearance After Shelby

Outcome	Diff-in-Diff	Classical SE	Bootstrapped SE	95 pct. CI	p-value
Total Reg. Counts	2222.33	439.27	1386.55	(-495.25, 4939.92)	0.11
Total Turnout Counts	1674.61	770.98	786.36	(133.38, 3215.85)	0.03

D More Detail on EAVS Analyses

Though the EAVS began in 2004, we use data from 2008 onward due both to low response rates and varying question formats in previous years (Feder and Miller, 2020). We focus on responses from counties, omitting observations provided at the state or township level, to keep our analyses comparable to other work on the EAVS as well as the rest of the paper. We clean the data to account for a variety of different numeric codes that have been used to indicate missing values, and also to remove some implausible values.The EAVS data often includes values of 0 when the information is in fact unknown, and where possible we replace those values with missingness. For example, if all jurisdictions in a state report zero votes in a given year, we assume that those zeroes indicate a data issue rather than true vote counts.

We also adjust the data in several ways based on other work. Following concerns about data quality expressed in the EAVS codebook, we omit data from Iowa in 2018. And we use publicly-available code from the Pew Elections Performance Index (which relies on the EAVS dataset) to clean the code further.[26] In years where specific corrections are available for states with data issues (such as where the EPI team collected updated data directly from election officials and then manually corrected the EAVS dataset), we borrow those corrections from the EPI code. We also follow the EPI code in implementing a number of data quality checks, like making sure that subcategories (such as types of registrations) sum up to total categories (such as overall registration counts).

[26] See https://doi.org/10.7910/DVN/WOV3HY

E More on CCES

E.1 Mobilization Trends

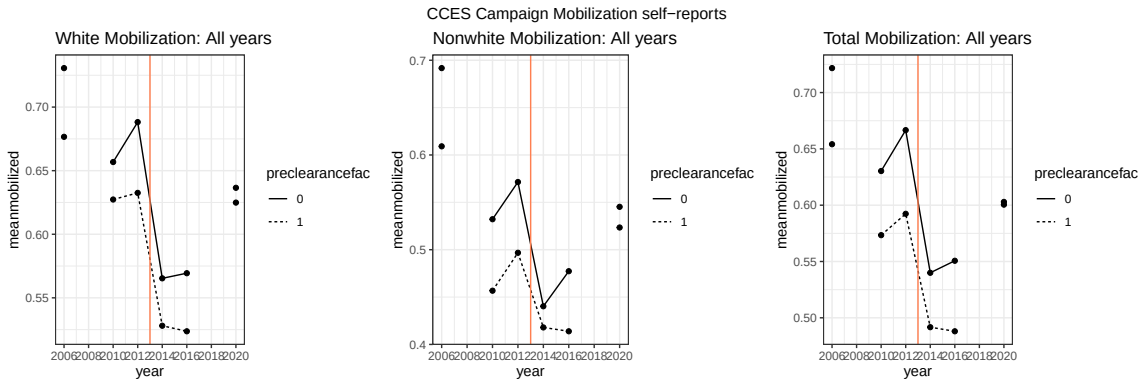


Figure 23: Time trends in self-reported campaign mobilization among CCES respondents, by race. In all panels, the dotted line represents mean values in preclearance or formerly-preclearance counties, while the solid line represents non-covered places.

F Pre-registration

Although this is an observational analysis and not an experiment, we pre-registered our design before purchasing the Catalist data used in this project. Here we included an anonymized copy of that preregistration document, along with notes on how the analyses presented here depart from it.

For the most part, we have adhered closely to the pre-registration, with several exceptions:

- This paper focuses on the main set of outcome measures described in the pre-registration document, those related to minority registration and voting. The pre-registration describes several additional outcomes that we hoped to collect about substantive or descriptive representation of minority voters. Data about the identity of legislators as well as the mapping of districts to counties over time is scarce, and we have been unable to put together these outcomes so they are not included in the paper.

- The pre-registration did not discuss measurement error or whether the main Catalist analyses would be weighted or unweighted. As we discuss in the main paper, we think it makes sense to weight by group population size both because of the question we

are interested in (we care about voters' experiences regardless of where they live, not about counties') and because places with very small minority populations are prone to measurement error. But in this SI (above), we present both unweighted analyses and also estimates based on raw registration and turnout counts, not rates; we believe both these approaches indicate that our decision to weight the main analyses by group size does not drive the conclusions of the paper.

- Similarly, we described a robustness test that would use various ACS population estimate windows (1-year versus 5-year) to make sure that time lag in the population estimates was not driving the observed patterns. We do not present that test here because we think it is clearer and more apt to simply present the raw-counts analyses that fully drop the ACS data rather than using different variations of it: SI section B.3's estimates do a better job than that proposed robustness test of addressing the concern.

Voting and Representation After *Shelby*: Did pre-clearance matter?

[redacted]

January 2019

The passage of the Voting Rights Act in 1965 had an immediate effect on voter registration, turnout, and representation, particularly in southern states that had been systematically disenfranchising African-Americans (Schuit and Rogowski, 2017). Five decades later, the Supreme Court, in the *Shelby County v. Holder* decision, dramatically changed the voting rights landscape by invalidating Section 4 of the VRA. This effectively ended the "pre-clearance" process, under which localities with a history of discrimination were required to get Justice Department approval of changes to their election procedures. The majority decision suggested that, while the VRA had once been useful, such strict monitoring was no longer necessary. Defenders of the VRA and of pre-clearance argued that the law continued to provide important protections to minority voters, and that removing this portion could have catastrophic effects (Wilson, 2015; Herron and Smith, 2016).

Since the 2013 decision, several federal elections have taken place. These do not provide enough data to test long-run theories about shifts in the electorate or slow-moving policy changes, but they do merit a simple examination of whether there have been clear shifts in minority voting registration and turnout, as well as in legislative representation, since the decision.

In this project, we examine data from recent elections, both before and after the Shelby decision, in counties covered and not covered by Section 5 of the VRA. We will focus on the south, as this was the region most noted for large-scale disenfranchisement before the VRA, and most covered jurisdictions are located there. We will run a simple difference-in-differences analysis comparing trends in minority voter turnout and representation before and after the Shelby decision across covered and non-covered places. If we find evidence of immediate shifts in registration, turnout, or representation after 2013, we will dig further into possible mechanisms, such as changes in local electoral practices that could deter minority voters.

If we do not find that any of our outcome variables have been substantially affected by the *Shelby* decision, we will conclude that some of the concerns about immediate effects of the decision have not been borne out. This could be due to concerted effort by advocates to prevent electoral changes through other legal means, or to activists who organized to mobilize minority voters in the wake of the decision, or it could be because our time frame

is too short to see longer-run effects that may materialize later. A null result here will not necessarily mean that pre-clearance was unimportant or that Justice Roberts was right that it was no longer needed, but it will rule out short-run changes in voting and representation. More data will be available when time has passed and more elections have taken place. Given the importance of this question in light of American histories of vote suppression and political exclusion, we nonetheless believe it is worth using the available data to make an early analysis of the effects of the *Shelby* decision.

Data

We will collect data on several outcome measures that capture the main goals of the VRA as we understand them: ensuring the opportunity to register and vote, especially for minority groups that have historically faced discrimination, and improving meaningful representation in government.

1 Registration and Turnout

Our main analysis will focus on voter registration and turnout in the wake of the *Shelby* decision. For this analysis, we will need local (in most cases, county-level) estimates of registration and turnout within racial or ethnic categories. We do not trust survey estimates of turnout by race at this level of aggregation, both because of misreporting and because political survey samples are generally not set up to provide valid population estimates at the level of the county, much less county-level estimates within-race. Instead, we turn to voter file data: we will use actual individual-level records of registration and turnout, combined with imputed race and ethnicity.[1] These county-level estimates will be purchased from Catalist, a firm that collects and cleans state voter files to maintain a national database. We are in the process of negotiating with Catalist to purchase this data for 2008, 2010, 2012, 2014, and 2016; we may also include 2018 data if it becomes available during the time we are working on this project.

Voter files can give us an estimate of the total number of people (by race) that were registered and/or voted in a given election, but they can't give us meaningful estimates of

[1] In states where race is recorded on the voter file, we will have voters' self-reported race. In other places, we rely on Catalist's imputation of race, discussed further in Ansolabehere and Hersh (2012) and Hersh (2015), to construct estimates of the number of people who were registered to vote, and who voted, in several recent elections.

turnout rates: the proportion of *eligible* voters that actually voted.[2] To calculate turnout, we will use county-level estimates of the citizen voting-age population (CVAP) by race from the American Community Survey (drawn from tables B05003A-I) as the denominator. We will use the 5-year ACS population estimates (so for 2012 turnout, we will use the 2007-2012 estimates) because these provide the most complete data for the counties we are interested in.

We have not yet purchased the Catalist data for this analysis; we will do so after filing this pre-analysis plan.

2 Representation

We hope to use two measures of representation, one intended to capture the level of descriptive representation experienced by minority voters, and one intended to capture substantive representation.

We will measure descriptive representation using the proportion of Black or Latino state legislators representing a given county in a given election cycle. To the extent this increases, we will interpret that as more descriptive representation for Black/Latino voters. We will consider a county "represented" by a legislator if that legislator's district includes any part of the county [3]

We will collect data on representatives' race from a number of sources: interest groups such as NALEO often publish lists of elected officials, and we will supplement such available codings with our own codings (based on internet searches) to fill in any gaps.

We will also attempt to collect data on candidate emergence and primary elections, including the presence of minority candidates on primary ballots and their success in primary contests. However, we are uncertain about our ability to collect a complete and accurate dataset with these measures.

Then, we will measure substantive representation using at least one measure of pro-minority voting: House members' voting scores from the Leadership Coalition on Civil Rights. These exist back to 1969, so they provide an over-time measure of pro-civil rights voting. [4] We will use the approach described by Groseclose, Levitt and Snyder (1999) to make the scores comparable across time.

[2] We are less interested in the proportion of registrants that voted, since registration counts can vary for many reasons.

[3] We will also perform a robustness check where rather than looking at representation of "any part" of the county, we will allow for fractional representation (that is, measuring whether a Black legislator's district covers 1/4 of the county, or 1/2, etc.).

[4] ex: http://civilrightsdocs.info/pdf/voting-record/Voting-Record-October2016.pdf

We acknowledge that these scores do not capture our ideal measure (a comparison of minority voters' issue opinions and the votes cast by their representatives), but they do provide an accessible and useful measure of whether representatives appear to be voting in minority constituents' interests. We will continue exploring other avenues for measuring substantive representation at the local level, including survey measures.

We have not yet collected any of the above measures or merged them to county-level data on Section 4 coverage; we will begin this process after filing this pre-analysis plan.

Design

We will set this up as a difference-in-differences analysis, using data from before and after the decision and from places that were and were not affected by the decision (pre-clearance and non-pre-clearance places). Our outcome measures will be:

1. Black voter turnout rates
2. Hispanic voter turnout rates
3. Black voter registration rates
4. Hispanic voter registration rates
5. Proportion of Black officials representing any part of the county in Congress/state legislature (Black officials divided by all officials)
6. Proportion of Hispanic officials representing any part of the county in Congress/state legislature
7. Average LCCR voting scores for Congresspeople representing the county.

Our main analysis will focus on counties within the South, as defined by the Census Bureau. We will present estimates separately for presidential and midterm elections.

The actual specification for the difference-in-differences setup will depend on how plausible we find the parallel trends assumption for the simplest possible specification. Once we receive/collect the county-level outcome data described above, we will examine pre-treatment trends to see whether, for example, Black voter registration in covered and non-covered counties (places that were and were not affected by the Shelby decision) followed similar trends prior to 2013. If they do appear to follow parallel trends (and placebo tests find no "effect" for covered places in periods before the Shelby decision happened), then we will use the

simplest possible difference in differences specification. We will simply predict each outcome measure (such as Black turnout) using a dummy variable for whether the county was covered by preclearance, another dummy for whether the observation was taken after the Shelby decision, and the interaction between the two variables (this is what we are interested in).

However, if we find that pre-treatment trends for covered and non-covered places look quite different, we will instead use a triple-differences approach to try to find a better comparison for the first six outcome measures.[5] In this case, we will use white turnout (or registration/representation) in each county to try to capture time-varying forces that shape local participation and representation. We begin by presenting the example of Black voter turnout. In the triple-differences specification, we will use county-level turnout estimates, with each row of the dataset representing county turnout for a given racial group (black/white) for a given year. We will then predict turnout using, as before, a dummy variable for whether the county was covered by preclearance and a dummy variable for whether the observation is from after 2013. However, we will also include a dummy variable indicating whether the observation is for Black turnout or not, and then will include all two-way interactions between the three dummy variables, as well as the triple interaction (Covered * PostShelby * Black), which should yield the desired estimate of whether Black turnout dropped in affected counties in the wake of the Shelby decision (relative to white turnout in the same places). We will conduct analogous triple-differences analyses for the first six outcome measures laid out above.

We note that the preferred design here depends on pre-treatment trends in our observational data, which we do not yet have. If we can use the simplest diff-in-diff setup and think that the parallel trends assumption is reasonable, we would prefer to do that (it is simpler and should be better-powered). But if we find evidence that the parallel trends assumption is implausible, we will instead favor the triple-differences approach just described. Whichever approach we use, we will include the other's estimates in an appendix or online appendix, with discussion of how we made the decision to privilege one over the other. If we find that neither approach is tenable given the data (if, for example, we find that pre-treatment placebo tests using either approach consistently yield impossible "effects" from the Shelby decision before it even happened), we will conclude that the data and design we have chosen are not well-suited to address this question, and we will give up on this entire research project.

[5] There is no meaningful white analogue to the LCCR scores, so if we don't think the simple approach described above will work, we will drop this outcome measure.

3 Additional Tests

We will run several robustness checks, including:

- Including different states in our analysis: using a measure of "South" based on Confederacy membership, rather than Census designation, and just including all states.
- Using 1-year ACS population estimates to calculate registration and turnout, rather than 5-year estimates. This will necessarily shrink our sample of counties, as many small counties will not have population estimates reported. But the 1-year estimates are more current than the 5-year estimates in our main specification, so this specification should let us make sure any findings aren't being driven by population shifts that throw off our population estimates.
- In some specifications, we will include controls for some potential time-varying confounders that could be driving turnout, such as election competitiveness, though we note that in some circumstances these measures could introduce post-treatment bias.
- On measures where it is possible, we will also try to run a within-North-Carolina design, taking advantage of the fact that a substantial number of NC counties were covered while others were not.

4 Extensions

If we find that minority turnout and registration (and possibly representation) decreased after *Shelby*, we will then try to discover the mechanisms by which this happened. We could examine changes to local election processes after the decision. We would also try to collect data on the racial composition of primary candidate fields in the wake of the case, to get a sense of whether candidate recruitment has changed.

If we find no effect, we will look into possible countervailing forces. For example, some political scientists have speculated that activism and mobilization would keep minority turnout relatively high for the few elections after *Shelby*. We could test this by looking at CCES self-reports of whether people were contacted during the campaign season and asked to vote, especially by non-campaign actors. Similarly, we could look for data on spending by national get-out-the-vote groups, as a measure of whether mobilization efforts increased in an attempt to counterbalance any effects of the *Shelby* decision.

References

Ansolabehere, S. and E. Hersh. 2012. "Validation: What Big Data Reveal About Survey Misreporting and the Real Electorate." *Political Analysis* 20(4):437–459.
URL: *http://pan.oxfordjournals.org/cgi/doi/10.1093/pan/mps023*

Groseclose, Tim, Steven D Levitt and James M Snyder. 1999. "Comparing Interest Group Scores across Time and Chambers : Adjusted ADA Scores for the U.S. Congress." *American Political Science Review* 93(1):33–50.

Herron, Michael C and Daniel A Smith. 2016. "RACE, SHELBY COUNTY, AND THE VOTER INFORMATION VERIFICATION ACT IN NORTH CAROLINA." *Florida State University Law Review* 43:465–506.

Hersh, Eitan D. 2015. *Hacking the electorate: How campaigns perceive voters.* Cambridge University Press.

Schuit, Sophie and Jon C. Rogowski. 2017. "Race, Representation, and the Voting Rights Act." *American Journal of Political Science* 61(3):513–526.

Wilson, McKenzie. 2015. "Piercing the Umbrella: The Dangerous Paradox of Shelby County v. Holder." *Seton Hall Law Journal* 39(xii):181–205.