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

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Abstract

Nearly five decades after the passage of the Voting Rights Act, the Supreme Court’s 2013 decision in *Shelby County v. Holder* dramatically changed the law. The court effectively removed the “preclearance” process that had required places with a history of discrimination to get federal approval before changing their voting procedures. Dissenting justices and voting-rights advocates feared that this decision could lead to massive changes to election administration, and ultimately to lower rates of voter participation in minority communities. In this paper, we evaluate the impact of this decision on election practices and on Black and Hispanic voter registration and turnout. Using administrative data on voters 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. If anything, it appears minority registration and turnout in affected places have increased since 2013. 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.

Preliminary Draft: Findings Subject to Change

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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 the Justice Department’s 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 some 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”\(^1\) and make it harder “to affirmatively protect [minority] communities from the spread of restrictions.”\(^2\) In the immediate aftermath of *Shelby*, states did begin 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 ID 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

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\(^1\)Leigh Chapman, director of the voting rights program at the Leadership Conference on Civil and Human Rights, quoted here

\(^2\)John Yang, the president and executive director of Asian Americans Advancing Justice-AAJC, quoted here
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. 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 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 to public discussion of the Voting Rights Act, and also to political science discussions about the importance of mobilization for voter participation.

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3 As we note later in the paper, previous research on the direct effects of these specific changes on minority voter participation have found limited effects; we discuss that literature and its interpretation in Section 4.
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 several 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. 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 Department of Justice to pre-empt potentially discriminatory changes before they even went into effect.

This process of “pre-clearance” continued to operate robustly up until the court’s 2013 decision in Shelby: covered jurisdictions submitted almost 400,000 proposed changes to

4The use of exclusionary measures such as whites-only primary elections, literacy tests, tests of good character, separate ballot boxes, the conversion of single-member districts to multi-member districts, extending term lengths for offices held by white incumbents, and many others ensured extremely low rates of turnout among Black Americans in the areas singled out by Section 4 (Rosenberg, 1991). Just 6.7% of Mississippi’s Black voting age population was registered to vote in 1965, and no state originally subjected to preclearance under Section 4 saw rates of Black registration above 40% (compared to an average of over 70% for white voters (Grofman, Handley and Niemi, 1992; Cascio and Washington, 2014)
Figure 1: Counties covered by “pre-clearance” under Section 4 of the 1965 Voting Rights Act (highlighted in light blue)
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.

Broadly speaking, 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 preclearance also 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; Naidu, 2012). 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, relatively little 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. We attribute the dearth of research on the impact of *Shelby* to the difficulty of studying it. Relatively few election cycles have elapsed between 2013 and the present, offering researchers limited opportunities to observe both changes to voting procedures and their effects on registration or turnout. And it has been challenging both to define appropriate outcome measures and then to collect them. If election officials in formerly preclearance jurisdictions *did* intend to engage in vote suppression after *Shelby* freed them from federal oversight, what changes might they make? Even with a comprehensive list of these possible changes, obtaining reliable data that catalogued them systematically would be extremely difficult. To deal with these issues, 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. If that was the case, 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 preclear-
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 pre-clearance 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 to warn of the possibility that these jurisdictions might revert to old patterns of vote suppression, “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-possible 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 turnout 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 making sure 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 certain types of election changes occurred more in previously-covered places than other places in the wake of the Court’s decision.

The second way that Shelby could affect minority voter turnout is through counter-mobilization. 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 counter-mobilization efforts by advocacy groups could work even if voters were not aware 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 that highlighted threats to voting rights may have been 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 derrent 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 didn’t 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. 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; Belli et al., 1999) and to yielding very noisy estimates of minority turnout, so we do not use survey data here. Instead, we rely on voter-file data collected from state elections offices, 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. We contracted with 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 registered voters 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

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5 We focus on Black and Hispanic voters as two large and geographically-dispersed groups of voters that have historically faced vote suppression efforts. There are other groups that could potentially 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.

6 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.
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 given that local population could change over the ten-year period spanned by our data, we may also 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.\textsuperscript{7} 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 who are 18 and older living in Autauga 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.\textsuperscript{8}

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 the decision, and ask whether minority voter turnout in these places looked different after the 2013 decision than before it. 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 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

\textsuperscript{7}We rely on the 2009 American Community Survey CVAP estimates to estimate CVAP in 2008, because the five-year estimates we use here only became available in 2009.

\textsuperscript{8}Section B.1 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.
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. Preclearance and non-preclearance counties show very similar turnout trends before 2013. The trends in Hispanic registration look slightly less-well-matched, due to some shifts in counties’ estimated CVAP from the ACS in 2010-2012. We continue with the simplest difference-in-differences specification here, but in Section B.5 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 following 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 \textit{Shelby v. Holder} decision: this indicator 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. Including these two fixed effects implies that \( \beta \) and \( \delta \) can be interpreted as the average changes in registration or turnout associated with being subject to preclearance and being in the period after \textit{Shelby}, respectively, \textit{within} a given county and year. Similarly, \( \tau \), our treatment effect of interest, can be interpreted as the average difference in group turnout or registration between preclearance and non-preclearance counties in the period after \textit{Shelby} relative to the period before.\(^9\) Throughout the paper, we present block-bootstrapped standard errors (Bertrand, Duflo and Mullainathan, 2004).

Our main specification for estimating \( \tau \) uses estimates of Black and Hispanic voter

\(^{9}\)We do not pursue the decomposition approach described in Goodman-Bacon (2018) because we do not have variation in treatment timing: the court acts in 2013, ending preclearance for all our previously-covered counties at once.
registration and turnout rates constructed from Catalist and Census data as described 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.\textsuperscript{10} 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.

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 11 in the SI presents similar estimates in terms of absolute numbers of voters rather than turnout rates, which is slightly harder to interpret but shows 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.

\textsuperscript{10}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 the 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 are yield similar conclusions.
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.5 of the Supporting Information discusses robustness of these patterns to a range of alternate specifications. These include restricting our analysis 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 estimates based on the PanelMatch approach described in Kim, Wang and Imai (2018) [in process]. The SI also presents a triple-differences analysis that compares changes in minority turnout to changes in white turnout to see whether the 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 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 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 ineffective at suppressing voting, or were potentially countered by grassroots mobilization efforts? In this section, we examine some of the details of what happened after Shelby.

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 to their election practices to the federal Department of Justice. With that requirement removed, one possible outcome was that states and municipalities would make dramatic changes to their election laws or practices, including changes 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. The exact units and years covered vary with the data sources.  

These outcome measures are far from a complete picture of potential changes to state

\[\text{footnote}{We also tried using self-reported voter wait times from the Cooperative Congressional Election Study (CCES) to see whether minority voters in previously-covered places faced longer wait times after 2013. However, due to concerns about data quality and parallel trends, we relegate this analysis to the SI.}\]
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 et al., 2015; Barreto et al., 2019; Grimmer and Yoder, 2019; Cantoni and Pons, 2019). 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 seem like 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.\textsuperscript{12} 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” ID 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.\textsuperscript{13}

Figure 4 shows the time trends in voter ID laws in previously covered and non-covered places between 2001 and 2018.\textsuperscript{14} Preclearance states were more likely to have any ID law

\textsuperscript{12}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.

\textsuperscript{13}If a state passed a law in 2011 that took effect in 2013, we only consider that state to have a law in place from 2013 onward. Similarly, if a law was not implemented in a given year due to ongoing litigation, we do not count it as active.

\textsuperscript{14}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 for the purposes of this state-level analysis. 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 not statistically distinguishable from zero.
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 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.

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 Results for Preclearance After Shelby

<table>
<thead>
<tr>
<th>Outcome</th>
<th>Diff-in-Diff</th>
<th>Classical SE</th>
<th>Bootstrapped SE</th>
<th>95 pct. CI</th>
<th>p-value</th>
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<tbody>
<tr>
<td>Any ID Law</td>
<td>-0.04</td>
<td>0.04</td>
<td>0.10</td>
<td>(-0.23, 0.15)</td>
<td>0.70</td>
</tr>
<tr>
<td>Photo ID Law</td>
<td>0.44</td>
<td>0.04</td>
<td>0.15</td>
<td>(0.15, 0.74)</td>
<td>0.003</td>
</tr>
<tr>
<td>Strict ID Law</td>
<td>0.25</td>
<td>0.04</td>
<td>0.12</td>
<td>(0.02, 0.48)</td>
<td>0.04</td>
</tr>
</tbody>
</table>

**EAVS data** Next, we turn to data on local election administration. The Election Administration and Voting Survey is conducted during election years by the US Election
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.

Assistance Commission. Since 2004, the EAC has sent surveys to local 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.\textsuperscript{15}

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 construct-

\textsuperscript{15}We discuss the process of cleaning this data, as well as other questions we examined but did not use due to data-quality or parallel-trends concerns, in Section D of the SI.
ing 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. The EAVS measures are suggestive of some post-Shelby electoral changes, but there is substantial uncertainty around these estimates. We discuss each of the three measures in turn.

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. 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 2013. 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).

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

<table>
<thead>
<tr>
<th>Outcome</th>
<th>Diff-in-Diff</th>
<th>Classical SE</th>
<th>Bootstrapped SE</th>
<th>95 pct. CI</th>
<th>p-value</th>
</tr>
</thead>
<tbody>
<tr>
<td>Registration Purge Rate</td>
<td>0.03</td>
<td>0.002</td>
<td>0.02</td>
<td>(0, 0.06)</td>
<td>0.08</td>
</tr>
<tr>
<td>Provisional Reject Rate</td>
<td>0.0001</td>
<td>0.0001</td>
<td>0.001</td>
<td>(0, 0)</td>
<td>0.89</td>
</tr>
<tr>
<td>Pollworkers per Reg. Voter</td>
<td>0.0001</td>
<td>0.0001</td>
<td>0.0002</td>
<td>(0, 0)</td>
<td>0.78</td>
</tr>
</tbody>
</table>

16 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. We hope that future research will take a closer look at poll closures using administrative data.

17 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.

The second panel of Figure 5 shows a measure of the provisional ballot rejection rate.
over time in affected and unaffected jurisdictions.\textsuperscript{18} Having many provisional ballots cast and ultimately rejected could indicate a number of issues with the voting process: inaccurate registration data, confusing voting instructions that make it hard for people to find their polling place, or poorly-trained pollworkers. Jurisdictions affected by the \textit{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 2013, the trends appear to diverge, with previously-covered places increasing their provisional-ballot rejections more steeply than unaffected places; this pattern is consistent with it becoming harder to vote in these affected places post-\textit{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 again interpret these estimates with caution.

The final panel of Figure 5 shows trends in the number of pollworkers per registered voter in affected and unaffected places. 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 \textit{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 election administration in previously-covered places changed somewhat in the wake of the \textit{Shelby} decision.

\subsection*{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

\textsuperscript{18}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: different states use provisional ballots at different rates for a variety of 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.
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.

We begin by noting that this paper’s main estimates hint at the presence of counter-mobilization, since we see increases in Black and Hispanic voter registration and turnout 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.” Though it is difficult to quantify all of the get-out-the-vote efforts of many disparate organizations, we think it is plausible that they ramped up in the wake of the *Shelby decision*.

We turn to survey data to look for evidence of these get-out-the-vote efforts. The Cooperative Congressional Elections Study (CCES) is run every two years and includes a set of publicly-available “common content” questions. From 2010 through 2016, the common content included a question asking whether respondents 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. The CCES’ mobilization question only asks about campaign contact, not mobilization efforts by other organizations, so we also take a more indirect approach to looking for mobilization. We follow Cantoni and Pons (2019) in constructing a summary index of political activities, including whether a person made a political donation, displayed a political sign, volunteered for a campaign, or attended a

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public meeting during an election cycle.\footnote{We construct this index as \textit{Cantoni and Pons} (2019) describe: we average together the z-scores of the five included items (donated to a campaign, amount donated, displayed sign, volunteered for campaign, attended public meeting) to yield a single index.}

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-\textit{Shelby} trends, since these questions were asked in only a handful of years before the decision.\footnote{Appendix Figures 22 and 23 plot these measures over time.} Still, we present these analyses as a preliminary look at the phenomenon of countermobilization. We approximately follow the specification of \textit{Cantoni and Pons} (2019), though we focus on a \textit{“Shelby v. Holder”} treatment rather than the voter ID laws they considered. We ask whether non-white voters\footnote{Given sample limitations, we group together everyone who reported belonging to a racial or ethnic group besides white.} experience different mobilization trends (relative to white voters) in places that were and were not affected by the \textit{Shelby} decision.

Table 3 presents the results of models focused on the campaign-mobilization question (columns 1-2) and on the political-behavior index (columns 3-4). The coefficient of interest is the interaction between “preclearance” (whether a jurisdiction was covered by preclearance before 2013), “shelby” (whether the observation is from before or after the 2013 \textit{Shelby} decision), and “nonwhite.” In all specifications, this coefficient is positive, suggesting more mobilization of nonwhite voters in previously-covered places after the \textit{Shelby} decision. This pattern is broadly consistent with a story about countermobilization, though we note that the estimates are only sometimes statistically distinguishable from 0.

### 4.3 Discussion of Mechanisms

What should we make of the evidence presented in this section? Section 4.1 established that previously-covered places changed some of their election practices in the wake of \textit{Shelby}, suggesting that their behavior had previously been constrained by preclearance.
Table 3: Self-Reported Mobilization (CCES)

<table>
<thead>
<tr>
<th></th>
<th>Mobilization</th>
<th>Summary Index</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>preclearance</td>
<td>−0.052*</td>
<td>−0.067*</td>
</tr>
<tr>
<td></td>
<td>(0.004)</td>
<td>(0.006)</td>
</tr>
<tr>
<td>shelby</td>
<td>−0.203*</td>
<td>−0.162*</td>
</tr>
<tr>
<td></td>
<td>(0.011)</td>
<td>(0.013)</td>
</tr>
<tr>
<td>nonwhite</td>
<td>0.009</td>
<td>0.112*</td>
</tr>
<tr>
<td></td>
<td>(0.017)</td>
<td>(0.014)</td>
</tr>
<tr>
<td>preclearance:shelby</td>
<td>0.003</td>
<td>−0.003</td>
</tr>
<tr>
<td></td>
<td>(0.010)</td>
<td>(0.017)</td>
</tr>
<tr>
<td>preclearance:nonwhite</td>
<td>−0.048*</td>
<td>−0.119*</td>
</tr>
<tr>
<td></td>
<td>(0.007)</td>
<td>(0.010)</td>
</tr>
<tr>
<td>shelby:nonwhite</td>
<td>−0.026</td>
<td>−0.050*</td>
</tr>
<tr>
<td></td>
<td>(0.021)</td>
<td>(0.021)</td>
</tr>
<tr>
<td>preclearance:shelby:nonwhite</td>
<td>0.018</td>
<td>0.021</td>
</tr>
<tr>
<td></td>
<td>(0.016)</td>
<td>(0.026)</td>
</tr>
<tr>
<td>Constant</td>
<td>0.733*</td>
<td>0.712*</td>
</tr>
<tr>
<td></td>
<td>(0.007)</td>
<td>(0.008)</td>
</tr>
</tbody>
</table>

Race-by-state FE’s   X   X   X   X
Race-by-year FE’s     X   X   X   X
Survey Weights       X   X
Observations          221,926 221,926 272,283 272,283
R^2                   0.063  0.041  0.030  0.018
Adjusted R^2          0.062  0.041  0.029  0.018

Note: *p<0.05
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. And the net effect of Shelby on Black and Hispanic participation appears to be a small increase in 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 (Biggers and Smith, 2018; Biggers, 2019; Valentino and Neuner, 2017). Further, voters may be invited to participate by grassroots organizations seeking to counter vote suppression; a high-profile recent example of this kind of work 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 elections 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 across jurisdictions.

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. But it is harder to systematically measure changes in election practices across many jurisdictions, and harder still to link those changes to reduced voter

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23 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).
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 very 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,\(^{24}\) though 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

\(^{24}\)They report point estimates of up to one percentage point reduction in overall voter turnout, not statistically distinguishable from zero; they report that the analyses can rule out turnout reductions of 2.7 percentage points or more.
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 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 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? 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 substantially 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 how short-term our analysis is. The question of *Shelby*’s effect on voters was so pressing that we thought it important to begin preliminary investigations.

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25. 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.
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. We are only now approaching the first full redistricting cycle since the Shelby decision, and that process will merit additional attention.
References


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.


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.26 There are also several townships in Michigan 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.

26In the case of jurisdictions in Virginia that had “bailed out” of coverage by 2013, we continue to include them as covered here. 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 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 weird patterns or big 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 up to the state level to be comparable. 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 also its provided weights to handle over-time changes in response rates.27

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. Figure 6 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 the 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. In general, 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 presents the same comparison of the Catalist and CPS data, this time focusing on 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%. In Figure 7, 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.

27For a helpful introduction to the cpsvote package, see: https://cran.r-project.org/web/packages/cpsvote/vignettes/voting.html
Comparing Hispanic Turnout from Catalist Data to CPS State–Level Estimates

Figure 6: Comparing Catalist Hispanic turnout estimates to CPS-derived estimates
For the most part, the estimates are 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 recent findings of turnout over-reporting on the CPS, as in Ansolabehere, Fraga and Schaffner (2020)). As in Figure 6, the years are clustered as expected, but we do not observe a pattern of one year’s estimates looking systematically different across the two datasets.

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 would be 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 treated across the datasets. Figure 8 compares our Catalist total-turnout estimates to Leip’s estimates, again with the diagonal line representing equivalence in the two datasets’ estimates. The two datasets appear to have very similar county turnout numbers, and slight differences (points off the line) do not appear systematic across years.

Figure 9 performs the same exercise, this time looking at county registration numbers. Again, the estimates line up quite tidily on the 45-degree line for most county-years. The small cluster of five points in the middle of the plot, where the Catalist estimates are lower than the Leip data, are all estimates from Cook County, Illinois in various years. We are not sure why the datasets diverge for this county, though we wonder whether it might have something to do with 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 which we sequentially drop each state from the dataset and re-run the analysis).
Figure 7: Comparing Catalist Black turnout estimates to CPS-derived estimates
Figure 8: Comparing Catalist county turnout estimates to Leip data
Figure 9: Comparing Catalist county registration estimates to Leip data
B.2 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 10 shows time trends of these measures for preclearance and non-preclearance counties.

Figure 11 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 somewhat 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.
Figure 10: 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 pre-clearance or formerly-preclearance counties, while the solid line represents non-covered places.
Figure 11: Difference-in-Differences for Black and Hispanic Turnout and Registration
B.3 Placebo Tests

One potential threat to inference in the difference-in-differences framework rests in the possibility that something other than the event under study actually drives an observed effect. In our case, that might mean that something close to contemporaneous with the *Shelby* decision, though not necessarily the decision itself, may have both increased the tendency to restrict voting in preclearance jurisdictions and driven minority turnout upward in those areas. Though this is unlikely given that it was precisely the *Shelby* decision that legalized changes to the voting practices and procedures we measure as one of our outcomes, we also test for this possibility by implementing our design using “placebo” treatment years. We test the idea, following Bertrand, Duflo and Mullainathan (2004), that if something other than the *Shelby* decision drives our outcomes then the effects on treatment timing that we observe in our main results are a fluke, and it’s therefore possible to observe our effect (or, indeed, other significant effects) if we assume “treatment” occurs in another year. Figure 12 demonstrates that this is not the case. 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. 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.
Figure 12: Difference-in-Differences Results for Placebo Treatment Years
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 13. If anything, these estimates suggest slightly larger turnout effects among minority voters than white voters, and roughly similar registration effects, though we note they are noisy and we urge caution in interpreting them.

Figure 13: Differences in Turnout and Registration between white and nonwhite voters
B.5 Robustness to Alternative Specifications

B.5.1 State-Level Analyses

Following Bertrand, Duflo and Mullainathan (2004), we further validate our results by aggregating to the state level. Since a large portion of policy that affects voting and election administration is passed and implemented at the state level, we expect the effects of changes to voting laws and procedures to be highly correlated across counties within states. We block-bootstrap our main results in order to account for this and conservatively estimate standard errors, but an even more conservative approach involves aggregating everything to the state-year level. Tables 4 and 5 summarize 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 generate registration and turnout rates by state. Following our previous analysis, we weight by group population in 2008 in order to upweight states with large subgroup populations because these are states with the likely lowest measurement error and states with the largest affected populations. States designated as preclearance include those states previously under statewide coverage (see Footnote 14); states that contain several covered jurisdictions, but are not covered statewide, are designated as untreated. However, these results are robust to the inclusion of North Carolina as a preclearance state. These results are consistent with the main results we report in this paper: point estimates across all groups suggest that registration and turnout rates increased in formerly preclearance areas after the Shelby decision. These results are not statistically distinguishable from 0 in the case of Black voters, but it is important to point out that this results in part from the power sacrificed by aggregating up from the county to the state level.

Table 4: Difference-in-Differences Results for Registration at the State Level

<table>
<thead>
<tr>
<th></th>
<th>Dependent variable:</th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Black</td>
<td>Hispanic</td>
<td>White</td>
<td>Total</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Preclearance x Shelby</td>
<td>0.012</td>
<td>0.019***</td>
<td>0.019***</td>
<td>0.020**</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.010)</td>
<td>(0.007)</td>
<td>(0.006)</td>
<td>(0.008)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>300</td>
<td>300</td>
<td>300</td>
<td>300</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>R²</td>
<td>0.776</td>
<td>0.929</td>
<td>0.858</td>
<td>0.865</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Adjusted R²</td>
<td>0.726</td>
<td>0.913</td>
<td>0.826</td>
<td>0.835</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Residual Std. Err (df = 244)</td>
<td>26.418</td>
<td>16.391</td>
<td>16.569</td>
<td>56.179</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

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 14 for preclearance criteria at the state level.

B.5.2 County Data Availability over Time

In addition to artificially re-setting treatment to years other than 2013 and demonstrating, via null effects, that events occurring around the treatment period (but not the
Table 5: Difference-in-Differences Results for Turnout at the State Level

<table>
<thead>
<tr>
<th>Dependent variable:</th>
<th>Black (1)</th>
<th>Hispanic (2)</th>
<th>White (3)</th>
<th>Total (4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Preclearance x Shelby</td>
<td>0.011</td>
<td>0.048***</td>
<td>0.012*</td>
<td>0.014*</td>
</tr>
<tr>
<td></td>
<td>(0.008)</td>
<td>(0.008)</td>
<td>(0.007)</td>
<td>(0.008)</td>
</tr>
<tr>
<td>Observations</td>
<td>300</td>
<td>300</td>
<td>300</td>
<td>300</td>
</tr>
<tr>
<td>$R^2$</td>
<td>0.950</td>
<td>0.945</td>
<td>0.949</td>
<td>0.950</td>
</tr>
<tr>
<td>Adjusted $R^2$</td>
<td>0.939</td>
<td>0.933</td>
<td>0.937</td>
<td>0.939</td>
</tr>
<tr>
<td>Residual Std. Error (df = 244)</td>
<td>21.264</td>
<td>18.832</td>
<td>17.724</td>
<td>53.836</td>
</tr>
</tbody>
</table>

Note: Results based on aggregate state registration by group, weighted by state-group population in 2008. See footnote 14 for preclearance criteria at the state level.

Treatment itself did not produce the turnout and registration rates we observed for minority voters, we did additional robustness checks to ensure robustness over counties and time. First, as Table 7 shows, we observe ACS data for significantly fewer counties in 2008 than we do in subsequent years due to the structure of the American Community Survey. One way to ensure our results hold is to remove 2008 from the data and show that our results are generally similar. We do this in Figure 14. In fact, Figure 14 represents our results for an analysis in which we iteratively drop every year in our data, one at a time, to show that our results are not specifically dependent on events occurring in any one year. Figure 14 shows that our results are consistent with the main specification we present in this paper even if we exclude 2008 and any other given year. Standard errors in this and subsequent analyses are similarly block-bootstrapped to account for the clustering of treatment and election policymaking authority at the state level.

Finally, another concern about time rests with pooling results from presidential and midterm elections. Our main specification contains results from both election types, and while the year fixed effect prevents us from strictly pooling election results across those years, it’s worth confirming that our results obtain even if we focus on subsamples of strictly presidential or strictly midterm election years. We do this in Figures 15 and 16. These results are similarly consistent with our main specification, if somewhat noisier as a result of using significantly smaller numbers of observations. These begin to 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.
Figure 14: Difference in Differences Results for Dropped Years
Figure 15: Difference-in-Differences Results: Presidential Election Years
Figure 16: Difference-in-Differences Results: Midterm Election Years
B.5.3 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. We believe this is justified for two reasons. First, larger counties (in terms of a relevant population) are less likely to have significant measurement error in the construction of voters / CVAP ratios (since, in small counties, being off by a count of 5 registered voters may mean a 50% or higher error rate in a population of 10-15 people total). Second, we want to emphasize activity in counties that house the largest numbers of minority residents since we believe these areas may be the targets of vote suppression efforts. Still, it is important to 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 10. In addition to this, we show the results of our main analysis of turnout and registration rates without weighting in Figure 17. 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.

![Figure 17: Difference-in-Differences Results with Unweighted Data](image)

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B.5.4 The South

The VRA’s original target jurisdictions for pre-clearance were all states in the Deep South. While the definition of pre-clearance 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. Per (Cascio and Washington, 2014) and other recent research in this area, we define “the South” as the 11 original Confederate States: Alabama, Arkansas, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, Tennessee, Texas, and Virginia. Arkansas, Florida, North Carolina, and Tennessee were never pre-clearance in their entirety, so counties within these states form a viable comparison group within the southern region. Figure 18 shows that our results, showcasing positive trends in minority registration and turnout rates in pre-clearance areas (relative to non-preclearance areas) after Shelby, hold 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.

Figure 18: Difference-in-Differences Results for Southern Counties
B.5.5 Excluding Individual States

Another concern about the robustness of the results may rest in the possibility that our results are driven primarily by changes to registration, turnout, or population in a single state. The implication of such a finding would be that, while one state (or a small collection of pre-clearance and non-pre-clearance states) in particular experienced the positive turnout and/or registration effects we report in the main results as the result of idiosyncratic changes in election administration or mobilization, these effects might not be generalizable to the full set of counties in pre-clearance and non-preclearance states. To investigate this possibility, we iteratively drop single states from our analysis in order to examine whether differences in turnout and registration by group remain consistent. Figures 19 and 20 show that our results do not depend exclusively on the presence of specific states. Difference-in-differences results for Black and Hispanic voters remain consistently positive even if specific states are included. 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 results.

Figure 19: Difference-in-Differences Results for Black Turnout and Registration Excluding Individual States

(a) Black Registration

(b) Black Turnout
Figure 20: Difference-in-Differences Results for Hispanic Turnout and Registration Excluding Individual States
B.6 Covariate Balance and Nonparametric Approaches to Differences-in-Differences

B.6.1 Including Covariates

The data used in this study encapsulates a wide array of counties with substantial variation in the populations of minority voters and other important characteristics. For instance, in our data, the proportion of residents in a county who are nonwhite ranges from 0 to over 96%. While our approach to identification relies on the assumption that these observable and latent characteristics remain constant within counties, which obviates the possibility of confounding from county-level characteristics, it’s useful (and potentially more precise) to examine treatment effects within the set of pre-clearance and non-preclearance counties that are most alike along a set of relevant county-level characteristics (like minority population). We do this in several ways. First, we achieve this by explicitly controlling for a series of county-level characteristics that may affect both registration and turnout and approaches to election law and administration. We use data from the ACS to incorporate information on 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. Results incorporating these control variables appear in Table 6. These results consistently show positive trends in turnout for minority voters in pre-clearance (relative to non-preclearance) areas after Shelby.

Table 6: Difference-in-Differences Results for Preclearance After Shelby with County-Level Controls

<table>
<thead>
<tr>
<th>Outcome</th>
<th>Diff-in-Diff</th>
<th>Classical SE</th>
<th>Bootstrapped SE</th>
<th>95 pct. CI</th>
<th>p-value</th>
</tr>
</thead>
<tbody>
<tr>
<td>Black Reg. Rates</td>
<td>0.02</td>
<td>0.00</td>
<td>0.01</td>
<td>(-0.00, 0.04)</td>
<td>0.16</td>
</tr>
<tr>
<td>Black Turnout Rates</td>
<td>0.02</td>
<td>0.00</td>
<td>0.01</td>
<td>(0.00, 0.03)</td>
<td>0.03</td>
</tr>
<tr>
<td>Hispanic Reg. Rates</td>
<td>0.02</td>
<td>0.00</td>
<td>0.01</td>
<td>(0.00, 0.03)</td>
<td>0.01</td>
</tr>
<tr>
<td>Hispanic Turnout Rates</td>
<td>0.04</td>
<td>0.00</td>
<td>0.01</td>
<td>(0.01, 0.06)</td>
<td>0.00</td>
</tr>
<tr>
<td>White Reg. Rates</td>
<td>0.02</td>
<td>0.00</td>
<td>0.01</td>
<td>(0.00, 0.03)</td>
<td>0.01</td>
</tr>
<tr>
<td>White Turnout Rates</td>
<td>0.01</td>
<td>0.00</td>
<td>0.01</td>
<td>(-0.00, 0.03)</td>
<td>0.09</td>
</tr>
<tr>
<td>Total Reg. Rates</td>
<td>0.02</td>
<td>0.00</td>
<td>0.01</td>
<td>(0.00, 0.03)</td>
<td>0.01</td>
</tr>
<tr>
<td>Total Turnout Rates</td>
<td>0.01</td>
<td>0.00</td>
<td>0.01</td>
<td>(-0.00, 0.03)</td>
<td>0.12</td>
</tr>
</tbody>
</table>

B.6.2 PanelMatch

[Under construction]
B.7 Other Catalist Figures

This section provides more descriptive information about the Catalist dataset used for this project. Figure 21 shows time trends in overall registration and voting for preclearance/non-preclearance places, as well as for white residents’ registration and voting; this plot is comparable to Figure 10 but for different subgroups of the population. Table 7 provides counts of county observations for specific years and outcomes.

Figure 21: Time trends in overall and white voter registration and turnout. In all panels, the dotted line represents mean values in preclearance or formerly-preclearance counties, while the solid line represents non-covered places.
Table 7: Number of County Observations for Each Year and Outcome

<table>
<thead>
<tr>
<th>Year</th>
<th>bregrate</th>
<th>bregcount</th>
<th>hregrate</th>
<th>hregcount</th>
<th>wregrate</th>
<th>wregcount</th>
<th>hvoterate</th>
<th>hvotecount</th>
<th>wvoterate</th>
<th>wvotecount</th>
</tr>
</thead>
<tbody>
<tr>
<td>2008</td>
<td>1,291</td>
<td>3,141</td>
<td>1,382</td>
<td>3,141</td>
<td>1,396</td>
<td>3,141</td>
<td>1,291</td>
<td>3,141</td>
<td>1,382</td>
<td>3,141</td>
</tr>
<tr>
<td>2010</td>
<td>2,166</td>
<td>3,130</td>
<td>2,289</td>
<td>3,130</td>
<td>2,166</td>
<td>3,130</td>
<td>2,289</td>
<td>3,130</td>
<td>2,166</td>
<td>3,130</td>
</tr>
<tr>
<td>2012</td>
<td>2,205</td>
<td>3,130</td>
<td>2,391</td>
<td>3,130</td>
<td>2,205</td>
<td>3,130</td>
<td>2,391</td>
<td>3,130</td>
<td>2,205</td>
<td>3,130</td>
</tr>
<tr>
<td>2014</td>
<td>2,246</td>
<td>3,142</td>
<td>2,476</td>
<td>3,142</td>
<td>2,246</td>
<td>3,142</td>
<td>2,476</td>
<td>3,142</td>
<td>2,246</td>
<td>3,142</td>
</tr>
<tr>
<td>2016</td>
<td>2,289</td>
<td>3,142</td>
<td>2,532</td>
<td>3,142</td>
<td>2,289</td>
<td>3,142</td>
<td>2,532</td>
<td>3,142</td>
<td>2,289</td>
<td>3,142</td>
</tr>
<tr>
<td>2018</td>
<td>2,340</td>
<td>3,142</td>
<td>2,574</td>
<td>3,142</td>
<td>2,340</td>
<td>3,142</td>
<td>2,574</td>
<td>3,142</td>
<td>2,340</td>
<td>3,142</td>
</tr>
</tbody>
</table>

Registration and turnout rates (regrate/voterate) rely on Census ACS estimates, while counts (regcount/votecount) rely solely on Catalist data. Registration and turnout rates in the analysis are restricted to counties with at least 100 members of a given group; raw registration and turnout counts reflect the total numbers of counties for which data is available.
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. In this section, we look 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 different sources: the Catalist dataset we use in our main analysis, and county-level data from David 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 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 consistent with the findings of Raze (2019).

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

<table>
<thead>
<tr>
<th>Outcome</th>
<th>Diff-in-Diff</th>
<th>Classical SE</th>
<th>Bootstrapped SE</th>
<th>95 pct. CI</th>
<th>p-value</th>
</tr>
</thead>
<tbody>
<tr>
<td>Leip Total Registration</td>
<td>1,101.01</td>
<td>417.63</td>
<td>1,077.83</td>
<td>(-1011.5, 3213.51)</td>
<td>0.31</td>
</tr>
<tr>
<td>Leip Total Turnout</td>
<td>1,132.53</td>
<td>1,062.61</td>
<td>839.98</td>
<td>(-513.8, 2778.86)</td>
<td>0.18</td>
</tr>
</tbody>
</table>

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

<table>
<thead>
<tr>
<th>Outcome</th>
<th>Diff-in-Diff</th>
<th>Classical SE</th>
<th>Bootstrapped SE</th>
<th>95 pct. CI</th>
<th>p-value</th>
</tr>
</thead>
<tbody>
<tr>
<td>Total Reg. Counts</td>
<td>2209.18</td>
<td>439.12</td>
<td>1419.80</td>
<td>(-573.57, 4991.93)</td>
<td>0.12</td>
</tr>
<tr>
<td>Total Turnout Counts</td>
<td>1702.97</td>
<td>770.72</td>
<td>800.11</td>
<td>(134.79, 3271.16)</td>
<td>0.03</td>
</tr>
</tbody>
</table>
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. 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).

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28See https://doi.org/10.7910/DVN/WDV3HY
E More on CCES

E.1 Mobilization Trends

Figure 22: 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.
Figure 23: Time trends in a summary index of self-reported political participation among CCES survey respondents. In all panels, the dotted line represents mean values in preclearance or formerly-preclearance counties, while the solid line represents non-covered places.
E.2 Wait Times

As a final look at election administration as it is experienced by voters, we ask how long voters waited to cast their ballots. Election day line lengths do not directly reflect one single choice by election officials, but voter wait times can be shaped by officials’ decisions about how and where to deploy election resources, as well as how complicated the voting process is (Spencer and Markovits, 2010).

We rely on a question from the Cooperative Congressional Election Study that asks voters about their wait time, offering several time ranges to choose from and a free-text option for voters to enter wait times over an hour. We follow Pettigrew (2017) in recoding survey responses into minutes,\(^29\) and use these estimates to construct state-level estimates of voter wait times overall and by race.

Figure 24 plots trends in voter wait times for places that were and were not affected by the Shelby decision, with different panels showing patterns overall and by racial group. There are no observations for 2010 because the CCES did not ask about wait times in that year.

We note that voter wait times in affected and unaffected places look broadly similar since the Shelby decision, particularly for Black and Hispanic voters. The largest divergence between affected and unaffected places occurred in 2008, well before the 2013 decision: states covered by pre-clearance showed steep increases in wait times across all groups relative to the shallower increases seen in non-covered states. Depending on how we interpret these large increases in 2008, Figure 24 might make us wonder whether the parallel trends assumption is reasonable for this design, but there is nothing in the trends that suggests that the Shelby decision led to higher wait times for minority voters in previously-affected places. Table 6 presents difference-in-differences estimates consistent with that conclusion of no effect. The difference-in-differences point estimates are all negative, consistent with voter wait times having slightly decreased in previously-covered places post-2013, but none are statistically significant. Although states and counties changed some of their election practices post-Shelby, on average we don’t see evidence that these changes translated into higher wait times for voters. Still, given concerns about parallel trends, we urge caution in interpreting these analyses.

\(^{29}\)Specifically, we recode responses to the middle of the time range chosen, so a voter who selected the “ten to thirty minutes” response would be coded as having waited 20 minutes.
Figure 24: Patterns in voter wait times (in minutes) as reported in the CCES. In all panels, the dotted line represents mean values in preclearance or formerly-preclearance states, while the solid line represents non-covered places.

Table 10: Difference-in-Differences Results for Preclearance After Shelby

<table>
<thead>
<tr>
<th>Outcome</th>
<th>Diff-in-Diff</th>
<th>Classical SE</th>
<th>Bootstrapped SE</th>
<th>95 pct. CI</th>
<th>p-value</th>
</tr>
</thead>
<tbody>
<tr>
<td>Wait Times (All)</td>
<td>-2.32</td>
<td>1.35</td>
<td>1.99</td>
<td>(-6.22, 1.58)</td>
<td>0.24</td>
</tr>
<tr>
<td>Wait Times (Black)</td>
<td>-2.05</td>
<td>2.56</td>
<td>2.80</td>
<td>(-7.53, 3.44)</td>
<td>0.46</td>
</tr>
<tr>
<td>Wait Times (Hispanic)</td>
<td>-5.59</td>
<td>2.87</td>
<td>2.89</td>
<td>(-11.26, 0.08)</td>
<td>0.05</td>
</tr>
<tr>
<td>Wait Times (White)</td>
<td>-2.04</td>
<td>1.25</td>
<td>1.75</td>
<td>(-5.47, 1.4)</td>
<td>0.25</td>
</tr>
</tbody>
</table>