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

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Abstract

The Supreme Court's 2013 decision in *Shelby County v. Holder* dramatically changed the Voting Rights Act, ending the "preclearance" process that had required federal approval before places with a history of discrimination changed their voting procedures. Dissenting justices and voting-rights advocates feared the decision could allow changes to election administration that would suppress minority voter participation. This paper evaluates the decision's impact on election practices and on voting. *Shelby* yielded decisive changes in some practices that had been constrained by preclearance (voter identification laws), though evidence on potential indirect changes to election administration is mixed. These bounded changes to election practices do not appear to have translated into a degradation of minority voter participation or power over the period studied. Using administrative data and a difference-in-differences design comparing places affected and unaffected by the court's decision, we find minimal changes in Black-white voting gaps in the post-*Shelby* period; further analyses indicate that voter participation was generally stable or potentially increasing in previously-covered places.

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In 2013, the Supreme Court's decision in the *Shelby v. Holder* case reshaped the historic Voting Rights Act ("VRA"; "the Act"). The court's decision invalidated Section 4 of the VRA, effectively ending the "preclearance" process under which state and local governments with a history of discrimination were required to seek federal approval before making changes to their election procedures. This decision meant that the federal government would no longer strike potentially discriminatory changes to voting practices before they were implemented.

The VRA had been passed to combat widespread and persistent voter exclusion on the basis of race, and many advocates feared that removing preclearance would return the U.S. to the pre-VRA era. Some warned that the change would "open the floodgates to voter suppression" and make it harder "to affirmatively protect [minority] communities from the spread of restrictions."[1]

Concerned observers pointed out that the end of preclearance could herald a new wave of restrictions on minority voters in two ways. The first was direct: potentially discriminatory laws that had either been suspended in the federal review process or previously rejected by the federal government could be enacted and enforced. Indeed, several states enacted restrictive voter identification laws within a year of *Shelby*.

In addition to the direct legal implications of losing preclearance, voting rights advocates feared a loss of deterrence. Knowing that they were no longer being monitored might embolden jurisdictions to pass restrictions on minority registration and voting that they would not even have submitted for review before *Shelby*. North Carolina's H.B. 589 may have been an example and a warning. The bill imposed a sweeping set of restrictions on voters including: a photo ID requirement; restrictions on when voters could cast provisional ballots; the elimination of same-day registration, as well as elimination of pre-registration for young voters, straight-ticket voting and extended early voting hours. While the U.S. Court of Appeals for the Fourth Circuit Court struck H.B. 589 in 2016, noting that it targeted Black voters with "almost surgical precision" (NAACP v. McCrory, 831 F.3d 204 (4th Cir. 2016)), the law's broad reach signaled that state and local governments may be willing to push the legal frontier of restrictions forward. While North Carolina's law was stricken *after Shelby* under still-in-force Section 2 of the VRA, plaintiffs only succeeded in their challenge to the law after a three-year legal battle - a potentially unrealistic prospect for other groups of voters targeted by discriminatory laws or practices that were more geographically concentrated, less publicly salient, or both.

In this paper, we evaluate the effects of the *Shelby* decision along multiple dimensions. We start by investigating whether or not jurisdictions newly free from federal supervision system-

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

atically changed their election practices, beginning with one high-profile form of legal change that received a great deal of public attention (voter identification or "ID" laws), before turning to county-level decisionmaking about election administration. We find the clearest evidence for changes in the content of voter ID laws, consistent with those laws having been actively constrained by preclearance. Evidence of lower-level election-administration changes is more mixed, though we note the limitations of available measures of local election administration.

We then turn to the crucial question of whether the removal of preclearance translated into lower participation, and less political power, for minority voters in previously-covered places. We use voter file and census data in a county-level difference-in-differences approach to compare participation patterns in previously-covered places to the rest of the country in the years before and after the 2013 decision. We find that the *Shelby* decision did not significantly worsen registration or turnout gaps between Black and white voters or Hispanic and white voters over the period studied. For white, Black, and Hispanic registrants, we show that this is a story of registrations across all groups generally rising from 2014-2018 before falling in 2020 - in covered and non-covered places alike. Turnout, too, trends upwards for all three groups of voters individually in both covered and non-covered counties. Building on previous work that points out how election changes can affect turnout both directly and indirectly (Hopkins et al., 2017; Burden et al., 2014; Rosenstone and Hansen, 1993), we examine the role countermobilization in response to the *Shelby* decision may have played in what are ultimately positive trends in Black and Hispanic turnout after 2013. We find suggestive but limited evidence that minority voters in formerly covered areas were more likely to have been contacted by a party or organization, but the substantively small magnitudes in these effects lead us to interpret resilience in minority registration as a function of relatively limited state and local policy activity rather than extensive countermobilization in the face of highly effective suppressive efforts.

We further find that the decision has not, thus far, translated into consequences for descriptive representation; formerly covered areas are not significantly less likely to elect Black or Democratic members of Congress in the post-*Shelby* period. We do not conclude from these patterns that *Shelby* has had no impact on elections; indeed, states have taken advantage of the removal of preclearance to implement legal changes that impose additional burdens on voters even if they do not translate into substantial changes in relative or overall turnout (White, Nathan and Faller, 2015; Barreto, Nuño and Sanchez, 2009; Grimmer and Yoder, 2019; Cantoni and Pons, 2019; Zhang, 2022). And it remains possible that over time, the forces that led to the adoption of previously-barred laws such as strict voter identification statutes could eventually lead to the development of more-effective measures targeting minority voter participation. However, it appears that election changes in the immediate aftermath of the court's *Shelby* decision have not effectively suppressed minority voting or political power.

We expand on existing research in several ways. First, we rely on administrative data rather than survey-based self-reports of participation, which can produce biased estimates of participation. Second, we rely on a difference-in-differences strategy rather than directly examining changes in covered areas before and after *Shelby*. This allows us to rule out changes in minority registration and turnout that occured as a result of national trends or policy changes unrelated to *Shelby*. We also trace group-specific data on registration and turnout by year and county to observe how voters behaved in real-time rather than trying to estimate the way *Shelby* affected total registration and turnout in places with historically large minority populations without observing how members of precisely those populations behaved.

1 *Shelby v. Holder* and The End of Preclearance

The Voting Rights Act is a touchstone of American democracy. Enacted in 1965 to eliminate literacy tests - the last sweeping barriers to Black political participation in the South - the VRA had two central features. The first of these, enumerated in Section 2 of the Act, directly prohibits voting practices or procedures that discriminate on the basis of race, color, or membership in a language minority group. The second was a unique oversight regime that granted the federal government powers to review and preemptively challenge voting laws in the places with the worst histories of racial discrimination. This authority, also called "preclearance," rested on two planks. First, Congress identified places with the worst histories of racial discrimination according to a coverage formula spelled out in Section 4(b) of the VRA. Originally, jurisdictions identified through this coverage formula were places that had previously employed "tests or devices" such as literacy tests or tests of good moral character as prerequisites for registering to vote and saw less than 50% of their voting-age populations registered to vote as of November 1, 1964.[2] Second, Congress effectively placed jurisdictions identified under its coverage formula into a form of federal receivership under Section 5 of the VRA (Pildes, 2006). These jurisdictions, along with all political subdivisions within them, would have to submit any and all proposed changes to their election and voting rules to the Department of Justice or the United States District Court for the

[2]Election and voting rules in Alabama, Georgia, Louisiana, Mississippi, South Carolina, Virginia and 39 counties in North Carolina became subject to federal oversight in 1965. In 1970 and again in 1975, Congress expanded this list to include Alaska, Arizona, and Texas statewide, along with select counties and other local jurisdictions in California, Florida New York, New Hampshire, North Carolina, Michigan, and South Dakota. Congress extended Section 5 of the VRA for 25-year periods in 1982 and 2006 without making additional changes to the coverage formula in Section 4(b).

District of Columbia for review. Under Section 5, changes to voting practices or procedures in these jurisdictions could only be enacted if a federal review determined that they had no racially discriminatory purpose or effect; the Department of Justice ("DOJ") could sue jurisdictions that neglected to submit proposed changes for federal consideration (U.S. Commission on Civil Rights, 1975).

Questions over whether federal preclearance was a constitutional exercise of Congress' power to enforce the Fourteenth and Fifteenth Amendments or an unconstitutional violation of states' rights to oversee and maintain elections arose virtually as soon as the VRA passed (Pildes, 2006; Rhodes, 2017). The Supreme Court upheld preclearance under Section 5 in 1966 as a "legitimate response" to the "insidious and pervasive evil" of racial discrimination (*South Carolina v. Katzenbach* (383 U.S. 301 1966)), and neglected to issue further comments directly on its constitutionality in the majority opinion for 2009's *Northwest Austin Municipal Util. Dist. No. One v. Holder*. In 2013, Shelby County, Alabama, issued another form of challenge against the VRA's federal preclearance regime. Plaintiffs argued that the coverage formula had not been updated in decades. Since literacy tests were a thing of the past, and because Black and white voters had been registering and voting at roughly equal rates throughout the parts of the South originally targeted by the VRA since the 1980s (Davidson and Grofman, 1994), plaintiffs argued, preclearance represented an undue burden on jurisdictions that had long since ceased to violate Black voters' rights. The Supreme Court agreed in a 5-4 decision.

To be clear, the Supreme Court stopped short of declaring oversight itself unconstitutional.[3] Congress was left free to modernize its coverage formula to identify places with present-day evidence of pervasive racial inequality in political participation, but has not yet done so. Thus, the implication of the Supreme Court's decision in *Shelby* is that federal review of election laws and procedures in formerly covered areas cannot be enforced until a new coverage formula is adopted by Congress - effectively an indefinite "pause" on enforcement.[4]

Voting rights advocates responded to the *Shelby* decision with concern. The American Civil Liberties Union ("ACLU") issued a press release arguing that "The court's [*Shelby*] decision presents a real challenge to Americans' fundamental right to vote,"[5]. In her dissent, the late Justice Ruth

[3]Clarence Thomas dissented on this point in both *Shelby* and *Northwest Austin Municipal Util. Dist. No. One*, pushing the majority to explicitly declare that Section 5 exceeded Congress' constitutional authority to enforce the Fourteenth and Fifteenth Amendments, but both decisions avoid this question.

[4]See "About Section 5 of the Voting Rights Act." United States Department of Justice, Civil Rights Division.

[5]Laughlin MacDonald, special counsel and director emeritus of the ACLU's Voting Rights Project quoted in "Supreme Court Strikes Down Current Coverage Formula to Voting Rights Act." https://www.aclu.org/press-releases/supreme-court-strikes-down-current-coverage-formula-voting-rights-act-1

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)).

From a historical perspective, it made sense to worry. The jurisdictions previously subject to federal preclearance were the places that had built a nearly comprehensive institutional infrastructure to exclude Black voters before the VRA. These were places that had adopted whites-only primaries, literacy tests, tests of good character, separate ballot boxes, and other methods to curtail registration and voting by Black Americans (Rosenberg, 1991). These places also had robust histories of resistance to federal intervention on behalf of Black voters; jurisdictions that came under preclearance with the VRA's original passage in 1965 spent the period from 1965-1969[6] gerrymandering, converting single-member districts to multi-member districts, consolidating counties, changing elected positions to appointed ones, changing candidate registration requirements to make it more difficult for Black candidates to appear on ballots, and implementing a host of other measures in an effort to circumvent the VRA and dilute Black votes (Davidson and Grofman, 1994; Rosenberg, 1991; Aghion, Alesina and Trebbi, 2008). Electoral incentives, in some ways, also mirrored the political climate of the 1960s: as of 2012, state legislatures in all fully preclearance states except for Alaska and Virginia were Republican-controlled. Most were contending with substantial, cohesively Democratic-voting Black minority populations whose registration and turnout rates they had every incentive to suppress - even if only for partisan reasons (see Valentino and Neuner (2017) and Biggers and Hanmer (2017) for an overview).

Unease over the possibility that state and local legislative bodies might pass new restrictions targeting minority voters was further stoked by an apparent flurry of policy change immediately in the wake of the *Shelby* decision. Less than 24 hours after the court's decision, then-Texas Attorney General Greg Abbott issued a statement saying that the state's voter identification law, which had been suspended under federal review, would take effect immediately.[7] Soon after, North Carolina passed an expansive set of restrictions on early voting, registration, and polling

[6]In 1969, the Supreme Court clarified that federal oversight applied to voting rule changes beyond the registration process and the ballot box. Voting rules, rules governing what it took for candidates to appear on the ballot, converting offices from elective to appointive, and many other such changes were explicitly declared subject to preclearance in *Allen v. State Board of Elections* (393 US 544 (1969)).

[7]Statement released by Attorney General's office, found here.

station hours, and instituted a strict photo identification requirement. In 2018, the Brennan Center issued a report raising concern about another potential form of "democratic backsliding" in formerly preclearance areas: purges of the voter rolls. The report pointed out that places formerly covered by preclearance requirements purged voters from the rolls illegally (that is, too close to an election date to meet the National Voter Registration Act's ("NVRA") requirement of 90 days prior, or without notifying voters that they would have to re-register in due time) and seemed to be purging their voter rolls more aggressively than non-covered areas (Morris and Peréz, 2018). These changes could raise the costs of participation for voters who would have to acquire new forms of identification, re-register if they had been purged from the rolls, take time off or travel longer distances to vote at a more limited set of polling stations, etc.

Advocates also began linking election changes to voting patterns, pointing out that in some cases, raw turnout gaps between Black and white voters in formerly covered states looked considerably larger in 2020 than they had been in 2012 (Morris, Miller and Grange, 2021; Morris and Grange, 2023). The raw white-Latino turnout gaps, too, appeared higher in 2020 than they had been in 2012 for covered states (Morris, Miller and Grange, 2021). In the next section, we consider possible pathways by which *Shelby* could plausibly have changed voting participation, as well as describing predictions from the empirical literature on the turnout and turnout-disparity implications of these kinds of election changes.

2 Preclearance: Functioning and Possible Effects of Removal

What did preclearance actually do, and how might eliminating it affect the electoral landscape in areas formerly bound by it? The federal oversight implied by coverage under Section 5 had two possible types of effects on covered areas: a direct legal impact and a symbolic deterrent power. The federal government exercised direct legal authority to review potential changes to election laws, request more information about them and how they might affect the electorate, and, in some cases, issue objections that prevented discriminatory laws from being enforced. Between 1965 and 2013, the DOJ reviewed 556,268 proposed changes to election laws.[8] The DOJ outright objected to approximately 2,300 of the 400,000 proposed changes to voting laws and procedures they reviewed between 1982 and 2005 alone, and issued requests for more information in almost 14,000 cases (Fraga and Ocampo, 2006). The most direct effect of preclearance, then, was to prevent the impacts of over 2,000 potentially discriminatory legal changes from being realized.

[8] The Civil Rights Division of the DOJ makes this data available here.

Scholars and legal observers of the VRA have pointed to some legal limits on the direct effect of preclearance. From 1965 to 1969, the VRA's primary targets were the "tests and devices" that served as barriers to minority registration. Section 5, too, was almost exclusively focused on registration and ballot access in this period (Davidson and Grofman, 1994). In 1969's *Allen v. State Board of Elections* (393 US 544 (1969)), the Supreme Court vastly expanded the scope of preclearance to include precisely the measures that did not directly target minority registrants and voters, but tried to dilute their voting strength by making it more difficult for them to elect the candidates of their choice.[9] Most election laws the DOJ reviewed related to these forms of vote dilution rather than outright vote denial; just over 45,000 reviews were categorized as related to "voter registration procedures" by the DOJ. Additionally, experts have pointed out that the volume of reviews carried out by the DOJ under Section 5 dropped considerably over time; the DOJ issued just 76 objections to proposed changes between 2000 and 2012, 5 of which were directly related to registration procedures (Tokaji, 2014).

We make two points about such critical assessments of preclearance's direct impact. First, while the bulk of the DOJ's reviews after 1969 applied to potentially discriminatory vote dilution laws, the DOJ *had* begun to review measures that made it harder to vote as Texas, Alabama and other covered places proposed tighter voter identification laws (Tokaji, 2014). These restrictions were already on the rise by 2011 (Levitt, 2012), and could reasonably escalate in volume or severity in a post-*Shelby* world. Second, issuing formal objections to proposed laws was not the only way that the DOJ could push states and localities to alter potentially discriminatory legislation. Fraga and Ocampo (2006) show that even requests for additional information about legal changes under review appeared to shape jurisdictions' behavior.

In addition to a direct impact on election laws in covered places, Section 5 also had an important symbolic deterrence function. Preclearance issued a strong signal to incumbents throughout the South that the federal government was willing to monitor electoral institutions in covered areas. Even if the government didn't literally review and object to a specific proposed change, it *could*, which may have kept state and local governments in covered areas from even proposing laws they were certain the federal government would sue to strike - thus a drop in volume of submissions could also be interpreted as a signal that states and localities had internalized the federal government's nondiscrimination requirements. More than any specific set of objections

[9] These measures included changes like adding qualifications for candidates from new parties who wanted to run, purging voter rolls and reidentifying voters in specific jurisdictions, annexing or consolidating territories into new election districts, converting single-member districts to at-large districts, converting elected offices to appointed ones, and other changes that would not affect voters directly at registration or the ballot box, but could nonetheless reduce their representation (Davidson and Grofman, 1994; Parker, 1990; Rosenberg, 1991; Komisarchik, 2023).

issued by the DOJ, it is this symbolic oversight power that led elites in preclearance areas to view the process as invasive and unfair (Feder and Miller, 2020; Rhodes, 2017). Without the federal government watching, then, *Shelby*'s critics worried that governments in formerly covered areas could both (1) pass legal changes they would not have dared to propose before 2013, knowing that the DOJ would object, and (2) engage in forms of discretionary discrimination that weren't expressly subject to review under Section 5 but might still have attracted unwanted attention from federal overseers watching over state and local elections.

Advocates' concerns over purging the voter rolls represent a good example of fears that a lack of deterrence from Section 5 might spill over into other election behaviors not completely subject to preclearance in formerly covered areas. Several studies have pointed out more aggressive purging of the voter rolls by covered counties relative to non-covered counties after 2013 (Morris and Peréz, 2018; Feder and Miller, 2020); the Brennan Center's reports were covered across national media outlets, which warned that purges might be a harbinger of democratic backsliding after *Shelby*. "Purging" voters from registration lists can be a routine part of list maintenance, useful for ensuring the voting rolls are not clogged with people who are deceased or have moved away (Ansolabehere and Hersh, 2014; Shaw, Ansolabehere and Stewart III, 2015; Huber et al., 2021). But list purges can also be misused to remove people who actually belong on the list, and to disproportionately remove voters of color. Memorably, an "overzealous" 2000 effort to remove people with past felony convictions from the voter rolls mistakenly removed many eligible Black voters (United States Commission on Civil Rights, 2001; Tokaji, 2005).

The purging of voter rolls would have been partially subject to preclearance before *Shelby* in the sense that, if a state explicitly passed legislation requiring the Secretary of State or some other actor to purge voter rolls and require voters to re-register, such legislation would have been reviewed and potentially rejected by the DOJ if it was discriminatory.[10] But public-facing reports on vote purging (Morris and Peréz, 2018) did not focus only on places passing legal changes that would previously have been reviewable under preclearance. Instead, they appeared to picture a more diffuse process, by which local or state officials who had previously feared federal oversight and intervention in their elections would no longer feel constrained from using discretion in purging voters from the rolls in potentially-discriminatory ways.

Indeed, a theory of election officials more freely using discretion, even in realms that had not previously been directly subject to review by the federal government, points to a range of possible election changes in previously-covered places. Election officials might distribute election

[10] See, for instance, the DOJ's determination on Alabama's State Act 81-226 in 1981, available here

resources, such as funding for voting machines or training poll workers, in a way that disadvantaged minority voters. Election officials might be less informative or forthcoming with minority voters seeking information about how to register, or they might act on other biases while enforcing election laws (Atkeson et al., 2010; Alvarez, Atkeson and Hall, 2013; Atkeson et al., 2014) with increasing openness. This sort of diffuse or "symbolic" effect of *Shelby*'s removal of preclearance is rendered more plausible by past findings of differential behavior in covered and non-covered places during the era of pre-clearance, even for non-reviewable types of decisions. For example, a 2012 audit study of local election officials found ethnic discrimination in responsiveness to voter questions across the country, but did not find such discrimination in places subject to preclearance (suggesting that the very existence of federal monitoring, even in different realms of election policy, might be constraining election officials' use of discretion more generally) (White, Nathan and Faller, 2015). In a similar study that largely replicated those findings in the post-*Shelby* era, formerly-covered places no longer appeared different from the rest of the country (Hughes et al., 2020). Thus, we take seriously the possibility that a wide range of election practices, even those not explicitly subject to federal review pre-*Shelby*, could change in the wake of the decision.

How big an effect might such changes have had on voters? In brief, there is limited evidence that modern election changes of the sort that received public attention after the *Shelby* decision, such as voter ID laws, have substantial impacts on voter participation (or disproportionate turnout effects among minority voters). It is possible that we may not observe large effects on registration and turnout after the removal of preclearance, even in the presence of election changes, because the potential vote suppression strategies that have become salient since *Shelby* simply do not have the ability to reduce registration and turnout in the ways that earlier measures did.

Before the VRA, states and local governments employed literacy tests and tests of good character as a functional ban on registration and voting by Black Americans (Keele, Cubbison and White, 2021). Black Southerners hoping to vote before 1965 would have to figure out how to get applications and ballots past registrars who were unwilling to collect them and brave often violent enforcement of exclusionary measures. These measures were extremely effective at reducing registration and turnout; just 7% of Mississippi's Black citizen voting age population were registered in the spring before the VRA was signed into law (Davidson and Grofman, 1994; Grofman, Handley and Niemi, 1992). Removing these sweeping barriers had large, nearly immediate effects on Black political participation (Fresh, 2018; Ang, 2019). Black registration rates rose by nearly 70%, on average, within three years of the VRA's passage (Cascio and Washington, 2014).

These legal measures that imposed nearly insurmountable costs upon Black voters remain illegal under Section 2 of the VRA. A new generation of voting restrictions, including policies like voter ID laws, purges and subsequent re-registration requirements, are all similarly expected to operate by increasing costs for voters seeking to register and turn out. But the costs imposed by these modern policies are qualitatively different from previous measures both in their scale and their reach.

Let us consider, for example, one very salient type of election change: voter ID laws. Scholars have pointed out that minority voters might be disproportionately affected by voter identification laws in the sense that they are disproportionately represented among people who lack the forms of identification required to register and vote (Henninger, Meredith and Morse, 2021; Barreto et al., 2019; Fraga and Miller, 2022; Bentele and O'Brien, 2013; Rocha and Matsubayashi, 2014; Barreto, Nuño and Sanchez, 2009), and are asked to provide identification at higher rates than non-minority voters (Atkeson et al., 2010; Cobb, Greiner and Quinn, 2010). Thus, identification laws can impose racially disparate burdens on potential voters. As we discuss elsewhere in the paper, we consider these costs real and normatively important. But in considering how they might translate into changes in voting participation, we must consider both the size of the costs imposed and the share of the electorate exposed to them. The additional costs imposed by voter identification laws, for instance, are not prohibitive for the large share of voters who have identification or can easily obtain it. One recent study found that a maximum of just 0.31% of voters across elections in Michigan and Florida voted without identification (Hoekstra and Koppa, 2019); another placed this figure at approximately 0.45% of voters (Henninger, Meredith and Morse, 2021). Turnout impacts are further limited by the existing distribution of voting habits: among people without identification, voting rates tend to be quite low even prior to legal changes (Fraga and Miller, 2022; Highton, 2017; Stewart, 2013; Barreto, Nuño and Sanchez, 2009). Theoretically, then, it is not clear that we should expect large, negative changes in any group's registration or turnout rates as a result of additional identification requirements. Indeed, most empirical investigations into the impact of voter identification laws have found mixed or negligible effects on overall turnout and that of specific racial groups (Alvarez, Bailey and Katz, 2007; Hood III and Bullock III, 2012; Grimmer et al., 2018; Hoekstra and Koppa, 2019; Cantoni and Pons, 2019).

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. But as Grimmer and Hersh (2023) point out in their recent review of many types of voter-focused policies, the same logic may hold for most types of election changes that are currently available to policymakers, alone or in combination. And recent studies about the impact of removing preclearance on other much-discussed forms of potential vote dilution seem to further support the idea of there being limited tools available within the current legal regime; Stephanopoulos, McGhee and Warshaw (2023) similarly report that districting plans in formerly preclearance areas did not "retrogress," or reduce minority representation after *Shelby*. Such patterns are broadly consistent with the opinion in *Shelby*, not in the sense that discriminatory intent is necessarily a thing of the past, but in the sense that the legal tools still available to any political actors seeking to restrict minority votes may not be collectively effective enough to constrain political participation on a large scale. We certainly do not claim that there is no way for previously covered places to retrogress to the pre-VRA period; indeed, further legal changes to the VRA that allowed the return of first-generation vote suppressive tools such as literacy tests could be expected to have drastic effects on participation, as could any future policies able to disproportionately target large swaths of minority voters while imposing substantial enough costs to deter voting. We note only that available evidence on the types of present-day election changes most commonly discussed in the wake of *Shelby* predicts much more muted aggregate effects.

3 Election Changes

Under preclearance, covered places had to submit any proposed changes in their election practices to the federal government. With that requirement removed, one possible outcome was that states and municipalities would make dramatic election changes that would previously have been directly or indirectly constrained by federal oversight. States might pass voter identification laws that would not have passed muster under preclearance,[11] 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 systematically 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

[11]It is not the case that no voter ID law could be approved under a pre-clearance regime. Georgia's voter ID law, for instance, was granted preclearance approval prior to the *Shelby* decision. The voter ID laws that would have been prevented from taking effect were those for which the government found evidence of discriminatory effects. Additionally, states outside of the federal government's preclearance jurisdiction passed and augmented voter ID restrictions throughout this period.

from the voter rolls or to reduce polling-place resources after 2013. In each case, we use a simple difference-in-differences approach: we compare time trends from before to after the 2013 decision, between places that were and were not affected by the decision.

These outcome measures are far from a complete picture of potential changes to state and local election practices. Nor do they all represent practices that have been consistently linked to changes in minority voter participation. However, these are changes that can be observed using extant data, and we intend them as a test of the idea that jurisdictions changed their election practices when given the opportunity. We anticipate that a variety of other harder-to-observe changes could also have taken place; though our evidence cannot directly test for those other changes, these highly-visible measures are a natural place to start looking.

Voter ID laws We begin by examining states' implementation of voter identification laws, relying on the National Conference of State Legislatures' detailed history of voter ID.[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" requirement that actually required (rather than requesting) an ID in order to cast a regular ballot. For each of these three measures, we focus on whether the state actually had an active ID law in place in a given year (not a law that passed but would be implemented in future years or was delayed by litigation).

Figure 1 shows the time trends in voter ID laws in previously covered and non-covered places between 2001 and 2020.[13] Preclearance states were more likely to have any ID law in place than non-preclearance states, even before the *Shelby* decision. But the two groups appear to follow broadly common trends both before and after the decision: it doesn't seem that preclearance states began implementing many 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 ID law on the books by 2013).

However, the *content* of state laws changed dramatically after the decision. The central panel

[12]We collected the NCSL data from its website. For a handful of places with unclear legal status, and for 2016-2020, we supplement the NCSL data with information from Ballotpedia.

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

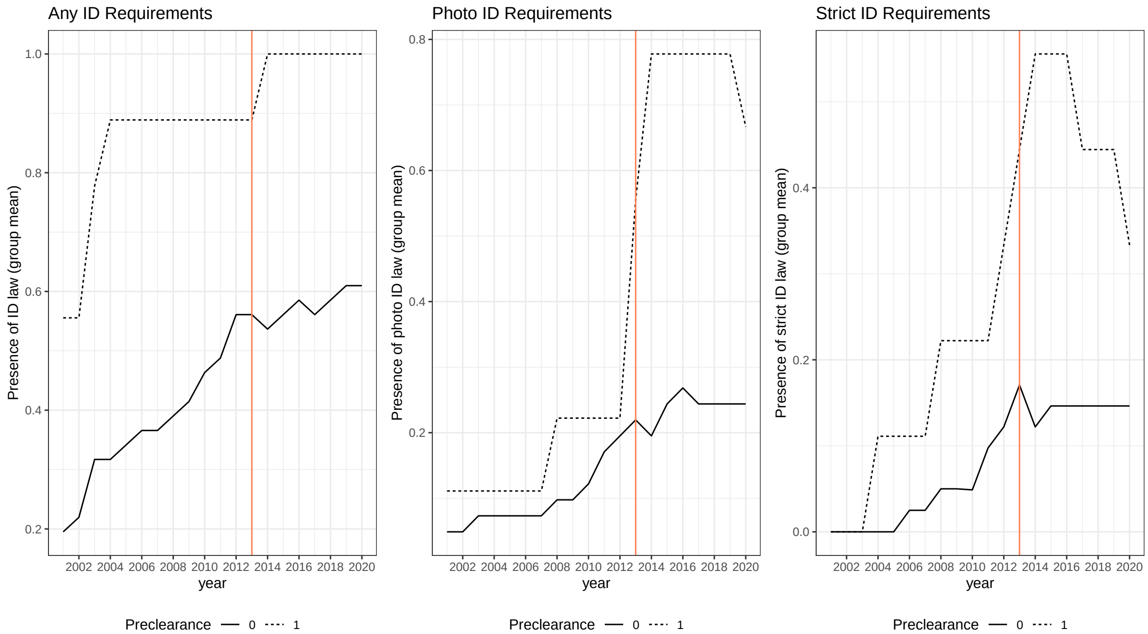

Figure 1: Time trends in types of voter ID laws as recorded by the NCSL. In all panels, the dotted line represents mean values in preclearance or formerly-preclearance states, while the solid line represents non-covered states.

of Figure 1 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 for all three outcomes: indeed, previously-covered states became substantially more likely to implement photo ID laws after the *Shelby* decision. The point estimate for strict ID laws also indicates a substantial change, though it is less clearly distinguishable from zero.

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

	Dependent variable:		
	Any ID Law	Photo ID Law	Strict ID Law
	(1)	(2)	(3)
Preclearance x Shelby	-0.038	0.443^{*}	0.247
	(0.098)	(0.151)	(0.130)
State fixed effects	X	X	X
Year fixed effects	X	X	X
Observations	900	900	891
R^2	0.784	0.700	0.575
Adjusted R^2	0.766	0.676	0.540
Note:			$^{*}p < 0.05$

EAVS data Next, we turn to data on local election administration. County-level election officials have substantial discretion in administering elections, as they are typically tasked with recruiting and training pollworkers, maintaining voter registration lists, siting polling places, and directing ballot counting. Voters of color have worse voting experiences than white voters on average (Chen et al., 2019), and election officials' decisionmaking could potentially contribute to racial disparities. There is evidence that local clerks and pollworkers discriminate in implementing and providing information about election laws, particularly in jurisdictions not subject to preclearance (Cobb, Greiner and Quinn, 2010; White, Nathan and Faller, 2015). It is plausible that local election officials freed from VRA oversight might make decisions about election administration that would disadvantage voters of color, though we note other work that has found election officials do not use their decisionmaking power to attempt to advantage their preferred group (along partisan lines: see Ferrer, Geyn and Thompson (2021)). We consider these outcomes to be a test of the "symbolic" effects of removing preclearance, as these kinds of practices would generally not have been explicitly subject to pre-clearance even before 2013.[14]

For measures of local election administration, we use the Election Administration and Voting Survey (EAVS), conducted during election years by the US Election Assistance Commission. Since

[14]For example, a jurisdiction might have needed to submit legal changes governing registration purges if they wanted to drastically change their rules, but individual decisions to remove voters from the rolls pursuant to existing rules would not have been subject to review.

2004, the EAC has sent surveys to election officials across the country, asking questions about their election practices and about registration and voting in their jurisdictions. We reviewed the survey for any questions that might indicate changes in local election administration that could potentially make it easier or more difficult for minority voters to participate. Section G of the SI discusses the process of cleaning this dataset. This analysis is at the county, rather than state, level, as counties are meaningful units both for EAVS data collection and for the local election processes considered here.[15]

We examine three measures of election administration, all displayed in Figure 2. We follow previous work in examining the removal or "purging" of registrants from the voter file (Feder and Miller, 2020). We follow the Pew Elections Performance Index in constructing a measure of the provisional ballot rejection rate (the number of provisional ballots cast but not counted divided by the total votes cast). Given public attention to poll closures (The Leadership Conference Education Fund, 2019), we also examine the number of pollworkers per registered voter as a measure of election-day capacity.[16] The EAVS measures are suggestive of some post-*Shelby* electoral changes, though there is substantial uncertainty around these estimates.

The top panel of Figure 2 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.[17] It appears that previously-covered places moved from removing similar shares of voters from the rolls (or even fewer) to removing substantially more voters than non-covered places, beginning in 2014. The first column 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), though with appropriately-clustered standard errors this estimate is too noisy to statistically distinguish from zero over the time period examined.

[15] Section A of the SI describes how we classify individual counties as previously "covered" or not covered by preclearance.

[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.

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

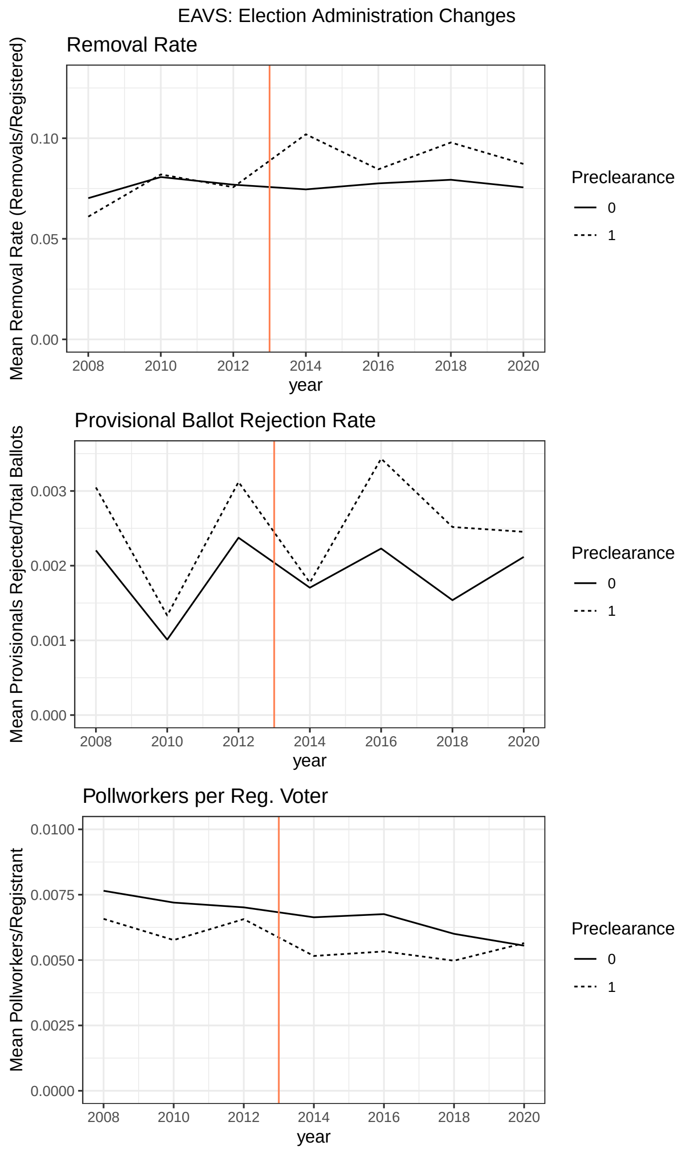

Figure 2: Time trends in election administration as reported in EAVS survey of jurisdictions. In all panels, the dotted line represents mean values in preclearance or formerly-preclearance counties, while the solid line represents non-covered places.

The second panel of Figure 2 shows a measure of the provisional ballot rejection rate over time

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

	Dependent variable:		
	Registration Purge Rate	Provisional Reject Rate	Pollworkers/Reg. Voter
	(1)	(2)	(3)
Preclearance x Shelby	0.027	0.00002	0.0005^{*}
	(0.015)	(0.001)	(0.0002)
County fixed effects	X	X	X
Year fixed effects	X	X	X
Observations	17,958	17,710	16,913
R^2	0.471	0.336	0.810
Adjusted R^2	0.369	0.202	0.770
Note:			$^{*}p < 0.05$

in affected and unaffected jurisdictions.[18] Having many provisional ballots cast and ultimately rejected could indicate 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 *Shelby* decision had somewhat higher provisional-rejection rates than other jurisdictions even before 2013, but covered and non-covered places follow similar trends in the pre-2013 period. After 2014, the trends appear to diverge, with previously-covered places increasing their provisional-ballot rejections more steeply than unaffected places; this pattern would be consistent with it becoming harder to vote in these affected places post-*Shelby*. But this increase is small enough in magnitude that we cannot statistically distinguish it from zero (see column 2 of Table 2), so we present these estimates with caution.

The final panel of Figure 2 shows trends in the number of pollworkers per registered voter. Affected places consistently use fewer pollworkers than unaffected places for most years before and after *Shelby*. But that difference does not appear to increase substantially after the Shelby decision, as seen both in the figure and in the third column of Table 3. If anything, it appears that in 2020 previously-covered places caught up to the rest of the country in their pollworker

[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: states use provisional ballots at different rates for many reasons, and we are particularly interested in the influence that the rejection of provisional ballots has on the overall vote count, not just on the count of provisional ballots.

numbers, which is reflected in a small but statistically-significant positive effect in column 3 of Table 3. This pattern is the opposite of what we might have expected if looking for limitations on voting options in previously-covered places in the wake of *Shelby*, though we note that 2020 was a particularly complex year for election administration in which different jurisdictions around the country may have made different (and temporary) choices about how to staff in-person polling places during COVID.

Conclusions about Election Changes As noted in Section 2 above, the *Shelby* decision could well have allowed for a range of changes in election administration. We distinguish between changes that had been explicitly constrained by previous federal decisions during the preclearance period (such as strict voter identification laws) and those that had not been explicitly barred by the Department of Justice but might nevertheless have been constrained by the presence of federal monitoring. Here, we see very clear evidence of states moving to implement voter identification policies that had previously been explicitly restricted under preclearance. But when we turn to the possibility of more "symbolic" impacts of *Shelby*, with places freed from preclearance feeling more empowered to make changes to local election administration that had not explicitly been subject to preclearance pre-2013, the evidence is more mixed. Two of the three local election-administration measures we examined in the EAVS data showed noisy but suggestive evidence of potentially higher costs of voting for voters purged from the rolls in previously-covered places after the *Shelby* decision, while the third measure (pollworker density) showed limited change in the opposite direction (driven largely by the 2020 election).

Some observers may interpret this pattern of changes as evidence of efforts to suppress voting, particularly among minority voters, while others may read it as election administrators freed from unnecessary federal oversight making newly-allowed changes to elections for other reasons. But these analyses, though limited in their ability to directly measure policymakers' internal motivations, rule out the possibility that the *Shelby* decision simply left elections in the US unchanged. If nothing about election practices had changed, we would think it especially implausible that the decision could be expected to shape voter participation. Now that we have seen some evidence of election changes, we turn to our core question: whether the *Shelby* decision led to meaningful changes in minority voter participation in previously-covered places.

4 Electoral Impacts

Next, we ask whether (observed or unobserved) election changes after the *Shelby* decision translated into measurable changes in voting. Given the importance of Black-white participation gaps in motivating the original passage of the VRA and evaluating the continued need for §4, we begin by looking at Black-white gaps in registration and turnout and then turn to additional participation and electoral outcomes.[19] For this analysis, we need a dataset with several characteristics. First, we need to go beyond aggregate data on overall turnout and registration: we need information about how voters of different racial groups fared, since most concerns about the *Shelby* decision were specifically about minority voting rights. And second, we need a dataset that allows us to precisely estimate participation rates for groups that represent a small share of the population in some places. Surveys of voter participation are prone to overstating turnout (Ansolabehere and Hersh, 2012; Burden, 2000) and to yielding very noisy estimates of minority turnout, so we avoid them. Instead, we rely on voter-file data drawn from state elections records, combined with estimates of voter identity.

For this project, we use a dataset constructed from the voter database maintained by Catalist, LLC, a voter-list vendor that collects and cleans voter-file data from state elections offices. Catalist's database includes individual observations for people registered in each state, as well as estimates of each registered voter's racial identity.[20] 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 2008-2020, as well as the number of people from each group that turned out to vote in each of those years. This dataset was constructed using a series of voter-file snapshots from previous years, and does not rely on a given voter's being registered as of 2020. This approach yields a dataset at the county-year level, with estimates of (for example) how many Black voters were registered as of 2008 in a given county, and how many Black voters turned out to vote.

The Catalist data yields raw counts of registrants and voters, but as local population could change over the twelve-year period spanned by our data, we want to calculate the *share* of eligible

[19] In Section C.1 in the SI, we present analogous estimates for Hispanic-White participation gaps, though these estimates carry some questions about parallel trends and measurement error. Like the main estimates presented here, they do not show minority voters losing substantial ground relative to white voters over the period studied.

[20] 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.

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.[21] For instance, the registration rate for Black voters in Autauga County, Alabama in 2010 would be 6,093 registered voters divided by an estimated 6,480 Black citizens aged 18 or older living in the county, or 0.94. To calculate voter turnout rates, we divide Autauga's 2,754 votes cast by Black voters by the same 6,480 eligible Black voters. We construct these rates for each county in each federal election year from 2008-2020.[22] We then construct Black-white racial gaps in registration and turnout by differencing these rates. For example, a county with 70% Black turnout and 75% white turnout would have a calculated Black-white turnout gap of -5% (negative values here denote higher white turnout).

Using this dataset, how can we tell whether the court's decision mattered? One possible approach would be to simply look at the set of places affected by it and ask whether participation gaps, or minority voter turnout, in these places looked different after the 2013 decision than before. But such an approach would not account for many other changes that could be happening in the background over this time period, like national trends in turnout. Instead, we use a difference-in-differences approach: we compare the over-time changes in affected places to 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 differ at baseline in the size of their registration and turnout gaps, their trends over time would have been similar were it not for the court's decision. This assumption cannot be explicitly tested for the period of our analysis, but Figure 3 displays trends from earlier periods as a first pass at evaluating the assumption's plausibility. Preclearance and non-preclearance counties show similar trends before 2013. We continue with several simple difference-in-differences specifications here, but in Section C.4 of the Supporting Information, we discuss a variety of alternative specifications.

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

$$Y_{it} = \tau \text{Covered}_i \cdot \text{Shelby}_t + \gamma \mathbf{X}_{it} + \text{County}_i + \text{Year}_t + \epsilon_{it}$$

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

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

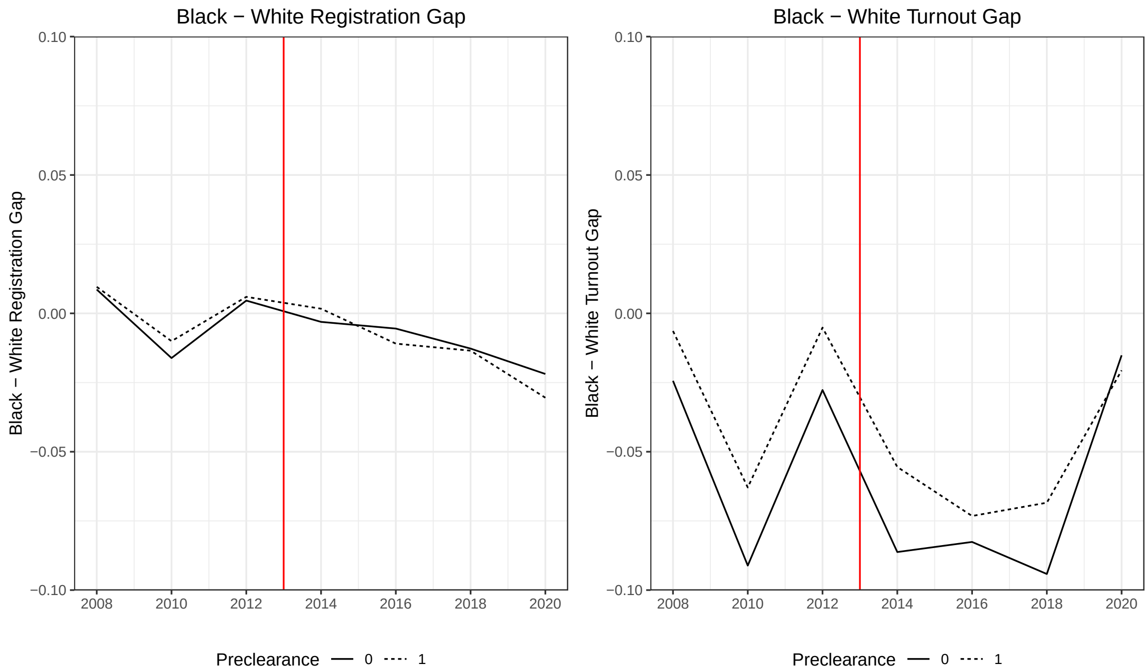

Figure 3: Time trends in Black-white registration (left) and turnout (right) gaps. Dotted lines represent weighted means for formerly-preclearance counties and solid lines represent weighted means for non-covered counties. Means are weighted by county-level Black citizen voting-age population (CVAP). Negative values denote higher white than Black participation.

Here, Y_{it} represents the relevant registration or turnout gap (Black-white or Hispanic-white). "Covered" is an indicator variable for whether county i was subject to preclearance before 2013. "Shelby" is an indicator for whether or not the year post-dates the *Shelby v. Holder* decision: this variable takes on a value of 0 for the years 2008-2012, and a value of 1 for the years 2014-2020. $\mathbf{X}$ represents a set of time-varying county-level covariates included in some specifications. These covariates include: total population, population density, proportion male, proportion over age 65, proportion nonwhite, proportion Hispanic, proportion married, proportion foreign-born, proportion high school graduates, and unemployment rate. We include two-way fixed effects in the form of a fixed effect for each county and a fixed effect for each year. Since treatment is time-based, lower order terms for coverage under preclearance and an indicator for being in a post-*Shelby* period are collinear with county and time fixed effects and are not estimated separately in our model. Our treatment effect of interest, τ, can be interpreted as the average difference in turnout or registration gaps between preclearance and non-preclearance counties in the period after *Shelby* relative to the period before. Throughout the paper, we cluster standard errors on the state (Bertrand, Duflo and Mullainathan, 2004), as preclearance is largely assigned at the state level.[23]

We weight these models by the estimated size of the Black population in each county. This approach limits the impact that measurement error in small counties can have on our estimates. Combining distinct datasets from Catalist and the Census occasionally yields strange patterns, as in counties with small Black populations where Catalist's estimated number of Black voters exceeds the Census' estimate of Black eligible voters in the county. Rather than censoring the estimates at 100% turnout (and potentially introducing other biases), we keep all estimates for counties with group populations above 100 people, but upweight larger and thus better-estimated counties. Unweighted estimates are shown in the SI and yield similar conclusions. Substantively, we are interested in turnout among voters, not among counties, so it makes sense to upweight the counties with more people in them.

The specification above is equivalent to a canonical two-group difference-in-differences estimator. Our dataset consists of between 3089 and 3142 counties each election year from 2008 to 2020. Approximately 900 of these counties were subject to preclearance until the *Shelby* decision in 2013, and are therefore all treated in 2014, 2016, 2018, and 2020 (and untreated in 2012 and prior years). The remaining counties are untreated for the entire period. Small variations in the number of counties included each election year result from differences in Census data availability at the

[23] An alternative block-bootstrapping approach yielded very similar standard errors and equivalent conclusions, so we present clustered standard errors for speed of calculation and code transparency.

county level in different years. The nature of the *Shelby* decision implies no variation in treatment timing: all preclearance counties under Sections 4(b) and 5 were simultaneously allowed to implement changes to voting law without direct federal supervision. Given the structure of treatment, τ corresponds to the average treatment effect on the treated (ATT) (Bertrand, Duflo and Mullainathan, 2004). While a wealth of recent literature has addressed difference-in-differences assumptions and estimation strategies when researchers do encounter variation in treatment timing (Goodman-Bacon, 2018; Callaway and Sant'Anna, 2020), these corrections specifically target cases more complex than the two-group case we present in this paper.

4.1 Estimates

Table 3 presents estimates of the effect of the *Shelby* decision on Black-white voter registration and turnout gaps in affected counties. Columns 1 and 4 report the simplest difference-in-differences specification described earlier in this section. Columns 2 and 5 expand this specification to include time-varying county-level controls derived from Census/ACS data. Columns 3 and 6 include state x year fixed effects in place of county and year fixed effects (included additively in our original specification). In this specification, the treatment effect is identified in a small number of states with within-state variation in preclearance at the county level[24]. Across these specifications, we do not see large or statistically-significant shifts in participation gaps after *Shelby*, consistent with what one sees in a visual inspection of Figure 3. It does not appear that previously-covered places diverged from the rest of the country after the *Shelby* decision. This finding is one of the core contributions of our paper: in the years immediately following the *Shelby* decision, we do not see evidence of Black voters losing ground in places no longer covered by preclearance.

The specific point estimates shown in Table 3, however, are negative: a coefficient of -.007, as seen in Column 4 of the table, suggests that the Black-white turnout gap grew by nearly three-quarters of a percentage point relative to what we would have expected in the absence of the *Shelby* decision's removal of preclearance, a shift of a magnitude that could be significant in very

[24]California, Florida, Michigan, North Carolina, New Hampshire, New York, South Dakota, and Virginia. We include the state x year fixed effects specification because it is a standard approach to robustness for cases where state-level features are important for outcomes and within-state variation is of interest. However, we note that the short list of states with internal variation in preclearance includes cases where we may not theoretically expect changes in the outcomes we track in this study. For instance, covered counties in South Dakota were added because they largely overlap with Native American reservations, leaving questions about whether we should expect to see changes in Black-white turnout gaps here. Further, because the key provisions of the VRA were originally aimed at the South, it's difficult to construct a substantive interpretation of the effects we observe in this specification because they represent a group of states largely outside this area.

close elections with racially-polarized voting. We thus investigate further the possibility that there are meaningful shifts happening in minority voter participation that we simply are not powered to detect in this setup. We turn to a range of other specifications and outcomes to look for evidence of systematic changes in minority participation.

Table 3: Difference-in-Differences Results for Black-White Registration and Turnout Gaps

	Dependent variable:					
	Black-White Registration Gap			Black-White Turnout Gap		
	(1)	(2)	(3)	(4)	(5)	(6)
Preclearance x Shelby	−0.004	−0.005	0.027	−0.007	−0.003	0.015
	(0.010)	(0.009)	(0.028)	(0.008)	(0.006)	(0.021)
County and Year Fixed Effects	✓	✓		✓	✓	
County Demographic Controls		✓	✓		✓	✓
State x Year Fixed Effects			✓			✓
Observations	15,784	15,782	15,782	15,784	15,782	15,782
Adjusted R^2	0.846	0.854	0.236	0.777	0.790	0.331

Note: Standard errors clustered by state. Time-varying county-level controls include: total population, population density, proportion male, proportion over age 65, proportion nonwhite, proportion Hispanic, proportion married, proportion foreign-born, proportion high school graduates, and unemployment rate.

We begin by considering patterns through time: do we see (even small/nonsignificant) shifts in participation immediately after the *Shelby* decision, as jurisdictions began changing their election practices, or do we see any potential gaps emerging later? A preliminary look at the simple descriptive trends plotted in Figure 3 suggests very little divergence between previously-covered places and the rest of the country (or in the case of turnout, an apparent improvement in the Black-white gap in previously-covered places) through 2018, with 2020 looking somewhat different from other years. This apparent shift is especially striking when considering the turnout gap (right panel of Figure 3): 2020 shows a substantial reduction in the Black-white turnout gap everywhere, but previously-covered places are outpaced by the changes in the rest of the country. This pattern of apparent reductions in participation gaps everywhere, including previously-covered places, is not necessarily what we would expect to see if the negative coefficients in Table 3 were a result of racially-disparate vote suppression efforts in previously-covered places. Nevertheless, we proceed with a more formal examination of *Shelby*'s effects through time.

Another simple way to look through patterns in time is to examine the relationship between registration or turnout gaps and preclearance year-by-year. That is: how much larger (or smaller) is the Black-white participation gap in previously-covered places than in the rest of the country in any given year, and how are these regional differences trending over time? We plot this approach in Figure 4, which shows the results of simple OLS regressions of Black-white registration (or turnout) gaps on a dummy for preclearance status for each individual year separately. Standard errors are clustered by state. We do not include fixed effects because each county has a single observation within each year, and we omit indicators for *Shelby* because these would contain no variation across units within a given year. These plots show no evidence of either strong patterns across estimated coefficients over time or coefficients that are significantly different from zero in most years.[25]

In interpreting these patterns, we first note that there is no evidence of a worsening registration or turnout gap in the years immediately following *Shelby*: as previously-covered states quickly changed their election practices to do things like implementing voter ID laws, we do not see an accompanying shift in racial voting patterns relative to the rest of the country. The apparently more negative coefficient in 2020, though still not distinguishable from zero, may lead some readers to wonder whether 2020 was a turning point or reflected some sort of strategy change among policymakers. Could it be that after seeing little change in the years immediately following *Shelby*, policymakers in previously-covered jurisdictions began experimenting with more creative (and more effective) ways of reducing minority voter participation? It is certainly possible, though we hesitate to make any such conclusions based on data from one pandemic-era election cycle (and without clear evidence about specific elections changes emerging in recent years). We encourage future researchers to collect additional years of election data as they become available to watch for any such trends. Meanwhile, we dig further into available dimensions of our dataset to look for additional evidence coherent with this interpretation of a gradually-shifting tide in election administration. Currently-available evidence does not seem especially consistent with such an interpretation.

First, we look to group-specific shifts in turnout after the *Shelby* decision. In section C.2 of the SI, we turn from analyses of racial gaps in participation to simply examining Black, Hispanic, and white registration and turnout over time in covered and non-covered places. Running an analogous difference-in-differences design to our main analysis but using in turn each group's registration and turnout rates as outcome measures, we find that if anything, registration and turnout among all groups examined has *increased* in previously-covered places after 2013, compared to the rest of the country. We cannot always distinguish these differences from zero, but the consistently positive coefficients help to rule out the possibility that minority voter participa-

[25]We also note the absence of strong time trends in the pre-*Shelby* period, consistent with the parallel-trends assumption required for the main difference-in-differences specification (and consistent with Figure 3).

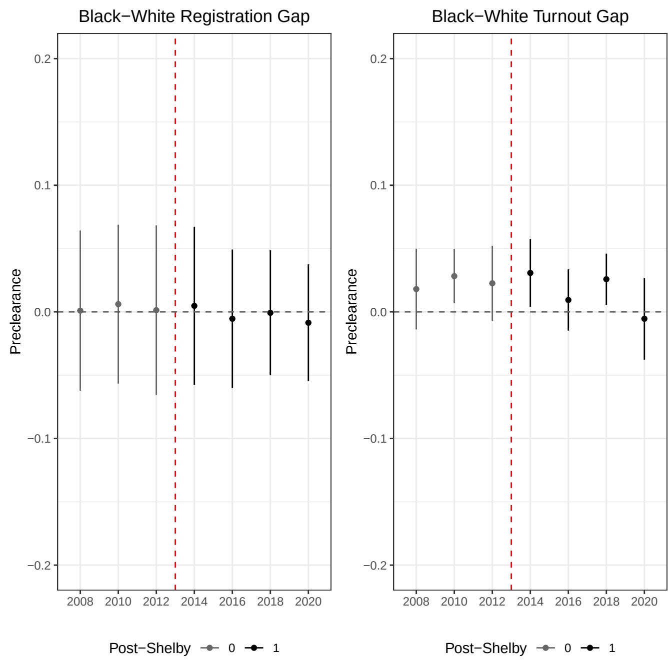

Figure 4: Black-white registration and turnout gaps through time (simple cross-sectional OLS estimates).

tion has been substantially reduced in previously-covered places in the wake of *Shelby*. Even in 2020, a year when we see some indication of an increased Black-white turnout gap, we still see Black turnout increasing in previously-covered places compared to other recent election years. These patterns cannot rule out the possibility that some outside force boosted all groups' participation while local election-administration decisions dampened that growth slightly for some groups (relative to what would have happened under preclearance), but we note that this pattern of across-the board turnout increases is not the first thing one would expect to observe under a system of highly-effective racialized vote suppression.

Next, we ask whether the electorate might have shifted in other ways that are not being captured by our demographic measures. In SI section E, we use 2008-2020 data on county-level vote outcomes to see whether Democratic voteshare changed in previously-covered places after *Shelby*; this approach should let us see if targeted election changes disproportionately reduced Democratic, rather than simply minority, voting. We see no evidence of such a shift: the point estimate in this difference-in-differences analysis, though not statistically distinguishable from zero, is a small positive (if anything, a several-percentage-point increase in Democratic voteshare in previously-covered places after *Shelby*).

4.2 Robustness

Across a range of outcome measures and specifications, we see relative stability in election participation in the wake of the *Shelby* decision. These findings may be surprising to some readers, but we do not think they are an artifact of our data or analytic choices. Section C.4 of the Supporting Information discusses robustness of these patterns to a number of alternate specifications. These include allowing for county-specific slopes or varying time trends, restricting analyses only to the South as well as to only presidential or only midterm years, and sequentially dropping specific years or states from the dataset.

Finally, we note that these findings are consistent with patterns seen in several other data sources. In the online appendix (Section E), 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 (while removing any concerns about race imputation). A difference-in-differences analysis like the one above finds similar patterns: if anything, registration and turnout appear to have increased in previously-covered places since 2013, relative to non-covered places. And in a paper similar in focus to this one, Raze (2021) analyzes survey estimates of minority voter participation from the CCES and finds "resilience" in that *Shelby* did not reduce (and may have increased) Black voters' relative share of the electorate in previously preclearance states. In short, a variety of data sources and model specifications point to unchanged or increased participation in previously-preclearance jurisdictions after *Shelby*.

We note that our analysis estimates the net effect of the *Shelby* decision on participation gaps and group participation; they report the average impact on participation across the full set of places included in the analysis. As such, we do not interpret these estimates as completely ruling out the possibility of vote suppression incidents in some specific jurisdictions. This note about interpretation also carries with it the question of whether there could be offsetting effects occurring: is it possible that participation gaps would have increased across previously-covered places, but for the intervention of some outside force? In SI Section D, we consider the evidence for a process of "countermobilization," or some voters becoming activated by grassroots mobilization efforts that emerged in previously-covered places after 2013. There are limited available data sources to test this possible mechanism, and the evidence we find for this pattern is equivocal: some survey evidence yields point estimates suggesting that voters of color may have been slightly more likely to be contacted with mobilizing messages in previously-covered places after 2013, but we cannot distinguish these estimates from zero, and their magnitude would imply relatively small increases in participation even under generous assumptions. We do not reject the possibility of such countermobilization efforts occurring or shaping elections, but we hope that future research can more thoroughly investigate the nature and scale of any such processes.

5 Downstream Outcomes: Legislative Representation

In addition to minority voter turnout, we also consider some downstream outcomes about legislative representation. We ask whether voters in previously-covered counties saw changes in who was representing them in Congress. Consistent with our pre-registered design, we begin with a look at descriptive representation, asking whether legislative identity shifted after *Shelby*. We also consider the content of that representation by examining legislative partisanship. Consistent with the core findings on voter participation in the prior section, we do not see evidence of shifts in these important downstream outcomes over the period examined.

We begin by looking at House representation: in the wake of the Shelby decision, were people in previously-preclearance counties any more or less likely to be represented by Black congresspeople? And were they more or less likely to be represented by Democrats? Figure 5 plots our data by preclearance status. From 2008-2020, we put together records of House members' identity and partisanship using lists published by Congress[26] and combined them with records from Congressional Quarterly of district numbers and partisanship. We then used crosswalks from the Missouri Census Data Center[27] to map House districts to counties for each redistricting cycle, yielding a county-level dataset analogous to our main dataset with indicators for whether any part of each county was represented by a Black or Democratic house member in any given year.[28] Figure 5 indicates that pre-2013 trends in these forms of representation look broadly similar for previously-covered places and the rest of the country.

[26] https://history.house.gov/Exhibitions-and-Publications/HAIC/Historical-Data/Hispanic-American-Representatives,-Senators,-Delegates,-and-Resident-Commissioners-by-Congress/, https://history.house.gov/Exhibitions-and-Publications/BAIC/Historical-Data/Black-American-Representatives-and-Senators-by-Congress/

[27] https://mcdc.missouri.edu/applications/geocorr.html

[28] Section F of the SI presents analogous figures for Latino representation as well as state legislative representation and similarly finds no clear patterns of change.

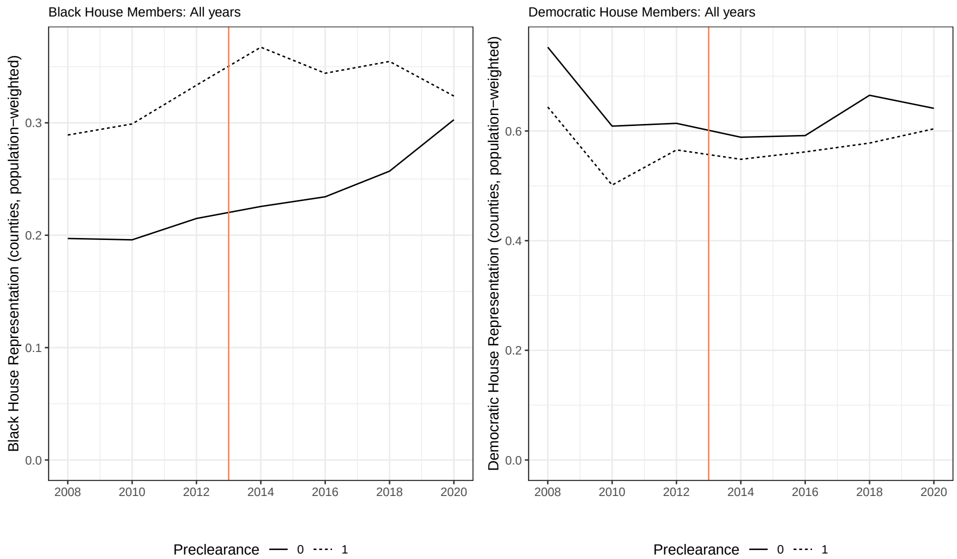

Figure 5: Time Trends in House Representation

Table 4 presents difference-in-differences estimates for these outcomes in the wake of the *Shelby* decision. As in the main estimates of registration and voting effects, these models use counties as units, weighting by county population, and cluster standard errors at the state level. There is no clear pattern of representational shifts in previously-covered places after the decision: point estimates suggest slightly less Black representation and slightly more Democratic representation, but none of these point estimates are statistically distinguishable from zero.

6 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. We see clear changes in voter identification laws, and mixed evidence of changes in local practices such as registration purges and provisional ballot rejections. It does not appear that Black-white registration gaps have substantially widened, or that Black or Hispanic registration or voter turnout have dropped in previously-covered places since that decision; if anything, it seems participation has increased across the board. These increases have occurred despite real changes in election practices in jurisdictions previously covered by preclearance. Our findings are consistent both with other work on the limited impact of the *Shelby* decision (Raze, 2021;

Table 4: DiD Results for Preclearance After Shelby: Congressional Representation

	Dependent variable:	
	Black Congressperson	Dem Congressperson
	(1)	(2)
Preclearance x Shelby	-0.010	0.039
	(0.049)	(0.030)
County fixed effects	X	X
Year fixed effects	X	X
Observations	21,651	21,651
R^2	0.845	0.802
Adjusted R^2	0.819	0.768
Note:		$^{*}p < 0.05$

Stephanopoulos, McGhee and Warshaw, 2023), and more broadly with recent work highlighting the limited effects of even large election-law changes on voting participation or election outcomes (Grimmer and Hersh, 2023).

What can we conclude from these patterns? We can rule out the possibility that *Shelby* left elections completely unchanged in places previously subject to preclearance: these jurisdictions have certainly used their release from preclearance to implement election laws (such as strict voter identification laws) that would not have been allowed under federal monitoring. These laws may well impose disproportionate burdens on minority voters along with other groups less likely to have identification, even if existing empirical literature does not link them to substantial turnout changes (White, Nathan and Faller, 2015; Barreto, Nuño and Sanchez, 2009; Grimmer and Yoder, 2019; Cantoni and Pons, 2019; Zhang, 2022). But consistent with past work on the turnout implications of such laws, we do not see these changes translating into a reduction in political participation or influence among minority voters over the period studied. This is not to say that participation is fully equal in previously-covered places. Rather, we join Fraga (2018) in noting with concern that Black-white participation gaps (and those of other groups) persist across the country, not only in places previously covered by Section 5.

Previous research on election administration had also suggested that there could be broader changes to election practices after *Shelby*, with the removal of federal monitoring leaving election officials feeling empowered to change even portions of election procedure that had not been directly subject to pre-clearance ("symbolic" effects of removing monitoring). We note the limitations of our ability to observe local decisionmaking about how to implement existing election laws, though Section 3 finds limited evidence of such changes over the period 2014-2020 in a federal survey of election administration.

Observers may nevertheless wonder whether short- and long-term effects of *Shelby* could diverge, especially given the apparent differences in our registration and turnout estimates when comparing 2020 data to prior years.[29] We hesitate to interpret existing data as evidence of some sort of "turning point" in 2020 without further years of data and more examination of plausible mechanisms for any potential change. It is theoretically possible that some jurisdictions may have been taking a "wait-and-see" approach to the court's decision in *Shelby*, and that depending on additional court decisions about other components of the VRA, they might feel increasingly emboldened to experiment with new and more targeted elections changes. However, given recent work on the limited policy tools available for even motivated actors to reshape the electorate in the present legal regime (Grimmer and Hersh, 2023), it would be valuable for the research community to more clearly articulate and systematically measure the presence of a range of potential elections changes. The continued presence of other portions of the VRA means that some historical forms of vote denial that comprehensively targeted minority votes (such as literacy tests) appear to still be off the table, leaving open questions about whether even highly-motivated political actors have the ability to effectively suppress minority voting via "second-generation" tools.

As such, we close this paper with an acknowledgement of its limited scope and an exhortation to future research. The question of *Shelby*'s effect on voters was so pressing that we thought it important to begin preliminary investigations using data from the first few election cycles after the decision, and on highly-visible electoral changes. But we acknowledge that some of the concerns raised by Justice Ginsburg and voting-rights advocates were about matters like vote dilution and the process of redistricting, not solely on individual voter participation, and also that some fear longer-term effects even in the absence of short-term ones. The historical record, as well as the current patterns of racially-polarized voting in previously-covered places, yields reasons to watch these jurisdictions closely. We welcome work like Stephanopoulos, McGhee and Warshaw (2023) on outcomes beyond those covered in this paper, and we encourage the collection of additional years and types of data, especially that which will help to understand the role of individual voters'

[29] As noted above, 2020 appears visually somewhat different in our descriptive plots of over-time trends in registration/turnout gaps and levels, and including this year in the data flips the point estimate of some effects from positive to negative, though all estimates remain statistically indistinguishable from zero (including year-specific 2020 estimates).

(and grassroots organizations') agency in navigating the political landscape in the wake of *Shelby*.

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Appendices

A Preclearance Definition

Our definition of "covered" counties (those previously subject to preclearance under Section 4 of the VRA) is drawn largely from a list provided by the Department of Justice.

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

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

B Validating Catalist data against other datasets

We validated the Catalist data we use in this project by comparing it to several other datasets, in hopes of noticing any strange patterns or major errors. We began with a comparison to Current Population Survey estimates. The CPS is often used to produce estimates of turnout by race at the state level, so we aggregated the Catalist dataset to the state level for comparison. We used state-level estimates of citizen voting age population from the ACS (for 2010-2018) to turn the raw Catalist turnout counts into turnout rates comparable to the ones calculated from CPS data. When calculating CPS turnout rates, we rely on the "cpsvote" R package (Lee and Gronke, 2020), using its "Hur-Achen" approach to nonresponse and its provided weights to handle over-time changes in response rates.

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

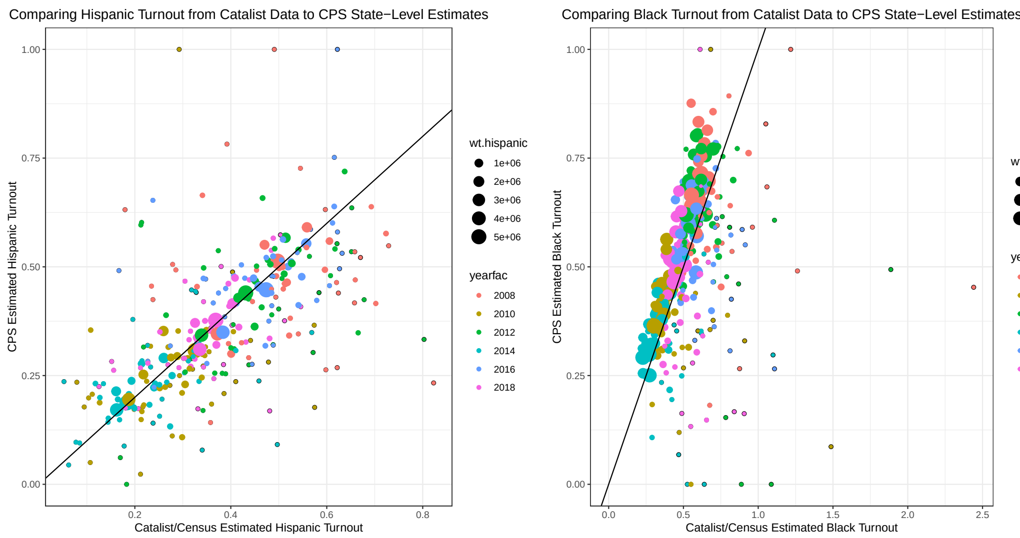


Figure 6: Comparing Catalist Hispanic and Black turnout estimates to CPS-derived estimates

The left panel of Figure 6 compares Catalist and CPS estimates of Black turnout. The diagonal line again shows equivalence between the Catalist and CPS estimates, though in this case the axis is stretched out by the presence of a few extreme outliers in the Catalist data. As noted in the main paper, there are a few places where small Black populations combined with measurement error in either the Catalist turnout estimates or the ACS estimates yield impossible turnout estimates of over 100%. The two points on the extreme right side of the plot are estimates from North Dakota, a state with a very small number of estimated Black eligible voters and thus a lot of room for measurement error to influence estimated turnout in fairly extreme ways. Given our population-weighted approach to the main estimates, we do not think counties in ND are likely to exert a large influence over our analyses. The estimates are broadly similar across the two datasets, particularly for places with large Black populations (represented by larger points), though the CPS estimates are on average slightly higher than the Catalist ones (consistent with turnout over-reporting on the CPS, as in Ansolabehere, Fraga and Schaffner (2020)).

Next, we compared our county-level Catalist estimates to estimates from David Leip's county-level elections data (obtained for 2008-2020 through the MIT library system). Leip's data reports aggregate registration and turnout counts for each county in each year, not estimates for specific racial groups. Still, we thought it worth summing up our Catalist data to produce county-level estimates of the total number of registered voters and ballots cast for each county year and comparing those to the Leip estimate to diagnose problems.

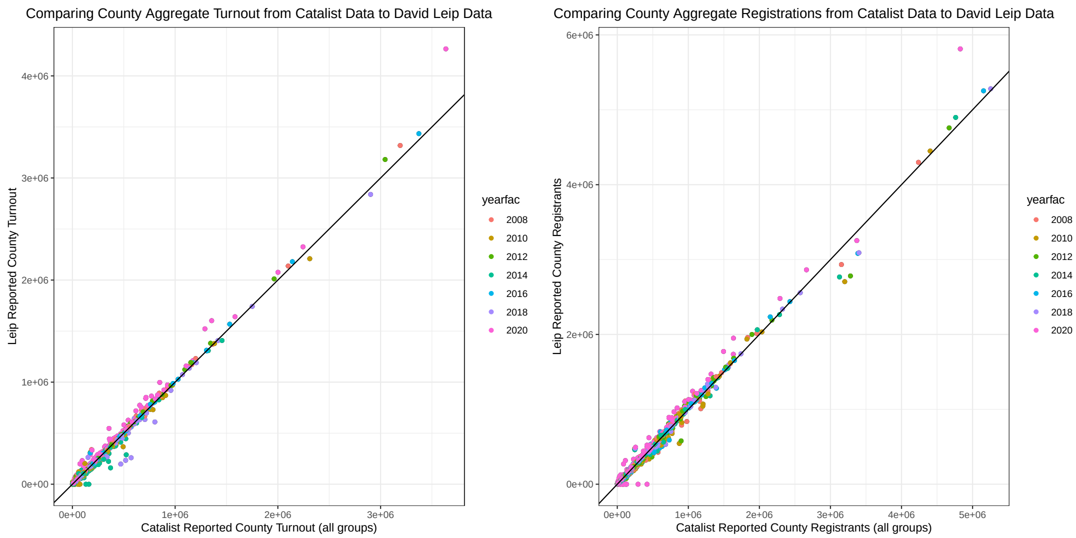


Figure 7: Comparing Catalist county turnout/registration estimates to Leip data

We were able to merge over 99% of the counties in our main dataset to counties in Leip's data using FIPS codes; the main source of missed matches was a difference in how Alaskan counties/-election districts were handled across datasets. The left panel of Figure 7 compares our Catalist turnout estimates to Leip's, with the diagonal line representing equivalence in the two datasets' estimates. The datasets have very similar county turnout numbers; slight differences (points off the line) do not appear systematic across years.The right panel of Figure 7 performs the same exercise for county registration numbers. Again, the estimates line up tidily on the 45-degree line for most county-years.

C Additional Analyses of Registration/Turnout

C.1 Hispanic-White Gaps

In this section, we present analyses of the Hispanic-white registration and turnout gap analogous to the Black-white analyses presented in the main paper. We begin, as in the main paper, by simply plotting the trends over time in both the registration and turnout gap in previously-covered and non-covered places in Figure 8. The pre-2013 trends in the registration gap appear broadly similar in covered and non-covered places, but the turnout gap trends do not appear as similar, so we present and interpret difference-in-differences estimates for this measure with caution. (For more formal consideration of these pre-trend issues, see section C.3 below, where a pre-treatment placebo test finds significant effects of *Shelby* on this outcome prior to 2013, which is of course impossible).

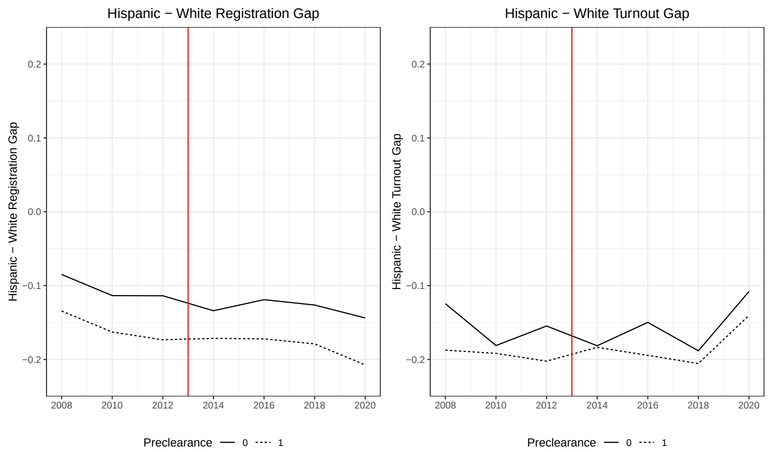


Figure 8: Time trends in Hispanic-White registration (left) and turnout (right) gaps. Dotted lines represent weighted means for formerly-preclearance counties and solid lines represent weighted means for non-covered counties. Means are weighted by county-level Black citizen voting-age population (CVAP).

Table 5 presents analogous DiD estimates to Table 3 in the main paper. Again, the first three columns present estimates of *Shelby* effects in affected places on the registration gap, while the last three columns examine the turnout gap. Comparably to the main paper, we do not see evidence of a widening Hispanic-white registration or turnout gap in affected jurisdictions: most of these point estimates are non-significant, and all of them are positively rather than negatively signed (a negative point estimate would mean that the Hispanic-white gap was widening, with white turnout further outpacing Hispanic participation). We see one statistically significant estimate on the Hispanic-white turnout gap (column 6, when including county-level covariates for precision), but we hesitate to conclude that the Hispanic-white turnout gap has actually *narrowed* in previously-preclearance places. In light of the pre-trend differences shown in Figure 8, we place less weight on these estimates for the Hispanic-white turnout gap than we do on the main-paper estimates about Black-white gaps.

Figure 9 presents dynamic estimates analogous to the main paper's Figure 4, this time focusing on Hispanic-white registration and turnout gaps. The first two point estimates (prior to 2013) serve as an additional way of considering possible parallel-trends violations, and as mentioned above, we see concerning pre-treatment "effects," particularly on the turnout gap.

Table 5: Difference-in-Differences Results for Hispanic-White Registration and Turnout Gaps

	Dependent variable:					
	Hispanic-White Registration Gap			Hispanic-White Turnout Gap		
	(1)	(2)	(3)	(4)	(5)	(6)
Preclearance x Shelby	0.002	0.003	−0.022	0.017	0.017*	−0.012
	(0.011)	(0.007)	(0.020)	(0.010)	(0.007)	(0.012)
County and Year Fixed Effects	✓	✓		✓	✓	
County Demographic Controls		✓	✓		✓	✓
State x Year Fixed Effects			✓			✓
Observations	17,177	17,175	17,175	17,177	17,175	17,175
Adjusted R^2	0.681	0.710	0.432	0.690	0.710	0.556

Note: Standard errors clustered by state. Time-varying county-level controls include: total population, population density, proportion male, proportion over age 65, proportion nonwhite, proportion Hispanic, proportion married, proportion foreign-born, proportion high school graduates, and unemployment rate.

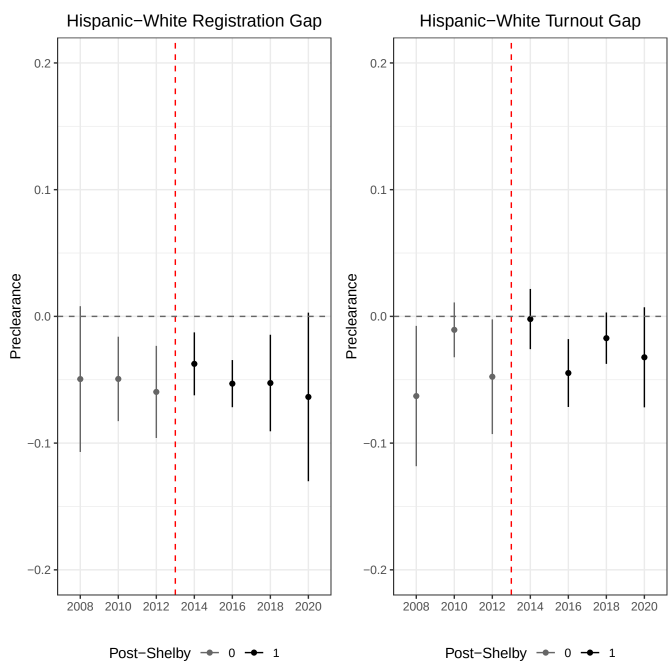


Figure 9: Dynamic estimates for Hispanic-White registration and turnout gaps

C.2 Group-specific Turnout

In this section, rather than considering participation gaps, we simply examine registration and turnout rates by specific groups in the dataset across time and space.[30] These group-specific estimates were the main estimates presented in an earlier working paper version of this paper.

We again begin by simply plotting registration and turnout rates over time in covered and non-covered places. Figure 10 plots each group's registration and turnout, weighted by county group populations (such that counties with a large Black population, for example, count more heavily in the Black population trend than those with smaller Black populations).[31] We also include as the fourth panel for each outcome a look at overall registration and turnout that does not incorporate any racial-classification data from Catalist (it simply looks at total registration and total turnout divided by total CVAP for the jurisdiction). For the most part, these outcomes appear to track closely in the pre-2013 period when comparing covered and non-covered places, which lends plausibility to the parallel trends assumption needed for the difference-in-differences setup[32]

Table 6 thus presents simple difference-in-differences estimates of the effect of the *Shelby* decision on group-specific registration and turnout rates in previously-covered places. As with estimates in the main paper, these are weighted by group population and drop counties with tiny (<100) populations, and standard errors are clustered by state. Consistent with the patterns shown in Figure 10 above, these estimates suggest that if anything, registration and turnout increased across the board in previously-covered places. Some of these point estimates are statistically distinguishable from zero and some are not, but all are positively signed and appear to rule out substantial *decreases* in minority voter participation in previously-covered places over the period studied.

C.3 Placebo Tests

One might wonder whether the kinds of estimates shown in the main paper could arise by chance, perhaps due to some other background "treatment" or some systematic issue with the data used. To assess this possibility, we run placebo tests where we implement the main analysis

[30]We focus on white, Black, and Hispanic voters as several large and geographically-dispersed groups of voters. Other groups could certainly 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 in this paper.

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

[32]We note some apparent trend differences in Hispanic registration rates, especially in 2012, which appear to trace back to variation in ACS population estimates during this period. We present estimates on Hispanic registration with caution.

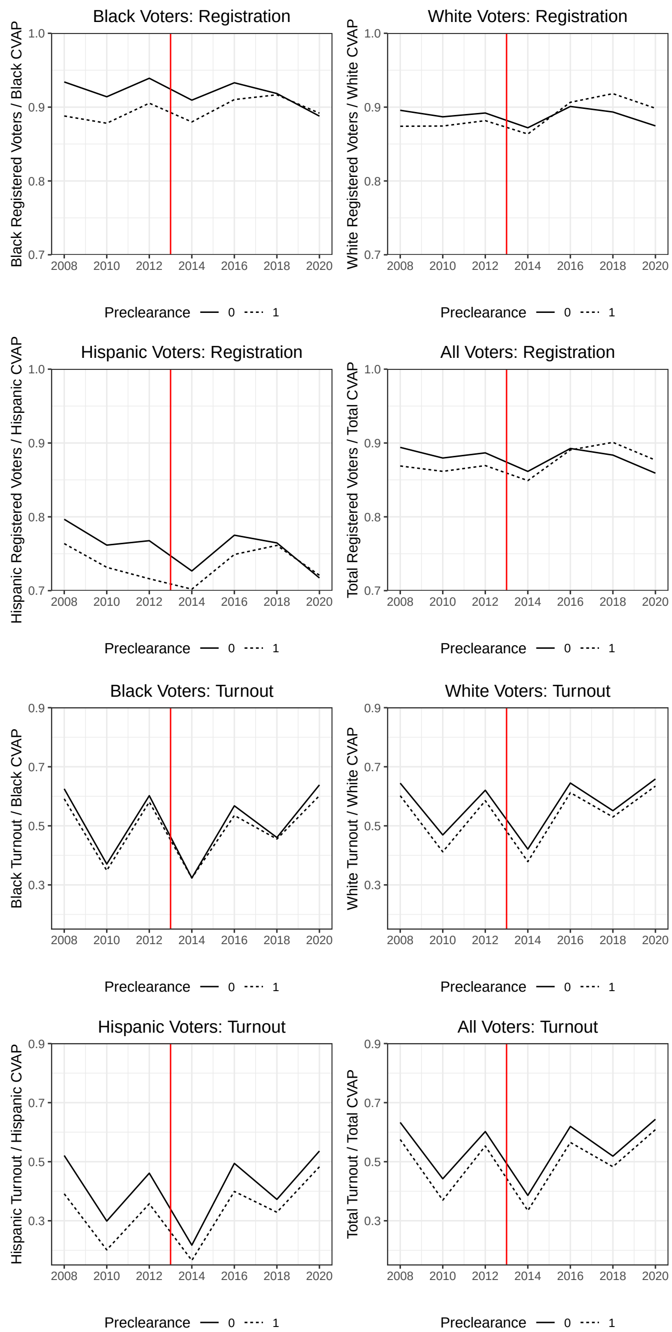


Figure 10: Registration and Turnout Trends (Group-specific)

Table 6: Group-Specific Registration and Turnout Estimates

	Dependent variable:					
	Black Reg.	Hispanic Reg.	White Reg.	Black Turnout	Hispanic Turnout	White Turnout
	(1)	(2)	(3)	(4)	(5)	(6)
Preclearance x Shelby	0.027	0.032*	0.030*	0.008	0.049*	0.031*
	(0.013)	(0.008)	(0.009)	(0.007)	(0.011)	(0.014)
Observations	15,784	17,177	21,891	15,784	17,177	21,891
R^2	0.854	0.863	0.822	0.916	0.920	0.905
Adjusted R^2	0.826	0.836	0.792	0.900	0.904	0.889
Note:						$^*p < 0.05$

using "placebo" treatment years. We set false decision years for the *Shelby* case in 2009 and 2011 (rather than 2013, as in reality) and report the results of our estimation procedure under these assumptions. We rely on these years because they are the only pre-treatment years for which data is available; including post-treatment years would risk incorporating real effects from any real treatment period. Table 7 presents the resulting estimates: no choice of placebo year produces statistically significant effects on Black-white turnout or registration gaps (the main specification used in the paper), and the estimates vary in direction. Table 8 presents the same exercise for Hispanic-white registration gaps, and here we see some concerning patterns of apparent "pre-treatment effects" on Hispanic-white turnout gaps that should make us interpret the main DiD estimates for this measure with caution (as noted in Section C.1 above).

Table 7: Difference-in-Differences Results for Black-White Registration and Turnout Gaps in Placebo Treatment Years

	Dependent variable:			
	Black-White Registration Gap		Black-White Turnout Gap	
	(1)	(2)	(3)	(4)
Preclearance x Shelby	0.003	−0.002	0.007	−0.0001
	(0.009)	(0.007)	(0.008)	(0.007)
Placebo Year	2009-2010	2011	2009-2010	2011
Observations	6,531	6,531	6,531	6,531
Adjusted R^2	0.904	0.904	0.852	0.852
Note:				*p<0.1; **p<0.05; ***p<0.01 Standard errors clustered by state.

Table 8: Difference-in-Differences Results for Hispanic-White Registration and Turnout Gaps in Placebo Treatment Years

	Dependent variable:			
	Hispanic-White Registration Gap		Hispanic-White Turnout Gap	
	(1)	(2)	(3)	(4)
Preclearance x Shelby	0.007	-0.004	0.036^{**}	-0.017^{***}
	(0.018)	(0.010)	(0.016)	(0.006)
Placebo Year	2009-2010	2011	2009-2010	2011
Observations	6,958	6,958	6,958	6,958
Adjusted R^2	0.881	0.881	0.834	0.829

Note: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$
Standard errors clustered by state.

C.4 Robustness to Alternative Specifications

C.4.1 County-Specific Trends

Table 9 presents estimates of the *Shelby* decision's effect on Black-white registration and turnout gaps in a specification that allows for time-varying slopes for each county in addition to county and year fixed effects. Including variable slopes by county allows us to capture and account for county-specific time trends in election climates that might affect racial turnout gaps for reasons apart from *Shelby*.

Accounting for county-specific time trends in addition to fixed effects for county and year yields small positive coefficients, non-significant in the case of the registration gap and significant in the case of turnout. The direction of these estimates suggests that if anything, the Black-white registration and turnout gaps in previously-covered places *shrank* in the post-*Shelby* period. These estimates differ slightly from those in the main paper in that they are positively-signed (rather than the insignificant negative coefficients in the main paper) but they continue to bolster our central conclusion that the post-2013 period had not seen substantial reductions in minority participation or political power in previously-covered places.

Table 9: DiD Estimates for Black-White Registration/Turnout Gaps with Varying County Slopes

Dependent Variables:	Black-White Registration Gap	Black-White Turnout Gap
Model:	(1)	(2)
Variables		
Preclearance × Shelby	0.0062	0.0155**
	(0.0045)	(0.0074)
Fixed-effects		
County	Yes	Yes
Year	Yes	Yes
Varying Slopes		
County	Yes	Yes
Fit statistics		
Observations	15,784	15,784
R^2	0.94853	0.89649
Within R^2	0.00056	0.00437

Clustered (state)) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

C.4.2 State-Level Analyses

Following Bertrand, Duflo and Mullainathan (2004), we further validate our estimates by aggregating to the state level. Table 10 summarizes our main difference-in-differences specifications at the state level. Here, registration and turnout levels are summed over counties within each state and year and divided by corresponding group CVAP in order to generate registration and turnout rates by state. Following our previous analysis, we weight by group population in order to upweight states with large subgroup populations.States designated as preclearance include those states previously under statewide coverage (see Footnote 12 of the main paper); 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 estimates are consistent with those we report in the main paper: we do not find evidence of large or statistically-significant increases in Black-white registration or turnout gaps in previously-covered places after the *Shelby* decision.

Table 10: Difference-in-Differences Results for Registration/Turnout Gaps at the State Level

	Dependent variable:	
	Black Reg. Gap	Black Turnout Gap
	(1)	(2)
Preclearance x Shelby	−0.005	−0.004
	(0.006)	(0.006)
Observations	356	356
R^2	0.849	0.778
Adjusted R^2	0.820	0.735
Residual Std. Error (df = 298)	19.863	18.536
Note:		$^{*}p < 0.05$ Results weighted by state-group population in 2008. See footnote 15 for preclearance criteria at the state level.

C.4.3 Different Time Periods

In addition to artificially re-setting treatment to years other than 2013 and finding the anticipated null effects, we run additional checks to ensure robustness over space and time. Figure 11 presents the estimates from an analysis in which we iteratively drop every year in our data, to show that our estimates are not dependent on events or data issues occurring in any one year. Estimates are broadly consistent with the main specification we present in the paper even when excluding any given year, though as noted in the paper the point estimates are even more precisely 0 when excluding 2020.

Further, in Table 11 we subset the main dataset to run separate analyses focused on midterm and presidential elections. These estimates are also consistent with our main specification, if noisier as a result of using fewer observations. As noted in the paper, 2020 appears somewhat different from other years, and accordingly the point estimates for the turnout gap in presidential years (the subset including 2020) appear more negative than those in midterm years, though still not statistically distinguishable from zero.

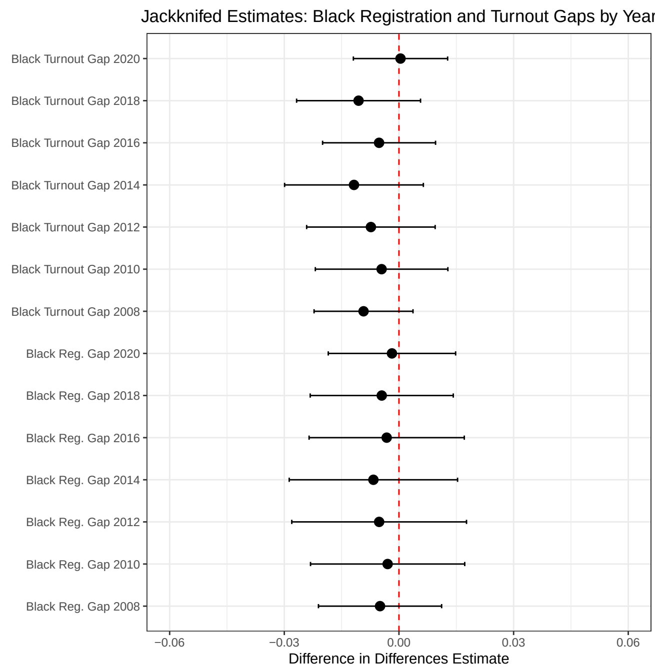


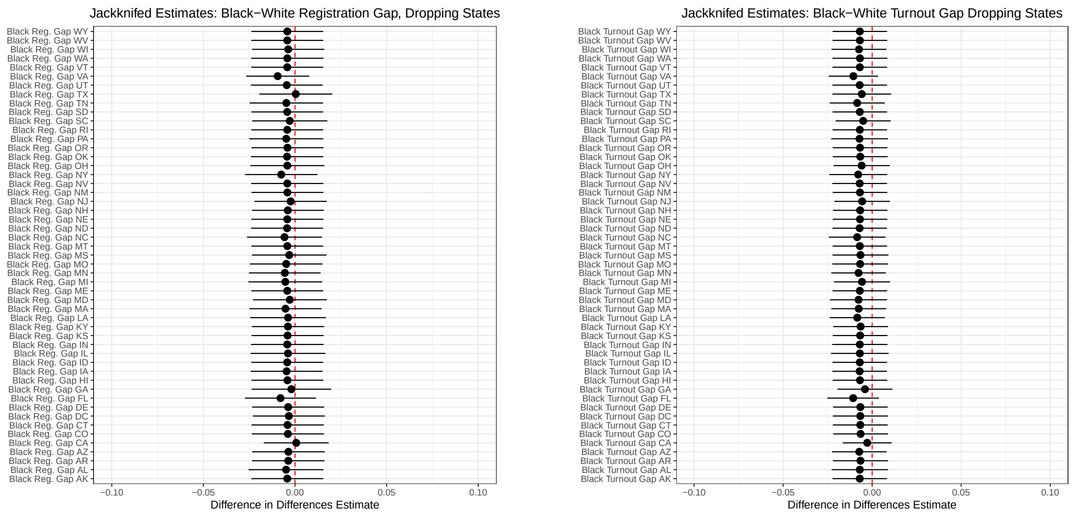
Figure 11: Difference in Differences Estimates for Dropped Years

Table 11: Difference-in-Differences Results for Registration/Turnout Gaps in Midterm/Presidential Election Years

	Dependent variable:			
	Black Reg. Gap	Black Turnout Gap	Black Reg. Gap	Black Turnout Gap
	Midterm Years		Presidential Years	
	(1)	(2)	(3)	(4)
Preclearance x Shelby	−0.003	0.001	−0.007	−0.017
	(0.008)	(0.005)	(0.012)	(0.012)
Observations	6,755	6,755	9,029	9,029
R^2	0.904	0.886	0.858	0.819
Adjusted R^2	0.851	0.823	0.803	0.750
Residual Std. Error	6.013 (df = 4339)	3.389 (df = 4339)	6.927 (df = 6538)	5.252 (df = 6538)
Note:				$^*p < 0.05$; Standard errors clustered by state.

C.4.4 Excluding Individual States

Another robustness concern is the possibility that our estimates are driven primarily by outcome changes in a single state, or perhaps by measurement error in one state's data. To investigate this possibility, we iteratively drop states from our analysis in order to examine whether differences in turnout and registration by group remain consistent. Figures 12 show that the results do not depend exclusively on the presence of specific states. Difference-in-differences estimates for registration and turnout gaps change very little when any individual state is excluded.



(a) Black-white Registration Gaps

(b) Black-white Turnout Gaps

Figure 12: Difference-in-Differences Estimates for Turnout and Registration Gaps Excluding Individual States

C.4.5 Weighting

As we discuss in Section 4, our main analysis weights counties by the size of the relevant minority group for which we analyze turnout and registration. Here, we verify that the conclusions we reach are not strictly an artifact of these population weights. We show this, in part, by using raw registration and turnout totals from Catalist in Section E below. In addition to this, we show the results of our main analysis of turnout and registration gaps without weighting in Table 12 .[33] These estimates are consistent with our main analyses in the sense that they do not show any indication of worsening Black-white registration or turnout gaps in previously-covered places after *Shelby*; rather, they find significant and implausibly-large reductions in Black-white gaps over this period (a positive coefficient here denotes a shrinking Black-white gap). We think these estimates carry questionable assumptions about parallel trends; unweighted registration- and turnout-gap estimates in covered/uncovered places do not track nearly as closely in the pre-period as our weighted measures do, potentially due to strange population-estimate fluctuations in small places. As such, we present them with caution, but we hope to provide transparency into

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

the impacts of our analytic decisions and to illustrate that removing weights from the analysis does not provide evidence that reverses the conclusions of the paper.

Table 12: Difference-in-Differences Results for Registration/Turnout Gaps with Unweighted Data

	Dependent variable:	
	Black Reg. Gap	Black Turnout Gap
	(1)	(2)
Preclearance x Shelby	0.117*	0.056*
	(0.020)	(0.012)
Observations	15,784	15,784
R^2	0.903	0.858
Adjusted R^2	0.884	0.831
Residual Std. Error (df = 13264)	0.232	0.147
Note:	$^*p < 0.05$; Standard errors clustered by state.	

C.4.6 The South

The VRA's original target jurisdictions for preclearance were all states in the Deep South. While the preclearance formula expanded over time, the South's large Black citizen population and robust history of minority vote suppression rendered it especially subject to federal scrutiny until *Shelby*. In Tables 13 and 14, we examine trends in turnout and registration gaps in the South alone. Following our pre-analysis plan, we use two different definitions. One approach narrowly defines "the South" as the 11 original Confederate states: AL, AR, FL, GA, LA, MS, NC, SC, TN, TX, and VA. Arkansas, Florida, North Carolina, and Tennessee were never preclearance in their entirety, so counties within these states form a comparison group within the southern region. We also use the U.S. Census Bureau's broader definition of the South, which includes the following states: AL, AR, DE, Washington DC, FL, GA, KY, LA, MD, MS, NC, OK, SC, TN, TX, VA, and WV.

Table 13 relies on the Confederacy definition and Table 14 on the Census definition. These estimates are generally consistent with the paper's results, showing no change in Black-white registration/turnoug gaps in preclearance areas (relative to non-preclearance areas) after *Shelby*. The one exception is the small positive coefficient on the Black-white turnout gap when limiting to the former Confederacy. The positive direction of this estimate suggests that Black turnout growth may have slightly outstripped white increases in participation in previously-covered places over the post-2013 period, though we interpret these estimates with some caution given the smaller sample size (and fewer untreated places) when limiting to the former Confederacy. Nevertheless, this robustness test does not indicate that Black-white turnout gaps widened in previously-covered places after *Shelby*, even when focusing our attention narrowly on the region of the country most often discussed as the target of the VRA.

Table 13: Difference-in-Differences Results for Registration/Turnout Gaps in Former Confederate States

	Dependent variable:	
	Black Reg. Gap	Black Turnout Gap
	(1)	(2)
Preclearance x Shelby	0.011	0.020*
	(0.012)	(0.007)
Observations	7,238	7,238
R^2	0.848	0.808
Adjusted R^2	0.822	0.775
Residual Std. Error (df = 6153)	5.017	4.102
Note:	$^*p < 0.05$; Standard errors clustered by state.	

Table 14: Difference-in-Differences Results for Registration/Turnout Gaps in Census South Region

	Dependent variable:	
	Black Reg. Gap	Black Turnout Gap
	(1)	(2)
Preclearance x Shelby	−0.003	0.008
	(0.013)	(0.009)
Observations	8,782	8,782
R^2	0.849	0.794
Adjusted R^2	0.823	0.757
Residual Std. Error (df = 7453)	5.141	4.177
Note:	$^*p < 0.05$; Standard errors clustered by state.	

D Looking for Evidence of Countermobilization

In this section, we look for evidence that efforts to register and mobilize Black and Hispanic voters increased after the *Shelby* decision, or that voters saw more reason to turn out. This question is important both because of a prominent hypothesis that there could be counterbalancing effects in the wake of *Shelby* (that is, effective vote suppression efforts being met with grassroots countermobilization efforts that fend off net decreases in voting), as well as simply providing a chance to consider the role of on-the-ground activism in shaping voter participation. In practice, we encounter substantial data limitations and find relatively inconclusive patterns. We encourage other researchers to continue to seek out new approaches to measuring grassroots voter activism.

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."[34] Though it is difficult to quantify all of the get-out-the-vote efforts of many disparate organizations, it is plausible that they ramped up in the wake of the *Shelby* decision. However, it is difficult to track such efforts systematically across space and time, and recent work has pointed out the

[34]See, for instance, the MacArthur Foundation

limited size of any such expected effect given what is known about GOTV efforts (Grimmer and Hersh, 2023).

We first turn to survey data to look for evidence of such efforts. The Cooperative Election Study (CES, formerly CCES) is run every two years. In several recent election years (2006-2020, excluding 2008 and 2018), the survey asked whether people had been contacted during the election cycle by a campaign organization or candidate. We use this question, combined with information about respondents' county of residence and self-reported racial identity, to see whether campaigns' GOTV efforts targeted at voters of color increased in previously-covered places after the *Shelby* decision. This question does not capture all possible mobilizing activity, since it is focused on campaigns and not other groups' efforts, but it gives a consistent view of mobilization efforts across time and geography.

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

We begin by asking whether voters experience different mobilization trends in places that were and were not affected by the *Shelby* decision. Columns 1 and 2 of Table 15 indicates that voters in previously-covered places reported extra campaign mobilization after the *Shelby* decision. These voters saw about three percentage points' higher rates of campaign contact after the decision than would otherwise have been expected (shown by the interaction between "preclearance" and "post-*Shelby*" in the table).

In the second half of Table 15, we ask whether that additional mobilization was focused on minority voters, as we would expect if it were driven by efforts from groups worried about voting rights. Here, the coefficient of interest is the interaction between "preclearance" (whether a jurisdiction was covered by preclearance before 2013), "Post-Shelby" (whether the observation is from before or after the 2013 *Shelby* decision), and "Non-white." In both specifications, this coefficient is positive, suggesting more mobilization of nonwhite voters in previously-covered places after the *Shelby* decision. This pattern is consistent with a story about countermobilization, though the estimates are noisy and not statistically distinguishable from 0. We also acknowledge Grimmer and Hersh (2023)'s point about the importance of considering the magnitude of these effects: taken at face value, they imply about a 5.6-percentage point increase in non-white voters reporting campaign mobilization efforts in previously-covered places after 2013. Making even generous assumptions about the effectiveness of campaign contacts and about how survey responses relate to actual mobilization efforts (imagining, for example, that increased campaign contacts might also be paired with non-campaign mobilization work that reached an even larger number of voters) still implies fairly limited effects of the kinds of mobilization reported here. Such increases in mobilizing contact could perhaps produce increases in participation of somewhere below one

Table 15: Self-Reported Mobilization (CES)

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

percentage point (among targeted population groups), but could not, for example, be offsetting what would otherwise be a large (multiple percentage point) reduction in voting driven by policy changes.

Next, we turn to another survey dataset to look at voters' perceptions of the electoral system. As noted above, some research in political psychology finds that voters can react strongly to perceived attempts to disenfranchise them. Voters could potentially react to the *Shelby* decision or the electoral changes that followed with backlash, perhaps becoming more likely to vote in the wake of those changes.[35] We are not aware of any panel survey that asks voters directly whether they think voting rights are under threat. However, the Survey on the Performance of American Elections (SPAE) asks voters whether they believe their vote was counted as intended in the most recent election. Although this is not a question explicitly about voting rights, we expect it to capture respondents' views about the integrity of the electoral system in their area, which should give an idea of whether they are concerned about voting access for people like them.

The SPAE surveys registered voters in a sample of states about their experiences in each federal election (Stewart, 2017). We rely on responses to the question "How confident are you that your vote in the General Election was counted as you intended?" from 2008, 2012, 2014, and 2016.[36] We focus on the share of voters reporting that they were "not too confident" or "not at all confident" that their votes were counted as intended; Figure 13 plots this measure over time for all respondents and for white and non-white voters separately. In these simple unweighted plots, it appears respondents in places previously covered by preclearance follow similar trends to those in other places before the *Shelby* decision, but then show much higher rates of skepticism about the electoral process after the decision; this pattern is particularly striking among non-white voters.

Table 16 again presents difference-in-differences estimates for all voters and then considers nonwhite voters specifically. Columns 1 and 2 show that after the 2013 *Shelby* decision, voters in previously-covered places became several percentage points more likely to say they doubted their vote was counted as intended. In exactly the places where we might think voters would be turning out due to a sense of foreboding about voting rights, we see more voters expressing a lack of confidence in elections. Columns 3-4 ask whether this pattern is especially pronounced for non-white voters, as we would expect if minority voters were responding to perceived threats to voting rights after the *Shelby* decision. When we include an interaction between previous preclearance status, the post-*Shelby* period, and voter race, we see a pattern consistent with higher rates of concern among minority voters in affected places. The "preclearance * Post-Shelby * Non-white"

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

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

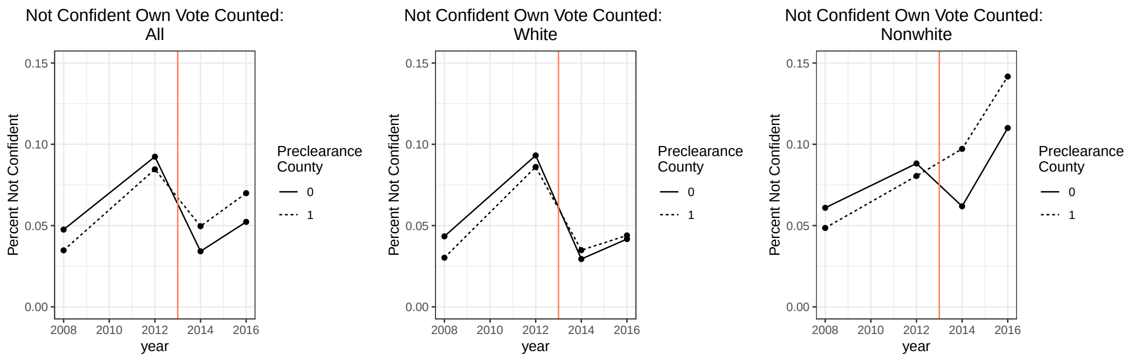


Figure 13: SPAE respondents' lack of confidence in vote counting by race and by home county's preclearance status.

coefficient suggests that nonwhite voters in affected places became several percentage points more likely to say they were not confident about vote-counting after the *Shelby* decision, though this coefficient is somewhat noisily-estimated and cannot be statistically distinguished from 0. Again, we also urge attention to the size of these estimates when thinking through how large a shift in voter participation could potentially be attributed to such voter concerns.

D.1 New Registrations from Catalist (Countermobilization)

In addition to the survey data presented above, we also look for evidence of countermobilization using a dataset constructed by Catalist of new voter registrations recorded in each county over each two-year election cycle from 2008 to 2018. For each election year (presidential and midterm), the dataset uses an aggregate snapshot of the voter file taken shortly after the election to tally up the number of new voter registrations added to the voter file in each county over the previous two years since the prior election.[37] For example, a person who moved to Cobb County, Georgia and registered to vote in 2011 would be recorded in the 2012 "new registrations" data for that county, as would a person who had previously lived in the county unregistered but decided to register in summer 2012. These estimates are based not on comparing the total number of registrants in a county at different time points, but on the dates that each individual person's registration appeared on the voter file.

This dataset should allow us to see whether new registrations increased in previously-preclearance counties after the *Shelby* decision. If voting-focused organizations worked to contact and register unregistered people or to help them update outdated registrations to reflect their current ad-

[37]This time window means that we have election years from 2010 through 2018 in this dataset: the 2010 observation captures new registrations taking place between the 2008 and 2010 elections. Aggregate data does not disaggregate by race.

Table 16: Lack of Confidence that Own Vote Counted Correctly (SPAE)

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

dresses, this dataset should capture the results of those efforts. We note that efforts to contact and turn out already-registered people would not be captured by this dataset.

We run a similar difference-in-differences analysis to the one presented in Section 4 above, but the outcome measure is now the share of voting-eligible residents of a county who appear as newly registered in a given year. As above, we cluster standard errors by state and weight by county population (in this case, total population rather than group-specific estimates, since we do not have new-registrations data by race). Table 17 displays the difference-in-differences estimate from this approach: the point estimate is positive, consistent with new registrations increasing very slightly in previously-covered places after 2013, but is noisily-estimated and cannot be distinguished from zero. It is possible that on-the-ground efforts to help voters register ramped up in counties affected by the *Shelby* decision, but this analysis does not allow us to say with certainty that those efforts occurred or succeeded.

Table 17: New Registrations (from Catalist) as a share of CVAP

	Dependent variable:
	New Registrations Rate
Preclearance x Shelby	0.002
	(0.008)
County fixed effects	X
Year fixed effects	X
Observations	15,669
R^2	0.802
Adjusted R^2	0.753
Note:	$^{*}p < 0.05$

E Other Outcomes: Total Registration/Turnout, Partisan Votes

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

Table 18 presents difference-in-differences estimates calculated from the Leip data for 2008-2020, while Table 19 presents estimates from the Catalist dataset. The estimates vary slightly in size and are not statistically distinguishable from zero, but they generally point to increases in overall registration and turnout in previously-covered places after *Shelby*, consistent with our main findings and also with those of Raze (2021).

Table 18: Leip Difference-in-Differences Results for Preclearance After Shelby

	Dependent variable:	
	Leip Total Registration	Leip Total Turnout
	(1)	(2)
Preclearance x Shelby	2,615.573	1,010.780
	(1,678.148)	(1,108.047)
County fixed effects	X	X
Year fixed effects	X	X
Observations	20,744	21,728
R^2	0.992	0.936
Adjusted R^2	0.990	0.925
Note:		$^{*}p < 0.05$

Table 19: Raw Registration and Vote Counts from Catalist data

	Dependent variable:	
	Total Registrations (count)	Total Votes Cast (count)
	(1)	(2)
Preclearance x Shelby	2,685.045	1,479.223
	(1,522.313)	(1,051.294)
County fixed effects	X	X
Year fixed effects	X	X
Observations	21,916	21,916
R^2	0.994	0.951
Adjusted R^2	0.993	0.943
Note:		$^{*}p < 0.05$

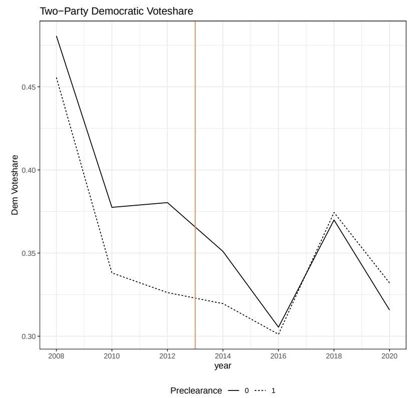


Figure 14: Time Trends in Democratic Voteshare

As part of considering whether the composition of the electorate shifts in some way that is not visible in our analysis of voter racial/ethnic demographics, we also look at partisan vote counts. Here, we use Leip data (again for 2000-2020) to use two-party Democratic voteshare as the outcome measure for an analogous difference-in-differences setup. Table 20 presents that analysis (again at the county level with standard errors clustered on state). There is no indication that Democratic voting declined in previously-covered places after the *Shelby* decision; if anything, the point estimate in the first column suggests a slight increase in Democratic voteshare in House races, though it is not statistically distinguishable from zero. The second column uses Democratic voteshare in gubernatorial races and finds small and non-significant negative shifts.[38]

F More on Legislator Identity

In the main paper, we consider whether downstream representational outcomes shift after *Shelby*, including the identity of House members. We also look at state legislative representation, though data is less available here. Figure 15 plots our data on Latino and Democratic representation in state assemblies. Here, we could not find consistent over-time measures of Black legislative identity, but we relied on lists produced by the National Association of Latino Elected Officials to identify Latino legislators and used data from the MIT Election and Data Science Lab and Carl Klarner to identify legislators' terms, districts, and partisanship. We then mapped legislative districts to counties as above, using Geocorr crosswalks. As seen in Figure 15, the pre-2013 trends in these measures (of having a Latino or a Democratic state assembly representing all or part of a county) do not track perfectly across covered and uncovered places, so we approach the

[38]Because state elections occur on various time frames, including all gubernatorial races would introduce strange compositional shifts in the panel from year to year. We focus here on the majority of states that hold their gubernatorial races in midterm years, so the data for this analysis includes 2010, 2014, and 2018.

Table 20: Two-Party Voteshare Difference-in-Differences Results for Preclearance After Shelby (using Leip elections data)

	Dependent variable:	
	2-Party D Voteshare, House Races	2-Party D Voteshare, Gubernatorial
	(1)	(2)
Preclearance x Shelby	0.019	−0.013
	(0.014)	(0.021)
County fixed effects	X	X
Year fixed effects	X	X
Observations	21,611	6,469
R^2	0.846	0.890
Adjusted R^2	0.820	0.834
Note:		$^*p < 0.05$

difference-in-differences analysis with some caution. However, Table 21 presents DiD estimates for these two outcomes. Again, we see no clear pattern of representational shifts: point estimates suggest slightly more Latino representation and slightly less Democratic representation, but neither estimate is distinguishable from zero.

Here we also present analogous figures/tables on House representation to those shown in the main table for Black and Democratic representation, this time focusing on whether a county is represented (all or in part) by a Latino member of Congress. Again, we do not see perfect comparability in covered/non-covered trends in the pre-period (in Figure 16, though they look broadly similar. Similar to the estimates presented in the main paper, Table 22 estimates a small decrease in Latino House representation in previously-covered places, but this estimate is not statistically distinguishable from zero.

G 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

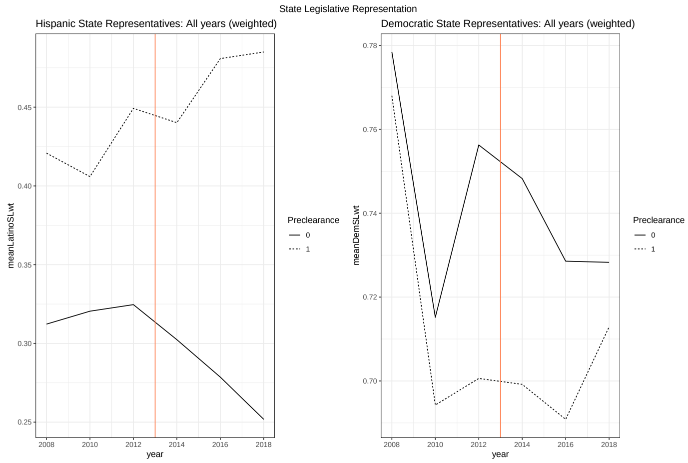


Figure 15: Time Trends in State Legislative Representation

Table 21: DiD Results for Preclearance After Shelby: State Legislative Representation

	Dependent variable:	
	Latino State Legislator	Dem State Legislator
	(1)	(2)
Preclearance x Shelby	0.085	−0.004
	(0.077)	(0.031)
County fixed effects	X	X
Year fixed effects	X	X
Observations	14,705	14,705
R^2	0.781	0.815
Adjusted R^2	0.732	0.775
Note:		*p<0.1; **p<0.05; ***p<0.01

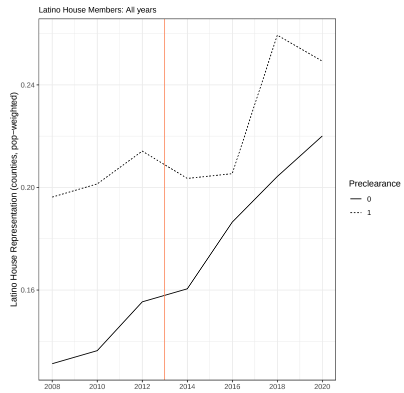


Figure 16: Time Trends in Congressional Representation

Table 22: DiD Results for Preclearance After Shelby: Congressional Representation

	Dependent variable:
	Latino Congressperson
Preclearance x Shelby	−0.026
	(0.021)
County fixed effects	X
Year fixed effects	X
Observations	21,651
R^2	0.861
Adjusted R^2	0.837
Note:	$^{*}p < 0.05$

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.[39] 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).

H Pre-registration

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

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

- The preregistration described our preferred approach as the simplest difference-in-difference specification with the outcome measure being group-specific registration and turnout rates. We have been convinced by journal reviewers that it makes sense substantively, given the history of the VRA, to center the analysis focused on racial gaps in registration and turnout rather than the simpler levels specification. But SI section C.2 continues to present those simpler group-specific estimates and they are discussed in the main paper.

- This paper focuses primarily on the main set of outcome measures described in the pre-registration document, those related to minority registration and voting. The pre-registration describes several additional outcomes that we hoped to collect about substantive or descriptive representation of minority voters. Data about the identity of legislators as well as the mapping of districts to counties over time is scarce, and the measures we have been able to construct do not map exactly to those described in the pre-registration. We have been unable to find systematic over-time data on Black state legislators, so our state legislative analysis focuses on Latino identity. However, we do examine both Black and Latino identity within Congress. Further, we realized on examination of the LCCR scores that they do not vary much within party, so we thought it more straightforward and equally informative to consider substantive representation via legislators' partisanship rather than their LCCR scores.

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

- The pre-registration did not discuss measurement error or whether the main Catalist analyses would be weighted or unweighted. As we discuss in the main paper, we think it makes sense to weight by group population size both because of the question we are interested in (we care about voters' experiences regardless of where they live, not about counties') and because places with very small minority populations are prone to measurement error. But in this SI (above), we present unweighted analyses and also estimates based on raw registration and turnout counts, not rates; we believe both these approaches indicate that our decision to weight the main analyses by group size does not drive the conclusions of the paper.
- Similarly, we described a robustness test that would use various ACS population estimate windows (1-year versus 5-year) to make sure that time lag in the population estimates was not driving the observed patterns. We do not present that test here because we think it is clearer and more apt to simply present the raw-counts analyses that fully drop the ACS data rather than using different variations of it.

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

[redacted]

January 2019

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

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

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

If we do not find that any of our outcome variables have been substantially affected by the *Shelby* decision, we will conclude that some of the concerns about immediate effects of the decision have not been borne out. This could be due to concerted effort by advocates to prevent electoral changes through other legal means, or to activists who organized to mobilize minority voters in the wake of the decision, or it could be because our time frame is too short to see longer-run effects that may materialize later. A null result here will not necessarily mean that pre-clearance was unimportant or that Justice Roberts was right that it was no longer needed, but it will rule out short-run changes in voting and representation. More data will be available when time has passed and more elections have taken place. Given the importance of this question in light of American histories of vote suppression and political exclusion, we nonetheless believe it is worth using the available data to make an early analysis of the effects of the *Shelby* decision.

Data

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

1 Registration and Turnout

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

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

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

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

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

2 Representation

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

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

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

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

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

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

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

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

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

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

Design

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

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

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

The actual specification for the difference-in-differences setup will depend on how plausible we find the parallel trends assumption for the simplest possible specification. Once we receive/collect the county-level outcome data described above, we will examine pre-treatment trends to see whether, for example, Black voter registration in covered and non-covered counties (places that were and were not affected by the Shelby decision) followed similar trends prior to 2013. If they do appear to follow parallel trends (and placebo tests find no "effect" for covered places in periods before the Shelby decision happened), then we will use the simplest possible difference in differences specification. We will simply predict each outcome measure (such as Black turnout) using a dummy variable for whether the county was covered by preclearance, another dummy for whether the observation was taken after the Shelby decision, and the interaction between the two variables (this is what we are interested in).

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

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

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

3 Additional Tests

We will run several robustness checks, including:

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

4 Extensions

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

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

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