Quarterly Journal of Political Science, 2025, 20: 269–305

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

*Authors are listed in alphabetical order and contributed equally. For helpful comments on this project, we thank Connor Huff, Christopher Lucas, Max Palmer, Jon Rogowski, and the members of the MIT Junior Faculty Research Lunch group and UR brown-bag series, as well as seminar participants at Columbia, Cornell, Stanford, the Harris School, UCLA, University of California Merced, University of Wisconsin Madison, NYU, and Yale. We thank Diana Camilla Valerio, Laurel Bliss, Wenyan Deng, Alejandro Frydman, Caitlin Fukumoto, Benjamin Muñoz Rojas, Rorisang Lekalake, Athena Sanchez, Ning Soong, Anna Weissmann, Brian Williams, and Chloe Wittenberg for excellent research assistance. Ariel White gratefully acknowledges the support of the Russell Sage Foundation's visiting scholars program.

Online Appendix available from:
http://dx.doi.org/10.1561/100.00023045_app
Supplementary Material available from:
http://dx.doi.org/10.1561/100.00023045_supp
MS received 11 April 2023; revised 28 March 2024; accepted 05 April 2024
ISSN 1554-0626; DOI 10.1561/100.00023045

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.

Keywords: Voting rights; turnout gap; Voting Rights Act; *Shelby v. Holder*

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 United States 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

[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.

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 3-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 systematically 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 to 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 (Burden *et al.*, 2014; Hopkins *et al.*, 2017; 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 (Barreto *et al.*, 2009; Cantoni and Pons, 2019; Grimmer and Yoder, 2019; White *et al.*, 2015; 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.

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

The VRA 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 pre-emptively 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 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* (557 U.S. 193 (2009)). 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

[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).

[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.

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 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 to 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

[4]See "About Section 5 of the VRA." 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

[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)).

to circumvent the VRA and dilute Black votes (Aghion *et al.*, 2008; Davidson and Grofman, 1994; Rosenberg, 1991). 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 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 (i.e., 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 and Grange, 2023; Morris *et al.*, 2021). The raw white-Latino turnout gaps, too, appeared higher in 2020 than they had been in 2012 for covered states (Morris *et al.*, 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.

[7]Statement released by Attorney General's office, found at https://perma.cc/SL53-AFSG.

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.

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, five 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

[8]The Civil Rights Division of the DOJ makes this data available at https://www.justice.gov/crt/section-5-changes-type-and-year.

[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; Komisarchik, 2023; Parker, 1990; Rosenberg, 1991).

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 did not 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 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 (Feder and Miller, 2020; Morris and Peréz, 2018); 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; Huber *et al.*, 2021; Shaw *et al.*, 2015). 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 (Tokaji, 2005; United States Commission on Civil Rights, 2001).

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 resources, such as funding for voting machines or training for 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 (Alvarez *et al.*, 2013; Atkeson *et al.*, 2010, 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 pre-clearance (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 *et al.*, 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

[10]See, for instance, the DOJ's determination on Alabama's State Act 81-226 in 1981, available at https://www.justice.gov/crt/section-5-objection-letters.

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 *et al.*, 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 voting age citizens were registered in the spring before the VRA was signed into law (Davidson and Grofman, 1994; Grofman *et al.*, 1992). Removing these sweeping barriers had large, nearly immediate effects on Black political participation (Ang, 2019; Fresh, 2018). Black registration rates rose by nearly 70%, on average, within 3 years of the VRA's passage (Cascio and Washington, 2014).

The 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 because they are disproportionately represented among people who lack the forms of identification required to register and vote (Barreto *et al.*, 2009, 2019; Bentele and O'Brien, 2013; Fraga and Miller, 2022; Henninger *et al.*, 2021; Rocha and Matsubayashi, 2014), and are asked to provide identification at higher rates than non-minority voters (Atkeson *et al.*, 2010; Cobb *et al.*, 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 *et al.*, 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 (Barreto *et al.*, 2009; Fraga and Miller, 2022; Highton, 2017; Stewart, 2013). 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. 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 *et al.*, 2007; Cantoni and Pons, 2019; Grimmer *et al.*, 2018; Hoekstra and Koppa, 2019; Hood III and Bullock III, 2012).

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 *et al.* (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.

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 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

[11] It is not the case that no voter ID law could be approved under a preclearance 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.

[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.

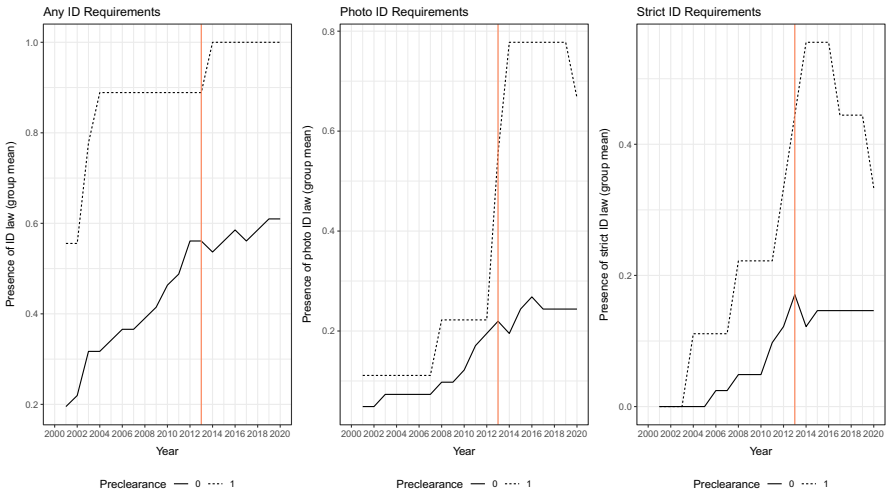

Figure 1: Time trends in types of voter ID laws as recorded by the NCSL. In all panels, the dotted line represents mean values in preclearance or formerly preclearance states, while the solid line represents non-covered states.

whether the state 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 does not 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 by 2013).

However, the *content* of state laws changed dramatically after the decision. The central panel of Figure 1 demonstrates that both groups of states followed similar trends in the implementation of photo ID laws prior to *Shelby*, but that

[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.

Table 1: NCSL difference-in-differences estimates for preclearance after *Shelby*.

	Dependent variable:		
	Any ID Law (1)	Photo ID Law (2)	Strict ID Law (3)
Preclearance × Shelby	−0.038	0.443*	0.249
	(0.098)	(0.151)	(0.130)
State fixed effects	X	X	X
Year fixed effects	X	X	X
Observations	900	900	900
R^2	0.784	0.700	0.574
Adjusted R^2	0.766	0.676	0.540

Note: $^*p < 0.05$.

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: 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.

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 *et al.*, 2010; White *et al.*, 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 *et al.* (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 preclearance even before 2013.[14]

For measures of local election administration, we use the EAVS, conducted during election years by the U.S. Election Assistance Commission (EAC). Since 2004, the EAC has sent surveys to election officials across the country, asking questions about their election practices and about registration and voting in their jurisdictions. We reviewed the survey for any questions that might indicate changes in local election administration that could potentially make it easier or more difficult for minority voters to participate. Section 2 of the Supplementary Materials 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

[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.

[15]Section A of the Supplementary Materials 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.

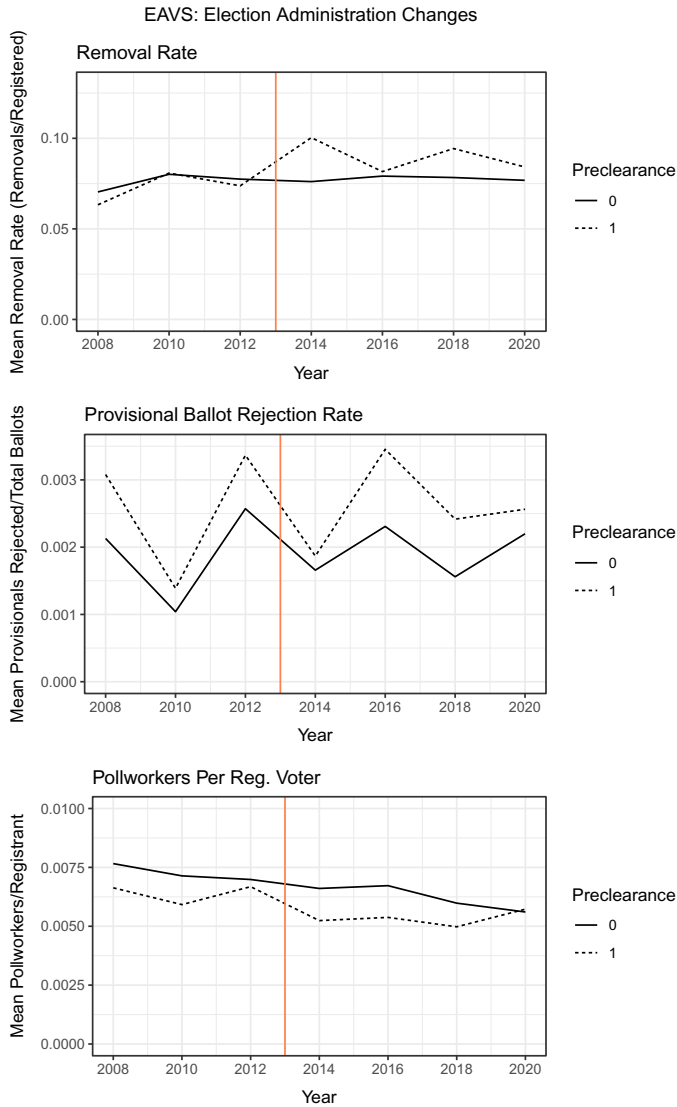

Figure 2: Time trends in election administration as reported in EAVS survey of jurisdictions. In all panels, the dotted line represents mean values in preclearance or formerly preclearance counties, while the solid line represents non-covered places.

Table 2: EAVS difference-in-differences estimates for preclearance after *Shelby*.

	Dependent variable:		
	Registration purge rate (1)	Provisional reject rate (2)	Pollworkers/ Reg. voter (3)
Preclearance × *Shelby*	0.023	0.0001	0.0005*
	(0.013)	(0.001)	(0.0002)
County fixed effects	X	X	X
Year fixed effects	X	X	X
Observations	18,192	18,033	17,209
R^2	0.449	0.335	0.811
Adjusted R^2	0.342	0.199	0.771

Note: $^*p < 0.05$.

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.

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

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

[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.

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 1 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 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 we note in the section "Preclearance: Functioning and Possible Effects of Removal" 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 DOJ 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. These analyses

provide evidence that at least some election practices changed after *Shelby*. 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.

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 a federal preclearance regime, 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 precise estimation of 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

[19]In Section C.1 in the Supplementary Materials, 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 from each racial group in each year from 2008 to 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 12-year period spanned by our data, we want to calculate the *share* of eligible voters who registered or voted in an area. To do this, we divide Catalist's counts by Census Bureau estimates of the citizen voting-age population (CVAP) for each corresponding racial category in each county.[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 to 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

[21] We rely on the 2009 American Community Survey CVAP estimates to estimate 2008 CVAP because the 5-year estimates we use only became available in 2009.

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

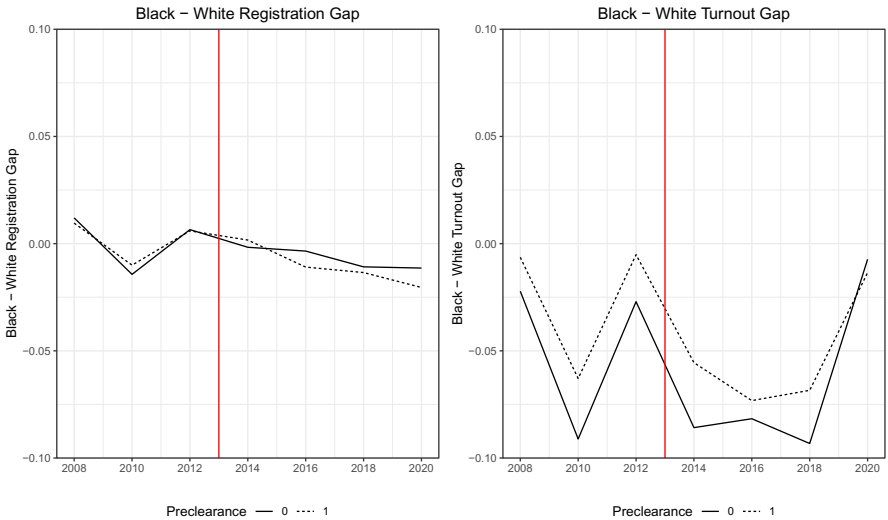

Figure 3: Time trends in Black-white registration (left) and turnout (right) gaps. Dotted lines represent weighted means for formerly preclearance counties and solid lines represent weighted means for non-covered counties. Means are weighted by county-level Black CVAP. Negative values denote higher white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

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 Supplementary Materials, we discuss a variety of alternative specifications. We implement this difference-in-differences approach by estimating the specification:

$$Y_{ct} = \alpha + \delta_t + \delta_c + \tau \text{Preclearance}_c \cdot \text{Shelby}_t + \beta' \mathbf{X}_{ct} + \epsilon_{ct}$$

Here, Y_{ct} represents the relevant registration or turnout gap (Black-white or Hispanic-white). "Preclearance" is an indicator variable for whether county c 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, the lower order term for being in a post-*Shelby* period

is collinear with time fixed effects and is 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 *et al.*, 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 Supplementary Materials and yield similar conclusions.

The specification above is equivalent to a canonical two-group difference-in-differences estimator. Our dataset consists of between 3,089 and 3,142 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 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 *et al.*, 2004). While a wealth of recent literature has addressed difference-in-differences assumptions and estimation strategies when researchers do encounter variation in treatment timing (Callaway and Sant'Anna, 2020; Goodman-Bacon, 2018), these corrections target cases more complex than the two-group case we present in this paper.

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 5 report our preferred specification, the simplest difference-in-differences setup described earlier in this section. Columns 2 and 6 expand this specification

[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.

Table 3: Estimated effects of removing preclearance on participation gaps (diff-in-diff).

	Dependent variable:							
	Black-white registration gap				Black-white turnout gap			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Preclearance × *Shelby*	−0.004	−0.003	0.027*	0.025*	−0.007	−0.002	0.015*	0.030*
	(0.010)	(0.009)	(0.005)	(0.007)	(0.008)	(0.006)	(0.007)	(0.006)
County fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Year fixed effects	✓	✓			✓	✓		
County demographic controls		✓		✓		✓		✓
State × year fixed effects			✓	✓			✓	✓
Observations	15,759	15,757	15,759	15,757	15,759	15,757	15,759	15,757
Adjusted R^2	0.837	0.842	0.857	0.862	0.785	0.797	0.848	0.856

Note: *$p < 0.05$.
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.

to include time-varying county-level controls derived from Census/ACS data. Columns 3 and 7 include state × year fixed effects in place of county and year fixed effects (included additively in our original specification). Columns 4 and 8 use these same state × year specifications and add time-varying county-level controls. In these final two specifications for registration and turnout, 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 substantial increases in participation gaps after *Shelby*, consistent with what one sees in a visual inspection of Figure 3. The only statistically significant shifts we see in any specification suggest that participation gaps may have *decreased* after *Shelby* when comparing counties within partially covered states; see footnote 24 for a discussion of the limitations of these estimates. It does not appear that previously covered places diverged from

[24]California, Florida, Michigan, North Carolina, New Hampshire, New York, South Dakota, and Virginia. We include the state × 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.

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, suggesting that the turnout gap between white and Black voters increased relative to what we would have expected in the absence of the *Shelby* decision's removal of preclearance. This shift in magnitude, though small, could be significant in very 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.

We begin by considering patterns through time: do we see (even small/non-significant) 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

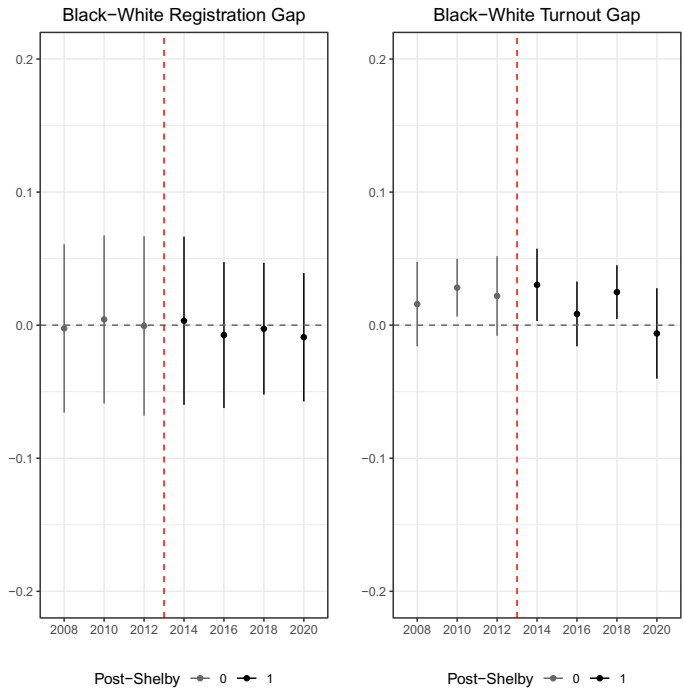

Figure 4: Over-time differences between preclearance and non-preclearance counties in Black-white registration and turnout gaps. Each year represents a separate OLS regression of the relevant registration or turnout gap on preclearance status.

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

[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).

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 Supplementary Materials, 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 participation 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 the Supplementary Materials, 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*).

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 Supplementary Materials 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 Supplementary Materials (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*.

One recent working paper reports slightly different conclusions from the ones reached in this paper and the ones mentioned above. Morris and Miller (2024) reports that *Shelby* significantly widened Black-white participation gaps in the years after the decision. Morris and Miller (2024) uses slightly different datasets and analytical choices than those reported here. The most impactful of these differences appears to be the choice not to weight by county population as we do in our main specifications. Indeed, when Morris and Miller (2024) reports a specification quite similar to ours, it shows point estimates similar to the ones we report in Table 3, despite differences in the years of data included and the sources of those datasets (Morris and Miller, 2024). As noted above, we believe population weighting is the appropriate choice for this research question. Both our analysis and that in Morris and Miller (2024) calculate turnout gaps at the county level, but counties vary a great deal in their population size and composition and are rarely a politically relevant unit for aggregating votes.

Treating all counties equally implicitly means treating individual voters unequally, as it paints a picture of Black voting experiences that disproportionately upweights the experiences of individual Black voters in small-population areas and downweights those in higher-population ones. And as a matter of estimation, we note that counties with small populations from any particular group will be especially prone to measurement error in voter race. Since approaches like Bayesian Improved Surname Geocoding infer voter race from a combination of voter names and the demographics of the surrounding area, it is especially challenging to correctly identify Black or Latino voters in places where few of those voters live, places that will disproportionately drive an unweighted county-level analysis. We discuss differences between these papers in more detail in the Supplementary Materials Section H.

Finally, we note that our analysis estimates the net effect of the *Shelby* decision on participation gaps and group participation; we 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 the Supplementary Materials 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.

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

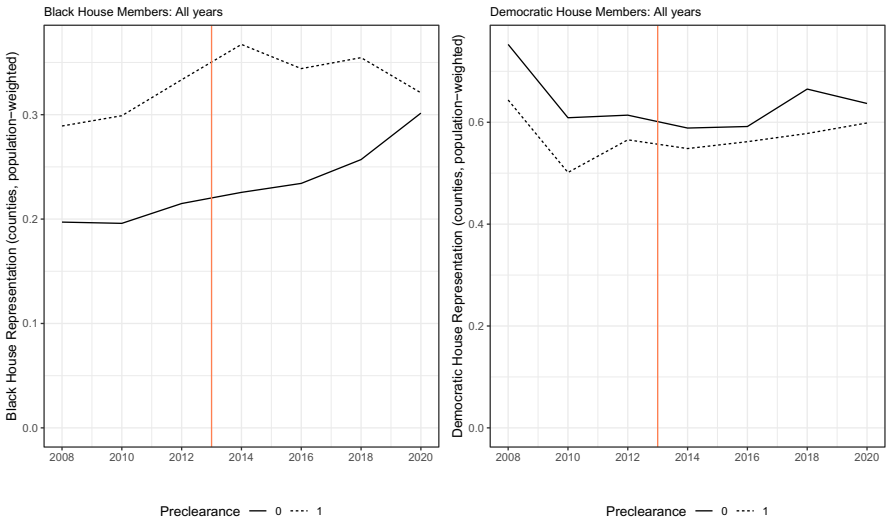

Figure 5: Time trends in house representation in covered and non-covered jurisdictions.

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 to 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

[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 Supplementary Materials presents analogous figures for Latino representation as well as state legislative representation and similarly finds no clear patterns of change.

Table 4: Estimated effects of removing preclearance on congressional representation (diff-in-diff).

	Dependent variable:	
	Black Congressperson (1)	Dem Congressperson (2)
Preclearance × *Shelby*	−0.010	0.039
	(0.049)	(0.030)
County fixed effects	X	X
Year fixed effects	X	X
Observations	21,842	21,842
R^2	0.845	0.802
Adjusted R^2	0.819	0.769

Note: $^*p < 0.05$.

representation look broadly similar for previously covered places and the rest of the country.

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.

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; Stephanopoulos *et al.*, 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 (Barreto *et al.*, 2009; Cantoni and Pons, 2019; Grimmer and Yoder, 2019; White *et al.*, 2015; 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 preclearance ("symbolic" effects of removing monitoring). We note the limitations of our ability to observe local decisionmaking about how to implement existing election laws, though the "Election Changes" section 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,

[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). Further, results presented by Morris and Miller (2024) including 2022 paint a more dire picture of the turnout effects of *Shelby*.

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

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