# Voting Rights and the Resilience of Black Turnout [*]

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### Abstract

The Voting Rights Act of 1965 increased turnout among Black voters, which then generated economic benefits for Black communities. In *Shelby County v. Holder* (2013), the Supreme Court invalidated the enforcement mechanism responsible for these improvements, prompting concerns that states with histories of discriminatory election practices would respond by suppressing Black turnout. I estimate the effect of the *Shelby* decision on the racial composition of the electorate using triple-difference comparisons of validated turnout data from the Cooperative Congressional Election Study. The data suggest that the *Shelby* decision did not widen the Black-white turnout gap in states subject to the ruling.

**Keywords**: voting rights, racial turnout gap, voter suppression

**JEL Classification**: D72, J15, K16

# 1  Introduction

For its profound impact on the political participation and representation of Black Americans,
the Voting Rights Act of 1965 is often described as "the most successful piece of civil rights
legislation ever adopted" by Congress.[1] By eliminating poll taxes, literacy tests, and other
discriminatory election policies, the Voting Rights Act produced lasting increases in Black
turnout (Ang 2019; Filer et al. 1991; Fresh 2018), which then increased the number of Black local
elected officials and elevated support for subsequent civil rights legislation (Bernini et al. 2018;
Schuit and Rogowski 2017). Ultimately, the expansion of voting rights generated significant
economic benefits for Black communities (Cascio and Washington 2014; Avenancio-León and
Aneja 2019; Facchini et al. 2020).

Most of the political and economic consequences of the Voting Rights Act are attributable
to an enforcement mechanism known as preclearance. Preclearance required state and local
jurisdictions identified by a coverage formula to secure federal approval for any election-related
policy change.[2] The formula identified "covered" jurisdictions as those with histories of literacy
tests or significant racial or linguistic disparities in voting. To secure federal approval, a covered
jurisdiction had to demonstrate that the policy change would not restrict voting rights based
on race or membership in a language minority group. If federal authorities withheld approval,
then the jurisdiction could not legally implement the proposed change. Between 1965 and 2013,
federal authorities received 556,268 proposals (over 11,000 per year) for election-related policy
changes in covered jurisdictions.[3] At issue, then, is that the coverage formula was invalidated
by the Supreme Court of the United States, in *Shelby County v. Holder* (2013). In this way, what
is known as "the *Shelby* decision" ended 48 years of direct preclearance oversight. Previously
covered jurisdictions can now adopt new election policies *without* federal approval.

The removal of preclearance could enable officials in previously covered jurisdictions
to suppress voting for political advantage. While white voters in covered jurisdictions lean
Republican, Black voters exhibit strong and enduring support for Democrats (Kuriwaki 2020).

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[1] US Department of Justice. 2015. "Introduction to Federal Voting Rights Laws." August 6. `https://www.justice.gov/crt/introduction-federal-voting-rights-laws-1`. Accessed on December 29, 2020.

[2] *Allen v. State Board of Elections*, 393 U.S. 544 (1969).

[3] US Department of Justice. 2015. "Section 5 Changes by Type and Year." August 6. `https://www.justice.gov/crt/section-5-changes-type-and-year-2`. Accessed on December 29, 2020.

Any successful attempt at suppressing the Black vote would likely benefit Republican candidates. Political conditions at the time of the ruling were favorable to selectively manipulating the costs of voting—Republicans held the governorship and both houses of the legislature in 12 out of 15 states with covered jurisdictions.[4]

The *Shelby* decision appears to have prompted previously covered jurisdictions to adopt policies that increase the cost of voting, perhaps differentially for prospective Black voters. Hours after the ruling, the Attorney General of Texas announced that "the State's voter ID law will take effect immediately".[5] State election officials in Alabama and Mississippi implemented strict voter identification laws that had either failed to secure federal approval before *Shelby* or were under review at the time of the ruling.[6] Other covered states, such as North Carolina, also enacted voter identification laws with additional restrictions on early voting and registration (US Commission on Civil Rights 2018). A federal court later overtuned North Carolina's election policy changes on the grounds that they "target African Americans with almost surgical precision."[7]

Not all election policy changes in covered jurisdictions were blocked by the courts after *Shelby*. Covered states were more likely than uncovered states to purge voters of color from voter rolls (Brater et al. 2018; Feder and Miller 2020) and covered counties were more likely than uncovered counties to close polling places, though not in North Carolina (Shepherd et al. 2021).[8] The proliferation of election-related policy changes in covered jurisdictions suggests that preclearance oversight was binding.

Given the response of covered jurisdictions after the ruling, fears that *Shelby* could attenuate or even reverse the effects of the Voting Rights Act are well-founded. However, the worst-case scenario presupposes a change in the composition of the electorate, for which there

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[4] Ballotpedia. "State government trifectas, pre-2014." `https://ballotpedia.org/State_government_trifectas#State_government_trifectas.2C_pre-2014`. Accessed on November 25, 2020.

[5] Brennan Center for Justice. 2018. "The Effects of *Shelby County v. Holder*." August 6. `https://www.brennancenter.org/our-work/policy-solutions/effects-shelby-county-v-holder`. Accessed on February 4, 2021.

[6] Brennan Center for Justice. 2019. "New Voting Restrictions in America." November 19. `https://www.brennancenter.org/new-voting-restrictions-america`. Accessed on November 25, 2020.

[7] *North Carolina State Conference of the NAACP v. McCrory*, 831 F.3d 204 (2016).

[8] Arthur, Rob, and Allison McCann. 2018. "How the Gutting of the Voting Rights Act Led to Hundreds of Closed Polls." *Vice News*, October 16. `https://news.vice.com/en_us/article/kz58qx/how-the-gutting-of-the-voting-rights-act-led-to-closed-polls`.

is little existing evidence. I bring new evidence to bear on the impact of the *Shelby* decision by considering whether the removal of preclearance decreased turnout among eligible Black voters relative to white voters in previously covered states.

The impact of preclearance removal may depend on the relative importance of (i) increases in the cost of voting and (ii) counter-mobilization against new voting restrictions. Although increases in the cost of voting exert downward pressure on turnout, counter-mobilization can stimulate turnout (Valentino and Neuner 2017; Biggers and Smith 2020), making the net effect of policy changes made possible by the *Shelby* decision theoretically ambiguous. Understanding the impact of the *Shelby* decision therefore requires empirical evaluation.

I conduct such an evaluation using validated turnout and registration data from six biennial waves of the Cooperative Congressional Election Study (2008-2018) and state-level variation in preclearance coverage. To identify the impact of preclearance removal, I employ a triple-difference design that uses states that were unaffected by *Shelby* as a control group for covered states. Building upon difference-in-differences variation in preclearance oversight, I incorporate a third difference by comparing the turnout of Black and white voters within each state. This allows me to flexibly control for differential trends in aggregate turnout and registration rates that could otherwise confound estimates of the effect of *Shelby* on the relative turnout of Black voters. The resulting triple-difference comparison considers, in effect, how changes in Black-white voter participation gaps differ across covered and uncovered states after *Shelby*.

I find that the removal of preclearance requirements did not significantly reduce the relative turnout or registration of eligible Black voters. If anything, Black turnout may have *increased* relative to white turnout in covered jurisdictions after *Shelby*. I document that *Shelby* increased relative turnout in covered states during the 2016 election, with the largest effects concentrated among covered states where all levels of government were previously subject to preclearance. Midterm elections exhibit smaller effects that are statistically indistinguishable from zero, but point estimates remain positive. Across several increasingly flexibile specifications, I find little evidence to substantiate the claim that *Shelby* differentially restricted Black voter participation in covered states. The non-negative association of *Shelby* with relative

turnout and registration is consistent with counter-mobilization efforts outweighing increases in the relative cost of voting, on average.

I proceed in Section 2 by reviewing the extant literature on the Voting Rights Act, the *Shelby* decision, and the mechanisms through which *Shelby* could facilitate changes in relative turnout. I then turn to the data and research design in Section 3 before presenting results in Section 4. Finally, in Section 5, I discuss the implications and limitations of my findings.

# 2   Background

## 2.1   The Voting Rights Act

Before the Voting Rights Act was passed in 1965, many states required prospective Black voters to pass literacy tests and pay poll taxes before voting.[9] States with these barriers exhibited vast racial disparities in voter registration and turnout (US Commission on Civil Rights 2018). The Voting Rights Act and its subsequent revisions ameliorated racial disparities in voter participation by banning the use of literacy tests, providing the legal framework to challenge poll taxes and other discriminatory policies through litigation, and requiring federal approval of new state and local election policies in jurisdictions with histories of discrimination.

Before *Shelby*, litigation and preclearance were the primary enforcement mechanisms of the Voting Rights Act. Litigation targeted previously enacted election policies, and overturning a policy through this channel required plaintiffs to demonstrate that the policy was intended to discriminate or that it exhibited discriminatory effects (Ho 2017). In contrast, preclearance targeted policies before implementation, requiring covered jurisdictions to demonstrate to federal authorities (namely, the US Attorney General or the US District Court for the District of Columbia) that each proposed policy change was free of both discriminatory intent and discriminatory effects. A crucial distinction for the research design I outline in Section 3 is that preclearance enforcement was limited to covered states before *Shelby*, whereas the right to litigate applied, and continues to apply, nationally.

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[9] Literacy tests and poll taxes disenfranchised a broad class of otherwise eligible voters, but grandfather clauses re-enfranchised those whose ancestors were eligible to vote before the passage of the 15th Amendment, which guaranteed universal male suffrage. Few, if any, citizens of color had voting rights before the 15th Amendment was ratified, so grandfather clauses almost exclusively exempted white citizens from literacy tests and poll taxes.

The extant literature on the Voting Rights Act leverages the coverage formula to identify causal effects of preclearance oversight on voter registration, turnout, and political representation. Using difference-in-differences variation in preclearance coverage within North Carolina, Fresh (2018) shows that preclearance generated large increases in voter registration and turnout rates in covered counties after the Voting Rights Act was enacted. Similarly, Ang (2019) leverages the expansion of preclearance coverage in the 1975 renewal of the Voting Rights Act and finds that preclearance produced lasting increases in the turnout rates of newly covered states and counties. Supplemental results from survey data suggest that the increases in overall turnout were driven by differential increases in voting among citizens of color. To document the effects of the Voting Rights Act on representation in local government, Bernini et al. (2018) exploit triple-difference variation in preclearance exposure within states of the former Confederacy. They show that preclearance increased the number of Black local elected officials in relatively Black counties. Using data on congressional roll-call votes and difference-in-differences variation in preclearance coverage, Schuit and Rogowski (2017) demonstrate that members of Congress who represented covered jurisdictions were more likely to support civil rights legislation after 1965 than those who represented uncovered jurisdictions. Taken together, the evidence suggests that the Voting Rights Acts—and preclearance in particular—increased the political participation and representation of Black Americans.

Models of distributive politics predict that expansions of specific constituencies, such as those associated with the Voting Rights Act, can trigger changes in the distribution of public resources (Cox and Mccubbins 1986; Lindbeck and Weibull 1987). Specifically, if politicians elicit support by redistributing public resources, then an increase in the share of voters from a group with distinct preferences should induce an increase in public resources directed toward that group. For example, the re-enfranchisement of relatively poor, Black voters in the South appears to have increased government spending on redistributive social assistance programs (Husted and Kenny 1997).

The quasi-experimental literature on the downstream consequences of the Voting Rights Act reinforces the importance of distributive politics in shaping economic outcomes. Using

a triple-difference design that leverages within-state variation in racial composition and the use of literacy tests, Cascio and Washington (2014) show that the abolition of literacy tests in the South increased state transfers to predominately Black school districts. Using a similar research design, Facchini et al. (2020) find that preclearance decreased the rate at which Black individuals were arrested for non-felony offenses in covered counties with elected sheriffs and a high concentration of Black voters. To identify the effects of enfranchisement on labor market disparities, Avenancio-León and Aneja (2019) employ a triple-difference design that compares racial differences in labor market outcomes within covered counties to those within neighboring uncovered counties. They find that preclearance increased the relative wages of Black workers through additional government employment opportunities and enhanced anti-discrimination protections. Through re-enfranchisement and increased representation, the Voting Right Act generated substantial economic benefits for Black Americans.

Other episodes of mass enfranchisement also demonstrate how reshaping the electorate induces policy changes that affect economic outcomes. For example, the enfranchisement of women in the United States during the early twentieth century prompted significant increases in public health spending which then decreased child mortality (Miller 2008). Similarly, the *de facto* enfranchisement of less-educated Brazilians expanded access to public health care services which then increased utilization among uneducated mothers and improved infant health (Fujiwara 2015). Mass disenfranchisement exhibits comparable effects—most pertinently, the imposition of poll taxes and literacy tests on Black voters in the Southern United States after Reconstruction reduced the allocation of public goods in Black communities (Naidu 2012). A worry, then, is that the policy changes made possible by the *Shelby* decision could disenfranchise Black voters and ultimately reverse the economic gains brought about by the Voting Rights Act.

## 2.2  *Shelby County v. Holder*

On June 25, 2013, the Supreme Court ruled in a 5-4 decision that the coverage formula governing preclearance is unconstitutional. In an oft-cited passage of his majority opinion, Chief Justice Roberts argued that the coverage formula is "based on 40-year-old facts having no logical

relation to the present day," and thus violates equal sovereignty of the states. Moreover, he argued that preclearance was "intended to be temporary," but Congress repeatedly renewed these sections without major revisions, most recently in 2006.[10] While the court did not rule on the constitutionality of preclearance itself, the invalidation of the coverage formula rendered preclearance unenforceable. Congress has since failed to enact new coverage formula that would comply with the *Shelby* decision and restore preclearance oversight.

After the ruling, previously covered states enacted a variety of election reforms.[11] Beyond new voter identification laws and other legislation, the *Shelby* decision also triggered an increase in voter list maintenance activity—which can remove (or "purge") otherwise eligible voters from registration lists—and may have prompted the closure of polling places. Using a difference-in-differences design, Feder and Miller (2020) estimate that voter purge rates increased in covered counties after *Shelby*, corroborating the findings of a Brennan Center report that documented a differential increase in purge rates in covered states (Brater et al. 2018).[12] An analysis of polling place data by *Vice News* documents that polling place closures were more common in covered counties than in uncovered counties after *Shelby*.[13] In contrast, Shepherd et al. (2021) find no systematic change in polling place locations within North Carolina, a state with covered and uncovered counties. Although the evidence on difficult-to-observe local policy responses remains sparse, the evidence on voter purges demonstrates that election administration in covered states responded systematically to the *Shelby* decision.

The existing literature has yet to establish whether the *Shelby* decision suppressed turnout among citizens of color. Ang (2019) provides a preliminary difference-in-differences analysis of the impact of *Shelby* on turnout. An event study of county election returns suggests that the difference in turnout between covered and uncovered counties decreased in 2016 relative to the difference in 2012, and event studies of self-reported turnout from the November Current

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[10] *Shelby County v. Holder*, 570 U.S. 529 (2013).

[11] Fuller, Jaime. 2014. "How has voting changed since *Shelby County v. Holder*?" *Washington Post*, July 7. https://www.washingtonpost.com/news/the-fix/wp/2014/07/07/how-has-voting-changed-since-shelby-county-v-holder/?utm%7B_%7Dterm=.0e6ef3dabd32.

[12] Neither Feder and Miller (2020) nor Brater et al. (2018) use data that facilitate tests for race-specific heterogeneity, so it remains unclear whether Black voters were more likely to purged from the rolls.

[13] Arthur, Rob, and Allison McCann. 2018. "How the Gutting of the Voting Rights Act Led to Hundreds of Closed Polls." *Vice News*, October 16. https://news.vice.com/en_us/article/kz58qx/how-the-gutting-of-the-voting-rights-act-led-to-closed-polls.

Population Survey (CPS) voter supplement suggest that this decrease was concentrated among citizens of color. However, the same event studies also show that turnout was differentially increasing in covered jurisdictions before 2012, illustrating the difficulty of isolating the causal impact of *Shelby*. Using regression discontinuity and difference-in-differences comparisons of counties within North Carolina, Gibson (2020) finds no evidence that *Shelby* reduced turnout rates in covered counties, either overall or among specific racial subgroups. The North Carolina state government was also subject to preclearance, though, so the data foreclose on the ability to measure the consequences of statewide policy changes that would affect uncovered counties.

Whereas Ang (2019) and Gibson (2020) measure the impact of *Shelby* on absolute turnout (i.e., the Black turnout rate or the white turnout rate), I measure the impact of *Shelby* on relative turnout (i.e., the Black-white turnout gap). Changes in absolute turnout can provide important signals about the health of democratic institutions (e.g., a reduction in the turnout of citizens of any race could indicate an erosion of voting rights), but changes in relative turnout are important, too, in that they can bring about changes in the distribution of public resources. A focus on relative turnout also facilitates a research design that is robust to differential trends in absolute turnout rates between covered and uncovered states before *Shelby*.

## 2.3   Conceptual framework

Many of the policy changes made possible by the *Shelby* decision may have made voting more costly. For example, voter purges may have forced some prospective voters to re-register, a process that imposes nontrivial administrative burdens (Braconnier et al. 2017). Similarly, the closure or relocation of polling places may have imposed significant information and travel costs on voters (Cantoni 2020; Clinton et al. 2020). Absent some other mechanism, any increase in the cost of voting would decrease the likelihood of voting for those at the margin, putting downward pressure on aggregate turnout (Downs 1957; Riker and Ordeshook 1968). Thus, if the *Shelby* decision ultimately led to differential increases in the cost of voting for Black citizens, then one would expect Black turnout to decrease relative to white turnout.

Such a narrow focus on the costs of voting neglects the potential for counter-mobilization efforts to offset or even outweigh any new inconveniences of voting. Experimental evi-

dence suggests that exposure to news portraying voter identification laws as discriminatory and disenfranchising generates anger that materializes in stronger voting intentions among Democrats, but not independents or Republicans (Valentino and Neuner 2017). Likewise, quasi-experimental evidence suggests that challenging an individual's right to vote increases the probability that they will vote in subsequent elections, consistent with the predictions of psychological reactance theory (Biggers and Smith 2020). Other quasi-experimental evidence suggests that political campaigns respond to strict voter identification laws by intensifying outreach toward voters of color (Cantoni and Pons 2021).[14] Whether instigated by campaigns or voters themselves, the perception of selective disenfranchisement can trigger a backlash that mobilizes voters. If *Shelby* induced policy changes that were differentially salient for Black voters, then one would expect counter-mobilization to exert upward pressure on Black turnout relative to white turnout.

Given the potential for the *Shelby* decision to put downward pressure on turnout through increasing the cost of voting and upward pressure on turnout through counter-mobilization, the net effect of *Shelby* is theoretically ambiguous.[15] The sign of the estimated effect of *Shelby* on relative turnout can therefore elucidate the relative importance of increases in the cost of voting and counter-mobilization. A decrease in the relative turnout of Black voters, for example, would suggest that the effects of differential increases in the cost of voting outweigh the effects of race-specific counter-mobilization, whereas an increase in relative turnout would suggest that the effects of race-specific counter-mobilization outweigh the effects of differential increases in the cost of voting. The results in Section 4 support the latter case in which the counter-mobilization response dominates.

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[14] Increased campaign activity might explain, in part, why strict voter identification laws tend to have minimal effects on turnout (Neiheisel and Horner 2019; Cantoni and Pons 2021).

[15] Viewed this way, it is possible for *Shelby* to increase Black turnout while decreasing white turnout—even if *Shelby* induced policy changes that differentially increased the cost of voting for Black citizens. For example, it remains illegal for election officials to discriminate explicitly on the basis of race, so policy changes that target Black voters would likely affect some white voters, too. In this case, a sufficiently strong Black counter-mobilization response paired with a sufficiently weak white one would have the seemingly paradoxical consequence of simultaneously increasing Black turnout while decreasing white turnout.

# 3  Data and research design

## 3.1  Preclearance coverage

Preclearance coverage provides a source of identifying variation for estimating the impact of *Shelby* on the composition of the electorate. Figure 1 illustrates preclearance coverage when the coverage formula was invalidated in 2013.[16]

Fully covered states were explicitly identified by the coverage formula. Before 2013, all state and local authorities responsible for running or overseeing elections in fully covered states were required to secure preclearance for any policy change related to elections.[17] With the exception of Alaska and Arizona, most fully covered states were located in the South.

Partially covered states were not explicitly identified by the coverage formula, but each had at least one local jurisdiction subject to preclearance. The presence of a covered jurisdiction in an otherwise uncovered state effectively extended coverage to the state government, as a statewide policy change would likely affect voting in the covered jurisdiction. Before 2013, state officials overseeing elections in partially covered states were required to obtain preclearance for any election policy change, but most local officials—namely those in uncovered jurisdictions—were not. For example, Michigan had two covered townships. A statewide voter identification law would require voters in those townships to provide identification as a precondition to voting, which would necessitate federal approval for the policy. In contrast, local officials in Wayne County, Michigan—an otherwise uncovered jurisdiction—could move polling places or change voting hours without federal approval.

Uncovered states were not identified by the coverage formula and had no covered local jurisdictions. Neither state nor local officials overseeing elections in these states were required to obtain preclearance for election policy changes before the *Shelby* decision. While the West, Midwest, and Northeast census regions contained most uncovered states, there were seven uncovered states in the South. In the analyses that follow, I use voters in uncovered states as a control group to estimate the effect of *Shelby* on the composition of voters in fully covered

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[16] US Department of Justice. 2020. "Jurisdictions Previously Covered by Section 5." September 11. `https://www.justice.gov/crt/jurisdictions-previously-covered-section-5`. Accessed on December 29, 2020.

[17] Some counties in Virginia—a fully covered state—had "bailed out" of coverage before *Shelby*, which exempted them from preclearance requirements, but most counties were subject to preclearance.

and partially covered states. Given the differences in the intensity of preclearance oversight, I estimate separate effects for fully covered and partially covered states.[18]

## 3.2   Turnout and registration data

Measuring changes in relative turnout and registration among Black and white voters requires data on turnout and registration by race, but official election returns do not typically publish race-specific turnout rates and most states do not release race-specific registration rates. For this reason, I use repeated cross sections from the Cooperative Congressional Election Study (CCES; Kuriwaki 2020), a large-sample survey of the voting-age population administered after presidential and midterm elections. Each weighted cross section provides a representative sample of eligible voters within each state.

Unlike the November CPS and other national election surveys, the CCES validates self-reported turnout and registration against state voter files to correct for the tendency of respondents to over-report their political participation; only those with a verified record of voting are counted as having voted and only those with a verified registration record are counted as being registered. As a result, implied turnout rates from validated CCES turnout data are closer to actual state turnout rates derived from official election returns. Relying on validated voter participation data also sidesteps the potential for differential overreporting that coincides with exposure to the *Shelby* decision. The extent of overreporting in the CPS, for example, varies over time and across states (McDonald 2021), and may also vary by race (Ansolabehere et al. 2021).[19]

CCES data are available for each presidential and midterm election since 2006, but I discard observations from the 2006 midterm election because the validation of self-reported

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[18] One might anticipate additional margins of heterogeneity among covered states. For example, the Voting Rights Act of 1965 brought full preclearance oversight to states with the worst records of racial discrimination in election administration (i.e., Alabama, Georgia, Louisiana, Mississippi, South Carolina, and Virginia) whereas the 1975 reauthorization extended coverage to states that lacked voting disparities severe enough for coverage in the original 1965 legislation (i.e., Alaska, Arizona, and Texas). The results in Section 4 do not change, however, when I drop respondents from the expansion states, and point estimates are nearly identical when I estimate separate effects for expansion states and the initial group of fully covered states.

[19] To the extent that overreporting varies by race within states over time, triple-difference comparisons of self-reported voter participation could confound the effects of the *Shelby* decision with coincident changes in overreporting. In spite of the potential for differential overreporting, I provide estimates using CPS self-reports in the Online Appendix. As with data from the CCES, data from the CPS do not support the claim that *Shelby* caused significant reductions in relative turnout or registration among Black voters in previously covered states.

turnout against voter files was inconsistent across states in 2006 (Grimmer et al. 2018). I also discard observations from Virginia before 2012, as validation was unavailable there in 2008 and 2010 (Ansolabehere 2010, 2012). The resulting sample consists of 35,322 non-Hispanic Black and 239,597 non-Hispanic white respondents from six federal elections (2008–2018).

I present absolute turnout rates, expressed as the percentage of adults who cast a ballot, for Black and white voters in Panels A and B of Figure 2. Black turnout was lower in covered states than in uncovered states before *Shelby* and the same is true of white turnout during presidential elections. While Black turnout rates decreased between the 2012 and 2016 presidential elections, this decrease was less pronounced in uncovered states than in covered states. Similarly, in midterm elections, the increase in Black turnout from the 2010 election to 2014 and 2018 was more pronounced in covered states than in uncovered states. Turnout rates also decreased among white voters between the 2012 and 2016 elections, though the decrease was somewhat less pronounced in uncovered states. In midterm elections, white turnout increased in uncovered states from 2010 to 2018, but remained constant in fully covered states. Difference-in-differences comparisons of absolute turnout rates in covered and uncovered states before and after *Shelby* would suggest that the *Shelby* decision is associated with an increase in Black turnout and a decrease in white turnout.

I turn to relative turnout, expressed as the difference between Black and white turnout rates, in Panel C of Figure 2. Negative turnout differentials throughout the sample period indicate that the white turnout rate exceeded the Black turnout rate during each election. Downward trends in turnout differentials indicate that the Black-white turnout gap increased between the 2008 and 2012 presidential elections. While turnout differentials did not meaningfully change in covered states between the 2012 and 2016 presidential elections, they continued to decrease in uncovered states. In other words, relative to the change in uncovered states, the Black-white turnout gap narrowed in covered states after *Shelby*. A relative narrowing of the turnout gap is less evident in midterm elections, though there is little indication that the gap widened in covered states after *Shelby*. Both patterns survive further scrutiny in Section 4.

Voter registration evidences a similar set of patterns. In Panel D, for instance, the decline in Black registration between 2012 and 2016 is smaller in covered states than in uncovered

states, and in Panel E the decline in white registration is larger in covered states than in uncovered states. As with absolute turnout, difference-in-differences comparisons of absolute registration rates would suggest (albeit noisily) that *Shelby* increased Black registration and decreased white registration. Accordingly, in Panel F, Black-white registration gaps narrow in covered states between 2012 and 2016, but widen in uncovered states. With the possible exception of partially covered states in 2018, there is little indication that registration gaps widened in covered states after *Shelby*. As with relative turnout, the non-negative association of *Shelby* with relative registration survives further scrutiny in Section 4.

## 3.3   Empirical strategy

Naïve before-and-after comparisons of turnout or registration in covered states may confound the effects of *Shelby* with other factors that affect voter participation across elections, such as the presence of Black candidates for state or federal office (Washington 2006), differences in *ex ante* expectations about the closeness of elections (Bursztyn et al. 2017), or changes in media consumption (Gentzkow 2006; DellaVigna and Kaplan 2007; Gerber et al. 2009). Similarly, cross-sectional comparisons of covered states with uncovered states may confound the effects of *Shelby* with preexisting differences in participation—turnout rates, for example, were lower in fully covered states than in uncovered states before *Shelby*. Even a difference-in-differences comparison could confound differential trends in turnout or registration between covered and uncovered states with the impact of *Shelby*.

To isolate the causal effect of *Shelby* on the relative turnout and registration of Black Americans, I employ a triple-difference design that leverages within-state variation in pre-clearance coverage. Specifically, I compare the Black-white turnout or registration differential within covered states to the differential within uncovered states, before and after the *Shelby* decision. I execute this comparison by estimating

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\begin{aligned}
\text{Participation}_{irst} = {} & \beta \, \text{Black}_{ir} \times \text{fully covered}_s \times \textit{Shelby}_t \\
& + \delta \, \text{Black}_{ir} \times \text{partially covered}_s \times \textit{Shelby}_t \\
& + \alpha_{rs} + \alpha_{rt} + \alpha_{st} + X'_{irst}\Gamma + \varepsilon_{irst} \, ,
\end{aligned}
\tag{1}
$$

where Participation$_{irst}$ is an indicator equal to one if respondent $i$ of race $r$ voted (or was registered to vote) in state $s$ during election $t$ or zero if the respondent did not vote (or was not registered to vote). Race-by-state fixed effects ($\alpha_{rs}$) absorb time-invariant factors that affect the participation of Black or white voters within each state. Race-by-year fixed effects ($\alpha_{rt}$) absorb election-specific characteristics that affect participation among all Black voters or all white voters across the country. State-by-year fixed effects ($\alpha_{st}$) absorb election-specific characteristics that affect participation among all voters within each state, such as the presence of a Senate race in a midterm election. Respondent controls ($X'_{irst}$) adjust for differences in participation by age and gender. Each treatment interaction consists of a race indicator, a coverage indicator, and a post-treatment indicator.[20] The race indicator (Black$_{ir}$) equals one if respondent $i$ identified as Black or zero if the respondent identified as white. Coverage indicators (fully covered$_s$ and partially covered$_s$) equal one if state $s$ was covered before *Shelby* or zero if the state was uncovered. The post-treatment indicator (*Shelby$_t$*) equals one if election $t$ was held after 2013 or zero if the election was held before 2013. The triple-difference parameters ($\beta$ and $\delta$) capture the effect of *Shelby* on the relative participation of eligible Black voters in fully covered and partially covered states after 2013. Following the conservative approach of Cameron and Miller (2015), I make inference using cluster-robust standard errors that account for clustering at the state level.

A causal interpretation of $\hat{\beta}$ and $\hat{\delta}$ rests on two identifying assumptions. The first asserts common trends—that is, conditional on the full set of fixed effects and respondent controls, the Black-white turnout or registration differential in covered states would have evolved similarly to the differential in uncovered states had the Supreme Court upheld the coverage formula. The second asserts that consequences of the *Shelby* decision did not spill over into uncovered states.

I gauge the plausibility of common trends by estimating an event study analog of

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[20] Fixed effects absorb all lower-order terms.

Equation 1:

$$\text{Participation}_{irst} = \sum_{\tau \neq 2012} \beta_\tau \, \text{Black}_{ir} \times \text{fully covered}_s \times \mathbb{1}(t = \tau)_\tau$$

$$+ \sum_{\tau \neq 2012} \delta_\tau \, \text{Black}_{ir} \times \text{partially covered}_s \times \mathbb{1}(t = \tau)_\tau \qquad (2)$$

$$+ \alpha_{rs} + \alpha_{rt} + \alpha_{st} + X'_{irst}\Gamma + \varepsilon_{irst} \, ,$$

where $\beta_\tau$ represents the difference in Black-white turnout differentials between fully covered states and uncovered states in year $\tau$ relative to the difference in 2012 and $\delta_\tau$ represents the difference between partially covered states and uncovered states. Estimates of $\beta_\tau$ or $\delta_\tau$ that deviate from zero for elections before *Shelby* would suggest that covered and uncovered states do not share common trends in participation differentials. If both identifying assumptions hold, then estimates of $\beta_\tau$ or $\delta_\tau$ for elections after *Shelby* help illustrate how the effects of the decision have evolved over time.

## 4 Results

### 4.1 Event study

Estimates of the event study coefficients from Equation 2 lend support to the plausibility of common trends between covered and uncovered states. In Panel A of Figure 3, for instance, the differences in Black-white turnout differentials between fully covered states and uncovered states during the 2008 and 2010 elections are indistinguishable from the difference in 2012 at conventional significance levels ($p = 0.477$ in 2008 and $p = 0.562$ in 2010). The same is true of the relative differences in Black-white turnout differentials between partially covered states and uncovered states ($p = 0.89$ in 2008 and $p = 0.671$ in 2010). Furthermore, all four pre-2012 coefficients are jointly indistinguishable from zero ($F = 0.9$, $p = 0.463$). The analysis of voter registration in Panel B concludes similarly, as all four pre-2012 coefficients are individually indistinguishable from zero ($p = 0.255$ and $p = 0.353$ for fully covered states in 2008 and 2010, and $p = 0.889$ and $p = 0.798$ for partially covered states in 2008 and 2010) and jointly indistinguishable from zero ($F = 1.54$, $p = 0.189$). Taken together, these tests fail to reject

common trends between covered and uncovered states before *Shelby*.

Rather than diminishing the Black share of the electorate, *Shelby* may have done the opposite—the positive-signed, post-*Shelby* event study estimates in Panel A of Figure 3 suggest that, if anything, Black turnout increased relative to white turnout within covered states. The estimate of the 2016 event study coefficient for fully covered states, for example, indicates that the difference in relative turnout between fully covered and uncovered states was 7.7 percentage points higher ($p = 0.019$), on average, in 2016 than in 2012. For partially covered states, the average relative increase between the 2012 and 2016 elections is similar in magnitude, though indistinguishable from zero at conventional significance levels (5.9 percentage points, $p = 0.103$). Estimates for the 2014 and 2018 midterm elections, while positive-signed, are smaller in magnitude and statistically insignificant. The event study estimates in Panel B evidence a similar story for voter registration, though the magnitude of the relative increase between the 2012 and 2016 elections is smaller for partially covered states (3.9 percentage points, $p = 0.183$) than for fully covered states (7.6 percentage points, $p = 0.011$). In any case, Black voter participation does not appear to have decreased any more than white voter participation within covered states after *Shelby*.

## 4.2  Turnout

In Table 1, I present estimates of the effect of *Shelby* on relative turnout from triple-difference specifications based on Equation 1. Baseline estimates in column (1) suggest that *Shelby* had a positive effect on the relative turnout of Black voters, on average, in fully covered states (4.9 percentage-point increase, $p = 0.035$) and in partially covered states (3.8 percentage-point increase, $p = 0.001$). In other words, the *Shelby* decision is associated with a decrease in the Black-white turnout gap in favor of Black voters. While the estimates attenuate with the addition of controls for age and gender in column (2), they remain positive and statistically significant at the 10-percent level. The inclusion of state-by-race time trends in column (3) serves to probe whether the positive results are artifacts of differential trends within states. Although the increase in standard errors renders the triple-difference coefficients indistinguishable from zero, the point estimates remain positive and are somewhat larger in

magnitude than those in column (1). Like the event study in Panel A of Figure 3, the estimates in columns (1)–(3) indicate that, if anything, *Shelby* differentially increased Black turnout.

If the net effect of *Shelby* on turnout equals the sum of a negative turnout response to differential increases in the cost of voting and a positive turnout response to (race-specific) counter-mobilization, then evidence of a relative increase in Black turnout suggests that counter-mobilization among Black voters outweighed any differential increase in the cost of voting, on average. One might also expect the relative importance of the cost response and the counter-mobilization response to vary by election type. For example, Panels A and B of Figure 2 show that the decrease in turnout between presidential and midterm elections is consistently more pronounced for Black voters than for white voters. While race-by-year fixed effects absorb this variation, the contraction of Black turnout in midterm elections could nevertheless limit the potential reach of counter-mobilization efforts or otherwise improve the prospects of new discriminatory legislation, increasing the relative importance of differential increases in the costs of voting during midterm elections. Alternatively, it could be that individuals who vote in midterm elections are less responsive to voting restrictions than those who only vote in presidential elections, decreasing the relative importance of differential increases in the costs of voting during midterm elections. The pattern of event study coefficients in Panel A of Figure 3 suggests that the positive effects of the *Shelby* decision on relative turnout were smaller in magnitude for midterm elections, consistent with a weaker counter-mobilization response or a stronger cost response.

I explore this possibility in further detail in columns (4)–(6) of Table 1. Estimates from the preferred specification in column (5) indicate that the increases documented in the first three specifications mask differences between presidential and midterm elections. In presidential elections, the *Shelby* decision increased Black turnout relative to white turnout by 7.5 percentage points ($p < 0.001$), on average, in fully covered states and 5.2 percentage points ($p = 0.047$), on average, in partially covered states. These increases represent a full reduction of the Black-white turnout gap that existed in fully covered states during the 2012 presidential election and a 52-percent reduction of the gap that existed in partially covered states, and are consistent with the effects of counter-mobilization outweighing the effects of differential

increases in the cost of voting. In midterm elections, *Shelby* had no significant effect on the relative turnout of Black voters in fully covered states (1.9 percentage points, $p = 0.479$) or partially covered states (1.9 percentage points, $p = 0.238$). Still, the null findings for midterm elections remain consistent with counter-mobilization offsetting the effects of increases in the cost of voting.

Although the inclusion of race-by-state time trends in column (6) reduces precision, all triple-difference estimates remain positive and the coefficient for fully covered states in presidential elections remains statistically distinguishable from zero at the 5-percent level. The absence of negative turnout responses across several specifications, coupled with the relative increase in Black turnout during the 2016 presidential election, contradict the notion that the *Shelby* decision enabled previously covered states to reshape the electorate by suppressing the Black vote. More generally, these results highlight the potential for counter-mobilization to offset the effects of differential increases in the cost of voting.

While the data do not support that *Shelby* differentially reduced average turnout rates among Black voters, this does not imply that no individual was disenfranchised by the decision. A null effect of *Shelby* on Black turnout could reflect a scenario in which counter-mobilization increased turnout among some Black voters while increases in the cost of voting disenfranchised others. It also remains unclear whether counter-mobilization would continue to offset differential increases in the cost of voting in the long run. If mobilizing anger diminishes as differential increases in the costs of voting persist, then the *Shelby* decision could eventually suppress the Black vote. Still, the non-negative impact on the relative turnout of Black voters in the first three federal elections after *Shelby* is an encouraging result.

## 4.3   Registration

In Table 2, I repeat the same exercise for voter registration. Baseline estimates in column (1) suggest that *Shelby* had no discernible effect on the registration of Black voters relative to white voters in fully covered states (4.3 percentage points, $p = 0.102$) or in partially covered states (1.9 percentage points, $p = 0.278$), but estimates from the preferred specification in column (5) indicate that the baseline estimates mask heterogeneous effects by election type.

In fully covered states, the *Shelby* decision increased Black registration relative to white registration during presidential elections (7.2 percentage points, $p = 0.005$), but not during midterm elections (1.8 percentage points, $p = 0.56$). Both coefficients for partially covered states are indistinguishable from zero, though the point estimate for presidential elections (3.4 percentage points, $p = 0.223$) is larger in magnitude than the point estimate for midterm elections (0.3 percentage points, $p = 0.865$). As with the analysis of turnout in Table 1, no specification in Table 2 supports that *Shelby* widened the Black-white registration gap in covered states. If anything, *Shelby* may have narrowed the gap in fully covered states during the 2016 presidential election.

In light of evidence that covered states responded to *Shelby* by purging voters (Feder and Miller 2020; Brater et al. 2018), a non-negative effect of *Shelby* on relative registration further reinforces the potential role of counter-mobilization in maintaining Black voter participation. For example, if Black voters were more likely than white voters to be purged from the rolls in covered states after *Shelby*, then even a null effect of *Shelby* on relative registration would suggest that a significant number of Black voters re-registered after being purged, or that previously unregistered Black voters decided to register in response.

# 5  Conclusion

As part of the argument against the constitutionality of preclearance coverage, Chief Justice Roberts cited data from the November CPS as evidence that the Black-white turnout gap had diminished or even reversed in covered states since the Voting Rights Act was enacted. Researchers have documented, however, that self-reported CPS turnout data mask significant racial disparities in voting (Ansolabehere et al. 2021). Validated turnout data from the CCES, for example, show that significant racial gaps in turnout existed in covered states before *Shelby* and have since continued to exist.

Despite well-founded fears to the contrary, the *Shelby* decision does not appear to have widened participation gaps between Black and white voters in previously covered states relative to those in uncovered states. Triple-difference comparisons of validated turnout and

registration data suggest that *Shelby* had little effect on participation gaps during the first three federal elections that followed the 2013 decision, and, in the 2016 election, may have even increased the relative turnout of Black voters in covered states. These results are consistent with an accumulating body of evidence that suggests that voters and campaigns mobilize in response to increases in the cost of voting when those increases are perceived as threats to the franchise (Valentino and Neuner 2017; Biggers and Smith 2020; Cantoni and Pons 2021).

The resilience of Black turnout in previously covered states provides some grounds for optimism. That said, voters of color do continue to experience systematic barriers to voting (Chen et al. 2020) and questions remain about the impact of *Shelby* on state and local elections, which could pose important consequences for the provision of public resources and the enforcement of anti-discrimination laws.

Figure 1: Preclearance coverage
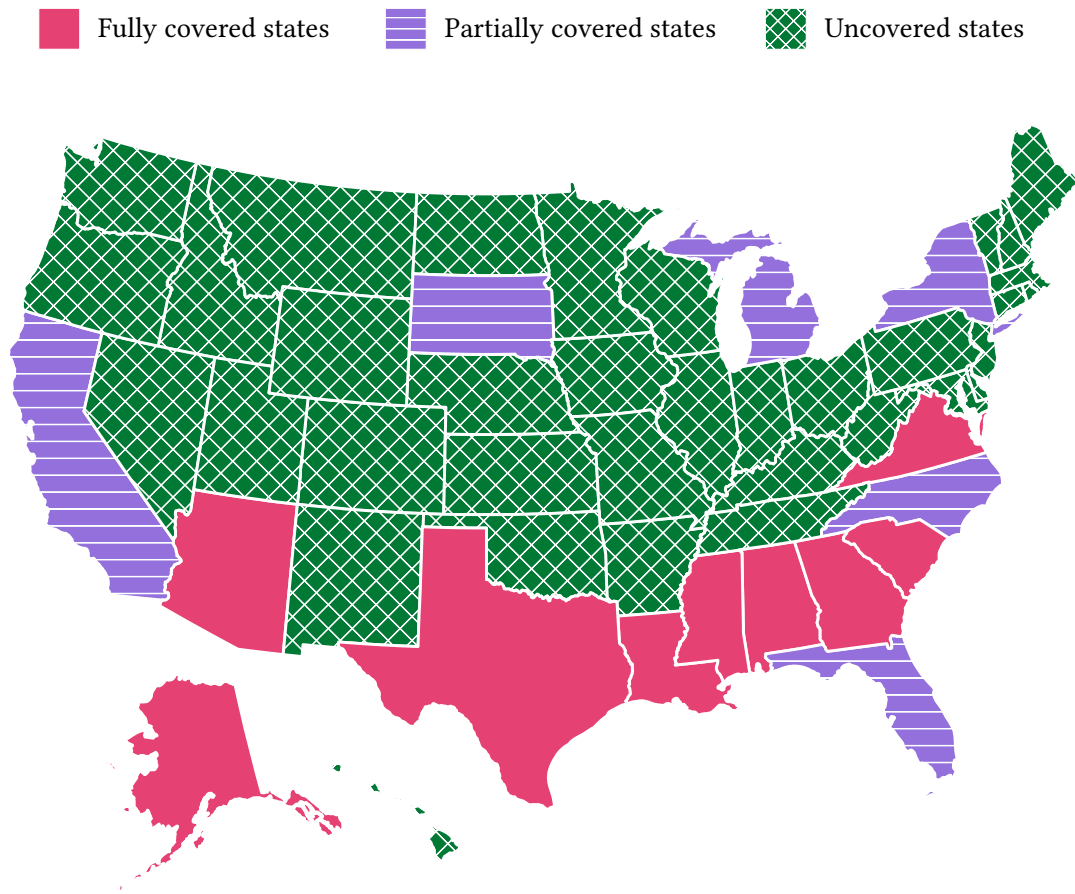


*Notes:* Preclearance coverage at the time of the *Shelby* decision. Before *Shelby*, all state and local authorities within fully covered states were required to obtain federal approval before changing any election policy. All state authorities within partially covered states were required to obtain federal approval, but most local authorities were not. State and local authorities within uncovered states were not required to obtain federal approval. The need for federal approval in covered states ended after the Supreme Court invalidated the coverage formula in 2013. *Source:* US Department of Justice.

## Figure 2: Voter turnout and registration by race and preclearance coverage
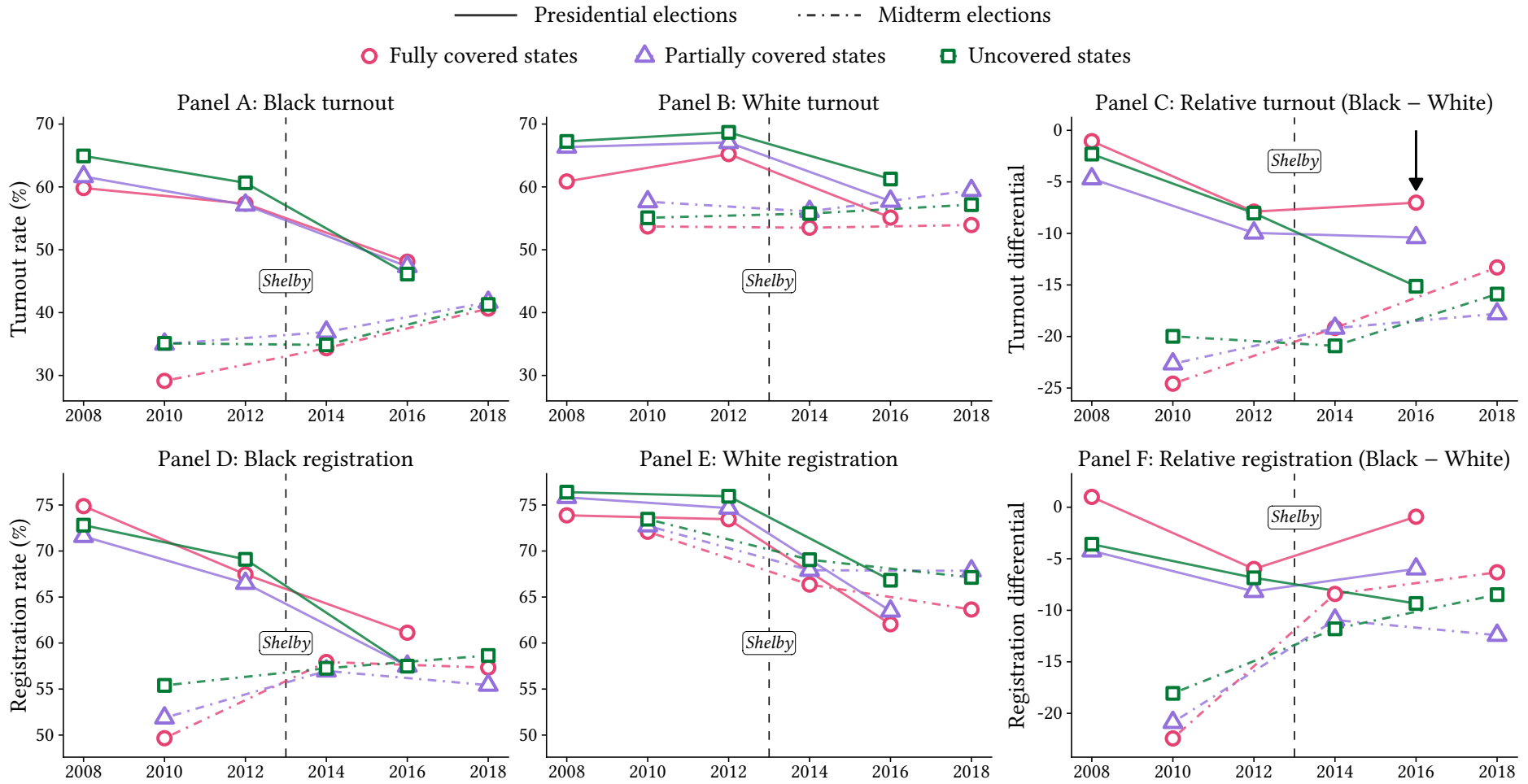


*Notes:* Implied turnout and registration rates derived from weighted averages of validated turnout responses of those in the analysis sample. The analysis sample consists of all Black and white CCES respondents, excluding those surveyed in Virginia during the 2008 presidential election and the 2010 midterms. *Data source:* Cooperative Congressional Election Study (Kuriwaki 2020).

Figure 3: Event study of the effect of *Shelby v. Holder* on relative turnout and voter registration
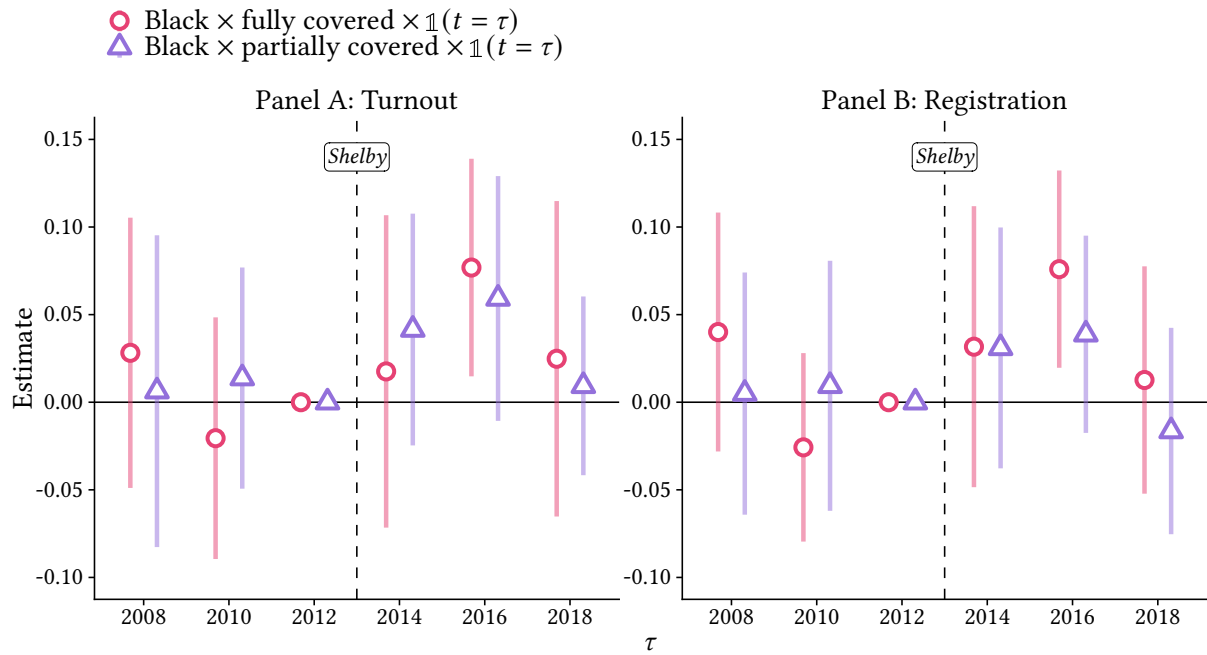


*Notes:* OLS estimates of event study coefficients from Equation 2. Each panel contains estimates from a single regression, and each regression includes race-by-state fixed effects, race-by-year fixed effects, state-by-year fixed effects, and controls for gender, age, and age squared. The sample includes Black and white respondents from six federal elections (2008–2018), weighted using CCES-provided sampling weights. Vertical bars outline pointwise 95% confidence intervals that are robust to clustering at the state level. *Data source:* Cooperative Congressional Election Study (Kuriwaki 2020).

Table 1: Effect of *Shelby v. Holder* on relative turnout

|  | Turnout | | | | | |
|  | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Black × fully covered × *Shelby* | 0.049** | 0.037* | 0.052 | | | |
|  | (0.022) | (0.021) | (0.046) | | | |
| Black × fully covered × *Shelby* × presidential | | | | 0.089*** | 0.075*** | 0.089** |
|  | | | | (0.020) | (0.019) | (0.043) |
| Black × fully covered × *Shelby* × midterm | | | | 0.029 | 0.019 | 0.032 |
|  | | | | (0.028) | (0.027) | (0.051) |
| Black × partially covered × *Shelby* | 0.038*** | 0.030** | 0.057 | | | |
|  | (0.011) | (0.012) | (0.042) | | | |
| Black × partially covered × *Shelby* × presidential | | | | 0.066*** | 0.052** | 0.080 |
|  | | | | (0.025) | (0.026) | (0.052) |
| Black × partially covered × *Shelby* × midterm | | | | 0.024 | 0.019 | 0.046 |
|  | | | | (0.014) | (0.016) | (0.041) |
| Observations | 274,919 | 274,919 | 274,919 | 274,919 | 274,919 | 274,919 |
| Effective observations (race × state × year) | 598 | 598 | 598 | 598 | 598 | 598 |
| Race × state fixed effects | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Race × year fixed effects | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| State × year fixed effects | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Demographic controls | | ✓ | ✓ | | ✓ | ✓ |
| Race × state time trends | | | | ✓ | | ✓ |

*Notes:* OLS estimates of triple-difference coefficients from Equation 1. Fixed effects absorb all lower-order terms. Demographic controls include gender, age, and age squared. The sample includes Black and white respondents from six federal elections (2008–2018), weighted using CCES-provided sampling weights. Standard errors (in parentheses) are robust to clustering at the state level. * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$. *Data source:* Cooperative Congressional Election Study (Kuriwaki 2020).

Table 2: Effect of *Shelby v. Holder* on relative voter registration

| | Registration | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Black × fully covered × *Shelby* | 0.043 | 0.036 | 0.078* | | | |
| | (0.026) | (0.025) | (0.046) | | | |
| Black × fully covered × *Shelby* × presidential | | | | 0.082*** | 0.072*** | 0.113** |
| | | | | (0.025) | (0.024) | (0.047) |
| Black × fully covered × *Shelby* × midterm | | | | 0.025 | 0.018 | 0.059 |
| | | | | (0.031) | (0.031) | (0.048) |
| Black × partially covered × *Shelby* | 0.019 | 0.013 | 0.052 | | | |
| | (0.017) | (0.015) | (0.051) | | | |
| Black × partially covered × *Shelby* × presidential | | | | 0.044 | 0.034 | 0.074 |
| | | | | (0.028) | (0.027) | (0.059) |
| Black × partially covered × *Shelby* × midterm | | | | 0.006 | 0.003 | 0.041 |
| | | | | (0.018) | (0.016) | (0.049) |
| | | | | | | |
| Observations | 274,919 | 274,919 | 274,919 | 274,919 | 274,919 | 274,919 |
| Effective observations (race × state × year) | 598 | 598 | 598 | 598 | 598 | 598 |
| Race × state fixed effects | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Race × year fixed effects | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| State × year fixed effects | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Demographic controls | | ✓ | ✓ | | ✓ | ✓ |
| Race × state time trends | | | ✓ | | | ✓ |

*Notes:* OLS estimates of triple-difference coefficients from Equation 1. Fixed effects absorb all lower-order terms. Demographic controls include gender, age, and age squared. The sample includes Black and white respondents from six federal elections (2008–2018), weighted using CCES-provided sampling weights. Standard errors (in parentheses) are robust to clustering at the state level. $^*$ $p < 0.1$, $^{**}$ $p < 0.05$, and $^{***}$ $p < 0.01$. *Data source:* Cooperative Congressional Election Study (Kuriwaki 2020).

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