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Did Changes to the Voting Rights Act Cause Electoral Backsliding in the States?

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Abstract

When announced in 2013, the U.S. Supreme Court's *Shelby County v. Holder* decision immediately raised concerns that its invalidation of the coverage formula stipulated in Section 4(b) of the Voting Rights Act (VRA) would foster voter suppression and other antidemocratic practices in jurisdictions subject to preclearance protections. Over a decade after this decision, I put these claims to the test. Using a synthetic difference-in-difference design (Arkhangelsky et al. 2021) and the State Democracy Index, a holistic measurement model that provides state-level estimates of electoral democracy from 2000-2018, I examine whether changes in state-level electoral democracy may have led to electoral backsliding in formerly fully and partially covered states in the years following the Court's decision. Ultimately, I find no evidence that electoral backsliding occurred in these jurisdictions in subsequent years. I conclude by discussing the significance of this finding and offering directions for future study.

Keywords: Voting Rights Act, *Shelby County v. Holder*, Preclearance, State Democracy, Electoral Laws, Electoral Democracy, Difference-in-Difference Methods

Word Count: 5794

From its inception in 1965 until the United States Supreme Court ruled it unconstitutional in its monumental *Shelby County v. Holder* (2013) decision, Section 4(b) of the *Voting Rights Act of 1965* (VRA) stipulated a formula that deemed certain states and localities 'covered,' and required them to seek 'preclearance' from the Department of Justice before changing their electoral laws. These 'covered' jurisdictions were selected on the basis of their history of engaging in antidemocratic practices, including denying and diluting the voting power of racial and ethnic minorities through cost-increasing electoral institutions.

Given this history, when the Supreme Court declared the coverage formula outlined in Section 4(b) unconstitutional, many prominent figures on the left immediately denounced this decision over concerns that abolishing these protections would result in a drastic decline in political participation and democratic well-being across these jurisdictions.[1] In her dissenting opinion, Supreme Court Justice Ruth Bader Ginsburg famously stated that "throwing out preclearance when it has worked…is like throwing away your umbrella in a rainstorm because you are not getting wet"[2] and that "there is no question…that the covered jurisdictions have a unique history of problems with racial discrimination in voting."[3] In an official White House statement, then-President Obama commented that the decision "upsets decades of well-established practices that help make sure voting is fair, especially in places where voting discrimination has been historically prevalent."[4] Indeed, the months following the Court's decision saw a flurry of activity in states previously covered by Section 4(b), including increased voter identification requirements and limits on access to polling places (Fuller 2014). It is

[1] Writing on behalf of the majority, Justice Roberts acknowledges the historical need for these protections, but argues that increased levels of minority participation in covered areas imply that "the conditions that originally justified these measures no longer characterize voting in the covered jurisdictions." (*Shelby County v. Holder*, 570 U.S. 529, 561 (2013), 535).
[2] Shelby County v. Holder, 570 U.S. 529, 561 (2013) (Ginsburg, J., dissenting), 590.
[3] Shelby County v. Holder, 570 U.S. 529, 561 (2013) (Ginsburg, J., dissenting), 576.
[4] "Statement by the President on the Supreme Court Ruling on Shelby County v. Holder." White House Office of the Press Secretary, 2013

possible – and indeed, in some cases likely – that many of these bills would have run into legal difficulties if proposed while preclearance requirements were still in effect.

The following analysis tests how much these pessimistic prognostications about the effects of the *Shelby County* decision reflect subsequent reality by asking: *to what extent did electoral backsliding occur in formerly covered states as a consequence of the Supreme Court's decision that Section 4(b) of the VRA was unconstitutional?*

In the years following the Court's decision, scholars of American electoral institutions and election law have attempted to assess the impact that the end of coverage has had on electoral participation in formerly covered jurisdictions. This paper contributes to the growing literature on electoral democracy in U.S. states in two important ways. First, while previous work has considered how the *Shelby County* decision impacted voter registration and turnout rates in formerly covered states, the literature has thus far yet to test whether the abolition of coverage protections have affected overall levels of electoral democracy and democratic health in these jurisdictions. Second, this paper uses estimates from a holistic measurement model, the State Democracy Index, to examine changes in levels of electoral democracy *writ large* in formerly covered states after the *Shelby County* decision, a novel contribution to the burgeoning literature on the health of electoral democracies (Waldner and Lust 2018, Garnett and James 2018, Grillo et al. 2023). I incorporate this measure in an innovative causal inference design, synthetic difference-in-differences, to determine how the *Shelby County* ruling affected levels of electoral democracy in formerly covered states. By combining features of standard difference-in-difference and synthetic control designs, the synthetic difference-in-difference estimator improves on the robustness of conventional difference-in-difference designs, especially in cases where the parallel trends assumption is unlikely to hold (Arkhangelsky et al. 2021).

The remainder of the paper proceeds as follows. I provide an overview of the existing literature investigating the impacts of the *Shelby County* decision on electoral participation in formerly covered jurisdictions. The paper then introduces the State Democracy Index (SDI), first introduced in Grumbach (2022), which is used as a measure of annual levels of electoral democracy in each state. This section also draws upon the democratization literature and a qualitative case study from South Carolina's 2011 voter identification law to establish why it is worthwhile to look at general trends of electoral health to evaluate the impacts of the Court's decision and motivate why the SDI is appropriate to measure these effects. I next introduce the synthetic difference-in-difference estimator, which I use to specify two models testing the theory that the Court's ruling should have led to lower levels of electoral democracy in former fully and partially covered states. Ultimately, neither model produces discernible effects, which leads me to conclude that the *Shelby County* decision did not noticeably reduce levels of electoral democracy in formerly covered states.

Race, Registration, and Turnout: the Existing Literature on *Shelby County*

In the years since the Court's decision, a number of studies have attempted to elucidate how the end of Section 4(b) has impacted voter registration and turnout in formerly covered jurisdictions, generally finding that the effects of the *Shelby County* decision on electoral participation and representation have not been as grim as predicted.

Raze (2022) uses a triple differencing method and validated turnout data from the Congressional Elections Study (CES) to identify how *Shelby County* impacted turnout in fully and partially covered states, compared to uncovered ones. These findings suggest that the Court's

decision did not increase turnout disparities between blacks and whites in formerly covered jurisdictions (Raze 2022).

Several other studies exploit the fact that six states were not subject to statewide preclearance restrictions under Section 5, but were instead subject to preclearance requirements on a county-by-county basis (Gibson 2020, Komisarchik and White 2022). This county-level analysis provides additional causal leverage on the effects of the Court's decision because it allows for the comparison of turnout and vote share outcomes across treated and untreated counties within the same state, which controls for electoral characteristics that may otherwise vary by state.

Most work that exploits county-level variations in coverage status does so in North Carolina, where forty out of 100 counties – the most of any partially covered state – were subject to preclearance under Section 5. Fresh (2018) uses a difference-in-difference design to examine how the introduction of preclearance laws in North Carolina following the passage of the *Voting Rights Act* affected minority voter registration and overall turnout rates in subsequent federal elections. This analysis yields mixed results, and ultimately concludes that "societal gains from the VRA…were modest" (714), Indeed, while county-level coverage protections in North Carolina boosted voter registration and turnout rates among both blacks and whites, preclearance was also linked to declining levels of Democratic vote share, likely from reactionary whites (Fresh 2018). Gibson (2020) uses regression discontinuity designs to explore how the Court's decision impacted 2016 Democratic turnout and vote share, finding similarly mixed results. While this analysis suggests that the abolition of preclearance decreased Democratic primary turnout in these counties, it ultimately concludes that the Court's decision had no impact on Democratic vote share and minimal effects on minority turnout rates (Gibson 2020).

Importantly, all of this research examines the ramifications of the *Shelby County* decision on voter registration and turnout rates in formerly covered jurisdictions, finding that the abolition of coverage generally had negligible effects on levels of political participation. Like previous work, my paper considers the impact of the Court's decision in both fully and partially covered states. However, my analysis is distinct from the rest of this literature in that it uses estimates of state-level electoral democracy as its primary outcome variable of interest. Such an approach enables this paper to make inferences about *Shelby County's* effects on levels of electoral democracy and democratic health *writ large,* rather than solely focusing on how this decision has impacted more conventional metrics of electoral participation.

The following section introduces the State Democracy Index. While doing so, it elaborates on the importance of undertaking this analysis by establishing a theoretical framework, grounded in the democratization literature, that explains why it is worthwhile to consider how *Shelby County* impacted levels of electoral health in formerly covered states and how doing so advances our understanding of the broader impacts of the Court's decision.

The State Democracy Index and the Importance of Measuring Electoral Democracy

In the United States, federalism gives states wide purview to regulate the franchise as they see fit (Keyssar 2009). This has essentially led to the creation of fifty different electoral systems, referring to the subnational election administration apparatuses developed by each individual state. These subnational institutional differences imply that it is plausible, if not likely, that states' levels of electoral democracy will differ significantly from one another, even absent larger institutional shocks like the *Shelby County* decision. Consequently, I define state-level

electoral democracy as the extent to which a state's electoral system holds fair and legitimate elections (Schumpeter 1942, Dahl 2003).[5, 6]

To capture the effects of the Court's decision on estimates of state-level electoral democracy over time, I rely on the State Democracy Index (SDI), a latent variable measure of electoral democratic health developed in Grumbach (2022).[7] The SDI is composed of 950 observations, each representing an estimated level of democracy for each of the fifty U.S. states from 2000-2018. To develop these estimates, the SDI uses Bayesian factor analysis modeling for mixed data to create an index composed of 51 measures of state-level electoral democracy (Grumbach 2022).[8] These measures cover a range of factors that affect electoral participation, representation, and responsiveness, ranging from whether or not a state has enacted cost-increasing electoral institutions, such as voter identification laws, to the extent to which state-level policy outcomes reflect popular opinion. Per Grumbach (2022), the individual factor loading score associated with each component determines how it affects a state's overall SDI score.

For example, states with strict voter identification laws will have lower SDI scores, *ceteris paribus*, than those that do not; whereas states that have enacted absentee voting or same

[5] This definition of electoral democracy is equivalent to one used in Grumbach (2022). This ensures that my use of this measure to gain leverage on electoral democracy as it relates to the impacts of the Shelby County decision is consistent with its intended use.

[6] It is also important to differentiate electoral democracy from liberal democracy, the latter of which references how well a democratic system upholds and protects the rights and liberties of its citizens (Grumbach 2022). I follow in the footsteps of Grumbach by considering state-level electoral democracy a subtype of overall American democracy. This is an especially important distinction for the purposes of this project because the electoral SDI only considers levels of electoral democracy and thus cannot provide any leverage on how levels of liberal democracy may have changed across states over time.

[7] Though loosely correlated with the Cost of Voting Index (COVI) (Grumbach 2022), the SDI goes beyond measuring the ease of voting in each state. Indeed, by estimating levels of state-level electoral democracy annually – including during midterm election and off-cycle years – and incorporating measures of the efficiency and fairness of state election administration, such as the completeness of a state's voting records, the index goes beyond COVI's goal of assessing the ease of voting in each state during presidential election years (Schraufnagel et al. 2022) to serve as a more holistic measure of overall democratic health in each state.

[8] The full SDI utilizes 115 measures of electoral, liberal, and egalitarian democracy (Grumbach 2022, 2023). In line with the approach used in Grumbach (2022), I only use the SDI's 51 indicators of electoral democracy.

day registration will have higher SDI scores than those that have not enacted such reforms.[9] When compiled together to create the SDI, these factors shape whether a state has a high or low estimated level of electoral democracy for any given year.

I define electoral health as how high (or low) each U.S. state's level of electoral democracy is for a specific year, which is given by each SDI estimate. That is, higher SDI scores suggest higher levels of electoral health, while the reverse holds true for lower scores. Consistent with other measures of electoral democracy that operationalize electoral democracy as a continuous variable,[10] the SDI is a continuous measure of electoral democracy with estimated levels of democracy ranging from the lowest score of -3.08 (Tennessee, 2018) to a high of 1.89 (Washington, 2018). By providing annually estimated levels of electoral democratic health in the years preceding and following the overturn of Section 4(b), the SDI therefore enables us to track how levels of electoral democracy within states shifted in response to the *Shelby County* decision. If SDI-estimated levels of democracy in formerly covered states abruptly declined in the wake of the Court's ruling, this suggests that the abolition of preclearance resulted in a non-negligible degree of electoral backsliding in the years that followed. On the other hand, if there is no noticeable change in these SDI estimates in subsequent years, this suggests that the abolition of coverage did not result in declining democratic health in these jurisdictions.

Consistently low SDI estimates in formerly covered states following the Court's decision may indicate that the end of preclearance led to electoral backsliding in these places. The democratization literature characterizes electoral backsliding as the process by which elections are made less competitive without entirely undermining the electoral process or system (Waldner

[9] For more information about the individual components included in the SDI index (and their associated factor loadings), see Figure 1 in Grumbach (2022).
[10] For one example of this, see the Varieties of Democracy (V-DEM) Electoral Democracy Index, which measures electoral democracy in continuous terms, rather than merely noting the presence or absence of free and fair elections in binary terms.

and Lust 2018). This may be accomplished by implicitly restricting participation without overtly or explicitly abolishing the norms of universal suffrage (Waldner and Lust 2018), but in a way that nevertheless results in a decline in the quality of elections (Garnett and James 2023).

For an example of preclearance restrictions standing in the way of an electoral reform that, when enacted, could have resulted in democratic backsliding, consider the example of voter identification laws. Citizens of a state that enacted a voter identification law following the ruling should find it more costly to vote than before such a regulation was enacted (Highton 2017). South Carolina stands out as an example of a state that was subject to full coverage under Section 4(b) of the VRA, but nevertheless attempted to implement a voter identification proposal in the years leading up to *Shelby County* decision. In December 2011, the U.S. Department of Justice (USDOJ) blocked Act R54, which would have required all South Carolina voters to present a valid form of photo identification in order to vote, under Section 5 of the VRA (Savage 2011). Writing on behalf of the USDOJ, Assistant Attorney General for Civil Rights Thomas E. Perez argued that the proposed law would disproportionately impact minority voters, drawing on evidence that nearly 82,000 minority citizens who were previously registered to vote lacked one of the acceptable forms of photo identification required to cast a ballot under the new law (USDOJ 2011).

The case of South Carolina's Act R54 shows that, prior to the *Shelby County* decision, preclearance worked as intended to prevent covered states from passing laws that would have contributed to electoral backsliding by implicitly restricting certain groups of citizens' ease of voting. To help evaluate these trends, the SDI includes an indicator of whether or not states have passed voter identification laws, finding that such laws are negatively associated with higher levels of state democracy (Grumbach 2022).

South Carolina's failed effort to implement a voter identification law helps explain that covered states were likely to have enjoyed higher levels of electoral democracy prior to the *Shelby County* decision because of the threat of federal intervention under Section 5. Because the Court's decision abolished federal oversight over electorally burdensome laws, this ruling enabled states to pass laws that could foster backsliding by making voting more difficult. Sure enough, in the years following the Court's decision, previously covered states used their newfound legal discretion to adopt voter identification laws that were substantially stricter than those enacted in states that had never been subject to preclearance (Komisarchik and White 2022). If *Shelby County* indeed led to electoral backsliding in formerly covered jurisdictions, then we should expect levels of electoral democracy in these states to decline resulting from the abolition of these coverage protections and the subsequent implementation of cost-increasing electoral regulations, such as voter identification laws.

The example of South Carolina's voter identification law illustrates a scenario where we would expect that SDI estimates should decline following the Court's decision, signaling a decrease in electoral health. Given this example, some might question why I do not use voter identification laws (or other specific electoral cost-increasing regulations) as my outcome variable instead of the broader electoral SDI. However, I argue that the justification given above helps explain why it is worthwhile to discern how the *Shelby County* decision has impacted electoral health more broadly. For all of these reasons, using the SDI to measure levels of electoral democracy elucidates *Shelby County's* impacts on an entirely new set of outcomes related to electoral health. Rather than using registration and turnout data to examine the ease of minority participation in formerly covered states – as previous studies have done – this analysis considers a broader set of outcomes related to electoral democratic health; and thus provides

leverage on the important question of whether or not the Court's decision led to electoral backsliding in affected states. In other words, rather than considering how the ruling impacted a singular policy outcome, use of the SDI allows for the understanding of how bundled changes to state electoral laws, each with different effects on SDI scores, impacts levels of electoral democracy within formerly covered states in the years following the Court's decision.

Testing *Shelby County's* Impacts on State-Level Electoral Democracy

To evaluate how levels of electoral democracy in formerly fully and partially covered states changed following the *Shelby County* ruling, I use SDI-estimated levels of state-level electoral democracy across all fifty states from 2000-2018 as my dependent variable in two synthetic difference-in-difference models.

Specifically, I test whether levels of electoral democracy in formerly fully and partially covered states have regressed in the aftermath of *Shelby,* each compared to states that were not subject to preclearance requirements. Table 1 outlines the specifications of each model in depth. Prior to the Court's decision, nine states were fully subject to preclearance requirements under the coverage formula given in Section 4(b). Furthermore, jurisdictions across six additional states were subject to county and municipal-level coverage requirements, even though preclearance was not mandated statewide. For this reason, I conduct two separate analyses. Model 1 evaluates how SDI scores in fully covered states changed following the Court's ruling compared to uncovered states, whereas Model 2 considers how partially covered states were affected by the decision (again relative to uncovered states). Because I consider two different groups of states as treated, I omit the other treated group not under consideration in each model. For example, Model 1, which estimates how *Shelby County* affected SDI scores in fully covered states, omits

the six partially covered states from its analysis, while Model 2 excludes fully covered states. This approach addresses treatment group contamination concerns that might compromise the validity of the synthetic difference-in-difference estimates.

	Model 1 *Fully Covered Only*	**Model 2** *Partially Covered Only*
Treated States	AL, AK, AZ, GA, LA, MS, SC, TX, VA	CA, FL, MI, NY, NC, SD
Excluded States	CA, FL, MI, NY, NC, SD	AL, AK, AZ, GA, LA, MS, SC, TX, VA
Total Sample Size	836	779
Treatment Period	2013-2018	2013-2018

TABLE 1: SYNTHETIC DIFFERENCE-IN-DIFFERENCE MODEL SPECIFICATIONS

I define the treatment period as beginning in 2013. Though not a federal election year, some states began implementing new laws immediately after the Court announced its decision, which would have gone into effect before the year was up. Perhaps most notably, North Carolina implemented a sweeping law that, among other changes, imposed voter identification requirements and eliminated same-day voter registration (Lerner 2023). Additionally, four states – three of which were fully subject to preclearance requirements prior to 2013 – hold off-year elections.[11] For these reasons, we should expect to see the effects of the Court's decision begin to alter levels of electoral democracy in states subject to the ruling in 2013.

Estimating these models allows for the comparison of how levels of electoral democracy, operationalized through the SDI, changed among the states subject to full and partial coverage as a result of the *Shelby County* decision. If there is a significant decline in SDI-given estimates of electoral health in the years following the ruling, this shows that *Shelby County* adversely

[11] Louisiana, Mississippi, New Jersey, and Virginia.

affected levels of electoral democracy in affected jurisdictions, and suggests the possibility that these states experienced electoral backsliding in the following years.

Synthetic Difference-in-Differences

Previous work uses standard difference-in-difference methods to evaluate the impacts of the *Shelby County* decision on state-level electoral outcomes (Gibson 2020, Komisarchik and White 2022). However, evaluating the pre-treatment trends of SDI estimates for treatment and control group states shows that the parallel trends assumption is unlikely to hold.[12] Because parallel trends between the treatment and control groups is a crucial identification assumption of difference-in-difference designs, this suggests that the conventional difference-in-difference estimator cannot accurately measure how the Shelby *County* decision affected levels of state democracy in formerly covered states.

To overcome these parallel trends violations, I use the synthetic difference-in-difference method and Synthdid R package introduced in Arkhangelsky et al. (2021) to estimate the effect of the *Shelby County* decision on electoral democracy via the SDI. Synthetic difference-in-differences builds off of the standard difference-in-difference design by integrating the synthetic control method's practice of deriving unit-specific weights to reweight control group states, placing greater weight on those that are most similar to treated states (Arkhangelsky et al. 2021). This method then combines these unit weights (ω_i) with time weights (λ_t) that balance the pre-treatment time trends of the SDI with those in the post-treatment period by emphasizing which years in the pre-treatment period, spanning from 2000-2012, are most similar to those in the post-treatment period, beginning in 2013. By incorporating this combination of

[12] See Appendix A for event study plots and a formal test of the validity of the parallel trends assumption for this study.

unit and time weights, the synthetic difference-in-difference estimator has been shown to increase the robustness of treatment effect estimates, especially in scenarios where parallel trends are unlikely to hold (Arkhangelsky et al. 2021).

Computing the Synthetic Difference-in-Difference Weights

Formally, I use the following equation to compute state weights (ω_i) that minimize the difference between the SDI score in the average treated state and a weighted average of SDI scores in untreated states in the years prior to 2013, when the *Shelby County* decision was announced.[13]

$$\text{argmin}_{\omega_0 \in R_+, \omega \in \Omega} \sum_{t=1}^{T_{pre}} \left(\omega_0 + \sum_{i=1}^{N_{co}} \omega_i Y_{it} - \frac{1}{N_{tr}} \sum_{i=N_{co}+1}^{N} Y_{it} \right)^2 + \zeta^2 T_{pre} \parallel \omega \parallel_2^2 \quad \textbf{[1]}$$

Here, ω_0 represents the average differences between the treated and control states in the pretreatment period. Consequently, this term produces a weighted average of the pretreatment control group states. Y_{it} is the SDI score in state *i* and year *t*, T_{PRE} is the number of pretreatment periods (the 13 years spanning from 2000-2012), *N* is the total number of treatment and control group states, Ω is the set containing all valid weights ω in which ω sums to one and all unit weights ω_i fall in the set between 0 and 1, and ζ is a regularization parameter derived in Arkhangelsky et al. (2021).

To compute the time weights (λ_t) that reweight the pre-treatment years in the years preceding the Court's decision so they are most similar to those in the posttreatment period, I minimize the following equation that is nearly identical to the above:

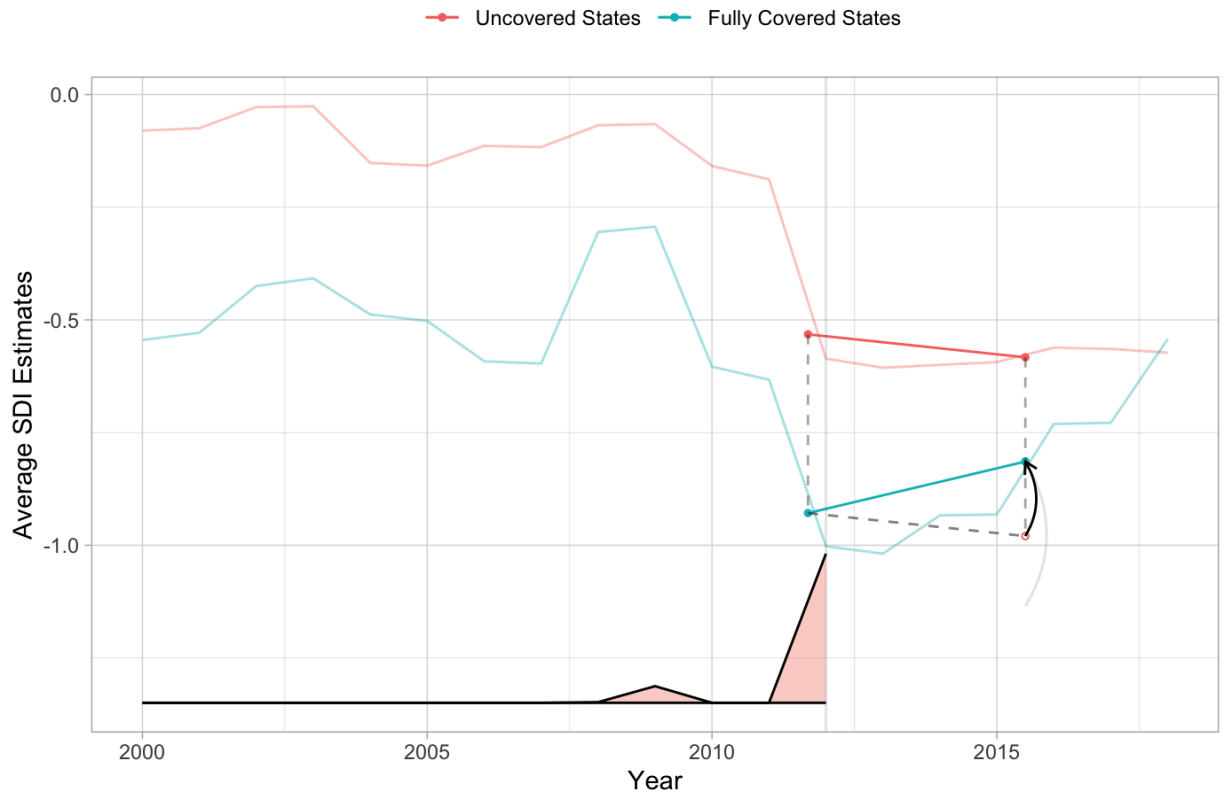
[13] Appendix B shows the distribution of unit weights assigned to control group states.

$$\text{argmin}_{\lambda_0 \in R, \lambda \in \wedge} \sum_{i=1}^{N_{co}} \left(\lambda_0 + \sum_{t=1}^{T_{pre}} \lambda_t Y_{it} - \frac{1}{T_{post}} \sum_{t=T_{pre}+1}^{T} Y_{it} \right)^2 \quad \textbf{[2]}$$

Finally, I use the products of the unit and time weights as the weights in the weighted difference-in-difference least squares regression given below:

$$\text{argmin}_{\tau, \alpha, \beta} \sum_{i=1}^{N} \sum_{t=1}^{T} (Y_{it} - \alpha_i - \beta_t - W_{it}\tau)^2 \hat{\omega}_i \hat{\lambda}_t \quad \textbf{[3]}$$

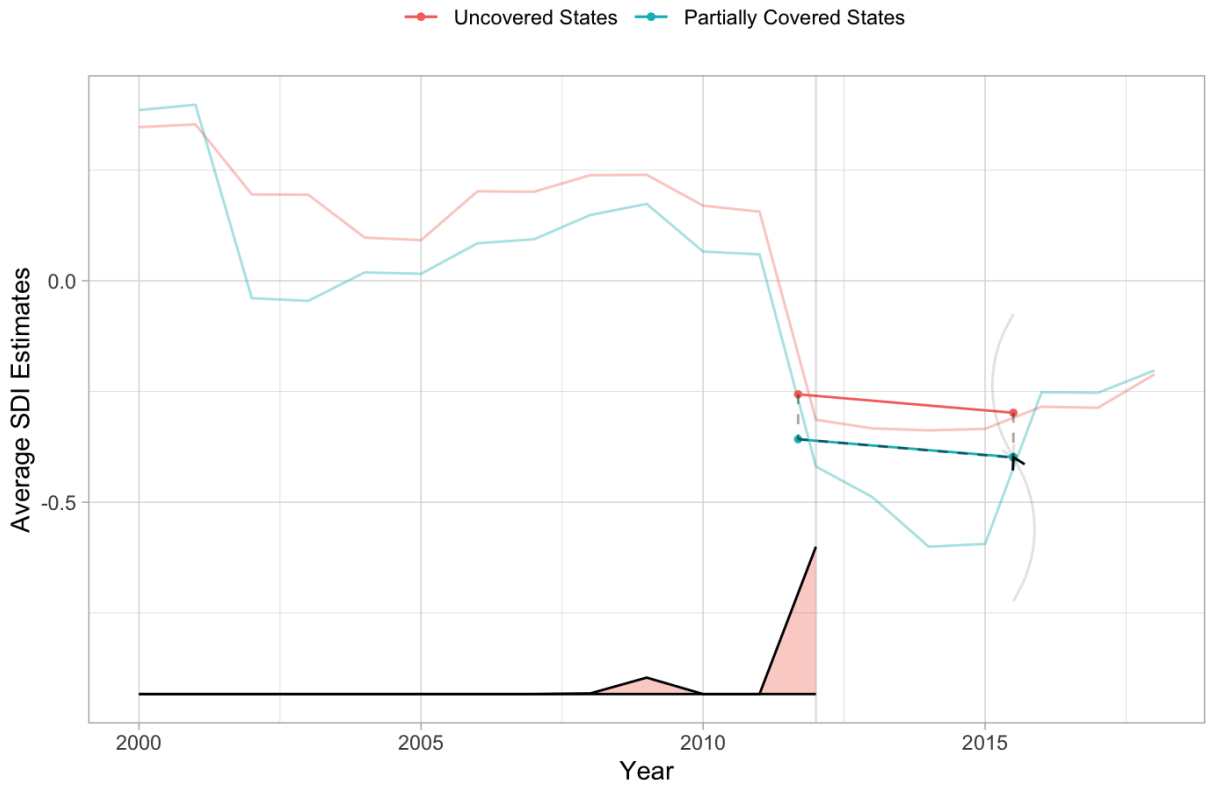
Where W_{it} is an indicator that shows whether state i was subject to preclearance at time t, τ is the treatment effect, and α_i and β_t are state and year fixed effects, respectively. The parallel trends plots in Figures 1 and 2 illustrate the reweighting of the treatment and control group subsets used in each model.



Notes: The black arrow denotes the magnitude of the treatment effect for each of the two models. The faint gray arrow represents the 95% confidence interval for the treatment effect estimate. The blue segment of the parallelogram shows the estimated change from the weighted pre-treatment average to the post-treatment average among fully covered states. The red segment shows the same for uncovered states. The size and shape of the pink triangles at the bottom of the figure indicate the distribution of the calculated time weights (λ_t) used to average pretreatment time periods to make them the most similar to the posttreatment periods.[14]

FIGURE 1: PARALLEL TRENDS PLOT, FULLY COVERED STATES (MODEL 1)

[14] synthdid/vignettes/synthdid.Rmd at master · synth-inference/synthdid · GitHub



Notes: See figure notes from Figure 1.

FIGURE 2: PARALLEL TRENDS PLOT, PARTIALLY COVERED STATES (MODEL 2)

***Shelby County's* Null Effect on Electoral Democracy**

Table 2 provides estimates of the four difference-in-difference models aimed at assessing the impact of the *Shelby County* decision on estimated levels of democracy in previously covered states. Models 1 and 2 are both synthetic difference-in-difference models that evaluate how the Court's ruling affected SDI estimates in fully and partially covered states, respectively. Models 3 and 4 estimate use a conventional difference-in-difference method to estimate the ATT for each of these treatment groups.[15] The synthetic difference-in-difference results from Models 1 and 2

[15] Though the results of a formal test of the parallel trends assumption presented in Appendix A suggest that parallel trends do not hold, I incorporate standard difference-in-difference estimates to compare with the synthetic estimates, a strategy used in other recent work using synthetic difference-in-differences, such as Lal and Thompson (2024).

are also illustrated in the parallel trends plots in Figures 1 and 2, where the black arrow denotes the magnitude of the treatment effect for each of the two models.

Table 2: Synthetic Difference-in-Difference Estimates of the Effect of *Shelby County* on Levels of State Democracy

	Outcome: SDI			
	Synthetic DiD		Standard DiD	
	Model 1	**Model 2**	**Model 3**	**Model 4**
ATT (Fully Covered States)	0.165		0.101	
	(0.173)		(0.235)	
ATT (Partially Covered States)		0.001		-0.088
		(0.349)		(0.635)
State FEs	Y	Y	Y	Y
Year FEs	Y	Y	Y	Y
N	836	779	836	779
Note:				*p<0.1; **p<0.05; ***p<0.01
				Standard errors (clustered by state) in parentheses

TABLE 2: ESTIMATED EFFECTS OF THE *SHELBY COUNTY* DECISION ON STATE DEMOCRACY INDEX SCORES

In contrast with expectations that the overturn of Section 4(b) would lower levels of democracy and contribute to electoral backsliding in formerly covered states, the estimates in Models 1 and 2 offer no evidence that this actually occurred. Furthermore, the conventional difference-in-difference estimates presented in Models 3 and 4 replicate this null result.

Among fully covered states, the treatment effect estimate of the synthetic difference-in-difference treatment effect estimate in Model 1 (0.165) suggests that, relative to uncovered states, fully covered states saw a moderate increase in state levels of democracy from

2013-2018. However, the standard error (0.173) implies that the possibility of no change cannot be rejected. Similarly, the treatment effect estimate in Model 2, which compares SDI scores in partially covered states to those in uncovered states, is virtually indistinguishable from zero (0.001).

The treatment effects estimated from the standard difference-in-difference models presented in Models 3 and 4 also align with these null findings. The magnitudes of the conventionally estimated treatment effects for fully and partially covered states (0.101 and -0.088, respectively) are marginally lower than the corresponding synthetic difference-in-difference estimates identified in Models 1 and 2. In line with the synthetic ATT estimates, however, neither conventional difference-in-difference estimate can be confidently distinguished from zero.

The results of all four models suggest that the predicted negative effects of the *Shelby County* decision on electoral health in formerly fully and partially covered states did not happen in the years following the ruling. In fact, there is no conclusive evidence of *any* positive or negative changes to levels of electoral democracy in affected states. Overall, the results of this analysis suggest that the effect of the Court's ruling on levels of democracy in states formerly subject to full and partial coverage is reliably null.

Where Do We Go From Here?

The results of the synthetic difference-in-difference analysis find no evidence of electoral backsliding within previously fully or partially covered states resulting from the overturning of Section 4(b) of the VRA. Though this lack of discernible effects may come as a surprise from a substantive perspective, it is consistent with other recent studies that examine the *Shelby County* decision's effect on voter registration and turnout and minority representation (Gibson 2020,

Komisarchik and White 2022, Raze 2022). This result also aligns with a related body of work that generally finds that other state-level electoral reforms that may be construed as increasing the costs of political participation, such as the passage of voter identification laws, do not actually do so in practice (Erikson and Minnite 2009, Alvarez et al. 2011, Rocha and Matsubayashi 2014, Highton 2017).

With that said, determining *why* the Court's decision did not result in electoral backsliding in formerly covered states is nevertheless a valuable direction for future research. Though both theory and empirics indicate that SDI captures a single (unidimensional) latent variable representing democratic health, additional work could be done to consider whether it can be principally disaggregated into multiple dimensions and, if so, whether they are equally sensitive to events such as the Shelby County decision.[16] Such an exercise could serve to better elucidate why the Court's ruling did not cause electoral backsliding in fully and partially covered states.

Another opportunity for future work could involve using a continuous difference-in-differences design to operationalize the percentage of a state's population subject to coverage prior to the Court's decision as a continuous treatment variable.[17] Because such a design defines treatment status as a continuous measure, rather than a binary variable that simply notes whether or not a state is fully or partially covered, it would allow for the inclusion of both fully and partially covered states in the same model, thus increasing the number of observations under consideration. Consequently, using a continuous difference-in-difference design to

[16] For example, there is a possibility that the SDI's 16 gerrymandering components are separable from its other 35 components (many of which pertain to the presence or absence of various restrictive and expansive state-level electoral institutions)

[17] I am grateful to an anonymous reviewer for this suggestion.

elucidate how the percentage of a state's residents subject to coverage prior to the *Shelby County* decision affects the SDI presents a worthwhile direction for future research.

Although these findings are in line with existing results about how state-level electoral institutions influence the ease of voting, they nevertheless contribute to the expanding body of literature on electoral institutions and political behavior. While previous studies use difference-in-difference (Fresh 2018, Gibson 2020, Komisarchik and White 2022) and triple differencing identification strategies (Raze 2022), this paper uses the recently developed synthetic difference-in-difference estimator to find similar results to prior work that uses conventional methods. Substantively, this analysis moves beyond previous work's focus on how the *Shelby County* decision impacted voter registration and turnout rates to instead consider its effects on state-level estimates of democratic health more generally by considering how annual State Democracy Index scores in formerly fully and partially covered states changed in the years following the ruling. While a decline in SDI scores in subsequent years would indicate that the abolition of coverage led to lower levels of electoral health in affected states, and suggest that the Court's decision led to electoral backsliding, my analysis finds no evidence of backsliding resulting from the overturn of Section 4(b). Justice Ruth Bader Ginsburg's metaphorical 'umbrella' is now more than a decade gone, and residents of formerly covered states still do not appear to be getting wet.

Appendix: Evaluating the Validity of the Parallel Trends Assumption

The parallel trends assumption is central to the validity of difference-in-difference designs.[18] This assumption states that the time trends of the dependent variable in the treatment and control groups remain parallel to each other over time, justifying an expectation that the difference between the pre and post-treatment periods for the treatment and control groups would have remained equivalent had the treatment group – in this case, fully and partially covered states – not been exposed to the intervention posed by the *Shelby County* decision.

To help determine whether the parallel trends assumption interferes with the validity of the treatment effect estimates, I create the following event study plots for Models 1 and 2, presented in Figures A1 and A2, respectively. Then, to formally test for parallel trends violations, I use the coefficients from each event study to conduct an F-test of the null hypothesis that these coefficients in the pre-treatment period are equal to zero.

The resulting p-values of both F-tests ($p < 0.01$) indicate that we can reject this null hypothesis (i.e., that the pre-treatment coefficients do not truly equal zero in either event study) at conventional levels. Consequently, this result does not offer strong support that either model is able to maintain the parallel trends assumption as is. Following the procedure put forth in Arkhangelsky et al. (2021), I decide to utilize a synthetic difference-in-difference estimator in both models to adjust for this violation by reweighing control group units to better satisfy the parallel trends assumption.

[18] The other identifying assumption underpinning difference-in-difference methods is the stable unit treatment value assumption (SUTVA). While I agree that it is important to consider potential SUTVA violations when utilizing difference-in-difference designs, I do not believe that the analysis presented in this paper has any spillover effects or other SUTVA violations. This is because my design considers a stable unit of analysis (U.S. states) that were all subject to the same treatment shock (the Shelby County decision) simultaneously. Thus, the set up of this design removes any major opportunities for treatment spillovers that might violate SUTVA.

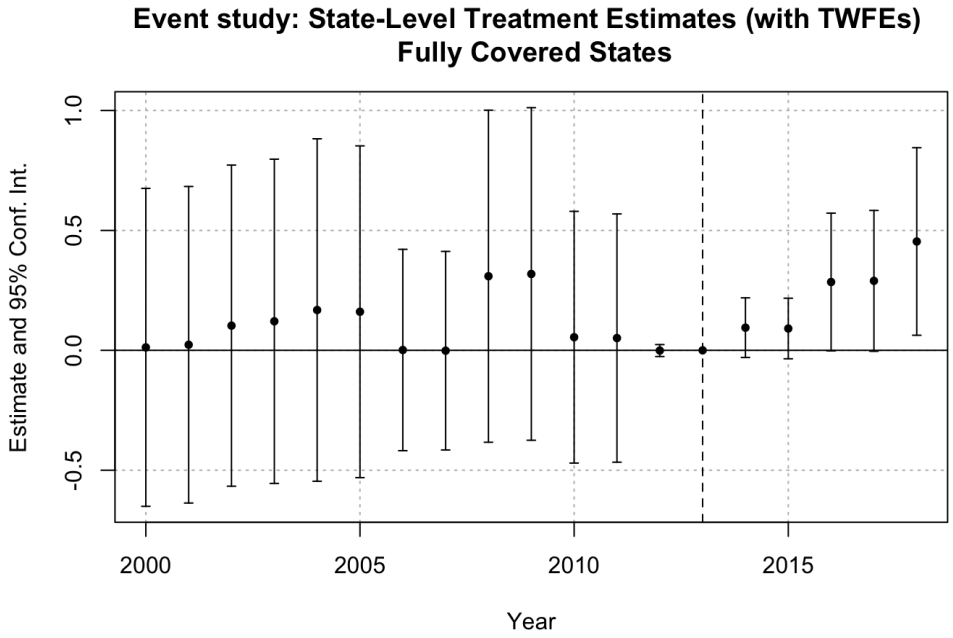


FIGURE A1: EVENT STUDY PLOT OF ESTIMATED TREATMENT EFFECTS, FULLY COVERED STATES

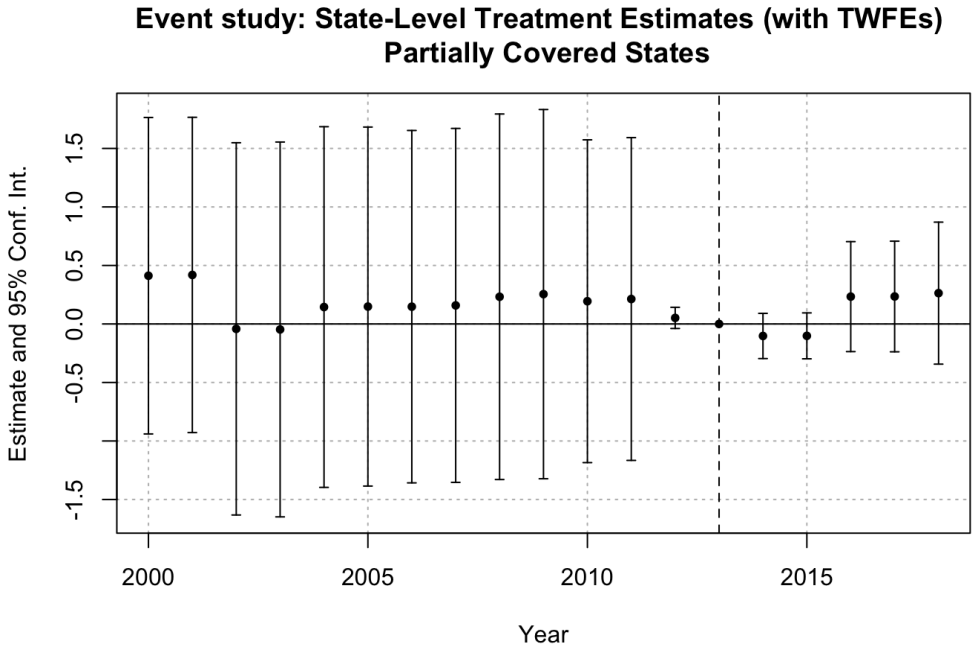


FIGURE A2: EVENT STUDY PLOT OF ESTIMATED TREATMENT EFFECTS, PARTIALLY COVERED STATES

Appendix B: Distributions of Synthetic Control Unit Weights (ω_i)

Per Arkhangelsky et al. (2021), the figures on the following pages plot the adjusted treatment effect outcomes (changes in SDI scores) among the control group units for Model 1 (fully covered states only) and Model 2 (partially covered states only). The sizes of the unit weights (ω_i) for each state are proportional to the size of the points.

Figures B1 and B3 plot the estimated changes in SDI scores associated with all control unit weights for Models 1 and 2, respectively. Figures B2 and B4 plot the changes in SDI scores associated with only the top ten highest weighted synthetic control units for each model.

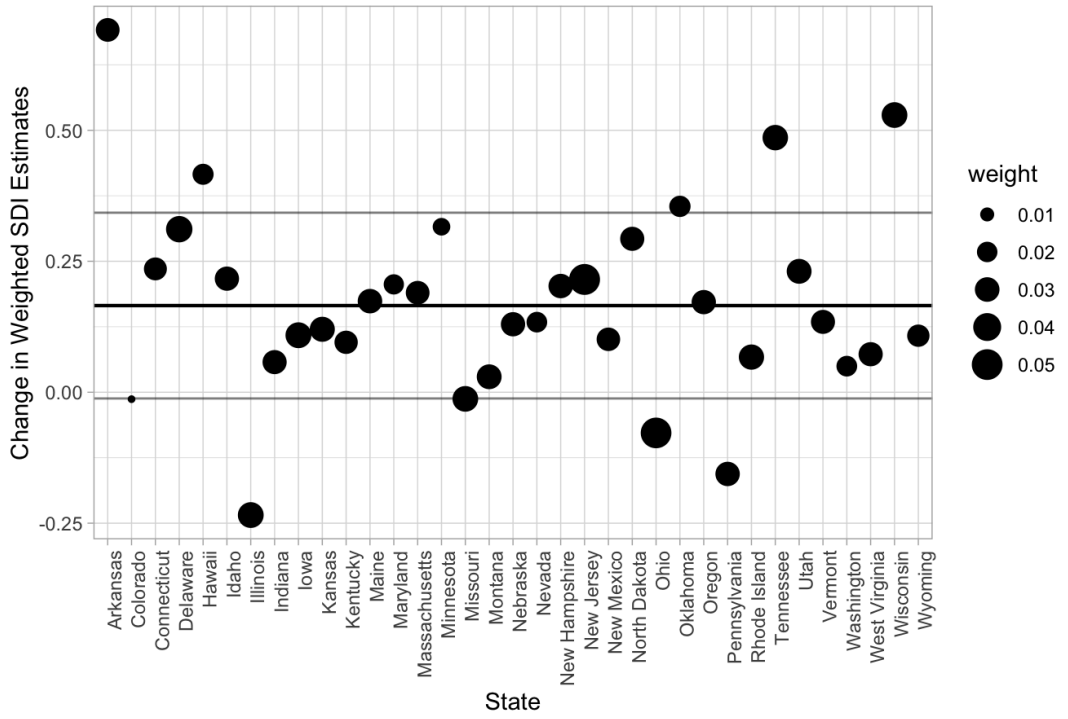


FIGURE B1: ESTIMATED TREATMENT EFFECTS WITH ALL ASSOCIATED SYNTHETIC CONTROL UNIT WEIGHTS FOR MODEL 1 (FULLY COVERED STATES)

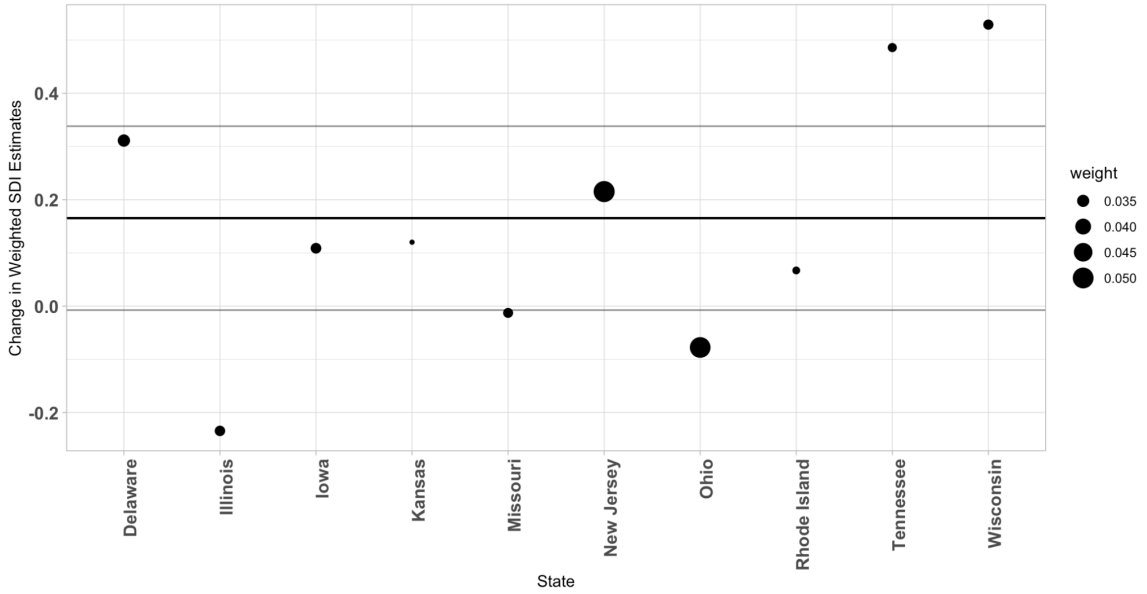


FIGURE B2: ESTIMATED TREATMENT EFFECTS WITH TOP TEN HIGHEST WEIGHTED SYNTHETIC CONTROL UNITS FOR MODEL 1 (FULLY COVERED STATES)

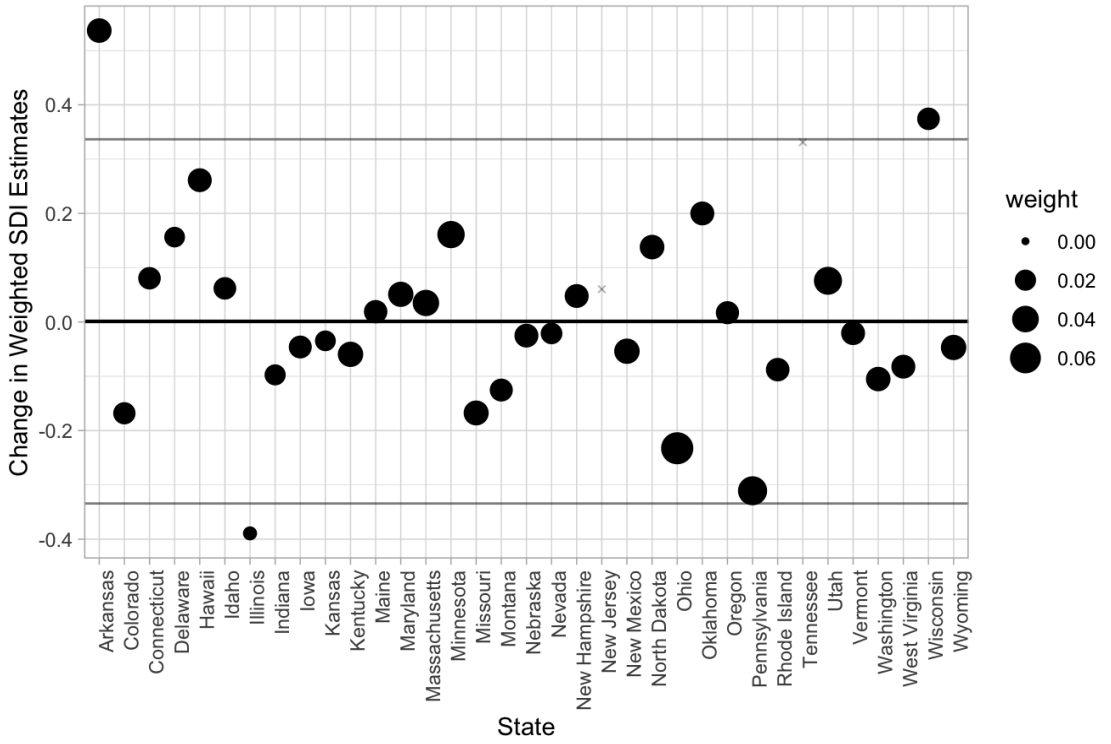


FIGURE B3: ESTIMATED TREATMENT EFFECTS WITH ASSOCIATED SYNTHETIC CONTROL UNIT WEIGHTS FOR MODEL 1 (FULLY COVERED STATES)

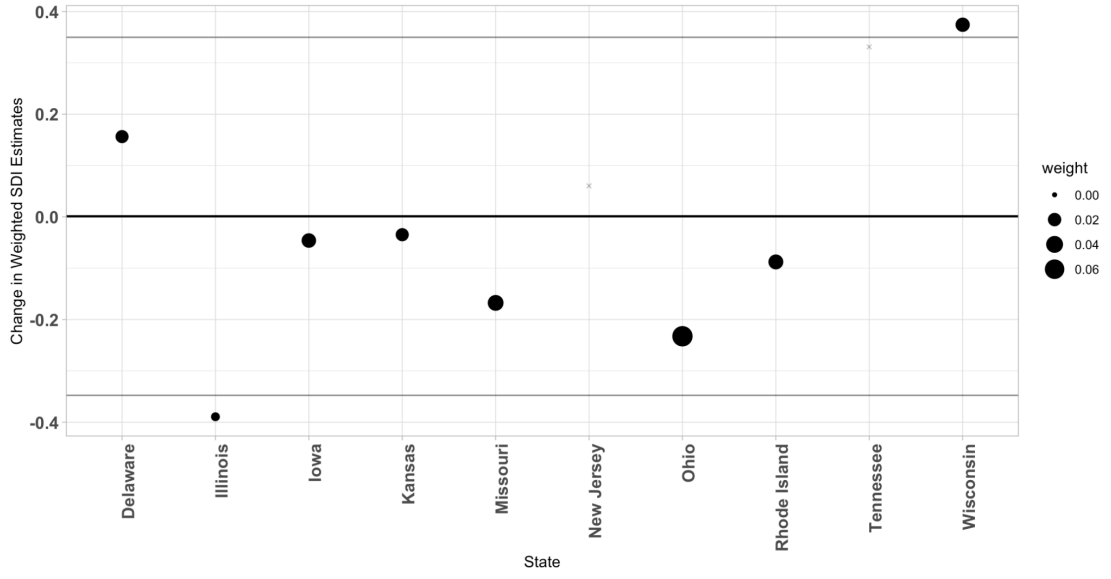


FIGURE B4: ESTIMATED TREATMENT EFFECTS WITH TOP TEN HIGHEST WEIGHTED SYNTHETIC CONTROL UNITS FOR MODEL 2 (PARTIALLY COVERED STATES)

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