Does Appointing Election Officials Produce Better Election Administration? Evidence from Georgia and Texas*

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Abstract

Does appointing rather than directly electing local election officials produce higher quality administrators and better-run elections? Given the technical and bureaucratic nature of election administration, as well as the low-information environment of local elected office, appointing election officials could increase both the quality of the pool of candidates and the accountability mechanism ensuring they do a good job. I leverage over-time variation in Georgia and Texas county election administration and a difference-in-difference design to credibly test the causal effects of appointing rather than electing local election officials on citizen participation. Appointed officials out-perform their elected counterparts, increasing voter turnout by 1.5 percentage points and raising registration rates as well. Additionally, officials directly elected in partisan contests appear to introduce a partisan slant to election results. These findings add to a nascent literature on the limits of elections in ensuring accountable officeholders and inform ongoing policy debates over local election administration in the US.

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1 Introduction

Unlike any other Western democracy, the US relies on a large number of autonomous local officials to conduct our elections (Hale, Montjoy, and Brown 2015). Many of these officials are directly elected with open partisan affiliations (Kimball and Kropf 2006). While direct elections offer citizens an opportunity to weigh in on how these local officials conduct elections, citizens may not have the right information to measure performance in low-information environments, and people skilled at administering elections may not be those most equipped to win elections. Furthermore, adverse incentives for elected partisan administrators may encourage some to intentionally seek lower participation rates, whereas the nature of appointed election officials may translate into increased resource investment into election administration. Do elections produce higher quality local election officials who, in turn, ensure smoother election administration and higher electoral participation? Or are appointed local election officials better equipped for the job?

One hurdle to answering this question is the fact that counties that elect local election administrators might differ systematically from counties that do not for a host of reasons beyond the selection method of the election official and in ways that are likely to affect participation rates. This means a simple cross-section comparison of counties that appoint and those that directly elect election officials will fail to uncover a causally identifiable effect. Another issue, common to all studies that seek to examine the effects of election administration or election laws on participation, is the fact that over time differences in voter turnout are largely due to when elections are held and who the candidates are. This makes studying the effects of statewide shifts difficult in the absence of within-state counterfactuals (Thompson et al. 2020).

I overcome both of these issues with a difference-in-differences research design that leverages over time county-level variation in election official selection method. To do so, I build a new dataset of changes in local election official selection in Georgia and Texas from 1964 to 2020. These two states have undergone staggered, widespread changes in their method of election official selection, with counties generally shifting from direct elections to bureaucratic appointments. I find that appointed local election officials increase voter turnout rates by 1.5 percentage points, relative to directly elected officials, and may increase registration rates as well. Additionally, those officials

directly elected with partisan affiliations appear to introduce a partisan slant to election results. In sum, it appears that appointed election officials make for superior administrators.

2 Selecting Local Election Officials

The United States is unique for its highly fragmented system of election administration (Hale, Montjoy, and Brown 2015). Despite a preference among citizens for elected nonpartisan boards of elections (Alvarez, Hall, and Llewellyn 2008), 61 percent of jurisdictions representing 45 percent of all voters elect an individual election official, while 46 percent of all jurisdictions representing nearly half of all voters have a Republican or Democratic-affiliated officials (Kimball and Kropf 2006). I first consider why we might expect appointed election officials to be better election administrators than elected officials, and then examine the specific cases of Georgia and Texas.

2.1 Why Might Appointments Produce Better Election Administration?

According to political economy theories of governance, elections allow voters to select higher-quality politicians and ensure their accountability to the electorate through the sanctioning mechanism of reelection (Besley 2006; Fearon 1999). In some empirical contexts, it appears that elections do achieve these goals, producing officials who are more competent, on average, than the constituents they represent (Dal Bó et al. 2017) and who work harder when they have the incentive of reelection (Alt, Bueno de Mesquita, and Rose 2011; Ferraz and Finan 2011; Fouirnaies and Hall 2018). In other contexts, however, elections may fail to achieve accountability, lowering the quality of the pool of candidates, creating weak accountability mechanisms, and producing adverse incentives.

First, elections alter the pool of candidates by selecting for those willing to run for office (Hall 2019). The skills that make someone a good politician may not align closely with the factors that make someone a good election official. If this is the case, then the election process itself may select out higher-quality candidates, simply due to the barrier to entry. Elected candidates must typically live within the county, whereas appointed administrators can be chosen from a broader geographic pool. Election administration has grown to be an increasing technical and expertise-driven endeavor, lending credence to the idea that appointed experts may be of higher average quality than elected officials.

Second, low-information environments can prevent elections from providing effective accountability mechanisms (Ashworth and Bueno de Mesquita 2008; Berry and Howell 2007; Besley 2006; Lim and Snyder 2010; Snyder Jr. and Strömberg 2010). Voters may be unable to select good candidates in the first place, as well as distinguish between highly and poorly performing election officials, leading them to rubber stamp whoever is in office. Compounding the problem is fact that elected election officials frequently have a portfolio of non-election responsibilities and unintuitive titles that further dilute the ability of voters to effectively monitor and sanction their performance.[1] A related issue is the frequent lack of competitive elections for these offices altogether. Whereas long tenures and few challengers could be a sign of voter contentment with the officeholder, it could alternatively mean a breakdown of the accountability mechanisms essential to ensuring good performance (Besley 2006). Appointments for election administrators are typically made by county elections boards. These entities likely have access to higher-quality information than the average voter, and may therefore be better able to select strong candidates for the job and effectively monitor them. County elections boards also are frequently comprised of elected politicians such as county officers, local party chairs, and county supervisors, all of whom have incentives to appoint qualified election officials in order to satisfy the electorate and maximize their own chances of reelection.

Third, elections may produce adverse incentives for officeholders to produce undesirable outcomes for political reasons (Sances 2016). Detached from partisan realities, a "responsive" local election official might try to minimize the costs of voting, such as by ensuring sufficient polling places and early voting opportunities, adequately trained and competent poll workers, and conducting robust information outreach. However, if political outcomes achieved by reducing participation is preferred by a majority of the presently voting electorate, then elected officials with partisan affiliations could be incentivized to concentrate costs on certain voters or discourage voting across the board, producing normatively adverse outcomes. This possibility seems most likely for officials directly elected with partisan affiliations and least likely for officials appointed by bipartisan election boards.

[1] Examples include probate judge in Alabama and Georgia; auditor in Iowa, South Dakota and Washington; and tax assessor in some Texas counties.

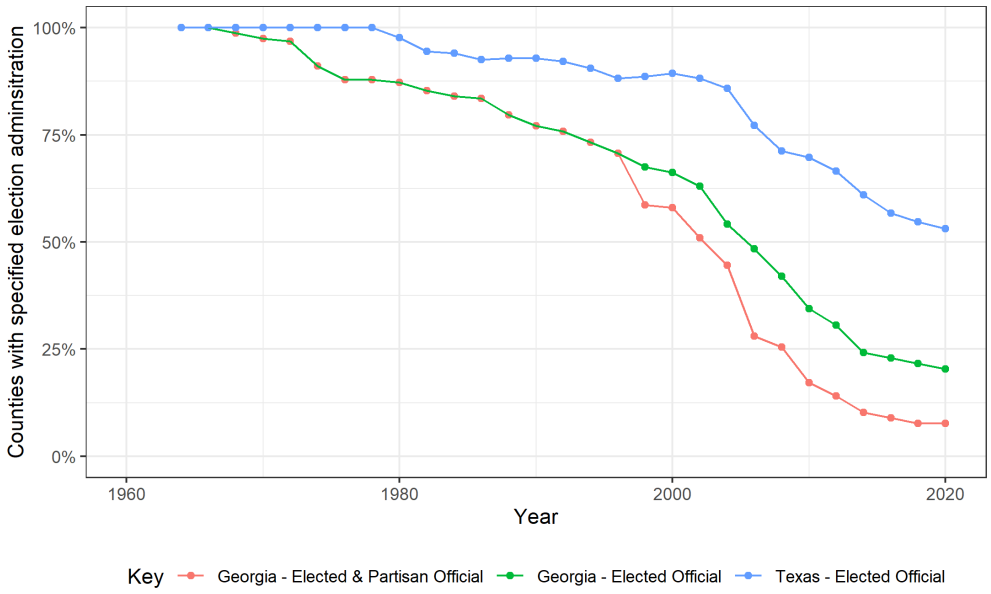
An additional reason why appointed election officials might outperform their elected counterparts is that they may have more time to dedicate to running elections. Appointed elections officials' sole job is to effectively administer elections. In comparison, most directly elected local election officials in the US undertake additional responsibilities beyond election administration. For instance, county clerks, the most common elected officers to administer local elections, typically have a variety of non-election duties such as maintaining legislative and judicial records and recording vital documents. Other shared elected election offices, such as tax assessors (used in South Dakota and some Texas counties) and probate judges (used in Alabama and Georgia) have more substantial non-election duties. This resource difference is likely to be most actuate in less populous counties, where sometimes only a single official administers elections.

Few studies have empirically examined differences between elected and appointed local election officials. In an experimental audit study of constituent communication rates, elected and appointed officials responded at similar rates (White, Nathan, and Faller 2015). On the other hand, a study of Wisconsin election officials found some evidence of preference diverge between elected and appointed clerks (Burden et al. 2013). Additionally, for appointed officials, voter turnout was lower among Republican clerks in Democratic electorates compared with appointed Democratic clerks in these electorates.

2.2 Selecting Local Election Officials in Georgia and Texas

Georgia and Texas present a unique opportunity to test the effects of electing versus appointing election officials on the quality of election administration. Historically, all counties in both states directly elected a partisan official who was tasked with overseeing voter administration—the probate judge in Georgia and the county clerk, district & county clerk, or tax assessor-collector in Texas. Beginning in 1967 in Georgia and 1979 in Texas, some counties began switching their local election official to an appointed, nonpartisan entity (a Board of Elections in Georgia and an individual Elections Administrator in Texas). Georgia introduced additional flexibility in the 1980s, allowing counties counties to establish nonpartisan probate judge elections. In both states, switches occurred in a staggered, irregular, and in some cases back-and-forth manner. The changes are more widespread in Georgia and have accelerated since 2000, to the point where only about

Figure 1: **Georgia and Texas Local Election Administration, 1964-2020**. This graph displays over time change in the selection method of local election officials in Georgia and Texas. Proportions are relative to the number of counties in each state—254 in Texas, 159 in Georgia. In both states, all appointed local election officials are nonpartisan. In Texas, all elected election officials are partisan, whereas in Georgia elected probate judges can be either partisan or nonpartisan.



half of counties in Texas and 1 in 5 counties in Georgia continue to task an elected official with voter administration responsibilities. These changes are illustrated in Figure 1

As in many other states, elected local election officials in Georgia and Texas are entrusted with broad statutory authority to administer elections (Ferrer, Geyn, and Thompson 2021) For instance, probate judges in Georgia determine precinct divisions, handle nomination petitions of candidates, publish notices and advertisements of elections, select and equip polling places, purchase and maintain election equipment, conduct early in-person voting, appoint and train poll officers, inspect the conduct of elections, receive and certify election results, prepare a budget estimate and appropriations request, conduct hearings to determine the eligibility of candidates, and administer photo ID provisions. Some Texas clerks also handle registration administration and voter list maintenance duties, although Georgia probate judges do not.[2] In short, these officials have responsibilities that

[2]All Georgia registration administration is undertaken by appointed registration boards. Texas county registrars are usually the tax assessor-collector, but may instead be the county clerk, district & county clerk, or in rare cases the county sheriff. All appointed election administrators in Texas counties handle both election and registration administration.

are substantial enough to suspect that variation in administrator quality could affect downstream aggregate outcomes such as turnout and registration rates.[3]

Georgia and Texas are both battleground election states with significant ongoing policy debates over election laws. Furthermore, counties in both states continue to actively consider changes to election official selection. This study thus contributes to ongoing debates about the optimal form of local election administration.

3 Data and Methods

3.1 Measuring the Selection Method of Local Election Officials in Georgia and Texas

I self-construct panel data on the selection method of local election officials in Georgia and Texas counties from 1964 to the present. In Georgia, each county shift between elected and appointed officials and between nonpartisan and partisan probate judge elections necessitated the passage of state legislation. I collect data on Georgia election administration type from three sources: the Digital Library of Georgia (1964–1999), Georgia Government Publications (1999–2001) and the Georgia General Assembly (2001–present). Prior to 1982, implementing legislation specified population bands rather than county names. Data from the U.S. Census was used to reconstruct which counties the implementing legislation affected.

Texas election administration changes are made by County Commissioner's Courts, in coordination with the affected statutory officers. Data on these changes is collected through two methods. First, I web scraped lists of each county's local election official provided on the Texas Secretary of State's website and archived on the WayBackMachine.[4] This provides yearly panel data between 2000 and 2020. Second, I made an Open Records Request to the Office of the Texas Secretary of State for documentation of all changes in county election administration between 1979 and 2020. The overlapping component of these records showed nearly identical alignment, though the State's documentation was notably incomplete. I use a combined dataset privileging the web scraped data

[3]In Georgia, probate judges who participate in a contested election are temporarily relieved of their duties by a three-person Board of Elections. In practice, this provision affects only a small percentage of elections because (1) only contested partisan probate judge general elections take place in November and (2) these races are rarely contested.
[4]https://www.sos.state.tx.us/elections/voter/county.shtml

for any discrepancies in the main analysis, and a combined dataset privileging the Open Records Request documentation in Section A.2.

3.2 Data

Presidential, gubernatorial, and senate races are used to measure election outcomes. Data on county-level vote totals for comes from Congressional Quarterly and David Leip's US Election Atlas. It spans from 1968 to 2020 for vote share and from 1970 to 2020 for turnout. Data on registration totals is from Leip's Election Atlas and includes presidential elections from 1996 and gubernatorial elections from 2004. Voting age population, the denominator in turnout and registration measurements, is from the U.S. Census, and is available from 1970. Turnout rate is measured as total votes for highest office by the voting age population. Registration rate is measured by dividing total registrants by the voting age population. Democratic vote share is measured as votes for the top-ticket Democratic candidate divided by votes for the top-ticket Democratic and Republican candidates.

I assemble a set of indicators of election administration policy using the US Election Assistance Commission's Election Administration and Voting Surveys (EAVS) from 2004 to 2020. This survey measures outcomes in every even-year general election for each county. I use this survey to measure the number of polling places, provisional ballots cast, provisional ballots rejected, absentee ballots rejected, and the number of registrants removed from the voter roll. Additionally, following Pettigrew (2017), I use data from the Cooperative Congressional Election Study to measure the share of voters who had to wait at the polls for more than 30 minutes for each county. This is available for general elections in 2006, 2008, and 2012–2018.

3.3 Design

Estimating the effect of local election administrator selection is difficult because counties that appoint officials likely differ from those that elect officials. Additionally, most year-to-year changes in turnout and participation are attributable to idiosyncratic factors such as candidates, election timing, and the public mood. I overcome both of these issues with a difference-in-differences research design. I leverage over time county-level variation in election official selection method in Georgia and Texas along with administrative datasets of voter turnout and registration rates. This

allows me to credibly test the causal effects of appointing versus electing election officials on citizen participation. With 254 and 159 counties, respectively, Texas and Georgia each have more counties than any other state, providing an ideal environment to maximize the power of the analysis.

I estimate the regression $Y_{it} = \alpha_i + \delta_t + \beta Appointed_{it} + \epsilon_{it}$, where Y_{it} is a measure of voter turnout in county i at election year t, α_i and δ_t are county and year fixed effects, respectively, $Appointed_{it}$ is a dummy variable taking 1 when counties appoint their local election official and 0 when counties elect their local election official, and β the causal effect of an appointed election official on voter turnout. In some specification, registration rate and democratic vote share are substituted for voter turnout. In others, the effects of partisanship are incorporated through an additional dummy variable, $\gamma Partisan_{it}$. Because all appointed election officials are also nonpartisan, this dummy variable measures the interaction of elected and partisan officials. In this case, the omitted category is elected nonpartisan officials.

The difference-in-differences design controls for all time invariant factors specific to each county, such as demographic and socioeconomic composition. It also controls for all time varying factors that vary in the same way for both counties that elect and those that appoint local election officials—for instance, across-the-board increases in turnout in the 2020 Presidential Election. It does not control for time varying factors that vary differently in counties with appointed election officials and those with directly elected officials. It is possible, for example, that counties that switch to appointed officials are actually growing at more rapid rates than those that stay with elected officials, and that turnout is trending down as a result.

To address these so-called "time-varying confounders", I incorporate Year by Democratic vote share and Year by Population fixed effects. The Year by Democratic vote share fixed effect compares within-county over time change to other counties with similar partisan makeup, whereas the Year by Population fixed effect compares within-county overtime change to other counties with similar populations. These account for the possibility that counties that switched their election administration may have also happened to shift either population or partisan trends in ways that are systematically related to turnout, registration, and/or Democratic vote share. Both population and partisanship are likely confounders, as counties that become more populous are more likely to change their election administration and the decision may also correlate with local partisanship.

These interacted fixed effects are each divided into quartiles and are measured pretreatment for each state.[5]

4 Results

In this section, I present evidence that appointing rather than electing local election officials results in higher quality administration in the form of increased turnout and registration rates. I then validate these findings using a range of alternative estimators and testing for anticipatory effects.

4.1 Appointing Election Officials Increases Citizen Participation

Table 1 displays the results of a two-way fixed effects regression estimating the effects of appointing rather than electing a local election official on citizen participation. Columns 1 through 3 test the effects on votes per voting-age resident, whereas columns 4 through 6 test the effects on registrants per voting-age resident. Both are measured as proportions out of 1. The coefficients can be interpreted as a percentage point difference in turnout and registration rates. All six regressions include county and year by state fixed effects. This means that comparisons are made on within-county changes in participation, relative to changes in other counties in the same year. The year by state interacted fixed effect makes within-county comparisons to other counties in the same year and state. This ensures that differential participation trends between Georgia and Texas are not driving the results. In this and all other regression specifications reported, robust standard errors are clustered by county.

Column 1 shows that counties switching from directly elected to appointed election officials see an average increase in presidential voter turnout of 1.8 percentage points, relative to counties that did not switch in the same year and state. The point estimate is precisely estimated, allowing us to confidently rule out effect sizes less than 1 percentage point.

It could be the case that counties with similar partisan compositions were on the same participation trajectory prior to their shift in administrator selection method. In other words, the result could be confounded by time-varying shifts in county-level participation that correlate both with

[5]For Georgia, population is measured using the 1960 census and partisanship is measured using Democratic vote share in the 1968 presidential election. For Texas, population is measured using 1970 census figures and partisanship is measured using Democratic vote share in the 1976 presidential election.

Table 1: Appointing Local Election Officials in Georgia and Texas Increases Citizen Participation (Presidential Elections, 1964-2020)

	Voter Turnout			Registration Rate		
	(1)	(2)	(3)	(4)	(5)	(6)
Appointed	0.018	0.017	0.015	0.010	0.012	0.010
	(0.004)	(0.004)	(0.004)	(0.006)	(0.006)	(0.006)
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year x State FEs	Yes	No	No	Yes	No	No
Year x State x Dem vote share FEs	No	Yes	No	No	Yes	No
Year x State x Population FEs	No	No	Yes	No	No	Yes
Observations	4,845	4,845	4,845	2,877	2,877	2,877

Robust standard errors clustered by county in parentheses.

partisanship and shifts to appointed election administration. I introduce year by state by Democratic vote share fixed effects to alleviate these concerns. In this column, differences in within-county turnout shifts are only made between counties in the same state, year, and partisan makeup. The result is nearly identical under this estimation strategy. Similarly, the inclusion of year by state by population fixed effects in column 3 makes comparisons between counties of similar sizes within the same state. The result is again similar.

It also appears that appointed election administrators oversee elections with higher registration rates. While the estimates are slightly noisier than those of turnout due to a shorter period of data availability and the nature of registration administration in Georgia and Texas, the coefficients are 1 percentage point in magnitude or higher, a substantively meaningful effect.[6] A null of no difference can be confidently ruled out in one of the three estimators.

This table provides strong evidence that appointed local election officials in Georgia and Texas increase citizen participation in presidential elections, relative to their directly elected counterparts. Regressions including Senate and Gubernatorial contests, shown in A.1, yields substantively identical findings. Table A.2 shows the results are also robust to the use of different criteria in constructing the election official selection data.

[6]This estimator is likely biased downward due to the fact that registration administration in Georgia has been conducted by appointed registration boards throughout the period of analysis.

4.2 Validating the Effect of Appointing Election Officials on Voter Turnout

In this section, I validate my main finding that appointed local election officials in Georgia and Texas produce higher presidential voter turnout than directly elected officials. I utilize a range of alternative difference-in-difference estimators and test the validity of the parallel trends assumption by examining anticipatory effects. I also employ a generalized synthetic control method, which relaxes the assumptions for needed for causal inference. These tests show the results to be robust to a range of specifications.

4.2.1 Validating the Staggered Rollout Design

Recent scholarship has identified potential problems with the standard generalized two-way fixed effects design that arise from nonuniform treatment effects (Borusyak, Jaravel, and Spiess 2021; Goodman-Bacon 2021; de Chaisemartin and D'Haultfœuille 2020; Callaway and Sant'Anna 2021; Sun and Abraham 2021). These issues stem from heterogeneous treatment effects and the staggered adoption of treatment across multiple time periods and units. If treatment effects vary across time or units, the comparisons made in the estimate will be biased due to the assignment of negative weights to some treatment effects. This is because under a staggered adoption design, units that switch early on from control to treatment are treated as controls in some comparisons and subtracted from the difference-in-difference estimand, even if they continue to experience dynamic treatment effects.[7]

Table 2 displays results from several alternate estimators that have been proposed to overcome the methodological issues of the generalized two-way fixed effects design. Due to limitations in utilizing some of these estimators and to ensure comparability, I drop year by state fixed effects and only include year and state fixed effects for all specifications.

Column 1 displays the results from a regular two-way fixed effect estimator similar to the one used in column 1 of Table 1, with county and year fixed effects are employed rather than county and year by state fixed effects. The second column displays results from the de Chaisemartin and D'Haultfœuille (2020) estimator, which is designed to be robust to heterogeneous treatment effects.

[7] I use the the method proposed by de Chaisemartin and D'Haultfœuille (2020) to diagnose the degree to which my effects estimates are robust to treatment effect heterogeneity. It appears there is some cause for concern. Using the regression specification shown in column 1 of Table 1, 39% of assigned weights have negative values, summing to -0.472. It is therefore important to validate my findings with other estimators that correct for this bias.

Table 2: Finding that Appointing Local Election Officials Increases Voter Turnout is Robust to Alternate Estimators

	Two-way FE	de Chaisemartin & D'Haultfoeuille	Callaway & Sant'Anna	Sun & Abraham
	(1)	(2)	(3)	(4)
Appointed	0.0328	0.022	0.054	0.025
	(0.005)	(0.003)	(0.011)	(0.017)
County FEs	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
Observations	4845	4271	4747	4747

Robust standard errors clustered by county in parentheses.

This regression specification employs dynamic effects with placebos. Column 3 displays results from the Callaway and Sant'Anna (2021) dynamic effects estimator and column 4 displays results from the Sun and Abraham (2021) estimator. Both approaches aggregate counties into cohorts that begin treatment at the same time, creating a "stacked difference-in-differences".[8] Doing so avoids negative weights, thereby addressing the weighting problems of the simple two-way fixed effects estimator.

Across all four estimators, we see similar or more substantial effects of appointing election officials as those reported in Table 1. Estimates range from 2 to 5 percentage points, indicating substantively meaningful effect sizes. We can confidently rule out a null of no difference in three of the four regressions. In short, my findings are robust to a range of alternative specifications.

4.2.2 Validating the Parallel Trends Assumption

The difference-in-differences design depends on an assumption of parallel trends in order to be causally credible. In other words, the observed effect is assumed to be due to the effect of treatment (i.e., a switch in the form of local election official selection) rather than differing trends in counties that switched their election administration from those that did not. This is an important

[8]First, always treated units are removed from the dataset. In practice, this only eliminates a handful of counties that were extremely early adopters of appointed election administrators. Next, each county's time period of first treatment is identified. The few counties that switch back are assigned positive treatments even after their reversal. Finally, those counties that are never treated (i.e., continue to have directly elected election officials) are separated out as the "true control" by which each cohort can be compared with.

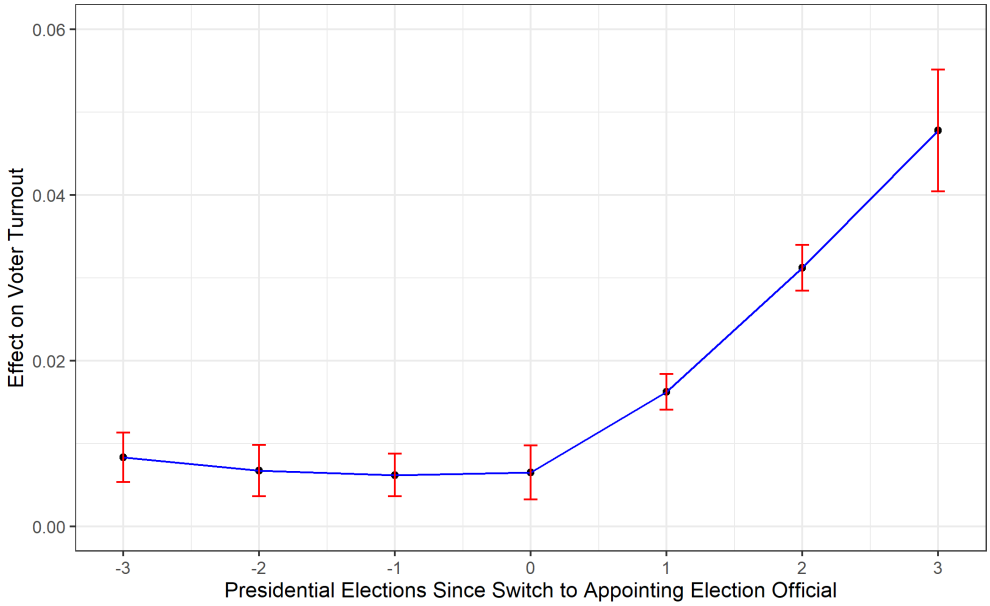
assumption to examine, because there are theoretically grounded reasons for expecting counties that switch from elected to appointed officials to already differ in time-varying ways that lead them to have different participation rates. For instance, counties growing in population could decide to switch to an appointed official in order to professionalize their election administration, at the same time that turnout is on the decline due to the population trend.

I test this assumption by incorporating lags and leads into the fixed effects estimator. I use a regression of the functional form $Y_{it} = \alpha_i + \delta_t + \beta_{-3}Appointed_{t+3} + \beta_{-2}Appointed_{t+2} + \beta_{t-1}Appointed_{t+1} + \beta_0 Appointed_0 + \beta_A ppointed_{t-1} + \beta_A ppointed_{t-2} + \beta_A ppointed_{t-3} + \epsilon_{it}$, where lead coefficients β_{-1}, β_{-2} (etc.) test for anticipation effects prior to the switch in local election official selection, β_0 tests for the immediate effect of the switch, and lag coefficients β_1, β_2 (etc.) test of long-run effects of the switch.

Figure 2 plots the results of a dynamic de Chaisemartin and D'Haultfœuille (2020) estimator with placebos, employing the same principles as the above estimator but correcting for two-way fixed effects bias as discussed in Section 4.2.1. The x-axis marks the presidential elections before and after a switch in local administration, with a switch occurring right before the 0-point election. Each point estimate is the difference in the change in turnout from the previous election of counties with appointed election officials rather than elected ones, at x presidential elections before or after each county's actual switch. Positive significant coefficients in the left half of the graph suggest modest evidence of anticipatory effects. In other words, it appears that counties that switched to appointing clerks were already experiencing higher turnout rates. However, these coefficients are relatively small and constant. On the other hand, there appears to be a strong dynamic effect on turnout after counties have switched their method of election official selection. The right half of the graph shows a steep linear increasing trend that begins after a county has switched to appointed administrators, evidence of potential learning effects for appointed administrators. This potential is explored further in Section 5.1.[9]

[9]I also test pretrending using a series of counterfactual estimators from (Liu, Wang, and Xu 2021) and display the output in Section A.3 in the Online Appendix. Three methods are tested: two-way fixed effects, interactive fixed effects, and matrix completion. All three methods display substantial and statistically significant treatment effects of appointing local election officials on turnout. However, the two-way fixed effects and matrix completion estimators show some pretrending concerns. These are attenuated somewhat in the interactive fixed effects figure. Finally, there appears to be no pretrending issues in a counterfactual plot of registration rates.

Figure 2: **de Chaisemartin and D'Haultfœuille (2020) Dynamic Heterogeneous-Robust Estimator of Effect of Appointing an Election Official on Voter Turnout**. Year 0 is the presidential election after a county's first switch from electing to appointing an election official. Each point is the estimated effect of appointing an election official on presidential voter turnout, at x presidential elections of exposure since first selecting the official via appointment. The bar lines above and below each point represent 95-percent confidence intervals. Estimates are from the de Chaisemartin and D'Haultfœuille (2020) estimator for dynamic heterogeneous-robust difference-in-difference designs with placebos, which corrects for bias due to heterogeneity in year and county treatment effects.

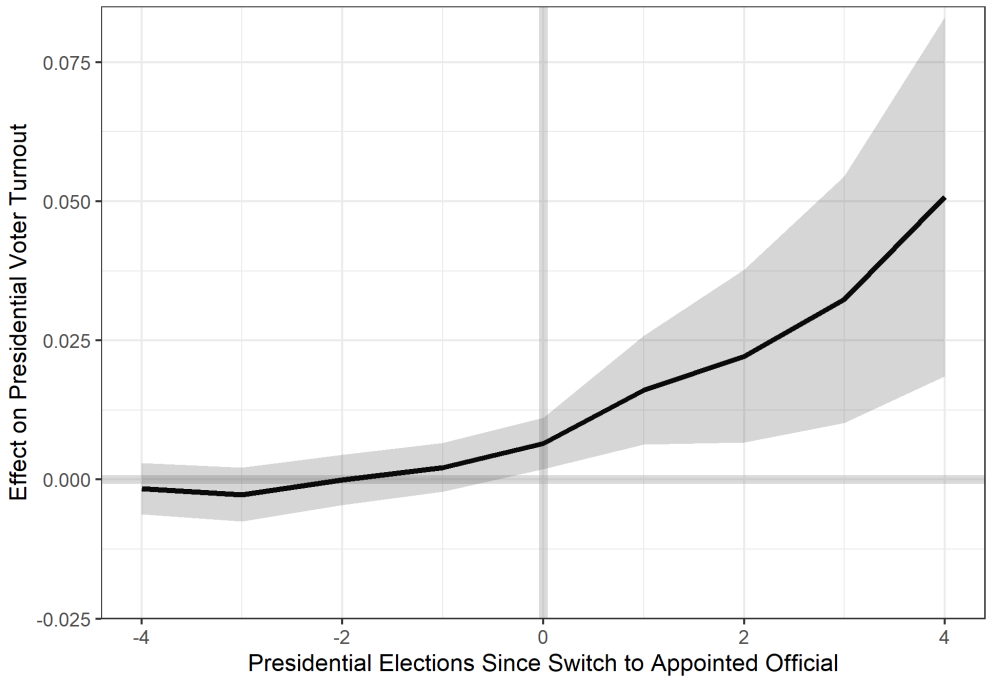


An underlying concern of the difference-in-difference estimation strategy is that treated and control units do not look like one another. In other words, if the places that switch from electing to appointing election officials are fundamentally different on some latent or difficult to observe characteristics, then this will undermine the causal validity of the regression specification. One way to overcome this concern and relax the parallel trends assumption is through the generalized synthetic control method. This method rebalances the data sample by matching treated and untreated units to ensure that treated units look like control units pre-treatment.

Figure 3 displays output from the Xu (2017) generalized synthetic control estimator, measuring the average effect of treatment on the treated units. This figure shows a statistically significant positive divergence from zero soon after counties switch to appointed local election administrators. In other words, it appears that once the sample is rebalanced to eliminate concerns about pretrend-

ing, a significant effect on turnout continues to be observed. This provides strong evidence that appointed election officials administer elections with higher turnout than their elected counterparts.

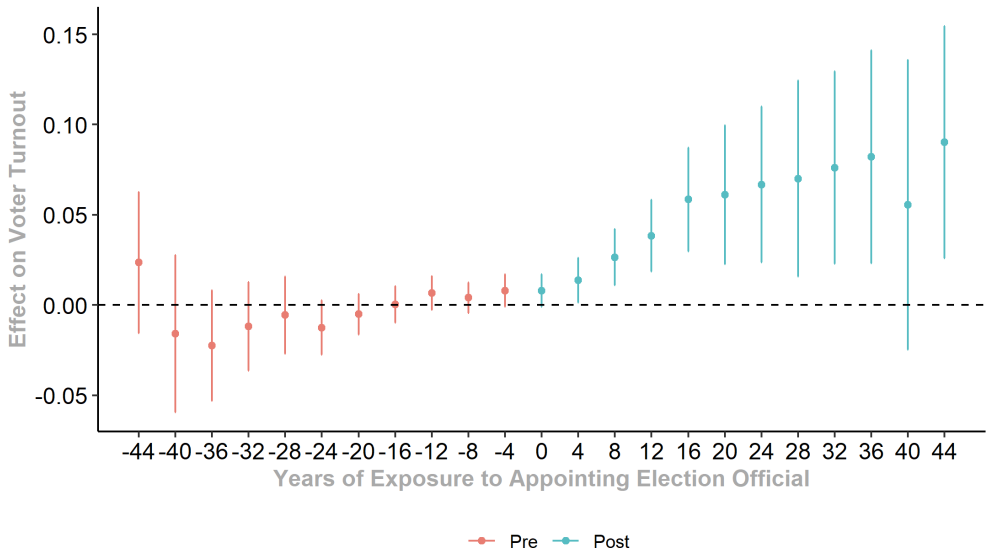
Figure 3: **Estimated ATT of Generalized Synthetic Control**. This graph displays a generalized synthetic control method of the two-way fixed effects regression testing the effects of appointing local election officials on presidential turnout. The specification includes two-way additive county and year fixed effects, a cross-validation procedure to select the number of unobserved factors within the interval of 0 and 2 presidential elections, and a parametric bootstrap procedure with 1000 samples. The black line is a dynamic estimated ATT effect of appointing an election official on turnout and the band is a 95% confidence interval.



5 When and Why Does Appointing Election Officials Increase Citizen Participation?

In this section, I explore treatment heterogeneity, state and administrator office-specific effects, the effects of partisan election administration, the impact of the Voting Rights Act, and whether appointed and elected election officials pursue different election administration policies.

Figure 4: **Average Effect of Appointed Election Officials on Voter Turnout by Length of Exposure to Appointing**. Year 0 is the presidential election after a county's first switch from electing to appointing an election official. Each point is the estimated effect of appointing an election official on presidential voter turnout, at x years of exposure since first selecting the official via appointment. The lines above and below each point represent 95-percent confidence intervals. Red points indicate pre-treatment effects, blue points indicate treatment effects. Estimates are from the Callaway and Sant'Anna (2021) estimator for dynamic two-way fixed effects designs, which corrects for bias due to the staggered rollout of county switches to appointed election officials.



5.1 Examining Dynamic, Group, and Time Period Effects of Appointing Election Officials

I use specifications from the Callaway and Sant'Anna (2021) estimator to examine dynamic, cohort, and time period effects of switching from elected to appointed election officials on presidential voter turnout. These are visualized in Figures 4, 5, and 6, respectively.

As seen in Figure 2, there appears to be significantly increasing improvements to voter turnout over time, relative to counties with elected officials, after the switch to appointed administration. Appointed officials do an better job of producing higher turnout, relative to their elected counterparts, as the tenure of their selection mechanism extends. Figure 4 provides additional evidence for this observation. This could be due to some combination of institutional learning effects and start-up costs, whereby appointed officials need the practice of administering a few elections to realize their full potential quality differential with elected counterparts.

Figure 5: **Average Effect of Appointed Election Officials on Voter Turnout by Cohort Group**. Each point is an estimate of the average group effect of appointing election officials on presidential voter turnout for counties that switch in the given cohort year. The lines above and below each point represent 95-percent confidence intervals. Estimates are from the Callaway and Sant'Anna (2021) estimator for dynamic two-way fixed effects designs, which corrects for bias due to the staggered rollout of county switches to appointed election officials.

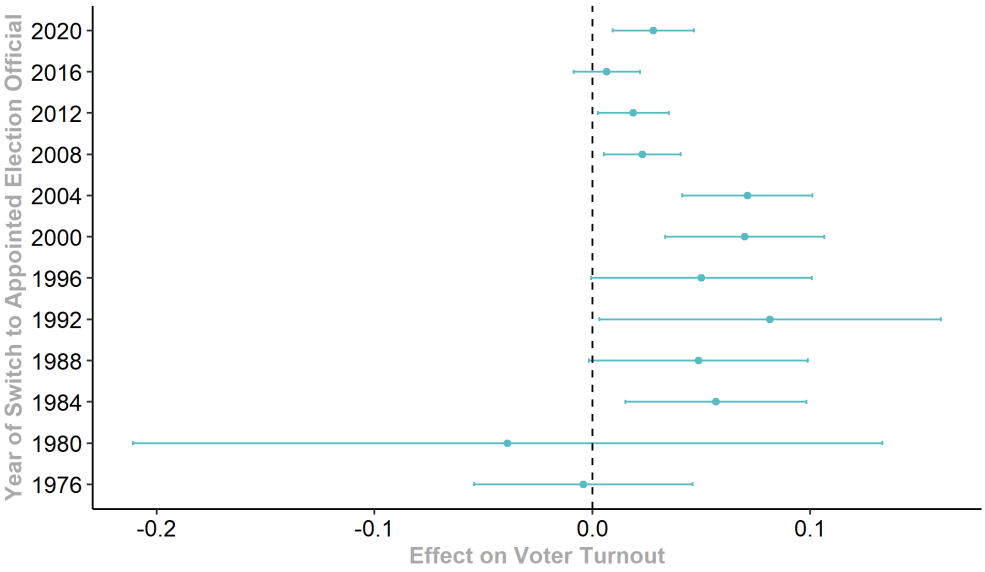
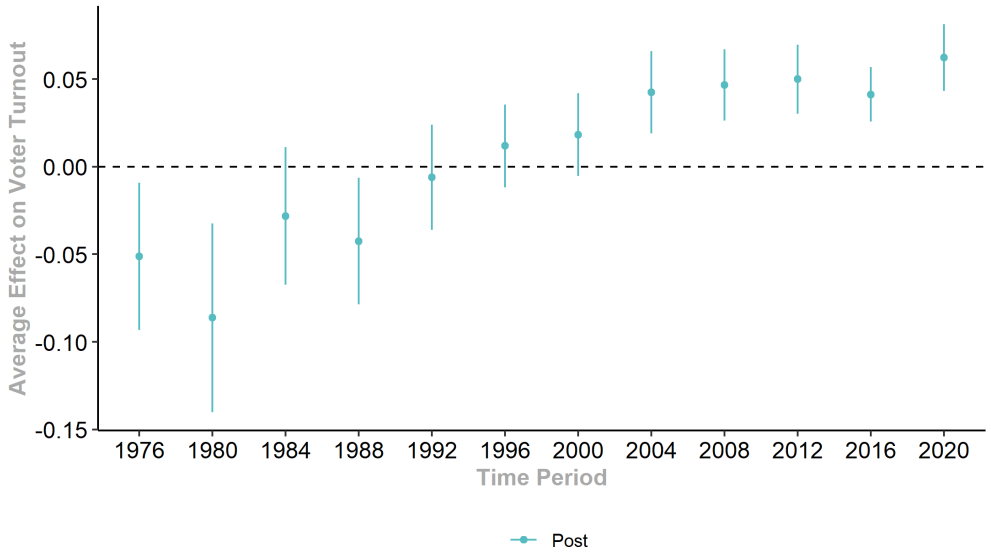


Figure 5 suggests that earlier adopters of appointed election officials have experienced stronger overall treatment effects than more recent adopters, whereas Figure 6 suggests that this is due to long-term accumulation rather than a diminishing effect over time. In fact, in more recent decades the positive effects of appointed election officials on turnout has, if anything, increased. This could be interpreted as more evidence of long-run dynamic gains in having appointed rather than elected officials run elections.

5.2 Exploring State and Office Heterogeneity

How generalizable is the main result? One test is to see if the effects of appointing election officials on citizen participation holds after splitting the data by state. I do so in Table A.3. The results reveal significant effects in both states. Interestingly, it appears switching to appointed election officials in Georgia has an effect on turnout nearly double the size than in Texas—2.4 percentage points versus 1.3 percentage points. In both states, a null of no effects can be confidently rejected. The impact of appointing election officials on registration rates is comparable in both counties as

Figure 6: **Average Effect of Appointed Election Officials on Voter Turnout by Time Period**. Each point is an estimate of the average time period effect of appointing election officials on presidential voter turnout. The lines above and below each point represent 95-percent confidence intervals. Estimates are from the Callaway and Sant'Anna (2021) estimator for dynamic two-way fixed effects designs, which corrects for bias due to the staggered rollout of county switches to appointed election officials.



well, but is much more precisely estimated in Texas and reaches conventional levels of significance. This is consistent with the fact that only in Texas do switches to appointed officials also encompass registrars.

Another way to test generalizability is to examine the different elected statutory offices that administer elections. Most directly elected election officials across the United States are clerks. If the results hold for Texas clerks, this is another indication that they generalize to other states. In Texas, directly elected local election officials are usually county clerks but in some counties tax-assessors take up the role. There have also been within-county switches in election administration officer between clerk and tax-assessor, in addition to changes to appointed elections administrators. Table A.4 shows that clerks significantly reduce voter turnout relative to appointed officials. However, it appears tax assessors do not systematically underperform relative to appointed administrators.

In summary, the results hold across both states and for county clerks.

5.3 Do Elections or Partisan Elections Explain the Results?

Are the observed effects the result of a switch between elected and appointed local election officials? Or does the partisan nature of most elected officials explain these results? The results so far have largely masked two changes that have taken place in most administrator selection transitions: from elected to appointed officials, and from partisan officials to nonpartisan officials. It might be the case that the partisan nature of elected office drives officials to administer elections in specific ways that differ from their nonpartisan appointed counterparts—for instance, by attempting to alter turnout in a way that advantages their copartisans. There certainly is ground for believing in a partisan effect. Georgia and Texas's long histories of race-based disenfranchisement, the strong association between race and partisanship (Abramowitz and McCoy 2019; Carmines and Stimson 1989; Frymer 1999), and the present efforts of Republican politicians to increase barriers to the ballot box all contribute to the possibility that adverse policy responsiveness rather than quality differences could explain some of the divergence between appointed and elected election officials.[10]

As an initial test, I examine whether appointed and elected officials administer elections in ways that leads to different partisan outcomes. Table 3 displays the results of a two-way fixed effects regression estimating the effects of appointing a local election official on democratic presidential vote share. The point estimates are negative, suggesting that appointed election officials administer elections in ways that increase Republican party vote share, relative to elected officials. These results seem contradictory to the findings on higher turnout, which conventional wisdom holds favors Democrats (Lijphart 1997; Piven and Cloward 1988). However, the point estimates are small and a null of no partisan difference can only be rejected at conventional levels of significance in one of the three specifications.

Georgia's history of county-level staggered switches between elected partisan, elected nonpartisan, and appointed election officials affords an opportunity at disentangle these effects, allowing me to disentangle the effects of direct election and partisan elections. I estimate the specification $Y_{it} = \alpha_i + \delta_t + \beta Appointed_{it} + \gamma Partisan_{it} + \epsilon_{it}$, where *Appointed* is again a dummy variable

[10]Some studies have found differences in the way that Democratic and Republic elected officials facilitate voter turnout (Burden et al. 2013), purge voters from the electoral roll (Stuart 2004), administer provisional ballots (Kimball, Kropf, and Battles 2006), site polling locations (McBrayer, Williams, and Eckelman 2020), and engage in constituent communication (Porter and Rogowski 2018). However, the most causally credibly study to date finds no observable differences in election administration between Democratic and Republican officials (Ferrer, Geyn, and Thompson 2021).

Table 3: Appointing Local Election Officials in Georgia and Texas May Reduce Democratic Presidential Vote Share (Presidential Elections, 1964-2020)

	Democratic vote share		
	(1)	(2)	(3)
Appointed	−0.009	−0.006	−0.016
	(0.007)	(0.007)	(0.007)
County FEs	Yes	Yes	Yes
Year x State FEs	Yes	No	No
Year x State x Dem vote share FEs	No	Yes	No
Year x State x Population FEs	No	No	Yes
Observations	5,004	5,004	5,004

taking the value 1 for appointed election officials and 0 for elected ones, and *Partisan* is a dummy variable taking the value 1 for partisan officials and 0 for non-partisan officials. Since all appointed officials are nonpartisan, this set-up is a simple decomposition of the effects of appointing officials into elected and partisan components. In other words, the omitted category is elected nonpartisan officials.

Table 4 mirrors Table 1, examining the effects of appointed election officials on voter turnout and registration rates. However, the effects are now decomposed into election and partisan components. The results provide clear evidence that elections themselves, and not the partisan nature of the office, drive the main results on citizen participation.

Table 5 makes the same decomposition, this time mirroring Table 3 above. It appears the appointed officials do not consistently alter partisan vote share. However, it appears partisan elected officeholders do compared with their nonpartisan elected counterparts. The point estimates suggest partisan election administrators boost Democratic presidential vote share by 2 to 3 percentage points compared to nonpartisan officeholders, a result that is statistically significant in two of the three specifications.

Table 4: Appointed Local Election Officials in Georgia and Texas Increase Citizen Participation Relative to Both Partisan and Nonpartisan Elected Officials (Presidential Elections, 1964-2020)

	Voter Turnout			Registration Rate		
	(1)	(2)	(3)	(4)	(5)	(6)
Appointed	0.020	0.019	0.014	0.021	0.021	0.017
	(0.009)	(0.009)	(0.009)	(0.011)	(0.011)	(0.010)
Partisan	0.002	0.002	−0.002	0.012	0.010	0.008
	(0.009)	(0.009)	(0.009)	(0.010)	(0.010)	(0.010)
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year x State x Office FEs	Yes	No	No	Yes	No	No
Year x State x Dem vs x Office FEs	No	Yes	No	No	Yes	No
Year x State x Pop x Office FEs	No	No	Yes	No	No	Yes
Observations	4,827	4,827	4,827	2,871	2,871	2,871

Robust standard errors clustered by county in parentheses.

Table 5: Partisan Elected Local Election Officials in Georgia and Texas May Slant Partisan Vote Shares (Presidential Elections, 1964-2020)

	Democratic vote share		
	(1)	(2)	(3)
Appointed	0.020	0.020	−0.002
	(0.013)	(0.012)	(0.012)
Partisan	0.033	0.029	0.018
	(0.014)	(0.013)	(0.012)
County FEs	Yes	Yes	Yes
Year x State x Office FEs	Yes	No	No
Year x State x Dem vote share x Office FEs	No	Yes	No
Year x State x Population x Office FEs	No	No	Yes
Observations	4,984	4,984	4,984

5.4 Have the Preclearance Provisions of the Voting Rights Act Constrained Elected Election Officials?

Due to a history of racial discrimination at the polls, Georgia and Texas were covered by the Section 5 preclearance provisions of the Voting Rights Act. Under these statutes, all election rules had to receive preclearance by the US Department of Justice to ensure that they did not disenfranchise or discriminate against African Americans. The preclearance provision was struck down by the Supreme Court's ruling in Shelby County v. Holder in 2013. This has led to a wave of voting restrictions ostensibly aimed at curtailing voter fraud. It has also increased the discretion election officials have to reduce polling places and make other administrative decisions without receiving federal approval. For instance, there is evidence of significant polling place reductions in some Georgia and Texas counties post-Shelby County.[11]

If the VRA has constrained the actions of local election officials, then conscious efforts by directly elected and partisan officials to reduce turnout should be more effective since the Supreme Court struck down the Act's Section 5 preclearance provisions in 2013. Table A.8 in the Online Appendix shows little evidence to support the idea that appointed election officials increase turnout rates more since the end of the preclearance provisions.

5.5 Do Elected Election Officials Follow Different Election Administration Policies?

Perhaps directly electing election officials curtails citizens participation because the type of officers who run for office pursue different election administration policies than those who are appointed into bureaucratic positions. Following Ferrer, Geyn, and Thompson (2021), I use the EAVS dataset to test a number of election administration policies. CCES data on voter wait times is also employed.

Table 6 displays the results of a two-way fixed effects regression of electing election officials on the following county-level variables: number of polling places, provisional votes share, provisional rejection rate, absentee rejection rate, registration removal late, and share of voters experiencing wait times greater than 30 minutes. While the nature of the data should caution any definitive takeaways from this analysis, there is no strong indication that directly elected officials pursue

[11] https://civilrights.org/democracy-diverted/

election administrative policies that differ from those of appointed officials. Including data from Gubernatorial and Senate elections, shown in Table A.7, reveals similar results.

Table 6: Appointed and Directly Elected Local Election Officials Pursue the Same Election Administration Policies (Presidential Elections, 1964-2020

	Polling Places	Prov Share	Prov Rejection	Absentee Rejection	Reg Removal	Wait Share
	(1)	(2)	(3)	(4)	(5)	(6)
Appointed	−0.007	0.001	−0.021	0.001	−0.006	−0.001
	(0.046)	(0.001)	(0.036)	(0.004)	(0.005)	(0.014)
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year x State FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,398	1,394	1,253	1,863	1,292	743

Robust standard errors clustered by county in parentheses.

6 Conclusion

Appointing election officials appears to produce more competent administrators, leading to greater citizen participation in elections. Using original data and a causally credible research design, I find that appointed election officials increase voter turnout rates by about 1.5 percentage points, relative to elected officials, and that registration rates are increased as well. This finding is robust to a number of alternate regression specifications. It also holds across multiple time periods and in both states studied. In sum, appointed local election officials in Georgia and Texas outperform their elected counterparts on multiple observable metrics.

These findings add to a nascent literature on the limits of elections in ensuring accountable officeholders and optimal policy decisions (Ashworth 2012). When it comes to election administration, it appears elections fail to select and sanction the best candidates for the job. Rather, a low-information environment, the technical nature of the job, and uncontested races conspire to make elections a detriment to selecting competent administrators rather than an advantage.

Future work should consider other observable implications of elected and partisan local election officials, such as potential effects on voter confidence, as well as other innovative ways to measure

quality and competence. Studying the politics of local election administration change—exploring when and why counties switch their selection strategies—is another important area for inquiry. Additionally, the significant variation in selection methods of state election officials merits attention.

These findings should inform ongoing debates over the best forms of election administration in the US. The case selection underlines the topic's importance: counties in Georgia and Texas are actively considering changes to their selection methods for choosing election officials. For instance, a number of state bills have been introduced in Georgia recently to transfer all probate judge elections to nonpartisan contests and to switch all local election administrators to appointed Boards of Elections. These debates have come into sharper focus since the events of the 2020 election and the country's renewed attention on local election administrators as vital caretakers of the democratic process.

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Online Appendix

Intended for online publication only.

Contents

A.1 Participation Effects with Presidential, Senate, and Gubernatorial Races

Table A.1 displays the results of a two-way fixed effects regression estimating the effects of directly electing a local election official on citizen participation. In addition to presidential election results shown in the main analysis, this regression also includes data from Senate and Gubernatorial contests.

Table A.1: Appointing Local Election Officials in Georgia and Texas Increases Citizen Participation (Presidential, Senate, and Gubernatorial Elections, 1964-2020)

	Voter Turnout			Registration Rate		
	(1)	(2)	(3)	(4)	(5)	(6)
Appointed	0.017	0.017	0.014	0.009	0.010	0.009
	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year x State x Office FEs	Yes	No	No	Yes	No	No
Year x State x Dem vs x Office FEs	No	Yes	No	No	Yes	No
Year x State x Pop x Office FEs	No	No	Yes	No	No	Yes
Observations	15,011	15,011	15,011	7,909	7,909	7,909

Robust standard errors clustered by county in parentheses.

A.2 Participation Effects with Alternative Administrative Data

Some conflicts between administrative and web scrapped data, as well as some data imputations from missing data. Table A.2 shows that the main finding that elected election officials reduce citizen participation is robust to an alternative specification privileging documents provided by the Texas Secretary of State over web scrapped data in creating the election official selection data.

Table A.2: Appointing Local Election Officials in Georgia and Texas Increases Citizen Participation (Presidential Elections, 1964-2020, FOIA Preferenced)

	Voter Turnout			Registration Rate		
	(1)	(2)	(3)	(4)	(5)	(6)
Appointed	0.018	0.017	0.015	0.010	0.012	0.010
	(0.004)	(0.004)	(0.004)	(0.006)	(0.006)	(0.006)
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year x State FEs	Yes	No	No	Yes	No	No
Year x State x Dem vote share FEs	No	Yes	No	No	Yes	No
Year x State x Population FEs	No	No	Yes	No	No	Yes
Observations	4,834	4,834	4,834	2,874	2,874	2,874

Robust standard errors clustered by county in parentheses.

A.3 Fixed Effects Counterfactual Estimators

As discussed in Section 4.2.2, I run a series of counterfactual estimations to test for anticipatory effects. The figures in this section were produced using Liu, Wang, and Xu (2021) estimators. The plots from three estimators are shown, each including a dynamic average treatment effect of the treated units estimate and an explicit test for pretrending. In the pretrending plots, two statistics are displayed: an F-test for zero residual and an equivalence p-value test. Low values of the F-test and high values of the equivalence p-value indicate evidence of pretrending.

Additionally, I test pretrending for registration rates and Democratic vote share using the Liu, Wang, and Xu (2021) two-way effects counterfactual estimator.

Figure A.1: **Estimated ATT using Two-Way Effects Counterfactual**. This graph displays a counterfactual prediction of the effect of switching from electing to appointing local election officials on voter turnout in presidential elections using the two-way fixed effects method. The black line shows the dynamic estimated effect of the switch on turnout at x presidential elections since the switch took place. The bar lines at the bottom of the graph show n-sizes for each estimated period (presidential election). Units that have insufficient observations under control are dropped. The specification includes a parametric bootstrap procedure with parallel computing with parallel computing using 200 samples.

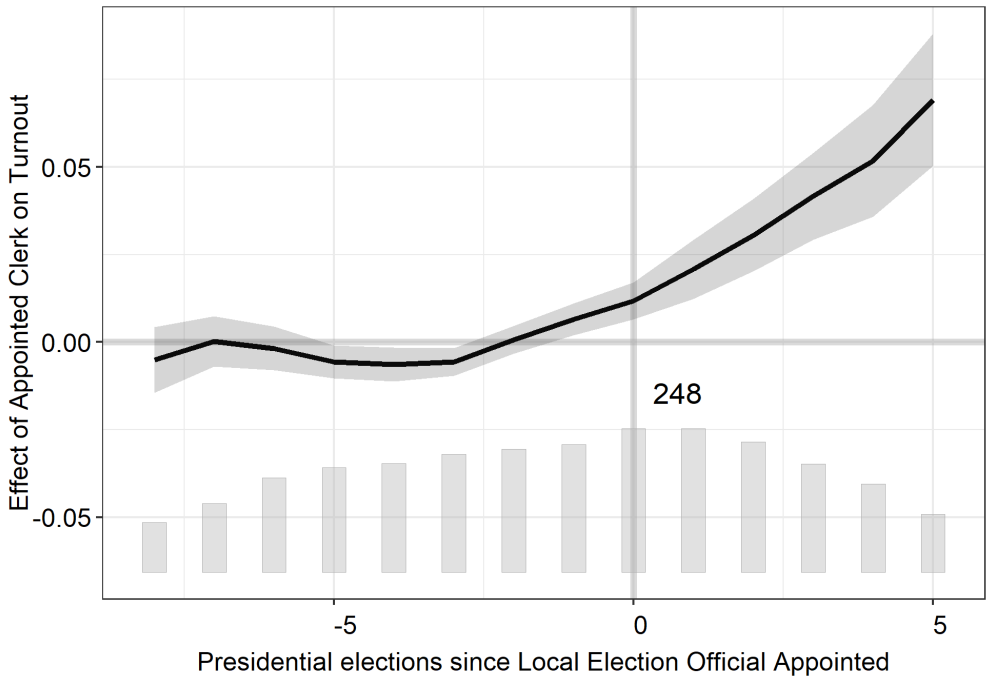


Figure A.2: **Estimated ATT using Interactive Fixed Effects**. This graph displays a counterfactual prediction of the effect of switching from electing to appointing local election officials on voter turnout in presidential elections using the interactive fixed effects method. The black line shows the dynamic estimated effect of the switch on turnout at x presidential elections since the switch took place. The bar lines at the bottom of the graph show n-sizes for each estimated period (presidential election). Units that have insufficient observations under control are dropped. The specification includes a cross-validation procedure to select the number of unobserved factors within the interval of 0 and 5 presidential elections, a parametric bootstrap procedure with parallel computing using 200 samples.

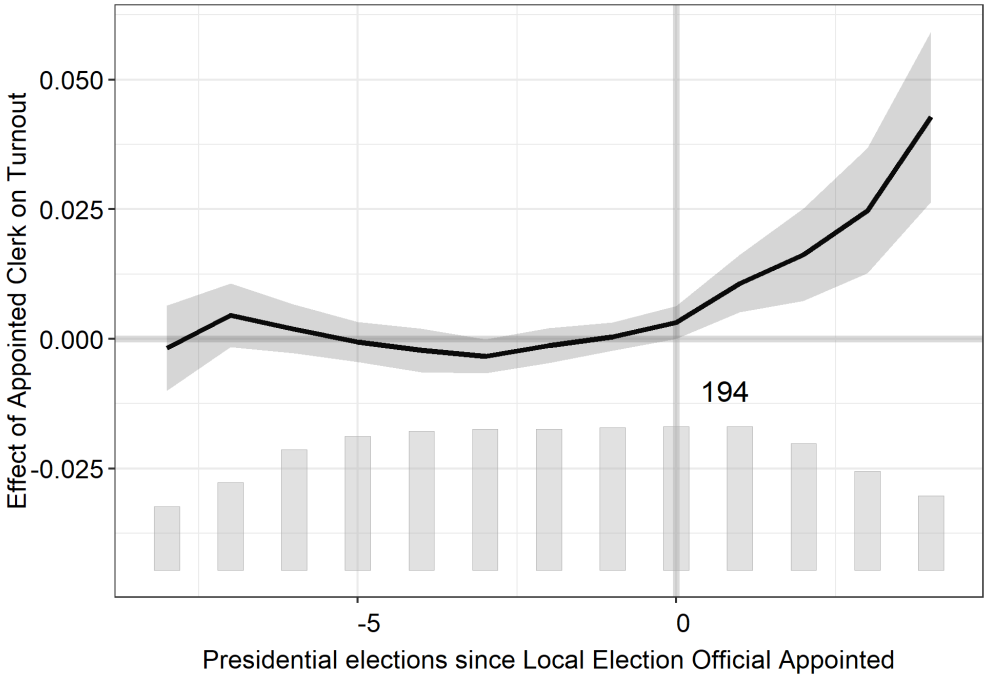


Figure A.3: **Estimated ATT using Matrix Control**. This graph displays a counterfactual prediction of the effect of switching from electing to appointing local election officials on voter turnout in presidential elections using the matrix completion method. The black line shows the dynamic estimated effect of the switch on turnout at x presidential elections since the switch took place. The bar lines at the bottom of the graph show n-sizes for each estimated period (presidential election). Units that have zero observations under control are dropped. The specification includes a cross-validation procedure to select the number of unobserved factors within the interval of 0 and 5 presidential elections, a parametric bootstrap procedure with 200 samples. parametric bootstrap procedure with parallel computing using 200 samples.

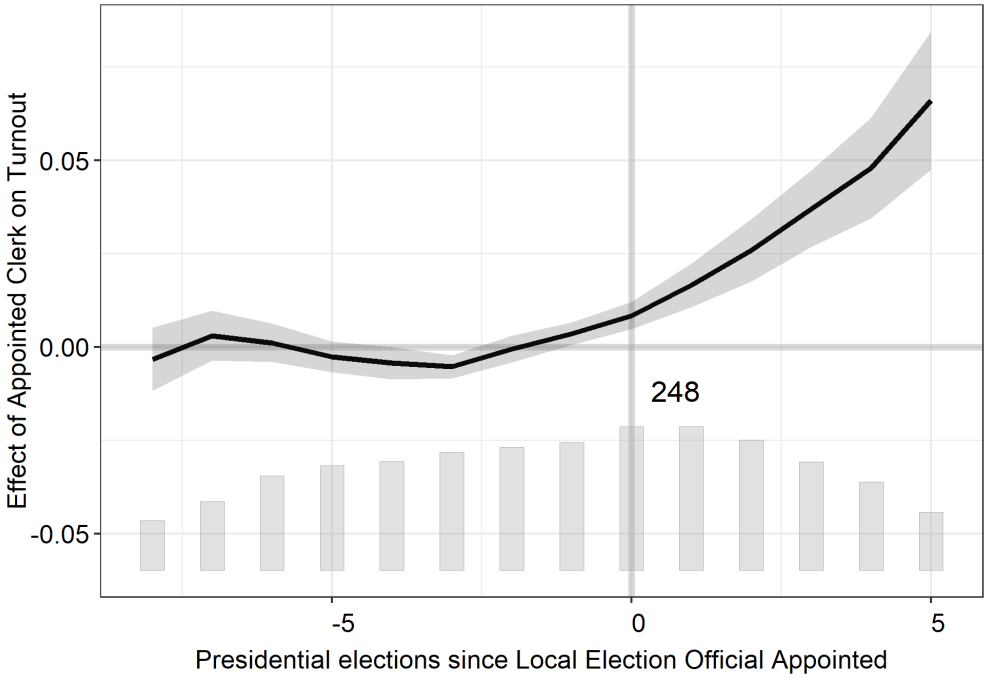


Figure A.4: **Two-Way Effects Counterfactual Test for No Pre-trend**. This graph displays a test for no-pretrend of the effect of switching from electing to appointing local election officials on voter turnout in presidential elections. The two-way fixed effects method is used. The black line shows the dynamic estimated effect of the switch on turnout at x presidential elections before the switch took place. The dotted horizontal lines indicate ranges of the equivalence tests. High values of the F p-value and low values of the equivalence p-value indicate no pre-trending.

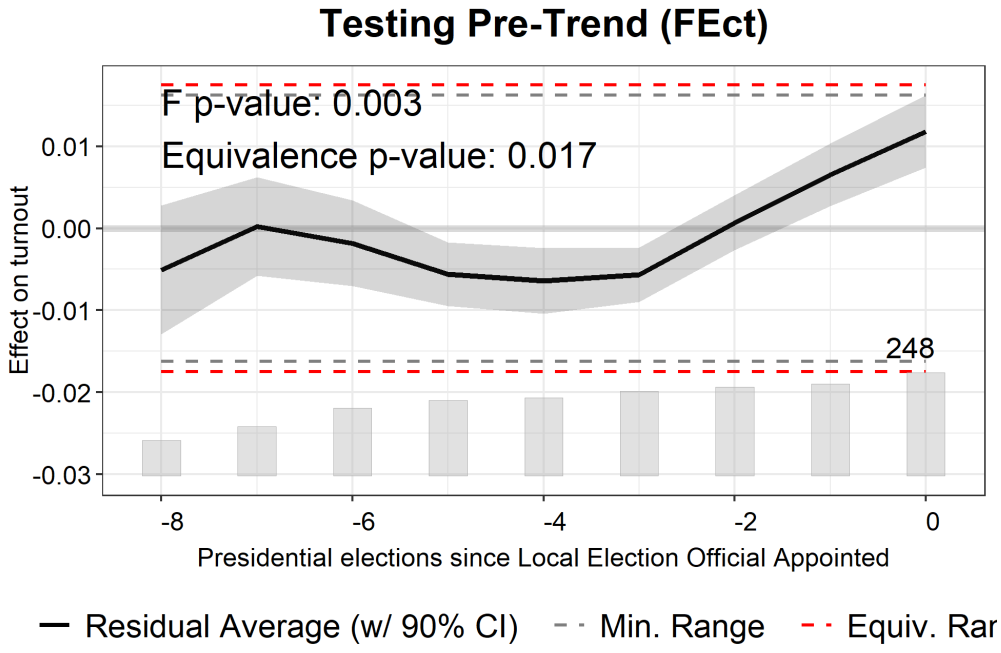


Figure A.5: **Interactive Fixed Effects Counterfactual Test for No Pre-trend**. This graph displays a test for no-pretrend of the effect of switching from electing to appointing local election officials on voter turnout in presidential elections. The interactive fixed effects method is used. The black line shows the dynamic estimated effect of the switch on turnout at x presidential elections before the switch took place. The dotted horizontal lines indicate ranges of the equivalence tests. High values of the F p-value and low values of the equivalence p-value indicate no pre-trending.

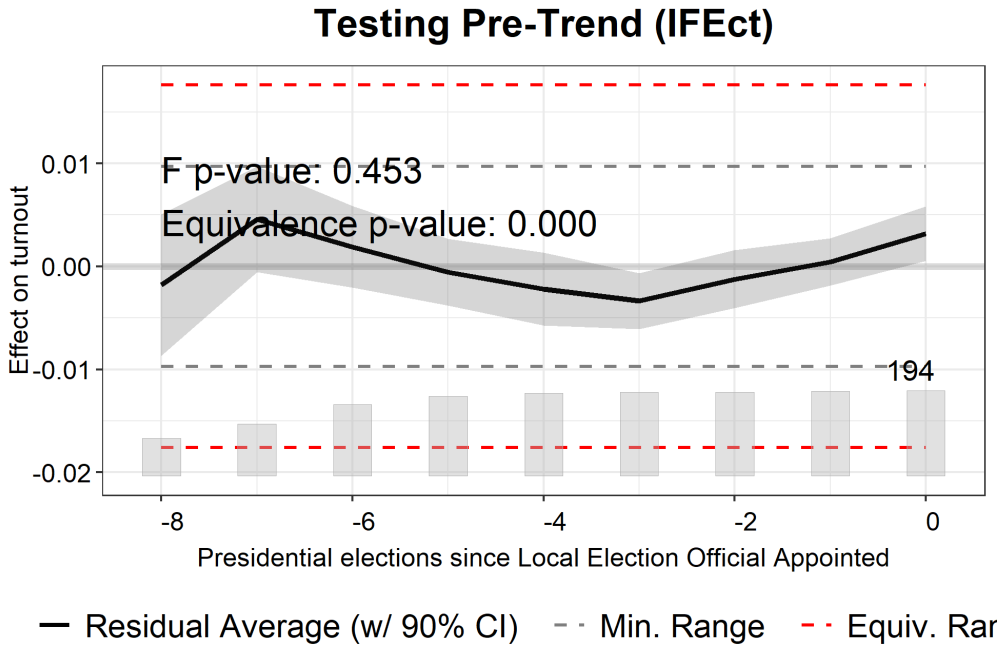


Figure A.6: **Matrix Completion Test for No Pre-trend**. This graph displays a test for no-pretrend of the effect of switching from electing to appointing local election officials on voter turnout in presidential elections. The matrix completion method is used. The black line shows the dynamic estimated effect of the switch on turnout at x presidential elections before the switch took place. The dotted horizontal lines indicate ranges of the equivalence tests. High values of the F p-value and low values of the equivalence p-value indicate no pre-trending.

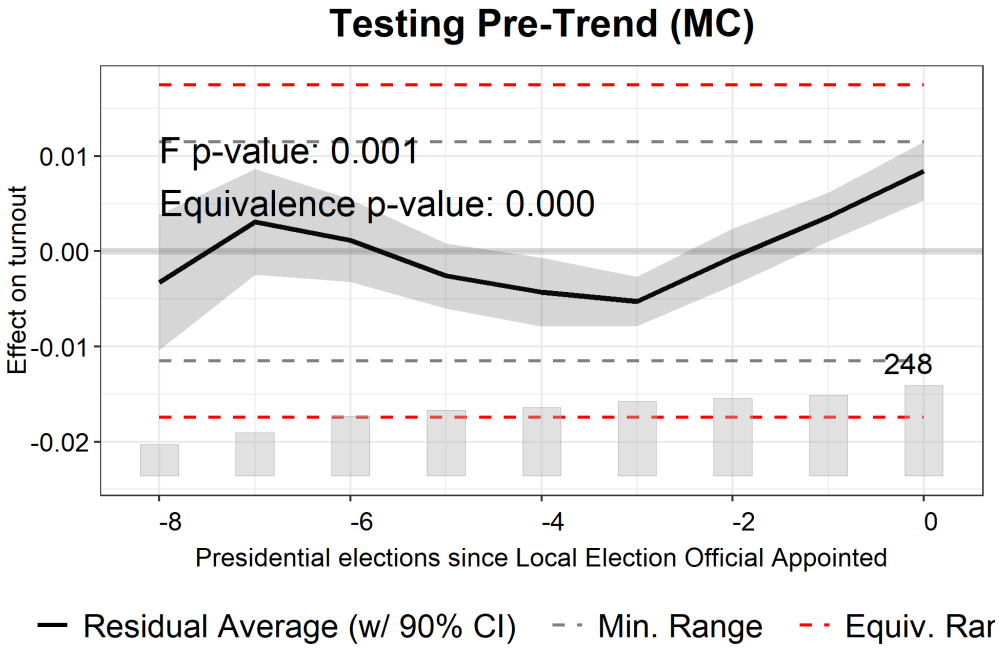


Figure A.7: **Estimated ATT on Registration using Two-Way Effects Counterfactual**. This graph displays a counterfactual prediction of the effect of switching from electing to appointing local election officials on voter registration in presidential elections using the two-way fixed effects method. The black line shows the dynamic estimated effect of the switch on turnout at x presidential elections since the switch took place. The bar lines at the bottom of the graph show n-sizes for each estimated period (presidential election). Units that have insufficient observations under control are dropped. The specification includes a parametric bootstrap procedure with parallel computing with parallel computing using 200 samples.

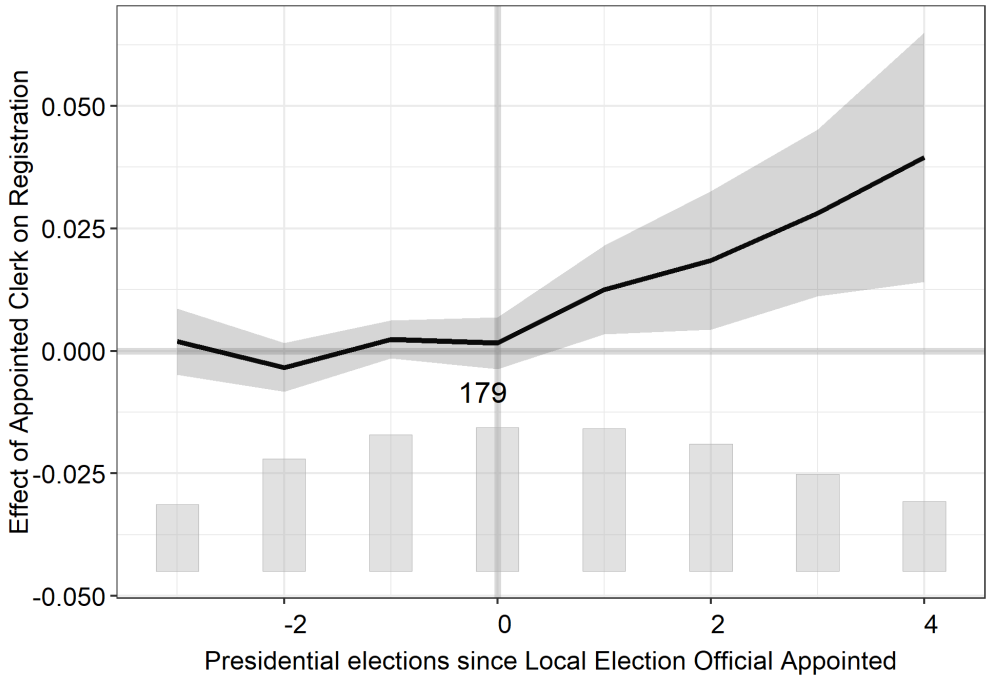


Figure A.8: **Two-Way Effects Counterfactual Test for No Pre-trend on Registration**. This graph displays a test for no-pretrend of the effect of switching from electing to appointing local election officials on voter registration in presidential elections. The two-way fixed effects method is used. The black line shows the dynamic estimated effect of the switch on turnout at x presidential elections before the switch took place. The dotted horizontal lines indicate ranges of the equivalence tests. High values of the F p-value and low values of the equivalence p-value indicate no pre-trending.

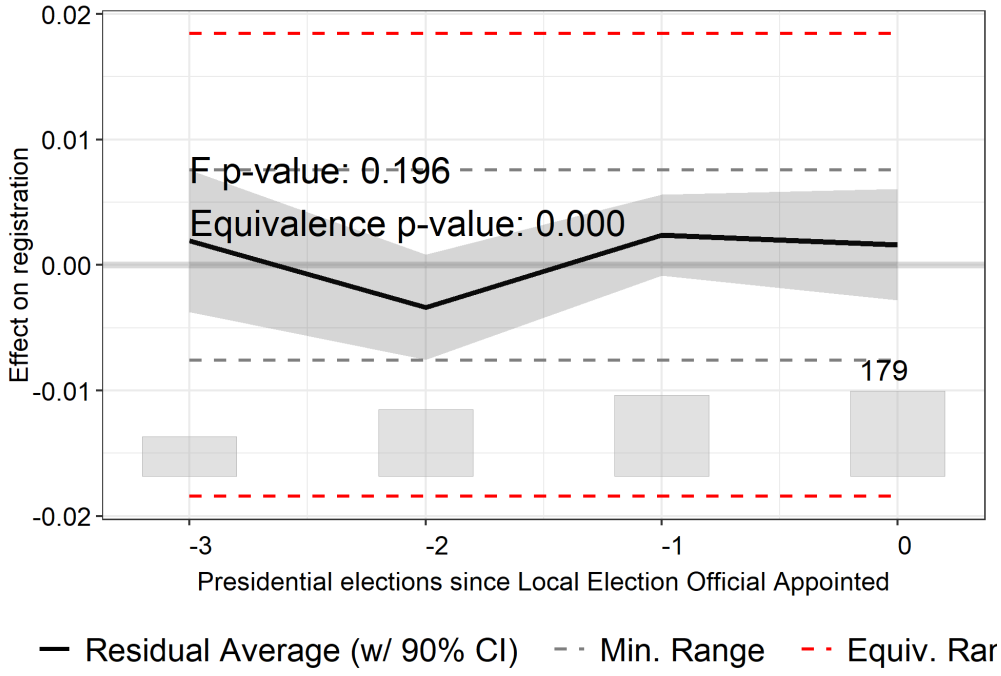


Figure A.9: **Estimated ATT on Democratic Vote Share using Two-Way Effects Counterfactual**. This graph displays a counterfactual prediction of the effect of switching from electing to appointing local election officials on Democratic vote share in presidential elections using the two-way fixed effects method. The black line shows the dynamic estimated effect of the switch on turnout at x presidential elections since the switch took place. The bar lines at the bottom of the graph show n-sizes for each estimated period (presidential election). Units that have insufficient observations under control are dropped. The specification includes a parametric bootstrap procedure with parallel computing with parallel computing using 200 samples.

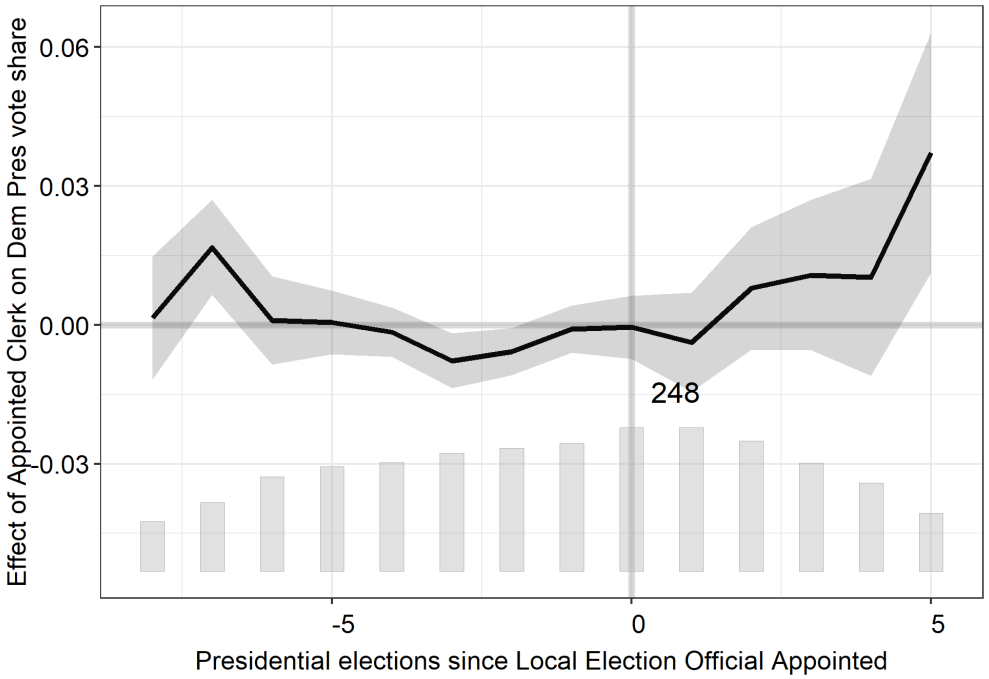
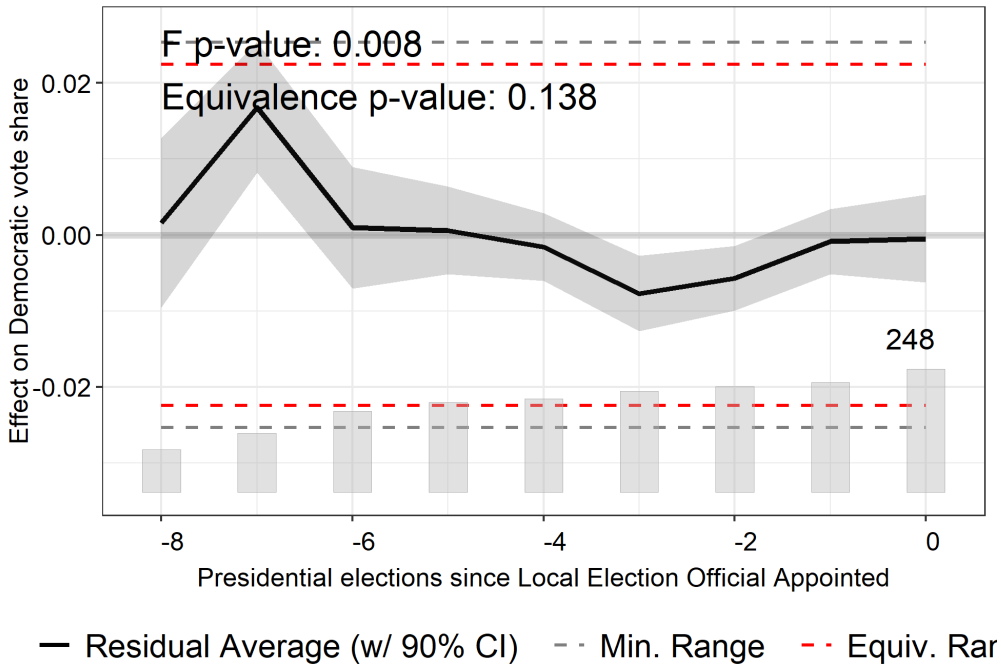


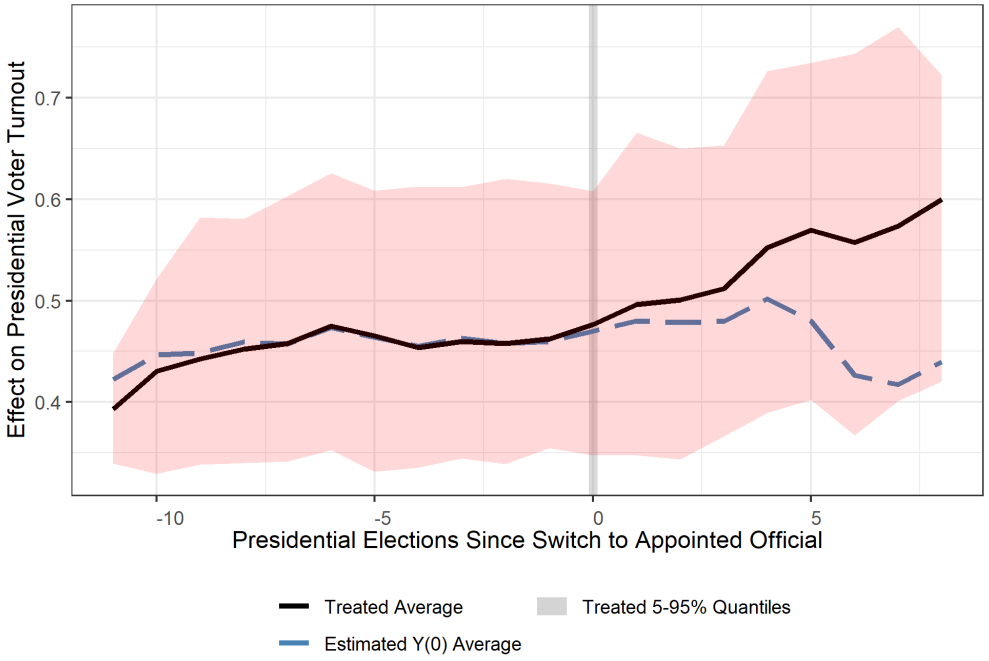
Figure A.10: **Two-Way Effects Counterfactual Test for No Pre-trend on Democratic Vote Share**. This graph displays a test for no-pretrend of the effect of switching from electing to appointing local election officials on Democratic vote share in presidential elections. The two-way fixed effects method is used. The black line shows the dynamic estimated effect of the switch on turnout at x presidential elections before the switch took place. The dotted horizontal lines indicate ranges of the equivalence tests. High values of the F p-value and low values of the equivalence p-value indicate no pre-trending.



A.4 Generalized Synthetic Control Validation Test: Estimated ATT

Figure A.11 displays output from the Xu (2017) generalized synthetic control estimator, showing both treated and estimated control trend lines. As explained in Section 4.2.2, this method allows for a relaxation of the parallel trends assumption by matching treated and untreated units to create balance pretreatment. The alignment of the treated and estimated control averages on the left part of the graph shows that this method has successfully matched treated and control units pre-treatment to overcome the pre-trending concerns identified in Section 4.2.2. Furthermore, it appears that the treated and synthetic counterfactual diverge significantly after a few presidential elections. This divergence continues to grow, although there are few observations at the far right side of the graph.

Figure A.11: **Generalized Synthetic Control of Effect of Appointing Election Officials on Turnout**. This graph displays a generalized synthetic control method of the two-way fixed effects regression testing the effects of appointing local election officials on presidential turnout. The specification includes two-way additive county and year fixed effects, a cross-validation procedure to select the number of unobserved factors within the interval of 0 and 2 presidential elections, and a parametric bootstrap procedure with 1000 samples. The black line is a dynamic estimated ATT effect of appointing an election official on turnout, whereas the dashed blue line is an estimated synthetic control result. The band is a 5% to 95% quantile band of the treated and control outcomes.



A.4.1 Exploring State and Office Heterogeneity Regression Tables

Table A.3 tests where the main results hold for both Georgia and Texas. The first and third columns are regressions including Texas counties only, whereas the second and fourth columns use only Georgia counties. I find that the results hold across both states. Table A.4 displays output from a two-way fixed effects regression testing the effects of various local election officers in Texas, with the omitted category appointed elections administrators. County clerk and district & county clerk offices are both counted as clerks for parsimony. I find that clerks significantly underperform relative to appointed election officials, but that elected tax assessors do not underperform.

Table A.3: Appointing Local Election Officials Increases Citizen Participation in both Georgia and Texas (Presidential Elections, 1964-2020)

	Turnout		Registration	
	(1)	(2)	(3)	(4)
Appointed	0.024	0.013	0.009	0.012
	(0.007)	(0.005)	(0.011)	(0.006)
State	GA	TX	GA	TX
County FEs	Yes	Yes	Yes	Yes
Year x State FEs	Yes	Yes	Yes	Yes
Observations	2,067	2,778	1,107	1,770

Table A.4: Clerks, Not Tax Assessors, Reduce Citizen Participation in Texas (Presidential Elections, 1964-2020)

	Turnout	Registration
	(1)	(2)
Tax Assessor	0.003	−0.008
	(0.011)	(0.014)
Clerk	−0.013	−0.012
	(0.005)	(0.006)
State	TX	TX
County FEs	Yes	Yes
Year x State FEs	Yes	Yes
Observations	2,778	1,770

A.5 Separating Elected and Partisan Effects with Presidential, Senate, and Gubernatorial Races

Table A.5: Appointed Local Election Officials in Georgia and Texas Increase Citizen Participation Relative to Both Partisan and Nonpartisan Elected Officials (Presidential, Senate, and Gubernatorial Elections, 1964-2020)

	Voter Turnout			Registration Rate		
	(1)	(2)	(3)	(4)	(5)	(6)
Appointed	0.010	0.010	0.007	0.015	0.014	0.010
	(0.008)	(0.007)	(0.008)	(0.009)	(0.009)	(0.008)
Partisan	−0.007	−0.008	−0.008	0.007	0.005	0.002
	(0.007)	(0.007)	(0.007)	(0.008)	(0.009)	(0.008)
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year x State FEs	Yes	No	No	Yes	No	No
Year x State x Dem vote share FEs	No	Yes	No	No	Yes	No
Year x State x Population FEs	No	No	Yes	No	No	Yes
Observations	14,953	14,953	14,953	7,899	7,899	7,899

Robust standard errors clustered by county in parentheses.

Table A.6: Partisan Elected Local Election Officials in Georgia and Texas Alter Democratic Presidential Vote Share (Presidential, Senate, and Gubernatorial Elections, 1964-2020)

	Democratic vote share		
	(1)	(2)	(3)
Appointed	0.022	0.024	0.0003
	(0.012)	(0.012)	(0.011)
Partisan	0.034	0.033	0.021
	(0.013)	(0.012)	(0.011)
County FEs	Yes	Yes	Yes
Year x State FEs	Yes	No	No
Year x State x Dem vote share FEs	No	Yes	No
Year x State x Population FEs	No	No	Yes
Observations	15,271	15,271	15,271

A.6 Election Administration Policy Effects with Presidential, Senate, and Gubernatorial Races

Table A.7 shows the effects of directly electing an election official on a range of election administration policies. As observed in Section 5.5, it appears that elected and appointed officials pursue a wide range of similar policies when in office.

Table A.7: Appointed and Directly Elected Local Election Officials Pursue the Same Election Administration Policies (Presidential, Senate, and Gubernatorial Elections, 1964-2020

	Polling Places	Prov Share	Prov Rejection	Absentee Rejection	Reg Removal	Wait Share
	(1)	(2)	(3)	(4)	(5)	(6)
Appointed	−0.019	−0.0002	−0.027	0.007	−0.002	0.020
	(0.037)	(0.001)	(0.032)	(0.006)	(0.004)	(0.013)
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year x State x Office FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,682	4,285	3,498	5,281	4,511	2,028

Robust standard errors clustered by county in parentheses.

A.6.1 Have Preclearance Provisions of the Voting Rights Act Constrained Elected Election Officials? Data Table

Table A.8 shows the results of a two-way fixed effect regression of the effect of switching between elected and appointed officials on, splitting the dataset into pre- and post- Shelby County v. Holder (2013) periods. I find no evidence that appointed election officials have increased their turnout advantage over elected officials since 2013.

Table A.8: No Difference in Post-Shelby County v. Holder Effect of Appointing Election Officials on Citizen Participation (Presidential Elections, 1964-2020)

	Turnout		Registration		Dem vote share	
	(1)	(2)	(3)	(4)	(5)	(6)
Appointed	0.014	0.008	0.006	0.008	0.001	−0.003
	(0.004)	(0.006)	(0.005)	(0.006)	(0.006)	(0.006)
Pre/Post Shelby	Pre	Post	Pre	Post	Pre	Post
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year x State FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	12,382	2,629	5,290	2,619	12,702	2,631

Robust standard errors clustered by county in parentheses.