

Does Appointing Election Officials Produce Better Election Administration?*

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Abstract

Does electing or appointing local officials produce better outcomes for citizens? The US stands out from peer countries in the number of local officials we elect. This is especially pronounced in the area of election administration, with most jurisdictions electing officials to run their elections. Using a newly collected dataset of election administration structures in 449 counties across three states over 60 years, along with a difference-in-differences design, I find that appointed officials out-perform their elected counterparts, increasing voter turnout by nearly two percentage points and raising registration rates by over one percentage point. I present evidence that appointed officials do not receive noticeably more resources than elected officials and do not have more relevant experience. Instead, appointed officials may be monitored and sanctioned better than elected officials. My findings highlight the limits of local elections in securing better government performance and inform ongoing 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 its elections (Hale, Montjoy, and Brown 2015). Many of these officials are directly elected rather than appointed (Kimball and Kropf 2006). Direct elections offer citizens an opportunity to weigh in on an official’s performance and can encourage officials to work harder (Alt, Bueno de Mesquita, and Rose 2011; Fourinaies and Hall 2022). Yet, citizens may not have the right information to measure performance in low-information environments, and people with the right skills for the job may not be those most equipped to win elections (Sances 2016; Whalley 2013). Do elections or appointments lead to better administrative outcomes such as more voter participation?

One hurdle to answering this question is 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. For instance, populous counties tend to appoint their election officials but also tend to have lower participation rates. Likewise, counties in the West tend to elect their officials and also tend to have higher turnout rates. This means a simple cross-section comparison of counties that appoint and those that directly elect clerks will fail to uncover a causally identifiable effect.¹

I overcome this issue with a difference-in-differences research design that leverages county-level variation in election official selection method. This design compares the change in turnout when a county switches from electing to appointing its election official to the change in turnout in other counties in the same state that continue electing clerks. In doing so, it eliminates concerns that certain counties inherently have a link with both clerk selection method and participation rates. I employ interacted population and partisanship fixed effects to address concerns that counties with certain attributes are both more likely to switch clerk selection methods and be on similar turnout trajectories.

To conduct this analysis, I build a new large-scale dataset of election administration structures in Georgia, Oregon, and Texas, encompassing 449 counties over 60 years. Counties in these states have undergone staggered changes in their clerk selection method, with virtually all shifts from direct elections to bureaucratic appointments. I find that when counties switch from elected to appointed

¹I occasionally refer to local election officials as clerks as shorthand. While clerks are the most common county election officials, there is wide variation in the position title across states and counties.

local election officials, their voter turnout rates increase by nearly 2 percentage points and their registration rates increase by over 1 percentage point. These findings are robust to a variety of different estimators; hold across multiple states, offices, and years; and concentrate in smaller jurisdictions. I present evidence that appointed officials do not receive noticeably more resources than elected officials and do not have more relevant experience. Instead, appointed officials may be monitored and sanctioned better than elected officials because voters lack necessary information and viable alternatives at the ballot box.

This paper adds to our growing understanding of the limits of local elections in securing better government performance. In the midst of significant shifts in U.S. election administration toward appointments (Ferrer and Geyn 2022) and declining trust in elections (Stewart 2021), it also informs ongoing debates over local election administration in the US.

2 Selecting Local Election Officials

The United States has a 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; Stewart 2021), 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 consider why we might expect appointed election officials to be better election administrators than elected officials, why participation rates are a useful measure of election quality, the findings of prior scholarship, and the specific cases of Georgia, Oregon, and Texas.

2.1 Why Might Appointments Produce Better Election Administration?

The discussion over whether a professionalized bureaucracy or democratic elections better ensures the accountability of public officials—sometimes labeled the “Friedrich-Finer debate”—has raged for nearly a century in the field of public administration (Al-Habil 2011). It has focused on the ethics of public officials. Friedrich argued that appointed bureaucrats might be better professionalized and held accountable through internal checks created by professional standards and technical

knowledge. Finer argued that external control via elections was the only way to ensure officials are held accountable to the public good.

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 being reelected (Alt, Bueno de Mesquita, and Rose 2011; Ferraz and Finan 2011; Fourinaies and Hall 2022). 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 (Sances 2016; Whalley 2013).

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 barriers to entry. Elected candidates must 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 bureaucrats may be of higher quality than elected officials. Additionally, local elections are rarely contested (Ferrer, Geyn, and Thompson 2021; Thompson 2020; Yntiso 2021). 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 mechanism that is essential to ensuring good performance (Besley 2006). If only one candidate is willing to run, this severely limits the ability of voters to select the highest quality candidates and punish them once in office.

Second, low-information and low-salience environments can prevent voters from using elections to effectively monitor officials and sanction them for poor performance (Ashworth and Bueno de Mesquita 2008; Berry and Howell 2007; Besley 2006; Lim and Snyder 2010). In theory, elections should provide voters with a more direct accountability mechanism than appointments (Burden et al. 2013). In the absence of sufficient information, however, voters may be unable to select good candidates in the first place or distinguish between highly and poorly performing election officials,

leading them to rubber stamp whoever is in office. Local media has been on the decline over the past few decades (Martin and McCrain 2019) and has increasingly devoted less attention to local politics (Lockhart 2021). Declines in the availability of local newspapers leads to less informed citizens and less competitive local races (Rubado and Jennings 2020). The large number of elected positions may also lead to voter fatigue and high ballot roll-off, with not many voters making it all the way to the bottom of the ballot where clerks and tax assessors are typically found (Augenblick and Nicholson 2015). Compounding the problem is the fact that election officials have a portfolio of non-election responsibilities and unintuitive titles that further dilute the ability of voters to effectively monitor and sanction their performance.² If election officials are acting rationally, we should expect them to shirk their duties in these circumstances, since their principals (the voters) may not have the tools necessary to effectively monitor and sanction them. In a study of California treasurers, Whalley (2013) found that municipalities that switched from elected to appointed treasurers enjoyed lower borrowing costs. He concluded that voters may be poorly equipped to judge performance, especially in complex policy areas.

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 indirect incentives to appoint qualified election officials in order to satisfy the electorate and maximize their own chances of reelection.

Elections may also create adverse incentives for officeholders to make politically motivated decisions that are normatively undesirable. For instance, electing rather than appointing assessors in New York exacerbates economic inequalities (Sances 2016), and electing rather than appointing municipal assessors in California leads to the adoption of more costly policies and higher borrowing costs (Whalley 2013). Detached from political 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 a majority of the presently voting electorate prefer political outcomes achieved

²Examples include probate judge in Alabama and Georgia; auditor in Iowa, South Dakota and Washington; and tax assessor in some Texas counties.

by reducing participation, then elected officials could be incentivized to concentrate costs on certain voters or discourage voting across the board. Appointments do not remove this possibility, but they may counterintuitively insulate officials from the pressures of responsiveness in ways that lead to socially desirable outcomes.

2.1.1 Voter Participation as a Measure of Election Administration Quality

I focus on turnout and registration rates as my primary measure of administrative quality for four reasons: local election officials have the ability to influence participation levels, they view increasing participation as part of the job, voter participation is a key component of election quality metrics, and I have access to high-quality data on participation rates. First, election officials typically have far-ranging duties and a significant degree of discretion in carrying out these duties (Ferrer, Geyn, and Thompson 2021). Some studies have found that clerks of different parties influence turnout rates (Bassi, Morton, and Trounstone 2009; Burden et al. 2013; but see Ferrer, Geyn, and Thompson 2021). Second, according to the 2020 Democracy Fund/Reed College Survey of Local Election Officials, over 67% of local election officials agree that encouraging voter turnout is part of their job, compared with fewer than 10% who disagree. Third, participation rates are widely viewed as key measures of election quality and, more broadly, important indicators of the health of the nation’s democracy. MIT’s Election Performance Index uses both voter turnout and voter registration in comparing election administration performance across states.³ Finally, high-quality data for both turnout and registration rates exist at the county level and, particularly in the case of voter turnout, are available going back many decades. This is not true of any other indicator of election quality.

2.1.2 Prior Scholarship

While a number of studies have studied the broader question of electing vs. appointing local officials (Sances 2016; Whalley 2013), few studies have examined the question specifically for local election officials. In an audit study of constituent communication rates, elected and appointed officials responded to correspondence at similar rates (White, Nathan, and Faller 2015). A cross-sectional study of Wisconsin election officials examined whether elected or appointed clerks oversee elections

³<https://elections.mit.edu/#/data/map>

with higher voter turnout (Burden et al. 2013). The authors argued that Wisconsin’s unique history of local election administration led to as-if random assignment of elected and appointed clerks, allowing them to interpret observational differences as causal effects of the selection method. They theorize that appointed officials are more insulated from public opinion than elected clerks, and thus pursue their own personal goals or the goals of the county officials who appointed them rather than the goals of the public. Since voters prefer that clerks make voting convenient whereas the appointing officials prefer minimizing costs, appointed clerks should oversee elections with lower turnout. The authors find that elected clerks have more liberal beliefs when it comes to voter access and desiring higher turnout and that appointed clerks administer elections with lower turnout. However, clerks who care more about increasing turnout do not actually produce higher turnout.

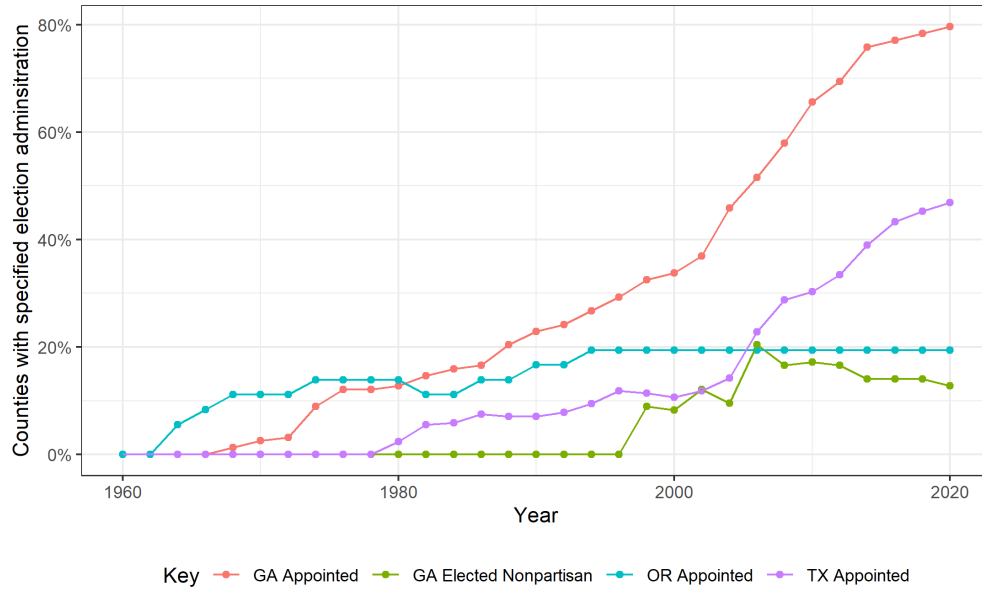
Clerks’ degree of responsiveness and the goals of their principals are both important for theorizing whether appointed or elected clerks will increase participation. It is unclear a priori whether voters and local elites’ preferences truly diverge on the goals of election administration. While local elites might prefer appointed officials to minimize costs, it is also possible that appointed officials have stronger leverage in the budget process than elected officials due to their close relationship with decision makers. Switching to an appointed official dedicated to elections rather than an elected official with multiple responsibilities could increase election resources. Likewise, not all voters value convenience. In fact, recent survey data suggests that voters are more polarized on whether local officials are responsible for increasing turnout than clerks themselves (Manion et al. 2021). Finally, if both voters and local elites value elections with higher turnout, then it is an open question whether appointments or elections will better ensure officeholders achieve that goal.

This is the first study to directly estimate the effects of switching from elected to appointed local election officials on the quality of election administration using a difference-in-differences design. I also examine whether appointments increase election expenditures, enhance the quality of officials, and improve monitoring and sanctioning.

2.2 Selecting Local Election Officials in Georgia, Oregon, and Texas

Georgia, Oregon, and Texas present a unique opportunity to estimate the effects of different selection methods on the quality of election administration. Historically, all counties in these states di-

Figure 1: **Georgia, Oregon, and Texas Local Election Administration Selection Methods, 1960-2020.** This graph displays over time change in the selection method of county election officials in Georgia, Oregon, and Texas from an original data collection. Proportions are relative to the number of counties in each state—254 in Texas, 159 in Georgia, and 36 in Oregon. In these states, all appointed local election officials are officially nonpartisan. In Texas all elected election officials are partisan, in Oregon all elected officials are nonpartisan, and in Georgia elected probate judges can be either partisan or nonpartisan.



rectly elected an official who was tasked with overseeing voter administration—the probate judge in Georgia, the county clerk in Oregon, and the county clerk, district & county clerk, or tax assessor-collector in Texas. In Georgia and Texas, election officials ran with partisan labels, whereas in Oregon election officials were nonpartisan. Beginning in 1963 in Oregon, 1967 in Georgia, and 1979 in Texas, counties began switching their local election official to an appointed nonpartisan entity (a Board of Elections in Georgia, an Elections Manager/Director in Oregon, and an Elections Administrator in Texas). Starting in the 1980s, Georgia also allowed counties to establish nonpartisan probate judge elections. In all three states, switches occurred in a staggered and irregular manner. The changes are most widespread in Georgia and Texas and have accelerated since 2000 in both states. Today, about half of counties in Texas and over 80% of counties in Georgia now task an appointed official with voter administration responsibilities. The changes are less widespread in Oregon and occurred earlier, with the last shift in 1993. Figure 1 illustrates these trends.

As in many other states, elected local election officials in Georgia, Oregon, and Texas are entrusted with broad statutory authority to conduct elections (Ferrer, Geyn, and Thompson 2021). 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.⁴ All Oregon clerks handle both voter and registration administration duties, although the extent of these duties is limited since Oregon moved to all vote-by-mail in 2000. In short, local election officials in all three states have responsibilities that are substantial enough to suspect that they could impact participation rates.⁵

Georgia and Texas are both battleground election states with significant ongoing political debates over election laws. Furthermore, counties in both states continue to actively consider changes to election official selection methods. They are not alone. Over a dozens counties and hundreds of municipalities across 10 other states have changed their selection method of clerks over the past few decades, almost all from elected to appointed officials (Ferrer and Geyn 2022). These changes are likely to continue given the renewed attention on election officials. This study thus contributes to an ongoing debate over the optimal form of local election administration.

3 Data and Methods

3.1 Measuring the Selection Method of Local Election Officials

I self-construct panel data on the selection method of local election officials in Georgia, Oregon, and Texas counties from 1960 to the present. In total, my dataset covers 60 years of election administration structure for 449 counties. In Georgia, each county shift between elected and appointed

⁴Appointed registration boards undertake all Georgia registration administration. 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.

⁵In 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.

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.

Shifts in Oregon local election administration are made by counties as part of the implementation or amendment of a home rule charter. These changes are catalogued in a report produced by the Association of Oregon Counties.⁶

Texas election administration changes are made by County Commissioner’s Courts, in coordination with the affected statutory officers. I collect data on these changes 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.⁷ This yields yearly panel data covering 2000 to 2020. Second, I filed 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 are nearly identical aligned, though the State’s documentation is notably incomplete. I use a combined dataset privileging the web scraped data for any discrepancies in the main analysis, and a combined dataset privileging the Open Records Request documentation in Section A.2 in the online appendix.

3.2 Data

I use presidential, gubernatorial, and senate races to measure election outcomes. Data on county-level vote totals is from Congressional Quarterly and David Leip’s US Election Atlas. It spans from 1968 to 2020.⁸ I use data on registration totals from Leip’s Election Atlas. This covers presidential elections from 1996 and gubernatorial elections from 2004. I measure voting age population, the denominator in turnout and registration measurements, using estimates from the National Cancer

⁶https://www.oregon.gov/oha/PH/DISEASES/CONDITIONS/CHRONICDISEASE/HPCDPCONNECTION/Documents/TA/policy_change_resources/county_home_rule_paper.pdf

⁷<https://www.sos.state.tx.us/elections/voter/county.shtml>

⁸I exclude Loving county, Texas from the analysis because its population is too small to reliably estimate participation rates.

Institute’s Surveillance, Epidemiology, and End Results Program, which is available from 1970.⁹ I measure registration rate by dividing total registrants by the voting age population.

I assemble a set of county-level 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 per 1,000 people, provisional ballots cast, provisional ballots rejected, absentee ballots rejected, and the number of registrants removed from the voter roll. Following Ferrer, Geyn, and Thompson (2021) and 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. This is available for general elections in 2006, 2008, and 2012–2018.

Finally, I conduct a series of mechanism estimations using data on election administration expenditures from Mohr et al. (2018) and on the quality of local election officials using the 2020 Democracy Fund/Reed College Survey of Local Election Officials.¹⁰

3.3 Design

It is difficult to estimate the effect of local election administrator selection because counties that appoint officials likely differ from those that elect officials for a host of reasons beyond the selection method of the election official and in ways that are likely to affect participation rates. For instance, populous, dense, and racially diverse counties are all more likely to appoint their election officials than sparsely populated, rural, and mostly white counties (Ferrer and Geyn 2022). They also tend to have lower participation rates for unrelated reasons (Leighley and Nagler 2017). Similarly, counties in Western states tend to elect their officials and also tend to have higher turnout rates than counties in other regions (Springer 2014). Given these correlations, a simple cross-sectional analysis of counties would result in a strong relationship between appointed officials and lower turnout—but this would not be evidence that appointing officials *cause* lower turnout. Even if all of these obvious differences are controlled for, there are likely unobservable or difficult to measure

⁹This data includes some voting-age residents who may be ineligible to vote due to citizenship status or criminal record. While this may make some estimates noisier, it is unlikely to introduce bias since few people decide where to live based solely on the selection method of a county’s local election official. The data I use is available at <https://seer.cancer.gov/popdata/>.

¹⁰<https://evic.reed.edu/leo-survey-summary/>

factors that make counties different in ways that happen to correlate both with their participation rate and the selection method of their clerk.

One solution would be to observe a county switching from one form of administration to another and measuring the difference in their turnout between elections before and after the switch. This analysis compares the county to itself, eliminating the problems of a simple cross-sectional analysis described above. However, it presents a new issue: most year-to-year changes in turnout and participation are attributable to idiosyncratic factors such as candidates, election timing, and the public mood, and likely dwarf the effect of selection method itself. This is a problem common to all studies that seek to examine the effects of election administration or policies on participation (Thompson et al. 2020). Simply observing changes at one point in time makes it impossible to distinguish whether the effect is due to the switch in selection method, or—more likely—to differences in the candidates on the ballot and the salience of each election.

I overcome both of these issues with a staggered difference-in-differences research design. I leverage over time county-level variation in election official selection method in Georgia, Oregon, and Texas along with administrative datasets of voter turnout and registration rates. The design compares the change in turnout when a county switches from electing to appointing its election official to the change in turnout in other counties in the same state that continue electing clerks. The comparison of turnout within the same county overcomes the problem that counties are inherently different for a whole host of observed and unobservable reasons, such as demographic and socioeconomic composition. The comparison of the difference in turnout between counties that switch to appointed clerks and those that continue to elect their clerks overcomes the problem that year-to-year changes in turnout are largely due to reasons besides the selection method of the local election official. So long as these year-to-year differences in turnout are commonly experienced across a state and not indirectly related to switches in clerk selection method, we can be confident that an observed difference in turnout in the counties that switch to appointed clerks is due to the selection method itself.

In summary, the differences-in-differences design allows me to credibly estimate the causal effects of switching from elected to appointed clerks on voter participation. With 254 and 159 counties, respectively, Texas and Georgia have more counties than any other state, ensuring that the analysis is sufficiently powered.

I estimate the regression $Y_{it} = \alpha_i + \delta_t + \beta \text{Appointed}_{it} + \epsilon_{it}$, where Y_{it} is a measure of voter turnout or registration in county i at election year t , α_i and δ_t are county and year fixed effects, respectively, and Appointed_{it} is a dummy variable taking 1 when counties appoint their local election official and 0 when counties elect their local election official. β is the causal effect of an appointed election official on voter turnout.

The causal interpretation of the difference-in-differences design rests on the parallel trends assumption. This means that counties that switch to appointed clerks are on similar turnout trajectories to those that do not switch, prior to the switch itself. It is possible to imagine that counties that switch to appointed officials are growing at more rapid rates than those that stay with elected officials, and that turnout is trending down as a result. In this case, bureaucratic appointed officials might be viewed as a way to professionalize the county’s election administration. Similarly, selection method might become a partisan issue. If more Democratic counties start to adopt appointed clerks, and Democrats reduce or increase their turnout relative to Republicans, then this would also result in the appearance of a causal relationship between appointments and turnout that was spurious.

All regressions include at the minimum Year by State fixed effects. This ensures that comparisons are only made between counties in the same state, addressing the possibility that states may be on different turnout trajectories. I further address parallel trending concerns by incorporating two additional sets of interacted fixed effects: Year by State by Democratic vote share and Year by State 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 switch their election administration may have also happen to shift either population or partisan trends in ways that are systematically related to turnout. Democratic vote share and population are both divided into quartiles and measured pretreatment for each state.¹¹

¹¹I measure Democratic vote share as votes for the top-ticket Democratic candidate divided by votes for the top-ticket Democratic and Republican candidates. For Georgia, population is measured using the 1960 census and partisanship is measured using Democratic vote share in the 1968 presidential election. For Oregon, population is measured using the 1960 census and partisanship is measured using Democratic vote share in the 1960 presidential election. For Texas, population is measured using 1970 census figures and partisanship is measured using Democratic vote share in the 1976 presidential election.

Even with these interacted fixed effects, it is still possible there are unobserved reasons why counties that switch to appointed clerks are on a different turnout trajectory than those that maintain elected offices. For instance, perhaps counties switch precisely because they are on a downward turnout trajectory and wish to reverse the trend. Or, counties that are on an upward participation trajectory might adopt appointed clerks as part of a series of initiatives that are effectively boosting turnout. I address these concerns by examining whether counties that are about to switch appear to have a distinct turnout trajectory from unchanged counties, and conducting a generalized synthetic control matching exercise to ensure that counties that switch are only compared to those that do not with similar turnout trajectories.

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, examine the validity of the parallel trends assumption, conduct a placebo analysis using registration rates in Georgia, and distinguish between the effects of selection method and partisanship.

4.1 Appointing Election Officials Increases Voter 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 estimate the effects on votes per voting-age resident, whereas columns 4 through 6 estimate the effects on registrants per voting-age resident. Both are measured as proportions out of 1. The coefficients can be interpreted as the average percentage point difference in turnout and registration rates when counties switch from elected to appointed clerks. 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 state and year. This ensures that differential participation trends between Georgia, Oregon, Texas are not driving the results. I cluster robust standard errors by county and omit 15 counties that switch back and forth between elected and appointed clerks.

Table 1: **Appointing Local Election Officials Increases Citizen Participation (Presidential Elections, 1972-2020)**

	Voter Turnout			Registration Rate		
	(1)	(2)	(3)	(4)	(5)	(6)
Appointed	0.019 (0.004)	0.019 (0.004)	0.017 (0.004)	0.016 (0.006)	0.018 (0.007)	0.009 (0.006)
Counties	433	433	433	276	276	276
Elections	13	13	13	7	7	7
Observations	5622	5622	5622	1931	1931	1931
Outcome Mean	0.50	0.50	0.50	0.81	0.81	0.81
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

Robust standard errors clustered by county in parentheses. Voter turnout and registration rate are measured as proportions out of 1. The number of observations is smaller in columns 4-6 because Georgia is excluded and because turnout data is available from 1970 but registration data is only available from 1996. 15 counties that switch from appointed to elected clerks are also excluded.

Column 1 shows that counties switching from directly elected to appointed election officials see an average increase in presidential voter turnout of 1.9 percentage points, compared with counties that do not switch. The point estimate is precisely estimated and substantively meaningful (effect size = .18), allowing us to confidently rule out effects of 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 partisanship and shifts to appointed election administration. I introduce year by state by Democratic vote share fixed effects to alleviate these concerns. In column 2, 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. Georgia is excluded from these specifications because registration duties are always undertaken by appointed registration boards. I conduct a placebo estimation of registration effects

including Georgia in Section 4.3. While the estimates are slightly noisier than those of turnout due to a shorter period of data availability and the exclusion of Georgia, the coefficients range from 0.9 to 1.8 percentage points in magnitude, a substantively meaningful effect (effect size = .18). A null of no difference can be confidently ruled out in two of the three estimators.

These estimations provide strong evidence that appointed local election officials increase voter participation in presidential elections, relative to their directly elected counterparts. Regressions including Senate and Gubernatorial contests are found in Section A.1 and yield substantively identical findings. Table A.2 shows the results are also robust to the use of different criteria in constructing the panel data of election official selection methods.

The results hold in multiple states, across multiple offices, and over multiple years and date ranges. In Table A.3 in the online appendix, I show that switching to an appointed election administrator increases voter turnout in both Georgia and Texas, the two states with enough counties to precisely estimate the results. One additional concern is that the effect only holds for certain elected offices. Table A.4 in the online appendix shows that switching from both elected probate judges and elected county clerks to bureaucratic appointments increases voter turnout. I run a series of Callaway and Sant’Anna (2021) estimators to estimate the dynamic effects of switching from an elected to an appointed election official. These results, found in Section A.4 of the online appendix, show that the positive effects of appointments on voter turnout appear over time and across multiple county cohorts and time periods. Finally, it is possible that low rates of turnout among African-Americans in the South due to the lingering effects of Jim Crow confound the results. I show in Table A.5 in the online appendix that the results also hold when only examining more recent elections.

I use EAVS and CCES data to explore whether appointed election officials pursue different election administration policies. The results, found in Section A.6 of the online appendix, suggest that appointed and elected administrators run elections with similar numbers of polling places per 1,000 residents, provisional ballot usage, provisional rejection rates, absentee ballot rejection rates, registration removal rates, and voter wait times.

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, Oregon, and Texas produce higher presidential voter turnout than directly elected officials. I utilize a range of alternative difference-in-difference estimators and examine the validity of the parallel trends assumption. I also employ a generalized synthetic control method which relaxes the assumptions needed for causal inference. These estimators 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 estimator when used in staggered adoption designs (Baker, Larcker, and Wang 2022; Borusyak, Jaravel, and Spiess 2021; Goodman-Bacon 2021; de Chaisemartin and D’Haultfoeulle 2020; Callaway and Sant’Anna 2021; Sun and Abraham 2021). These issues stem from heterogeneous treatment effects. If treatment effects vary across time or units, the estimate will be biased due to the assignment of negative weights to some comparison groups. This is because units that switch early on from control to treatment are treated as controls in some comparisons and subtracted from the difference-in-difference estimator, even if they continue to experience dynamic treatment effects.¹²

Table 2 displays results from additional estimators designed to help overcome the methodological issues of the staggered adoption two-way fixed effects design. All estimators include county and year by state fixed effects. The first column includes 15 counties that switch back from appointed to elected clerks. These are excluded from the main analysis because they can prove particularly problematic in the estimation, though they are few in number and have minimal effect on the results. Column 2 is identical to the specification found in column 1 of Table 1. In the third specification, counties that are always “treated”—in this case, those that use appointments from the beginning of data availability—are excluded to avoid problematic comparisons in the diff-in-diff estimation. The last two columns show the results of “stacked” diff-in-diff estimations (Cengiz et al.

¹²I use the method proposed by de Chaisemartin and D’Haultfoeulle (2020), an extension of the Bacon Decomposition (Goodman-Bacon 2021), 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, 42% of assigned weights have negative values, summing to -0.486. It is therefore important to validate my findings with other estimators that correct for this bias.

2019). This is one recent technique that has been developed to eliminate the biases in the naive two-way fixed effects estimator discussed above. Encouragingly, the point estimate grows with each additional best practice employed. The estimated effect of appointments on voter turnout is 2 percentage points once counties that switch back to appointments and those that always appoint are excluded. Both of the stacked estimators produce point estimates that are above 3 percentage points. All specifications are precisely estimated.

Table 2: Finding that Appointing Local Election Officials Increases Citizen Participation Is Robust to Alternative Estimators (Presidential Elections, 1972-2020)

	Voter Turnout				
	(1)	(2)	(3)	(4)	(5)
Appointed	0.017 (0.004)	0.019 (0.004)	0.020 (0.004)	0.036 (0.007)	0.030 (0.006)
Counties	448	433	425	425	278
Elections (avg)	13	13	13	12	4
Observations	5817	5622	5518	32831	9968
Outcome Mean	0.50	0.50	0.50	0.52	0.52
County FEs	Yes	Yes	Yes	Yes	Yes
Year x State FEs	Yes	Yes	Yes	Yes	Yes
Switchers Excluded	No	Yes	Yes	Yes	Yes
Always Treated Excluded	No	No	Yes	Yes	Yes
Stacked DiD	No	No	No	Yes	Yes
Shortened Event Window	No	No	No	No	Yes

Robust standard errors clustered by county in parentheses. Column 1 includes 15 counties that switch from appointed to elected clerks. Column 2 is identical to Column 1 in Table 1, and excludes counties that switch from appointing to electing their clerks. Column 3 additionally excludes counties that have not elected their clerk since 1968. Column 4 implements a stacked difference-in-difference regression following the procedure described by Cengiz et al. 2019. Column 5 additionally shortens the event window for each county to within 8 years before its switch and within 16 years after its switch.

I also employ the de Chaisemartin and D’Haultfœuille (2020) estimator and the Callaway and Sant’Anna (2021) dynamic effects estimator, separating the results by state to ensure that differential trending between states does not bias the estimators. The results are consistent with those shown in Table 2 and are found in Section A.7 in the Online Appendix.

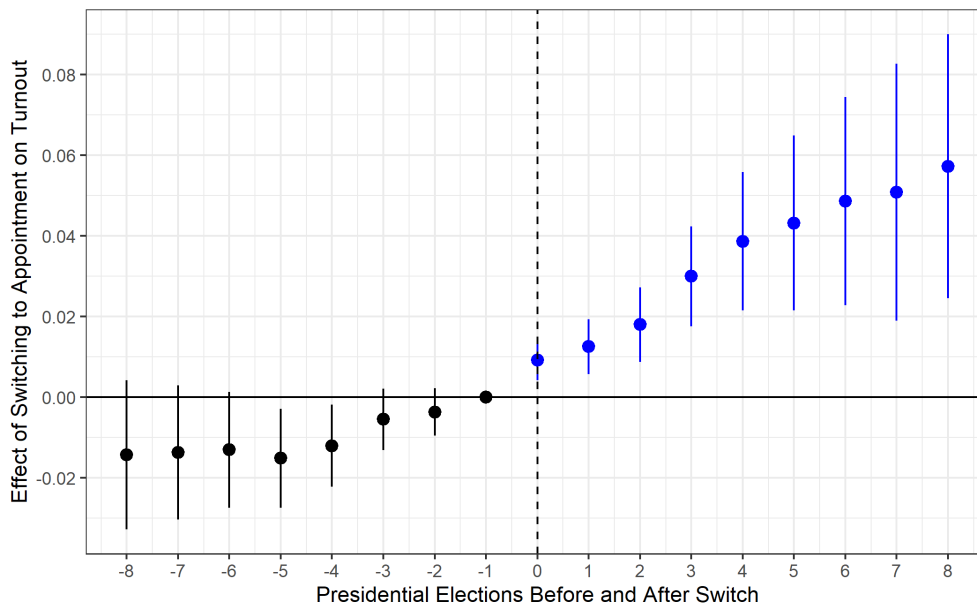
4.2.2 Validating the Parallel Trends Assumption

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 switching clerk selection methods rather than differing trends in counties that switched their election administration from those that did not. 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. Or, counties that have committed to increasing voter participation could switch to appointed election officials along with other policy changes.

I investigate the validity of this assumption using the Dube et al. (2022) local projections difference-in-differences event studies estimator. This estimator makes a series of pooled two-period two-group comparisons, estimating period-by-period effects and eliminating biases due to heterogeneous treatment effects. Figure 2 plots the results. The x-axis marks the presidential elections before and after a switch in local administration, with 0 marking the first election under an appointed clerk. 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. Negative coefficients in the left half of the graph suggest modest evidence of pre-trending. In other words, it appears that counties that switched to appointing clerks may already have been on a trajectory of higher turnout. However, these coefficients are relatively small and are not statistically distinguishable from zero in the twelve years leading up to a county’s switch. 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 increasing trend that begins after a county has switched to appointed administrators. I explore the reasons for this trend in Section A.4 in the online appendix.

An underlying concern of the difference-in-difference estimation strategy is that treated and control units do not look like one another. If the places that switch from electing to appointing election officials are fundamentally different on some unobserved characteristics, then this will

Figure 2: **Dube et al. (2022) Local Projections Difference-in-Differences Estimate 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 use the Dube et al. (2022) local projections difference-in-differences estimator for dynamic heterogeneous-robust difference-in-difference designs, which corrects for bias due to heterogeneity in year and county treatment effects.



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 estimation rebalances the data sample by matching treated and untreated units to ensure that treated units look like control units pre-treatment. State-specific generalized synthetic control estimates, found in Section A.8 of the online appendix, continue to show positive effects of switching to appointed officials on voter turnout.

4.3 Appointing Election Officials Only Boosts Registration Rates when switching from Elected Officials

In Texas, the switch from elected to appointed election officials involves both registration administration and voting administration duties. In Georgia, the shift only impacts voting administration; registration duties have always been carried out by appointed officials. Therefore, we should only

observe effects of appointed administration on registration rates in Texas. Table 3 displays the results of a placebo estimation on whether the switch to appointed election officials boosts voter registration rates in Texas and not Georgia.

Table 3: Appointing Local Election Officials Increases Voter Registration Only when Switching from Electing Officials (Presidential Elections, 1996-2020)

	Registration Rate	
	(1)	(2)
Appointed	0.011 (0.011)	0.016 (0.006)
Counties	157	241
Elections	7	7
Observations	1093	1686
Outcome Mean	0.71	0.81
State	GA	TX
County FEs	Yes	Yes
Year FEs	Yes	Yes
Robust standard errors clustered by county in parentheses.		

While the point estimate is positive for both Georgia and Texas, it is larger and only precisely estimated in the case of Texas. In Georgia, the effect cannot be confidently distinguished from zero. This is consistent with the fact that only in Texas do switches to appointed officials also encompass registrars, and thus we should only observe a significant effect in Texas. Due to data limitations, I am unable to estimate registration effects in Oregon.

4.4 Does Selection Method or Partisanship Explain the Results?

Are the observed effects the result of a switch from elected to appointed clerks, or are they due to the switch from an openly partisan office to an ostensibly nonpartisan position? The results so far have presented a bundled treatment of both selection method and partisanship. The partisan nature of elected office could lead election officials to act in ways that differ from their nonpartisan appointed counterparts—for instance, by attempting to alter turnout to advantage their co-partisans. 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 the divergence between appointed and elected election officials.¹³

Georgia’s history of county-level changes between elected partisan, elected nonpartisan, and appointed election officials provides an opportunity to disentangle the effects of selection method and partisanship. Table 4 displays estimations of voter turnout in Georgia separating out the effects of appointments and partisan elections, with the omitted category elected nonpartisan officials.¹⁴ The results provide strong evidence that elections themselves, and not the partisan nature of the office, drive the main results on voter turnout. Virtually all of the estimated effect is observed for a switch from elected to appointed administration, whereas the effect of switching between partisan and nonpartisan administration is not distinguishable from zero.

Table 4: Appointments, Rather than Partisanship, Drive the Effects on Voter Turnout (Georgia Presidential Elections, 1972-2020)

	Voter Turnout		
	(1)	(2)	(3)
Appointed	0.026 (0.010)	0.024 (0.010)	0.023 (0.010)
Partisan	0.001 (0.009)	0.000 (0.010)	-0.005 (0.010)
Counties	157	157	157
Elections	13	13	13
Observations	2029	2029	2029
Outcome Mean	0.47	0.47	0.47
County FEs	Yes	Yes	Yes
Year FEs	Yes	No	No
Year x Dem vs FEs	No	Yes	No
Year x Pop FEs	No	No	Yes

Robust standard errors clustered by county in parentheses.

¹³Some 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, Ferrer, Geyn, and Thompson (2021) find no observable differences in election administration between Democratic and Republican officials.

¹⁴I have incomplete data on partisan election administration for Camden and Laurens counties.

5 Why Does Appointing Election Officials Increase Voter Participation?

In this section, I examine whether the effects concentrate in smaller jurisdictions. I then explore three potential explanations for my findings: election resources, quality differences, and monitoring and sanctioning capacity. Smaller jurisdictions enjoy the greatest benefits to appointing administrators. However, adopting appointed clerks does not lead to more election resources in the jurisdictions they are most expected to, nor are there notable quality differences between appointed and elected officials. Rather, it appears the core reason why appointed officials perform better is because elections fail to adequately allow for voters to monitor and sanction election administrators.

5.1 Are the Effects Largest in Small Jurisdictions?

Are the effects spread evenly across jurisdictions, or do they concentrate in jurisdictions of a certain population? On its face, it makes sense that in counties where local election officials have fewer deputies, the actions of that official have a greater impact on participation rates. Table 5 displays the results of a series of difference-in-difference regressions similar to Table 1, except that the effects on small counties and large counties have been separated. This is determined by an even population cut for each state—about 22,500 people in Georgia, 50,000 in Oregon, and 18,500 in Texas. The top row can be interpreted as the effect of switching to appointed election officials for relatively populous counties, and the bottom row the effect for relatively less populous counties. It is immediately apparent that the effect concentrates in small counties. Appointed election officials produce turnout rates that are over 2 percentage points higher than elected officials in small jurisdictions. In contrast, the point estimates for larger jurisdictions are less than 1 percentage point and not statistically distinguishable from zero. A similar pattern is found with registration rates.

These findings are in line with three explanations: that smaller jurisdictions enjoy a boost in resources when switching to appointed officials, that smaller jurisdictions face hurdles to candidate recruitment that are alleviated with appointments, or that failures of electoral accountability are most acute in smaller jurisdictions. I explore these three explanations below.

Table 5: **Appointing Local Election Officials Increases Citizen Participation in Small Counties (Presidential Elections, 1972-2020)**

	Voter Turnout			Registration Rate		
	(1)	(2)	(3)	(4)	(5)	(6)
Appointed	0.008 (0.005)	0.007 (0.005)	0.008 (0.005)	0.005 (0.007)	0.003 (0.007)	0.006 (0.007)
Appointed X Small County	0.021 (0.009)	0.023 (0.009)	0.018 (0.009)	0.017 (0.013)	0.019 (0.013)	0.007 (0.013)
Counties	426	426	426	272	272	272
Elections	13	13	13	7	7	7
Observations	5538	5538	5538	1904	1904	1904
Outcome Mean	0.50	0.50	0.50	NA	NA	NA
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year x State FEs	Yes	No	No	Yes	No	No
Year x State x Dem vs FEs	No	Yes	No	No	Yes	No
Year x State x Pop FEs	No	No	Yes	No	No	Yes

Robust standard errors clustered by county in parentheses. The number of observations is smaller in columns 4-6 because Georgia is excluded and because turnout data is available from 1970 but registration data is only available from 1996.

5.2 Do Appointed Election Officials Increase Election Administration Expenditures?

One explanation for the observed effect is that switching to appointed officials boosts election administration resources, and that this leads to increased turnout. Sufficiently funding elections is essential to ensuring high quality administration (Mohr et al. 2019, 2020; Kropf et al. 2020; McGowan et al. 2021). Burden et al. (2013) argue that appointed officials are *less* able to advocate for more resources than their elected counterparts. However, it is possible that in smaller jurisdictions switching to a dedicated appointed local election official increases the amount of full-time equivalent (FTE) employees who work in election administration. Appointed 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. County clerks 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 greatest in less popu-

lous counties, where sometimes only a single official administers elections. According to the 2020 Democracy Fund/Reed College Survey of Local Election Officials, 34 percent of jurisdictions have no full-time election administrators and 17 percent have exactly one FTE.¹⁵

I use jurisdiction election administration expenditure data from Mohr et al. (2018). This dataset includes estimated yearly election administration costs for each county in Georgia starting from as early as 2005, though there is significant missingness and high within-county variance. This enables the use of a difference-in-differences regression design to credibly estimate the causal effect of switching to appointed election officials on logged total election expenditures. Table 6 displays the results. The first three specifications include appointments, and the latter three split out the effect for small and large counties. The point estimates are large, although imprecisely estimated. The coefficient in column 1 means that when counties switch to an appointed election official, they increase their election expenditures by approximately 60 percentage points on average. However, the effect actually concentrates in larger jurisdictions. Negative coefficients in columns 4, 5, and 6 mean that switching to appointed probate judges in small jurisdictions actually leads to a reduction in election administration expenditures. These results should be interpreted cautiously given the small sample size and data quality issues. Nonetheless, they are inconsistent with the idea that increased expenditures explain the why appointed clerks boost turnout.

5.3 Are Appointed Election Officials Higher Quality?

Perhaps appointed local election officials are more experienced, more educated, and therefore of higher quality than their elected counterparts. This could be due to some failure in elections that prevent voters from selecting the most qualified individuals—for instance, because of a limited pool of viable candidates, lack of contested elections, or the absence of high-quality information. It could also be due to geographic restrictions imposed by the practice of elections.

I use the 2020 Democracy Fund/Reed College Survey of Local Election Officials, a nationwide poll of election officials, to examine whether elected and appointed officials differ on important indicators of quality. Table 7 displays the output of a series of regressions estimating potential quality differences between appointed and elected officials. All specifications include state fixed effects and both log population and log population squared controls. These ensure that comparisons

¹⁵<https://evic.reed.edu/leo-survey-summary/>

Table 6: **Appointing Local Election Officials Does Not Increase Election Expenditures in Smaller Counties (Presidential Elections, 2008-2016)**

	Log Total Election Expenditures					
	(1)	(2)	(3)	(4)	(5)	(6)
Appointed	0.609 (0.171)	0.622 (0.163)	0.636 (0.165)	0.695 (0.234)	0.723 (0.205)	0.727 (0.234)
Appointed X Small County				-0.239 (0.272)	-0.243 (0.230)	-0.268 (0.272)
Counties	126	126	126	125	125	125
Elections	3	3	3	3	3	3
Observations	296	296	296	293	293	293
Outcome Mean	12.16	12.16	12.16	12.13	12.13	12.13
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	No	No	Yes	No	No
Year x Dem vs FEs	No	Yes	No	No	Yes	No
Year x Pop FEs	No	No	Yes	No	No	Yes

Robust standard errors clustered by county in parentheses. Data is from Mohr et al. (2018) and is only available for Georgia.

are only made between appointed and elected officials who oversee elections in similarly sized jurisdictions within the same state. Any differences that arise are likely due to the selection method itself rather than inherent differences in the places that elect and appoint clerks.

The results reveal few observable differences between appointed and elected officials. Appointed officials have lower tenure lengths on average than elected officials who oversee similar jurisdictions, and they also have fewer years of experience in election administration. While not statistically significant, the point estimate for average tenure is substantively meaningful (effect size = -.28). Both of these results suggest that appointed officials experience higher turnover than elected officials. This could be indicative of stronger monitoring and sanctioning.

Appointed officials tend to be slightly better educated, have more professional memberships in election administration, and are more likely to have worked as an election official in another jurisdiction. But most of the point estimates are substantively small and none attain conventional levels of statistical significance. Regressions run with a 5-category jurisdiction size fixed effect instead of the population controls produces similar results and are found in Section A.9 of the Online Appendix. In short, quality differences do not explain why appointing clerks leads to higher turnout.

Table 7: **Appointed Local Election Officials Are Similar in Quality to Elected Officials**

	Tenure (Years) (1)	Years in administration (2)	Education (3)	Professional Memberships (4)	Served Elsewhere (5)
Appointed	-1.884 (1.205)	-2.181 (1.743)	0.336 (0.209)	0.039 (0.085)	0.028 (0.043)
States	44	44	44	44	44
Observations	698	653	581	699	664
Outcome Mean	8.76	15.25	2.86	1.17	0.15
State FEs	Yes	Yes	Yes	Yes	Yes
Log Pop	Yes	Yes	Yes	Yes	Yes
Log Pop squared	Yes	Yes	Yes	Yes	Yes

Robust standard errors clustered by state in parentheses. Data is from the 2020 Democracy Fund/Reed College Survey of Local Election Officials and is filtered to only include chief local election officials. County is imputed from zip code to calculate population controls. Observations are weighted to be representative of the population of local election officials. Column 1 measures tenure with right-truncated values of greater than 20 years at 20 years and left-truncated values of less than one year as 0 years. Column 2 measures average experience working in election administration. Column 3 measures educational attainment on a 5-point scale: high school, some college, college, some graduate school, and graduate school. Professional memberships counts the number of memberships among the following four organizations: state association of local election officials, regional and/or local association of election officials, the Election Center (National Association of Election Officials), and the International Association of Government Officials (iGO). Column 5 measures whether clerks have served as election officials in other jurisdictions.

5.4 Are Elected Election Officials Inadequately Monitored and Sanctioned?

Previous scholarship has shown that elections may sometimes fail to achieve accountability, especially for local offices (Sances 2016). Contrary to the notion that highly localized democracy ensures accountability, the evidence presented here suggests that appointments might achieve better outcomes, especially in the smallest jurisdictions. I examine two facets of accountability: voters’ access to information about local election officials and contestation rates for clerks.

If failures of electoral accountability are driving the results, these are likely to be largest in less populous jurisdictions. Smaller jurisdictions are likely to have less congruent media markets (Snyder Jr. and Strömberg 2010), meaning that there is less information available about the candidates and thus fewer opportunities for voters to electorally sanction clerks for shirking their duties. Additionally, election officials receive minimal news coverage compared with other elected offices. A ProQuest Newspaper search from 2000–2022 returns 88,047 hits for “clerk” and only 13,818 hits for “county clerk”. In comparison, a search for “mayor” returns 585,359 hits. Returns for “Senator”, “Representative”, and “Governor” return 281,455, 309,393, and 400,086 hits, respectively. A

similar trend emerges when examining state-specific newspaper archives. Using Georgia Historic Newspapers from the Digital Library of Georgia, a 1960–2022 search for “probate judge” returns only 20,907 hits, “election superintendent” only 11,608, and “election supervisor” only 5,260. In comparison, “mayor” gets 50,417 hits, “Senator” over 55,655 returns, “governor” 42,356 results, and “Board of Commissioners” over 50,000 mentions. A similar exercise using The Portal to Texas History database returns 52,347 hits for “county clerk” and only 37,490 hits for “tax assessor”. In contrast, “sheriff” returns 122,303 hits, “mayor” returns 139,262 results, and “governor” returns nearly 120,000 hits. In summary, it appears voters have access to less information about their elected election officials than other elected offices.

Even if voters have access to high-quality information, it matters little if they do not have a choice at the ballot box. Contestation rates for most local races tend to be low (Thompson 2020; Yntiso 2021), and they are particularly so for local election officials. Ferrer, Geyn, and Thompson (2021) find that only 23% of general election races feature a contest between a Democrat and a Republican, and only 12% of all contests result in a race with a margin of victory of less than 20 percentage points. In the relatively small subset of Georgia races available (from 2004 to 2020), about a quarter of both small and large counties involve a contested general election. In general, however, it is likely that smaller counties will have smaller candidate pools and thus more uncontested elections.

In summary, voters rarely have access to much information about their local election officials, and they also rarely have an alternative choice on the ballot. Both of these factors may contribute to an inability to properly monitor and sanction elected clerks. On the other hand, local elites likely have access to very high-quality information about those they hire. Appointed officials are subject to periodic review and can be dismissed if performing inadequately. The fact that appointed officials tend to have shorter tenures and thus higher turnover rates than elected officials in similar jurisdictions is suggestive evidence of stronger monitoring and sanctioning.

6 Conclusion

Elections are designed to achieve accountability between officeholders and the public. When people have access to high-quality information, can make a choice between multiple candidates, and are

able to effectively sanction an officeholder who shirks their duty, agents will be incentivized to perform their best in order to win another term in office. However, if voters do not have access to adequate information or a sufficient choice on election day, there is little they can do to demand accountability from elected officials.

Using original data and a causally credible research design, I show that when counties switch from elected to appointed local election officials, their voter participation rates significantly increase. The findings are robust to alternate specifications, hold across multiple time periods and states, and are not explained by differences in election resources or the quality of officeholders. Rather, it appears that appointed officials are better monitored and sanctioned than their elected counterparts.

These findings add to both a longstanding debate in public administration (Al-Habil 2011) and a growing literature on the limits of elections in ensuring accountable officeholders (Ashworth 2012). They are in line with findings that appointing other local offices, such as municipal assessors and treasurers (Sances 2016; Whalley 2013), leads to better policy outcomes. Taken together, the results suggest that local elections may systematically fail to ensure accountability or create adverse accountability effects that have undesirable policy consequences. This is especially true considering information environments at the local level continue to deteriorate (Lockhart 2021; Martin and McCrain 2019), the tasks demanded of these officials grow more complex, and contestation rates remain low. These factors conspire to make elections a detriment to optimal administrative outcomes.

Appointments alone do not guarantee desirable administrative outcomes. The political context of the transition is equally important. In the 1960s, counties in the South eliminated elected offices in the wake of the Voting Rights Act for the express purpose of maintaining white power (Komisarchik 2018). The politicization of appointing authorities is emerging as a concern once again. For instance, several recently enacted bills in Georgia have created highly partisan election boards.¹⁶

Future work should consider other observable implications of electing vs. appointing clerks, such as effects on voter confidence, as well as other instances where local elections fail to achieve their intended effects. We need better measures of accountability outcomes as well (Carreri and Payson

¹⁶<https://www.washingtonpost.com/nation/2022/03/14/georgia-elections-fraud-purge/>

2021). Scholarship should work to distinguish between responsiveness and conflicts in principals' goals. Are appointments beneficial only when the desires of voters and local elites align? How often do they diverge, and what factors make preference convergence more likely? Measuring which issues and to what degree elites and voters have differing preferences could go a long way to clarifying the contexts where appointments produce more favorable outcomes.

These findings also inform ongoing debates over the best forms of election administration in the US. Counties in Georgia and Texas continue to actively change their selection methods for election officials. They are not alone. Hundreds of jurisdictions across a dozen states have begun appointing their local election officials over the past few decades (Ferrer and Geyn 2022). These changes are likely to continue, especially given the country's renewed attention on local election administrators as stewards of the democratic process.

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Online Appendix

Intended for online publication only.

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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 voter 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 Increases Citizen Participation (Presidential, Senate, and Gubernatorial Elections, 1970-2020)**

	Voter Turnout			Registration Rate		
	(1)	(2)	(3)	(4)	(5)	(6)
Appointed	0.019 (0.004)	0.019 (0.004)	0.016 (0.004)	0.013 (0.006)	0.013 (0.006)	0.009 (0.005)
Counties	440	440	440	282	282	282
Elections	13	13	13	7	7	7
Observations	17380	17380	17380	5513	5513	5513
Outcome Mean	0.42	0.42	0.42	0.80	0.80	0.80
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

Robust standard errors clustered by county in parentheses. Voter turnout and registration rate are measured as proportions out of 1. The number of observations is smaller in columns 4-6 because Georgia is excluded and because turnout data is available from 1970 but registration data is only available from 1996.

A.2 Participation Effects with Alternative Administrative Data

Conflicts arose between administrative and web scrapped data in Texas, and the main results included some data imputations from missing data. Table A.2 shows that the main finding that appointed election officials increase voter participation is robust to an alternative specification privileging documents provided by the Texas Secretary of State over web scrapped data and removing all data imputations in the election official selection dataset.

Table A.2: **Appointing Local Election Officials Increases Citizen Participation (Presidential Elections, 1972-2020, FOIA Preferred)**

	Voter Turnout			Registration Rate		
	(1)	(2)	(3)	(4)	(5)	(6)
Appointed	0.019 (0.004)	0.019 (0.004)	0.017 (0.004)	0.015 (0.006)	0.017 (0.007)	0.009 (0.006)
Counties	433	433	433	276	276	276
Elections	13	13	13	7	7	7
Observations	5614	5614	5614	1929	1929	1929
Outcome Mean	0.50	0.50	0.50	0.81	0.81	0.81
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

Robust standard errors clustered by county in parentheses. The number of observations is smaller in columns 4-6 because Georgia is excluded and because turnout data is available from 1970 but registration data is only available from 1996.

A.3 Exploring State and Office Heterogeneity

Is the main result generalizable across states and offices? Table A.3 estimates the effects of appointing election officials on voter participation for each state separately. The results reveal statistically significant effects in both Georgia and Texas. Interestingly, it appears switching to appointed election officials in Georgia has an effect on turnout nearly double the size of the effect in Texas—2.4 percentage points compared with 1.2 percentage points. In both states, a null of no effect can be confidently rejected. Due to the small number of counties involved, the estimation of the effect on turnout in Oregon is imprecise.

Table A.3: **Appointing Local Election Officials Increases Voter Turnout in Georgia and Texas (Presidential Elections, 1972-2020)**

	Voter Turnout		
	(1)	(2)	(3)
Appointed	0.025 (0.007)	0.015 (0.006)	-0.013 (0.017)
Counties	157	241	35
Elections	13	13	13
Observations	2041	3126	455
Outcome Mean	0.47	0.50	0.64
State	GA	TX	OR
County FEs	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes

Robust standard errors clustered by county in parentheses.

I also examine whether the effect holds across different statutory offices. Most directly elected election officials across the United States are county clerks. If the results hold for Oregon and 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 changes within counties between clerks and tax-assessors as the election official. Table A.4 shows that both elected clerks and probate judges produce significantly lower voter turnout relative to appointed officials (the omitted category).¹⁷ It appears that tax assessors do not underperform relative to appointed administrators, although these results are

¹⁷County clerk and district & county clerk offices are pooled for parsimony.

imprecisely estimated and not statistically distinguishable from zero. In sum, the results hold across multiple states and offices.

Table A.4: **Clerks and Probate Judges Reduce Citizen Participation (Presidential Elections, 1972-2020)**

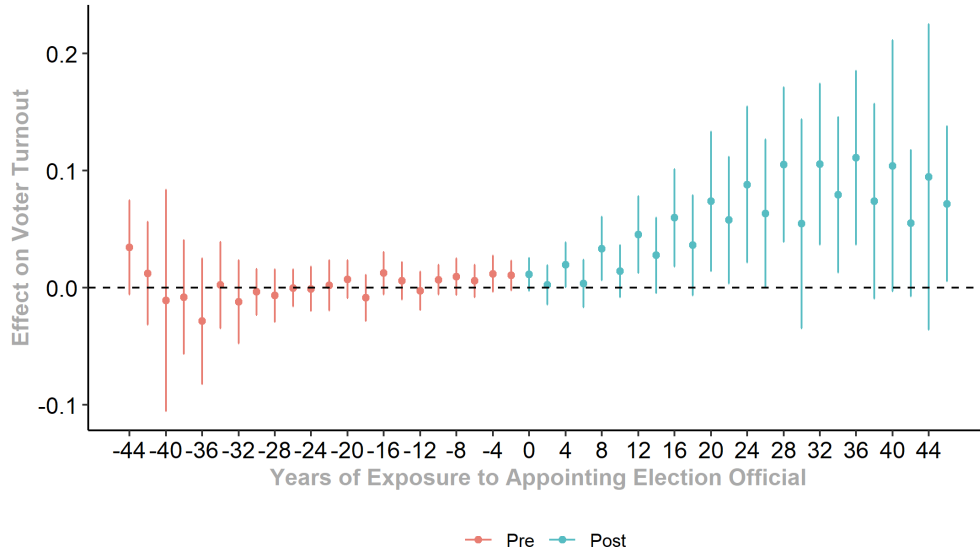
	Turnout (1)	Registration (2)
Tax Assessor	0.006 (0.014)	-0.007 (0.015)
Clerk	-0.016 (0.006)	-0.016 (0.006)
Probate Judge	-0.025 (0.007)	-0.011 (0.011)
Counties	433	433
Elections	13	7
Observations	5622	3126
Outcome Mean	0.50	0.77
County FEs	Yes	Yes
Year x State FEs	Yes	Yes
Robust standard errors clustered by county in parentheses.		

A.4 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. Because state-year fixed effects cannot be incorporated, I run separate estimations for Georgia and Texas and omit Oregon due to the small sample size. Dynamic effects for Georgia and Texas are visualized in Figures A.1 and A.2, cohort effects are visualized in Figures A.3 and A.4, and time period effects are visualized in Figures A.5 and A.6, respectively.

As seen in Figure 2, there appears to be increasing improvements to voter turnout over time for counties that switch to appointed administrators, relative to counties with elected officials. In other words, appointed officials do an increasingly better job of producing higher turnout as the tenure of their selection mechanism extends, or the value of appointed officials over elected ones

Figure A.1: **Average Effect of Appointed Election Officials on Voter Turnout by Length of Exposure to Appointing - Georgia.** 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 heterogeneous treatment effects.

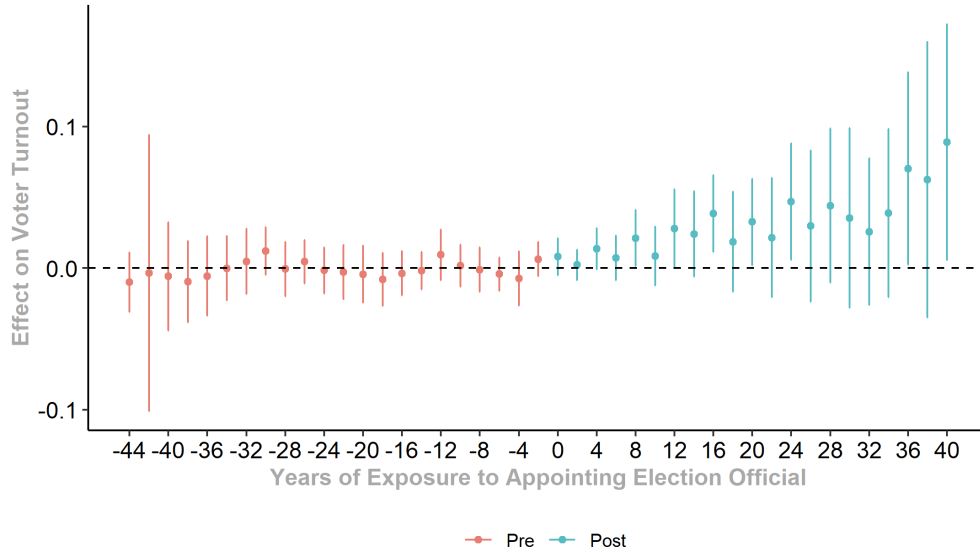


has grown over time. Figures A.1 and A.2 provide additional evidence for the dynamic effects of appointments on voter participation. This could be due to some combination of institutional learning effects and start-up costs of switching selection methods, whereby appointed officials need the practice of administering a few elections to realize their full potential. An equally plausible alternative is that the benefits to appointing relative to electing election officials has increased over time. This would lead those counties that switched earlier to appear to have a growing over time effect, when in reality all jurisdictions are experiencing increasing gains. This could be due to a combination of factors, including a declining local media environment, the increasingly technical demands of the job, and growing recruitment problems.¹⁸

Figures A.3 and A.4 display cohort treatment effects of the Callaway and Sant'Anna (2021) estimator. Although the estimates are noisy, they suggest that earlier adopters of appointed election

¹⁸<https://www.inquirer.com/politics/election/spl/pennsylvania-election-2020-officials-retiring-nightmare-20201221.html>

Figure A.2: **Average Effect of Appointed Election Officials on Voter Turnout by Length of Exposure to Appointing - Texas.** 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 heterogeneous treatment effects.



officials may have experienced stronger overall treatment effects than more recent adopters. Figures A.5 and A.6 display time period effects of switching to appointing election officials. They suggest that the greater effect of earlier adopters is due to long-term accumulation rather than a diminishing instantaneous 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 evidence of long-run dynamic gains in having appointed rather than elected officials run elections, as well as the idea that the gap in turnout produced by appointed and elected election officials has grown.

Figure A.3: **Average Effect of Appointed Election Officials on Voter Turnout by Cohort Group - Georgia.** 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 heterogeneous treatment effects.

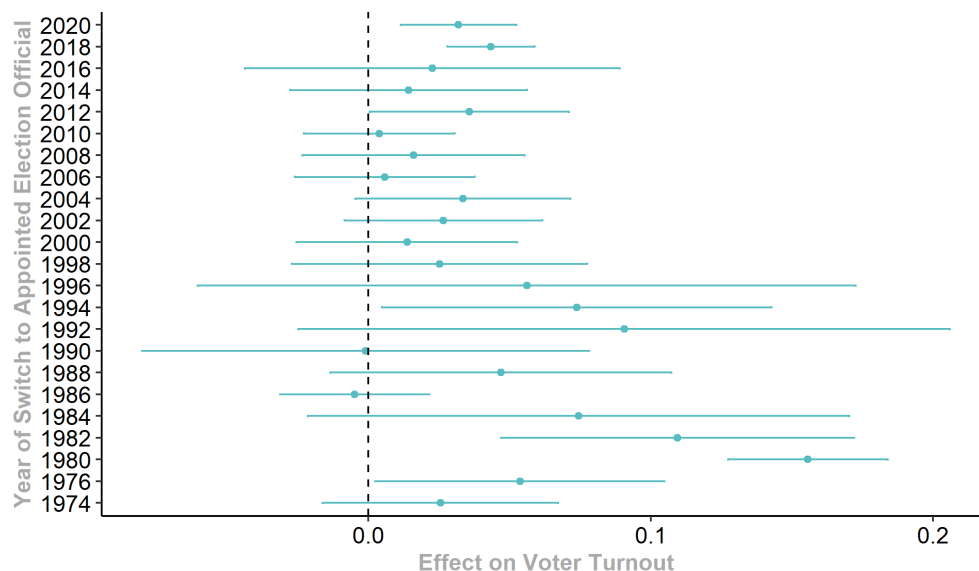


Figure A.4: **Average Effect of Appointed Election Officials on Voter Turnout by Cohort Group - Texas.** Each point is an estimate of the average group effect of appointing election officials on presidential voter turnout for counties that switch in the given cohort year. The lines above and below each 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 heterogeneous treatment effects.

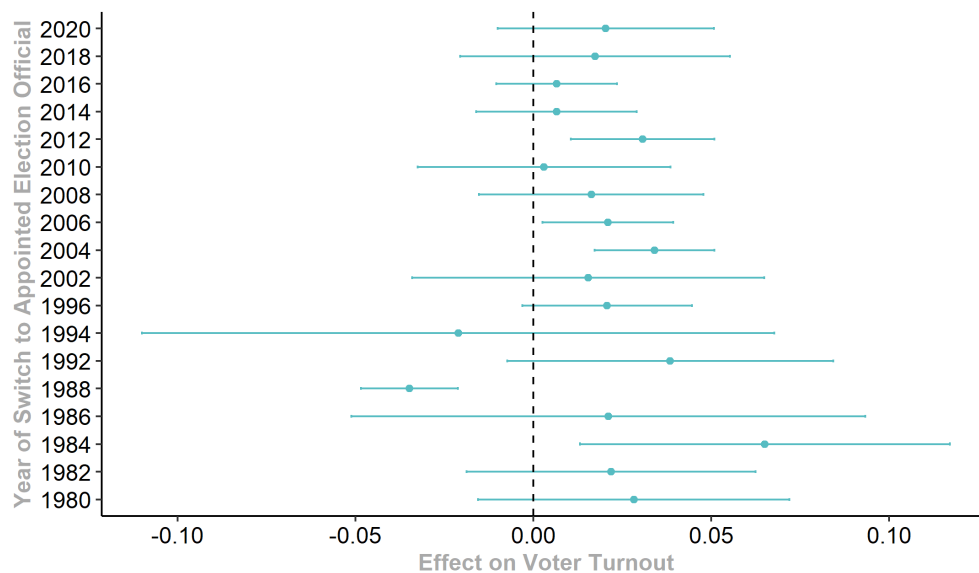


Figure A.5: **Average Effect of Appointed Election Officials on Voter Turnout by Time Period - Georgia.** Each point is an estimate of the average time period effect of appointing election officials on presidential voter turnout. The lines above and below each point represent 95-percent confidence intervals. Estimates are from the Callaway and Sant'Anna (2021) estimator for dynamic two-way fixed effects designs, which corrects for bias due to heterogeneous treatment effects.

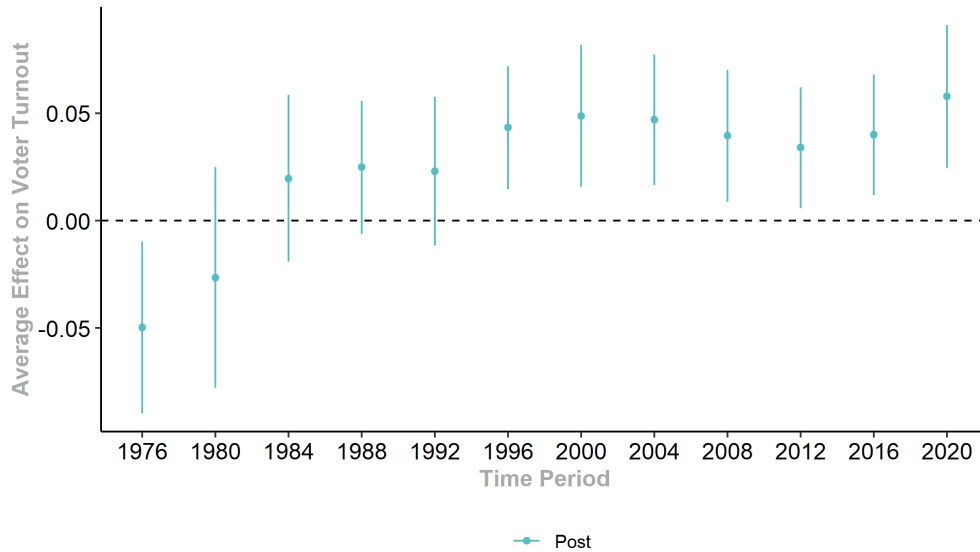
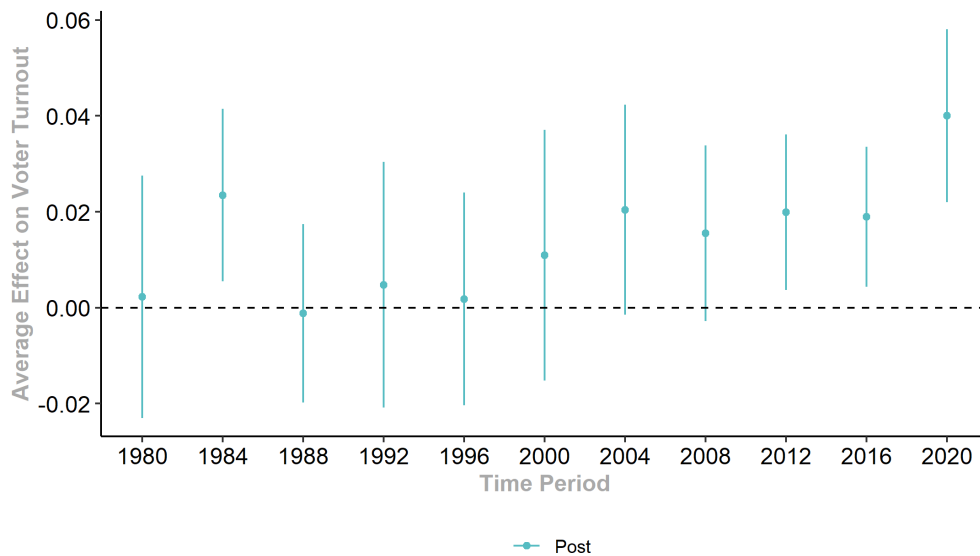


Figure A.6: **Average Effect of Appointed Election Officials on Voter Turnout by Time Period - Texas.** Each point is an estimate of the average time period effect of appointing election officials on presidential voter turnout. The lines above and below each point represent 95-percent confidence intervals.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



A.5 Are the Results an Artifact of the Jim Crow South?

One concern is that registration and turnout rates of African-Americans in Georgia and Texas were artificially low in the earlier periods of the dataset due to the lingering effects of racially targeted barriers to the ballot box. Even though the Voting Rights Act passed in 1965, African-American registration rates in South continued to trail those of white voters until many decades later. For instance, African American and white registration rates in Louisiana did not achieve parity until 2000 (Keele, Cubbison, and White 2021). If counties that switch to appointments are more likely to have large African-American populations (Komisarchik 2018), then the inclusion of these earlier years in the dataset could confound the relationship between appointments and voter turnout.

Table A.5 displays three cutoffs of the data: starting with the 1980 presidential election, the 1992 presidential election, and the 2000 presidential election. The main analysis displayed in Table 1 relies on turnout data beginning with the 1972 presidential election. Because registration data is only available from 1996, I focus on voter turnout here. The results show some evidence of diminishing effect sizes when only more recent data is considered. However, in all estimations the point estimates are substantively large and statistically distinguishable from zero. In the most restrictive analysis, considering data from 2000 onwards, counties that switch to appointed clerks are estimated to boost turnout by 1.1 percentage points.

Table A.5: Finding that Appointing Local Election Officials Increases Citizen Participation is Robust to Alternative Year Cutoffs

	Voter Turnout		
	(1)	(2)	(3)
Appointed	0.018 (0.004)	0.014 (0.004)	0.011 (0.003)
Counties	433	433	433
Elections	11	8	6
Observations	4758	3462	2598
Outcome Mean	0.51	0.51	0.52
Year Cutoff	1980	1992	2000
County FEs	Yes	Yes	Yes
Year x State FEs	Yes	Yes	Yes

Robust standard errors clustered by county in parentheses. Year cutoff indicates the first presidential election included in the analysis.

A.6 Do Appointed Election Officials Follow Different Election Administration Policies?

Perhaps directly electing election officials curtails voter 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 examine a number of election administration policies and use CCES data on voter wait times.

Table A.6 displays the results of a two-way fixed effects regression of appointing election officials on the following county-level variables: number of polling places per 1,000 residents, provisional votes share, provisional rejection rate, absentee rejection rate, registration removal rate, and share of voters experiencing wait times greater than 30 minutes. While the nature of the data should caution against any definitive takeaways, there is no indication that appointed officials pursue election administrative policies that differ from those of directly elected officials. All of the estimates are precisely estimated, except for voter wait times. Table A.7 includes data from Gubernatorial and Senate elections and reveals similar results.

Table A.6: **Appointed and Directly Elected Local Election Officials Pursue the Same Election Administration Policies (Presidential Elections, 2000-2020)**

	Polling Places (1)	Prov Share (2)	Prov Rejection (3)	Absentee Rejection (4)	Reg Removal (5)	Wait Share (6)
Appointed	-0.007 (0.051)	0.001 (0.001)	-0.030 (0.039)	-0.001 (0.004)	-0.008 (0.006)	0.002 (0.017)
Counties	433	433	423	433	433	339
Elections (avg)	4	4	4	5	4	3
Observations	1512	1509	1304	2002	1415	750
Outcome Mean	1.171	0.004	0.639	0.024	0.086	0.047
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year x State FEs	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors clustered by county in parentheses. Columns 1 through 5 use EAVS survey data from the US Election Assistance Commission. Column 1 measures the number of polling places per 1,000 residents, column 2 the share of votes cast provisionally, column 3 the share of provisional ballots rejected, column 4 the share of absentee ballots rejected, and column 5 the share of registrants removed from the list. Column 6 measures the share of voters in the CCES reporting a wait time of longer than 30 minutes.

Table A.7: **Appointed and Directly Elected Local Election Officials Pursue the Same Election Administration Policies (Presidential, Senate, and Gubernatorial Elections, 2000-2020)**

	Polling Places (1)	Prov Share (2)	Prov Rejection (3)	Absentee Rejection (4)	Reg Removal (5)	Wait Share (6)
Appointed	-0.017 (0.039)	0.001 (0.001)	-0.035 (0.034)	0.007 (0.006)	-0.005 (0.003)	0.023 (0.014)
Counties	440	440	434	440	440	364
Elections (avg)	3	4	3	5	4	3
Observations	4032	4575	3647	5669	4890	2056
Outcome Mean	1.114	0.003	0.610	0.028	0.096	0.042
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year x State x Office FEs	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors clustered by county in parentheses. Columns 1 through 5 use EAVS survey data from the US Election Assistance Commission. Column 1 measures the number of polling places per 1,000 residents, column 2 the share of votes cast provisionally, column 3 the share of provisional ballots rejected, column 4 the share of absentee ballots rejected, and column 5 the share of registrants removed from the list. Column 6 measures the share of voters in the CCES reporting a wait time of longer than 30 minutes.

A.7 Validating the Staggered Rollout Design with State-Specific Estimates

I run the de Chaisemartin and D’Haultfoeulle (2020) and the Callaway and Sant’Anna (2021) estimators separately for each state. This is to ensure that the results are not biased by differential trending between states. The results are displayed in Tables A.8, A.9, and A.10. The de Chaisemartin and D’Haultfoeulle (2020) estimator employs dynamic effects with placebos. The Callaway and Sant’Anna (2021) estimator employs dynamic effects after aggregating counties into cohorts that begin treatment at the same time. This estimator is very similar to the one displayed in column 4 of Table 2. First, always treated units are removed from the dataset. This 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 to treatment 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. Doing so avoids negative weights, thereby addressing the weighting problems of the simple two-way fixed effects estimator.

Table A.8: **Main Finding that Appointing Local Election Officials Increases Voter Turnout is Robust to Alternate Specifications - Georgia**

	Voter Turnout		
	Two-Way FEs (1)	de Chaisemartin and D'Haultfoeuille (2)	Callaway and Sant'Anna (3)
Appointed	0.025 (0.007)	0.014 (0.007)	0.058 (0.013)
Counties	157	157	154
Elections	13	13	13
Observations	2041	4455	2002
Outcome Mean	0.47	0.47	0.47
County FEs	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes

Robust standard errors clustered by county in parentheses.

These tables further validate the main findings. All estimators for Georgia and Texas return positive point estimates and are precisely estimated. Both of the alternative estimators for Oregon return positive values, with the Callaway and Sant'Anna estimator statistically significant. In summary, these results validate the main finding that appointed local election officials increase participation.

Table A.9: **Main Finding that Appointing Local Election Officials Increases Voter Turnout is Robust to Alternate Specifications - Oregon**

	Voter Turnout		
	Two-Way FEs (1)	de Chaisemartin and D'Haultfoeuille (2)	Callaway and Sant'Anna (3)
Appointed	-0.013 (0.017)	0.013 (0.005)	0.019 (0.008)
Counties	35	35	32
Elections	13	13	13
Observations	455	369	416
Outcome Mean	0.64	0.64	0.64
County FEs	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes

Robust standard errors clustered by county in parentheses.

Table A.10: **Main Finding that Appointing Local Election Officials Increases Voter Turnout is Robust to Alternate Specifications - Texas**

	Voter Turnout		
	Two-Way FEs (1)	de Chaisemartin and D'Haultfoeuille (2)	Callaway and Sant'Anna (3)
Appointed	0.015 (0.006)	0.011 (0.002)	0.032 (0.008)
Counties	241	241	253
Elections	13	13	13
Observations	3126	7835	3282
Outcome Mean	0.50	0.50	0.50
County FEs	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes

Robust standard errors clustered by county in parentheses.

A.8 Generalized Synthetic Control Estimators

Figures A.7 and A.8 displays output from the Xu (2017) generalized synthetic control estimator for Georgia and Texas, respectively, measuring the average effect of treatment on the treated units. Oregon is omitted due to the limited number of observations. This method allows for a relaxation of the parallel trends assumption by matching treated and untreated units to create balance pre-treatment. Both figures show positive divergences from zero after counties switch from elected to appointed local election administrators. In other words, it appears that once the sample is rebalanced to eliminate concerns about pretrending, an effect on turnout continues to be observed for each state. This provides additional evidence that appointed election officials administer elections with higher turnout than their elected counterparts.

Figure A.7: **Estimated ATT of Generalized Synthetic Control - Georgia.** This graph displays a generalized synthetic control method of the two-way fixed effects regression estimating the effects of appointing local election officials on presidential turnout in Georgia. 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.

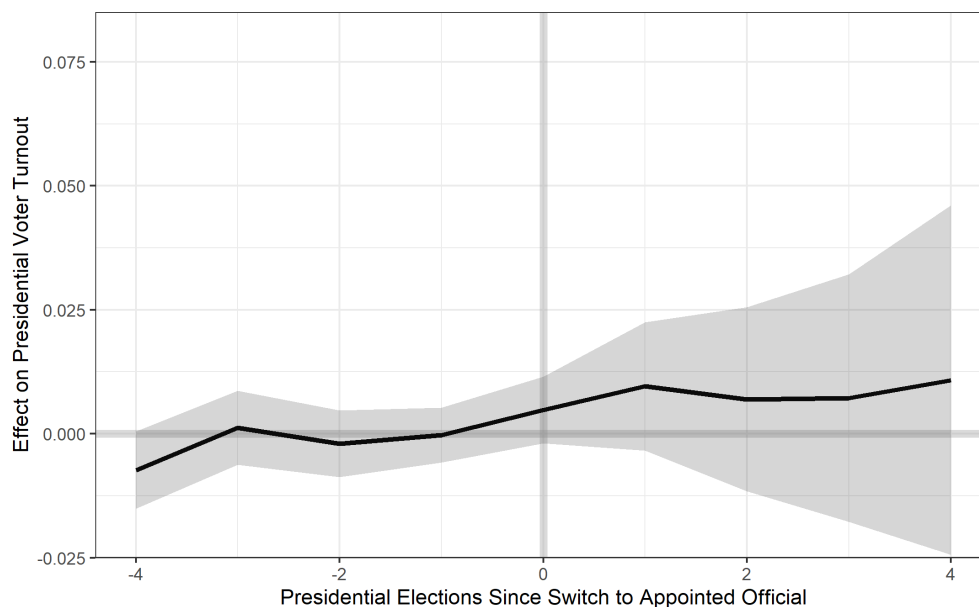
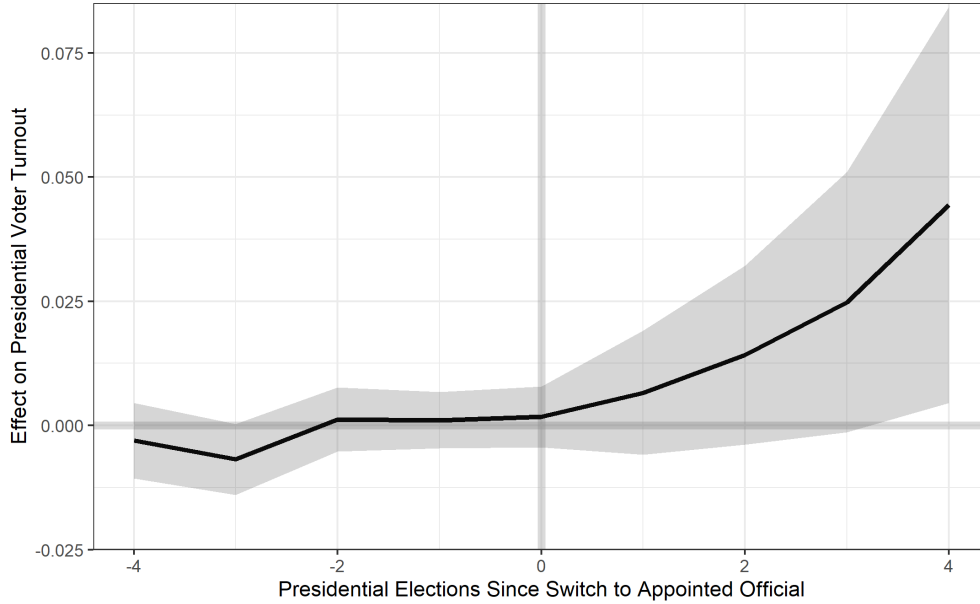


Figure A.8: **Estimated ATT of Generalized Synthetic Control - Texas.** This graph displays a generalized synthetic control method of the two-way fixed effects regression estimating the effects of appointing local election officials on presidential turnout in Texas. The specification includes two-way 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.



A.9 Finding that Appointed Election Officials Are Similar in Quality to Elected Officials Robust to Alternative Specification

Table A.11 shows that using a jurisdiction size fixed effect rather than logged population and logged population square controls returns similar results to those shown in 5.3. Because the original survey included zip code rather than county, an imputation exercise was required to back out county identifiers for each respondent, which were then matched with 2020 census data. The jurisdiction size fixed effect is original to the 2020 Democracy Fund/Reed College Survey, and thus of higher reliability.

Table A.11: **Appointed Local Election Officials Are Similar in Quality to Elected Officials - Alternative Population Controls**

	Tenure (Years) (1)	Years in administration (2)	Education (3)	Professional Memberships (4)	Served Elsewhere (5)
Appointed	-2.089 (1.294)	-2.485 (0.002)	0.288 (0.194)	-0.012 (0.075)	0.027 (0.041)
States	44	44	44	44	44
Observations	717	672	599	718	682
Outcome Mean	8.74	15.15	2.86	1.18	0.14
State FEs	Yes	Yes	Yes	Yes	Yes
Jurisdiction Size FEs	Yes	Yes	Yes	Yes	Yes

Robust standard errors clustered by state in parentheses. Data is from the 2020 Democracy Fund/Reed College Survey of Local Election Officials and is filtered to only include chief local election officials. Jurisdiction size fixed effects are derived directly from survey responses and break down into five categories: 0 to 5,000 people, 5,001 to 25,000 people, 25,001 to 100,000 people, 100,001 to 250,000 people, and greater than 250,000 people. Observations are weighted to be representative of the population of local election officials. Column 2 measures average experience working in election administration. Column 3 measures educational attainment on a 5-point scale: high school, some college, college, some graduate school, and graduate school. Professional memberships counts the number of memberships among the following four organizations: state association of local election officials, regional and/or local association of election officials, the Election Center (National Association of Election Officials), and the International Association of Government Officials (iGO). Column 5 measures whether clerks have served as election officials in other jurisdictions.