

To Elect or Appoint? Evidence from Local Election Administration.*

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Abstract

Do elected or appointed officials produce better outcomes for their constituents? Elections should improve representation by providing a direct link to voters. However, some argue that citizens may have too little information to select good leaders and hold them accountable, especially at the local level. In order to assess these conflicting claims, I examine the performance of local election officials, an office that has come under immense strain to deliver democratic elections and for which selection method is a live policy debate. Using an original collection of election administration structures in 1,116 counties across 13 states and over 62 years, I leverage changes in selection method over time to credibly measure differences in the election outcomes produced by local election officials based on whether they were elected or appointed. I find that appointed officials out-perform their elected counterparts, increasing voter turnout by one to two percentage points and raising registration rates by one percentage point. Appointed officials appear to boost election administration resources, more actively communicate with voters, and reduce voter wait times. I present evidence that the quality of selection and sanctioning are higher for appointed officials, leading to better educated and more closely monitored agents. My findings speak to the challenges in designing local institutions that advance and protect democratic ideals.

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1 Introduction

One of the challenging aspects of designing democracies is deciding which public officials to directly elect and which to appoint. America’s founders ratified a Constitution that relied almost exclusively on appointments. James Madison justified the indirect selection of the president via the Electoral College by reasoning that “A small number of persons, selected by their fellow-citizens from the general mass, will be most likely to possess the information and discernment requisite to such complicated investigations” (Madison 1788). In the 19th century, Jacksonian reformers expanded the practice of directly electing public officials to include senators, judges, state executives, and a multitude of county and municipal offices, whereas by the early 20th century, Progressive reformers sought to return many of these positions to appointments.

Scholars disagree about whether elections or appointments produce better outcomes for constituents. Elections should improve representation by providing a direct link between voters and their agents (Besley 2006; Ferraz and Finan 2011). However, the mass public may not have sufficient information compared to political elites, leading to the selection of less qualified officials and weaker accountability once in office (Gailmard and Jenkins 2009). Elections’ agency problems are likely strongest in local politics, where expertise is hardest to find and the public is least aware of their agents’ activities (Whalley 2013).

In this paper, I assess conflicting claims over selection method by studying the consequences of appointing vs. electing the people responsible for running elections. Unlike any other Western democracy, the U.S. delegates election administration duties to over 8,000 local officials who handle the minutiae of elections: registering voters, hiring poll workers, locating polling places, mailing ballots, tallying votes, and certifying results (Hale, Montjoy, and Brown 2015). These administrators have endured intense scrutiny in recent years, especially when former President Trump alleged the 2020 presidential election was stolen. Some officials were pressured to refuse to certify the election results,¹ and many have re-

¹<https://www.nytimes.com/2020/11/17/us/politics/michigan-certify-election-results.html>

ceived threats of violence over baseless accusations of malfeasance.² Some are elected and some are appointed (Kimball, Kropf, and Battles 2006)—a balance that is tipping more heavily towards appointments in recent decades while also becoming increasingly contested politically (Ferrer and Geyn 2023). Harris County, the third most populous county in the country, was forced by the Texas state legislature to switch its chief election official from an appointed to an elected position in 2023.³ Georgia’s state government considered taking over the administration of its most populous county after the legislature passed legislation in 2021 empowering it to do so.⁴ And Miami-Dade is being forced to return to an elected election supervisor after the approval of a potentially misleading referendum in 2018.⁵

Over 300 jurisdictions—nearly 1 out of 4 counties across 13 U.S. states that comprise 40% of the country’s population—have switched from electing to appointing their chief local election official since 1960. I leverage an exhaustive original collection of clerk selection methods spanning 1,116 counties and 28 federal elections to provide the strongest evidence to date for whether elected or appointed officials produce better outcomes for their constituents.⁶ Within-jurisdiction variation in selection method over time allows me to identify a precise effect on differences in election outcomes.

I use measures of voter participation such as turnout and registration rates as my primary outcome. Voter participation is one of the few reliable measures of election quality available over a large span of time. It is also an important one, frequently used in election quality indices such as the MIT Election Performance Index and the Varieties of Democracy Project. More than two-thirds of election officials consider increasing participation a central component of their job,⁷ as does their chief professional organization, the National Associa-

²<https://www.nytimes.com/2022/09/06/us/politics/midterms-elections-threats-security.html>

³<https://www.nytimes.com/2023/05/28/us/texas-voting-laws-harris-county.html>

⁴<https://georgiarecorder.com/2021/08/18/panel-begins-review-of-fulton-elections-ahead-of-potential-state-takeover/>

⁵<https://www.miamiherald.com/news/politics-government/election/article215034905.html>

⁶I occasionally refer to local election officials as clerks in shorthand. While clerks are the most common county election officials, there is wide variation in the position title across states and counties.

⁷https://evic.reed.edu/wp-content/uploads/2021/04/leo2020_codebook.pdf

tion of Election Officials.⁸ Finally, local election officials likely have the ability to influence participation rates given their far-ranging duties and discretion over administrative decisions (Burden et al. 2013; Kimball and Kropf 2006). Election administrator decisions over communication strategies (Merivaki and Suttman-Lea 2023), election expenditures (Grose 2022), and polling places (Yoder 2018) have all been shown to affect participation, as well as their indirect ability to shape voter wait times (Pettigrew 2017).

I find that when counties switch from electing to appointing their local election official, voter turnout in presidential elections increases by between 1 and 2 percentage points and registration rates increase by 1 percentage point. These findings are robust to a variety of different estimators; hold across multiple states, offices, years, and reform mechanisms; are largest in less populous jurisdictions; and do not differ by jurisdiction partisanship or come at the expense of increased partisan manipulation of elections. They are also substantively significant. A 2 percentage point boost to voter turnout in federal elections is equivalent to or larger than the effect of universal vote-by-mail (Thompson et al. 2020), automatic voter registration (McGhee, Hill, and Romero 2021), 10 additional days of early voting (Kaplan and Yuan 2020), or a door-to-door canvassing campaign (Green, McGrath, and Aronow 2013). I find suggestive evidence that part of the effect may be due to greater resource provision, contributing to additional staff, more active communication with constituents, and shorter wait times at the polls.

Through a series of mechanism tests, I show that the quality of selection and sanctioning is higher for appointed clerks than elected clerks. Appointed officials are more likely to hold a college degree and elected administrators rarely face competition at the polls. I identify three factors likely contributing to better sanctioning of appointed administrators: voters know little about their local election official, the differences between elected and appointed clerks are largest in areas lacking a local newspaper, and appointed officials have higher turnover rates.

⁸<https://www.electioncenter.org/about-us.php>

My findings speak to the challenges in designing local institutions that advance and protect democratic ideals. In the midst of unprecedented threats to that democracy, declining trust in elections (Stewart 2021), and partisan moves to shape election administration (Ferrer and Geyn 2023), this paper also informs ongoing debates over who should run elections in the U.S.

2 Selecting Public Officials

The United States is exceptional in the number of public officials we elect. By one count, there are approximately 520,000 elected officials in the country, with 96% of these politicians elected at the local level (Lawless 2012). I consider why we might expect appointing officials to be preferable to electing officials and the findings of prior scholarship on selection method.

2.1 Why Might Appointed Officials Produce Better Outcomes for Their Constituents?

According to political economy theories of governance, elections improve representation by allowing voters to select higher-quality politicians and ensuring their accountability to the electorate through the sanctioning mechanism of reelection (Besley 2006; Besley and Case 2003; Besley and Coate 2003; Fearon 1999). In some empirical contexts, it appears that elections do achieve these goals, producing officials who are more competent than the constituents they represent (Dal Bó et al. 2017), who work harder when they have the incentive of being reelected (Alt, Bueno de Mesquita, and Rose 2011; Christensen and Ejdemyr 2018; Ferraz and Finan 2011; Fourniaies and Hall 2022), and who better represent voters (Besley and Coate 2003). In other contexts, however, elections may have unintended consequences, 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 (Anzia and Berry 2011; Hall 2019). The skills that make someone a good politician may not align closely with the factors that make someone a good public 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 typically must live within the jurisdiction they are elected to, whereas appointed administrators can be chosen from a broader geographic pool. Additionally, technological advancements and population growth have led many local public duties to require greater expertise, including election administration (Hale, Montjoy, and Brown 2015). Local elections are rarely contested (Burden and Snyder 2021; Lappie and Marschall 2018; Marschall and Lappie 2018). In the 2020 general election, 78% of all county-level races went uncontested,⁹ and half of all elections for partisan office went uncontested in 2022.¹⁰ 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 candidate 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; Rogers 2023). 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, distinguish between highly and poorly performing election officials, or select on quality rather than ideology or other characteristics (Franchino and Zucchini 2015). Local media has been on the

⁹<https://organizations.ballotready.org/research/nothing-to-lose-uncontested-races-in-2020-and-their-implications>

¹⁰<https://www.nytimes.com/2024/09/04/us/missouri-uncontested-races-elections.html?smid=nytcore-android-share>. See also <https://www.civicpulse.org/post/how-many-local-elections-are-uncontested>.

decline over the past few decades (Martin and McCrain 2019) and has increasingly devoted less attention to local politics (Lockhart 2021). This has led to less informed citizens and less competitive local races (Rubado and Jennings 2020). The large number of elected positions may cause voter fatigue and high ballot roll-off, with not many voters making it all the way to the bottom of the ballot where local offices are typically found (Augenblick and Nicholson 2015). Voters might be especially poor judges of performance in complex and technical policy areas (Whalley 2013). The voter information gap for election officials is particularly acute, as election officials have a portfolio of non-election responsibilities and unintuitive titles that dilute the ability of voters to effectively monitor and sanction their performance.¹¹ If public officials are acting rationally, we should expect them to shirk their duties in these circumstances, since their principals (the voters) lack the information necessary to effectively monitor and sanction them. The information-poor environment voters face contrasts with the richer information environment that appointed officials' principals possess. Appointments for election administrators are typically made by boards of local elites and can include county officers, local party chairs, judges, and county supervisors. Many of these officers are elected and thus have a high degree of familiarity with the election process.

Finally, elections can create adverse incentives for officeholders to make politically motivated decisions that are normatively undesirable (Canes-Wrone, Herron, and Shotts 2001). Electing rather than appointing assessors in New York exacerbates economic inequalities (Sances 2016), electing rather than appointing city managers skews economic policies towards the wealthy (Lubell, Feiock, and De La Cruz 2009), and electing rather than appointing municipal assessors in California leads to the adoption of more costly policies and higher borrowing costs (Whalley 2013). If a majority of the voting electorate prefers political outcomes achieved by reducing participation, then elected officials could be incentivized to concentrate costs on certain voters or discourage voting across the board. Likewise, elected officials have won office with the present electorate, so they might be disinclined to pursue

¹¹Examples include probate judge in Alabama and Georgia; auditor in Iowa, South Dakota and Washington; and tax assessor in Texas.

actions to expand the electorate. 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.

2.2 Prior Scholarship on Selection Method

A number of studies have examined the differences between elected and appointed public officials in federal, state, and local contexts. Elected officials tend to be more responsive to their constituents (Gailmard and Jenkins 2009), but participation gaps could cause responsiveness to skew policy outcomes in ways that benefit the wealthy and whites (Hajnal and Trounstein 2014; Lubell, Feiock, and De La Cruz 2009; Sances 2016) and lead to more punitive judicial outcomes (Gordon and Huber 2007; Huber and Gordon 2004). Additionally, appointing local officials has been found to improve policy outcomes in some cases. In a study of California treasurers, Whalley (2013) finds that municipalities that switched from elected to appointed treasurers enjoyed lower borrowing costs. He concludes that voters may be poorly equipped to judge performance, especially in complex policy areas.

A cross-sectional study of Wisconsin election officials found that elected clerks produce higher turnout, although it relied on the assumption that elected and appointed clerks were assigned as-if randomly in the state (Burden et al. 2013). The authors theorized 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 appoint 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.

3 Data and Methods

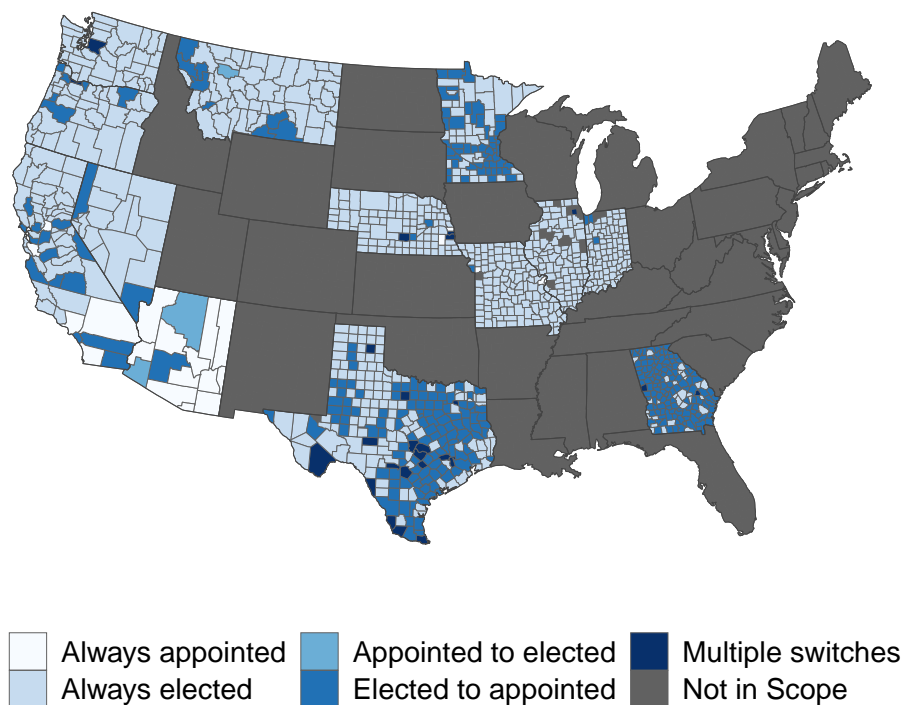
3.1 Measuring the Selection Method of Local Election Officials

I construct original panel data on the selection method of local election officials in 13 states from 1960 to 2022. In total, my dataset covers 62 years of election administration structures for 1,116 counties, encompassing over 30,000 county-federal election observations.

My sample consists of every state in the United States with at least one county-level change between appointing and electing clerks since 1960. These 13 states are a subset of the 42 states in the country where elections are primarily administered at the county level (Ferrer and Geyn 2023) and combined cover over 40% of the nation’s population. The states included are Arizona, California, Georgia, Illinois, Indiana, Minnesota, Missouri, Montana, Nebraska, Nevada, Oregon, Texas, and Washington.¹² Figure A.1 in the online appendix shows the selection method of election officials in all county-administered jurisdictions across the United States. Table A.1 shows that counties in the dataset are similar to those that are excluded. Figure 1 shows which counties enter into the dataset as well as whether they are always appointed, always elected, switch from elections to appointments, switch from appointments to elections, or have undergone multiple changes in selection method. The vast majority of counties that have switched since 1960 have moved from electing to appointing their clerks. In fact, 99.1% of counties switching their selection method have adopted appointments, and 93% of all singular switches have been in the direction of appointments. Four states in particular stand out for the number of switches: California, Georgia, Minnesota, and Texas. Figure 2 shows when each switch in selection method occurred. Counties have changed their clerk selection method in a staggered fashion over many decades, with switches accelerating since 2000. Table A.1.3 in the online appendix details the specific election authority used

¹²In states with multiple election authorities, I use the selection method for the authority with primary responsibility for administering elections on Election Day, as defined by Ferrer and Geyn (2023). I exclude five counties in Illinois and one in Missouri with nested municipal-level election administration.

Figure 1: **Local Election Administration Selection Methods, 1960-2022.** This graph displays over time change in the selection method of county election officials across all states with county-level administration where at least one change has occurred since 1960.

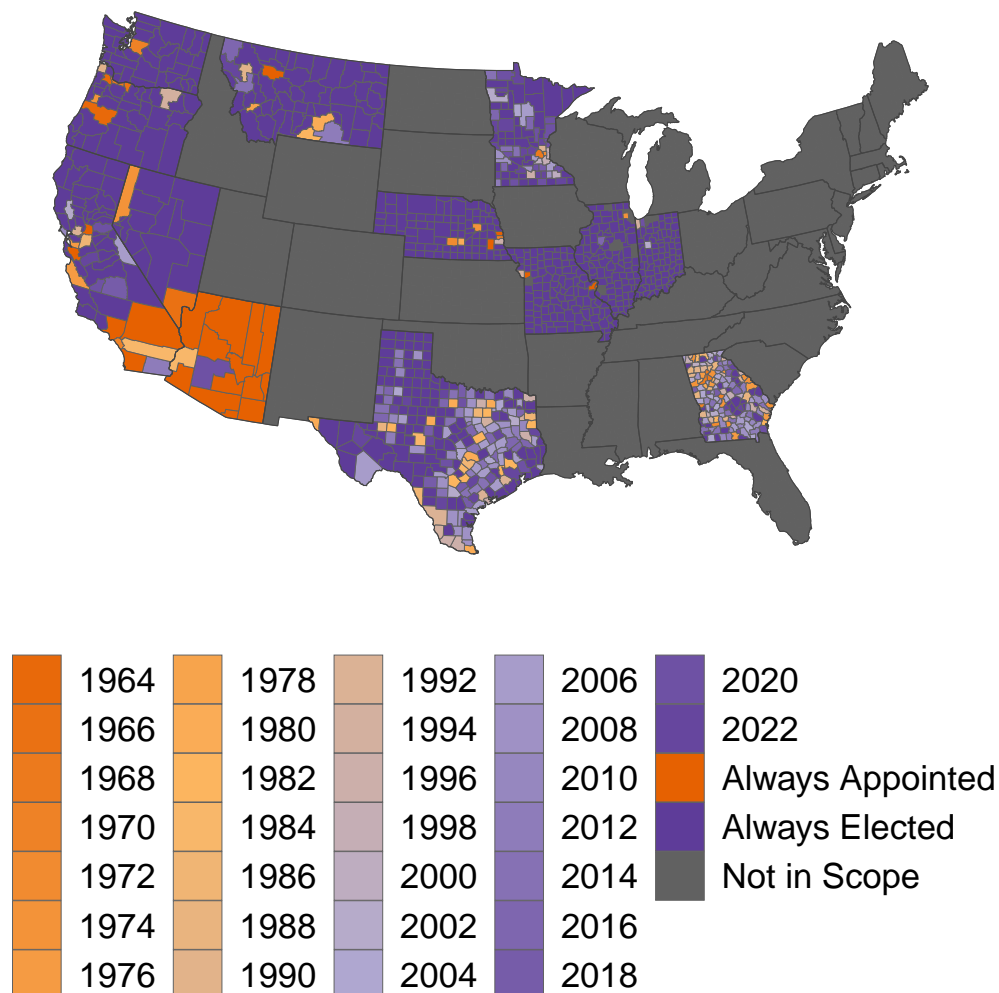


for each state, as was well as the number of counties falling into each clerk selection method category and the first and last year a change occurred.

Finally, Figure 3 graphs the extraordinary shift in selection method over time across these 13 states. The percentage of counties that appoint their election officials has grown from 2% in 1960 to 33% in 2022. The balance continues to shift towards appointments, with the trend accelerating over the past two decades. Election official selection method is a live, ongoing, and at times contentious policy debate.

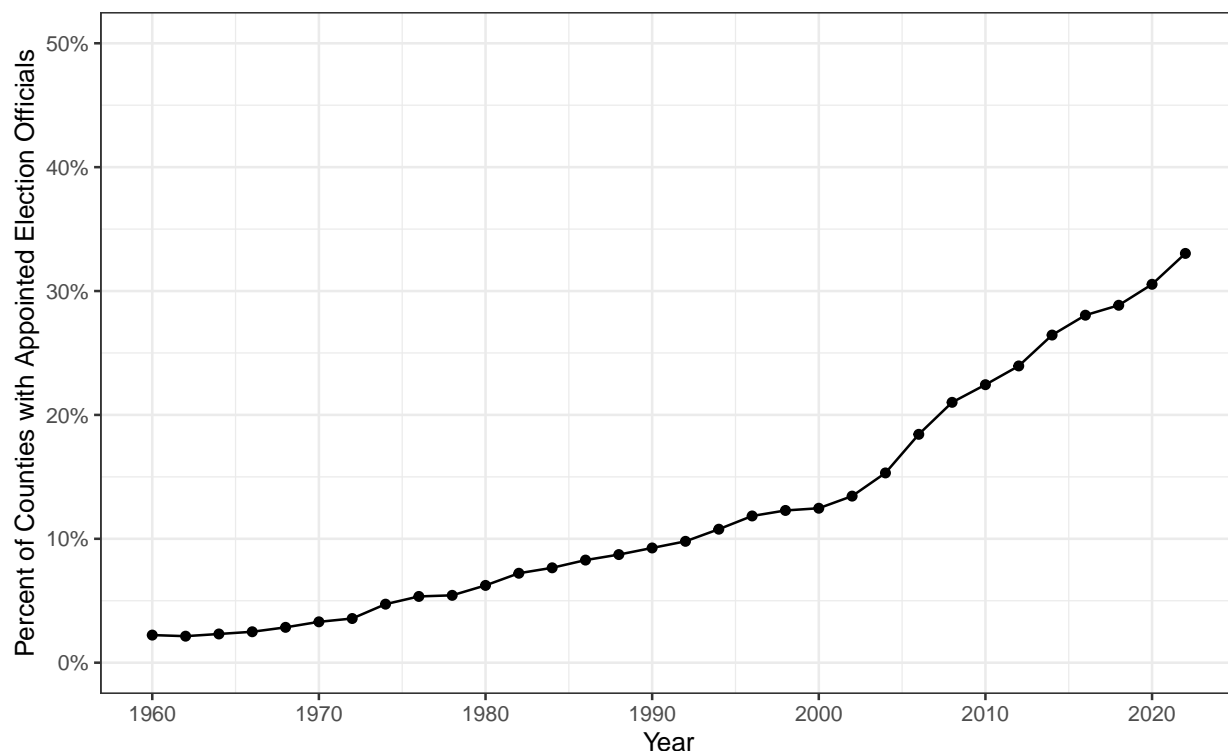
The mechanism and character of the changes vary widely across states. Most or all of the changes in California, Oregon, and Washington are due to the implementation of home rule charters that tended to make wholesale changes to local governance. Minnesota, Montana, and Texas devolve the power to switch selection methods to their counties, whereas California and Georgia typically require the passage of state legislation to enable a change.

Figure 2: **Local Election Administration Selection Cohorts, 1960-2022.** This graph displays the year county-level switches occurred between electing and appointing local election officials since 1960. In most cases, this switch is from electing to appointing the local election official. In counties where multiple switches occurred, the year of the first switch is reflected.



Some counties in California, Minnesota, Montana, and Washington hold binding referendums to initiate the reform, and several Midwestern states have population thresholds at which appointing their election official becomes possible or mandatory. In most cases, the switch

Figure 3: **Growth in Appointed Election Officials Across 13 States, 1960-2022.** This graph displays over time change in the selection method of county election officials across the 13 states with county-level administration where at least one change has occurred since 1960. In total, these states have 1,123 counties.



in selection method was not accompanied by any other substantive change to election policy or resource provision. For instance, in Georgia a state legislator that represents the affected county introduces a law to the legislature transferring election administration authority from the elected probate judge to an appointed board of elections that then selects an elections director. In Texas, the county commissioners enact the transfer of authority from an elected clerk or tax assessor to an appointed elections administrator. Minnesota presents a particularly minimal case of change, with county officials agreeing to a switch from election to appointment of the county auditor. In some cases, this does not even result in a change in leadership. I conduct a series of robustness tests isolating the effects of reform independent of other substantive policy changes.

Reasons counties state for making the switch include difficulty finding qualified candidates for office,¹³ a desire to professionalize the job,¹⁴ increasing efficiency and streamlining services,¹⁵ creating a dedicated position for election administration,¹⁶ or simply following in the footsteps of other counties in the state.¹⁷ Who receives appointing authority also varies, as well as whether they appoint an individual or a board (Ferrer and Geyn 2023).

Local election officials are entrusted with broad statutory authority to conduct elections (Ferrer, Geyn, and Thompson 2023). For instance, probate judges in Georgia determine precinct divisions, handle nomination petitions of candidates, publish notices and advertisements of elections, select and equip polling places, purchase and maintain election equipment, conduct early in-person voting, appoint and train poll officers, inspect the conduct of elections, receive and certify election results, prepare a budget estimate and appropriations request, conduct hearings to determine the eligibility of candidates, and administer photo ID provisions. Most clerks also handle registration administration and voter list maintenance duties, although these responsibilities are divided in Arizona, Georgia, and parts of Texas.

I use a combination of sources in order to identify the selection method of election officials across the dataset, including state legislative databases, home rule charters, newspaper archives, web scraped internet archives, Blue Book directories, public records requests, and correspondence with state and local election officials.

3.2 Data

I use presidential and midterm participation rates as my primary outcome measure. I focus on turnout and registration rates for four reasons: local election officials have the ability

¹³<https://www.fairmontsentinel.com/news/local-news/2023/07/19/faribault-county-looks-to-appoint-auditor-treasurer/>

¹⁴<https://www.houstonchronicle.com/opinion/editorials/article/harris-county-elections-legislation-hudspeth-18552129.php>

¹⁵<https://maplelakemessenger.com/2020/12/wright-county-considers-changing-auditor-treasurer-from-elected-to-appointed/>

¹⁶<https://www.timesrecordnews.com/story/news/local/2023/06/05/wichita-county-to-hire-election-administrator/70289429007/>

¹⁷https://www.unionrecorder.com/news/commissioners-discuss-possibly-creating-a-board-of-elections/article_43508cfc-6718-11ee-a035-13c8d8908b19.html

to influence participation levels, they view increasing participation as part of the job, voter participation is a key component of election quality metrics and the ultimate outcome of election quality, 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 (Kimball and Kropf 2006). 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 2023). Second, according to the 2020 EVIC 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. This is reflected in the National Association of Election Officials, which lists increasing participation as one of the main considerations for election officials.¹⁸ Third, participation rates are widely viewed as a key measure of election quality. MIT’s Election Performance Index uses both voter turnout and voter registration in comparing election administration performance across states,¹⁹ and Varieties of Democracy (V-Dem) uses electoral participation as one of its indicators of democratic health.²⁰ Voter turnout can be considered the ultimate effect of the quality of election administration. If voters have a poor voting experience, or are not readily or proactively provisioned with the information necessary to vote, then they are less likely to participate. Finally, high-quality data for turnout exists at the county level and is available going back many decades. This is not true of any other indicator of election quality, including voter confidence, voter wait times, number of polling places, and constituent communication.

Data on county-level vote totals is from Congressional Quarterly and David Leip’s U.S. Election Atlas and spans 1968 to 2022.²¹ 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 using estimates from the National Cancer Institute’s

¹⁸<https://www.electioncenter.org/about-us.php>

¹⁹<https://elections.mit.edu/#/data/map>

²⁰<https://www.v-dem.net/static/website/img/refs/codebookv12.pdf>

²¹I exclude Loving county, Texas from the analysis because its population is too small to reliably estimate participation rates.

Surveillance, Epidemiology, and End Results Program.²² 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 U.S. Election Assistance Commission’s Election Administration and Voting Surveys (EAVS) from 2004 to 2022, including the number of polling places per 1,000 people, provisional ballot rate, provisional ballot rejection rate, absentee ballot rejection rate, and the registration removal rate. Following Ferrer, Geyn, and Thompson (2023) and Pettigrew (2017), I use data from the Congressional Election Study to measure the share of voters who had to wait at the polls for certain lengths of time. This is available for general elections in 2006, 2008, and 2012–2018. I also use election official communication data provided by Thessalia Merivaki and Mara Suttman-Lea.

I probe mechanisms using data on election administration expenditures from Mohr et al. (2018), data on the prior experience of local election officials from the 2020 EVIC Survey of Local Election Officials,²³ data on local newspapers from Gentzkow, Shapiro, and Sinkinson (2014) and Sean Ewing, and data on voter knowledge of election officials from an original survey.

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. Table A.3 in the online appendix shows some of the differences between counties that appoint their clerk vs. those that elect their clerk. For instance, populous, dense, and racially diverse counties are all more likely to appoint their election officials than sparsely

²²This 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/>. It is available from 1970 to 2020. I extrapolate the estimates to 1968 and to 2022.

²³<https://evic.reed.edu/leo-survey-summary/>

populated, rural, and mostly white counties (Ferrer and Geyn 2023). They also tend to have lower participation rates (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 relationship between appointed officials and lower turnout—but this would not be evidence that appointing officials *causes* lower turnout. Even if all of these obvious differences are controlled for, there are likely unobservable factors that make counties different in ways that happen to correlate both with their participation rate and the selection method of their clerk.

I overcome this issue with a difference-in-differences research design. I leverage county-level changes in clerk method across 13 states to credibly measure the effects of a switch on participation. 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. So long as year-to-year differences in turnout are commonly experienced across a state and not indirectly related to switches in clerk selection method, I can be confident that an observed difference in turnout in the counties that switch to appointed clerks is due to the selection method itself.

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 reform. It is possible to imagine that counties that switch to appointed officials are growing at a more rapid rate than those that stay with elected officials, and that turnout is trending down as a result. In this case,

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 former compares within-county over time change to other counties with similar partisan makeup, whereas the latter compares within-county overtime change to other counties with similar populations. These account for the possibility that counties that switch their election administration may also happen to shift either population or partisan trends in ways that are systematically related to turnout. Democratic vote share and population are divided into quartiles for each state, allowing the grouping cut points to vary by state, and measured pretreatment.²⁴

Even with these interacted fixed effects, it is still possible there are unobserved confounders. I conduct a generalized synthetic control balancing exercise to ensure that counties that switch are only compared to those that do not with similar pretreatment turnout trajectories and randomization inference to investigate the likelihood of getting the observed results given the structure of the data.

4 Results

In this section, I present evidence that appointing clerks results in increased turnout and registration rates. I then validate this finding with a range of alternative estimators, conduct

²⁴I use the 1960 census for population and the 1968 presidential election for Democratic vote share. Democratic vote share is measured as votes for the top-ticket Democratic candidate divided by votes for the top-ticket Democratic and Republican candidates.

a placebo analysis using registration rates, distinguish between the effects of selection method and partisanship, show the effects are largest in less populous jurisdictions, and examine whether appointed officials benefit their principals' party or if the effect differs by jurisdiction 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 a local election official on citizen participation. Columns 1 through 3 estimate the effects on votes per voting-age resident and columns 4 through 6 estimate the effects on registrants per voting-age resident. Both are measured as proportions out of 1. The coefficients are the average percentage point difference in turnout and registration rates when counties switch from elected to appointed clerks. All six regressions include, at minimum, county and year by state fixed effects.

Table 1: **Appointing Local Election Officials Increases Citizen Participation (Even-Year General Elections, 1968-2022)**

	Voter Turnout			Registration Rate		
	(1)	(2)	(3)	(4)	(5)	(6)
Appointed	0.018 (0.003)	0.016 (0.003)	0.014 (0.003)	0.009 (0.004)	0.009 (0.004)	0.008 (0.004)
Counties	1116	1116	1116	942	942	942
Elections	28	28	28	13	13	13
Observations	31146	31146	31146	12216	12216	12216
Outcome Mean	0.50	0.50	0.50	0.84	0.84	0.84
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. Voter turnout and registration rate are measured as proportions out of 1. The number of observations is smaller in columns 4-6 because Arizona and Georgia are excluded and because turnout data is available from 1968 but registration data is only available from 1996.

Column 1 shows that counties switching from directly elected to appointed election officials see an average increase in voter turnout of 1.8 percentage points in even-year general elections, compared with counties that do not switch. The point estimate is precisely estimated, allowing us to confidently rule out effects smaller than 1.2 percentage points. It is also substantively meaningful. The effect size for turnout in even-year general elections is on par or larger than those generated by the most significant modern policy interventions designed to boost voter participation. It is equivalent to implementing universal vote-by-mail (Thompson et al. 2020) or adding 10 days of early voting (Kaplan and Yuan 2020). It is also double the turnout boost caused by implementing automatic voter registration (McGhee, Hill, and Romero 2021). This effect size is also significant compared to get-out-the-vote interventions. It is twice the average turnout effect of door-to-door canvassing, three times that of a direct mailing, and five times that of a phone call campaign (Green, McGrath, and Aronow 2013).

I introduce year by state by Democratic vote share fixed effects to alleviate this concern in column 2 to alleviate the concern that counties with similar partisan compositions were on the same participation trajectory prior to their shift in selection method. The result is similar under this estimation strategy. The inclusion of year by state by population fixed effects in column 3 makes comparisons between counties of comparable sizes within the same state and yields analogous results.

Appointed election administrators also appear to oversee elections with higher registration rates. Arizona and Georgia are excluded from these specifications because registration duties are always undertaken by appointed registration boards. The coefficients range from 0.8 to 1 percentage point in magnitude, and a null of no difference can be confidently ruled out in all three estimators. An event study plot of the effect of appointment on registration, shown in Section A.3.3, reveals some evidence of pre-trending, so this effect should be viewed with caution.

These estimates provide strong evidence that appointed clerks increase voter participation, relative to their directly elected counterparts. Regressions excluding midterm contests are found in Section A.2.1 and yield substantively similar findings. Table A.5 in the online appendix 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, for multiple reform mechanisms, and over multiple years and date ranges. In Table A.6 in the online appendix, I show that switching to an appointed election administrator increases voter turnout in three of the four states with at least 10 counties experiencing switches (Georgia, Minnesota, and Texas), and is imprecisely estimated in the fourth case (California). Table A.7 in the online appendix shows that switching from elected probate judges, auditors, and clerks to appointments increases voter turnout. In Table A.8 in the online appendix, I show that both county- and state-initiated reform mechanisms lead to a boost in turnout and that the findings are robust to excluding the few cases where the change was packaged with unrelated reforms. This alleviates concerns that the boost to turnout is an artifact of the way the reform in selection method was initiated. I also run a series of Callaway and Sant’Anna (2021) regressions in Section A.2.5 to estimate the dynamic effects of switching from an elected to an appointed election official. The positive effects of appointments on voter turnout appear over time and across multiple county cohorts and time periods.²⁵ Finally, it is possible that low rates of turnout among African-Americans in the South due to the lingering effects of repressive Jim Crow restrictions confound the results. I show in Table A.10 in the online appendix that the results hold using only more recent elections, with some attenuation in effect magnitude.

²⁵This provides evidence that the positive effects of appointment are not simply due to a novelty or Hawthorne-style effect in the immediate aftermath of a change. I am not able to reliably estimate the effects of switching from appointed to elected clerks due to the small number of counties that have switched in this direction.

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 produce higher voter turnout than directly elected officials. I utilize alternative difference-in-difference estimators and employ a generalized synthetic control balancing 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 two-way fixed effects estimator when used in staggered adoption designs (Baker, Larcker, and Wang 2022; Borusyak, Jaravel, and Spiess 2021; de Chaisemartin and D’Haultfoeuille 2020; Callaway and Sant’Anna 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.

To validate my main findings, I test a range of alternative specifications in Table A.11 in the online appendix, including removing counties that switch from appointments to elections, removing counties that use appointments throughout the dataset, and using stacked difference-in-difference estimators. All specifications result in precisely estimated effects on turnout between 2.1 and 3.5 percentage points. In Section A.3.2, I employ the de Chaisemartin and D’Haultfoeuille (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 introduce bias. The results are consistent with those shown in Section A.2.3.

4.2.2 Generalized Synthetic Control

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 appoint-

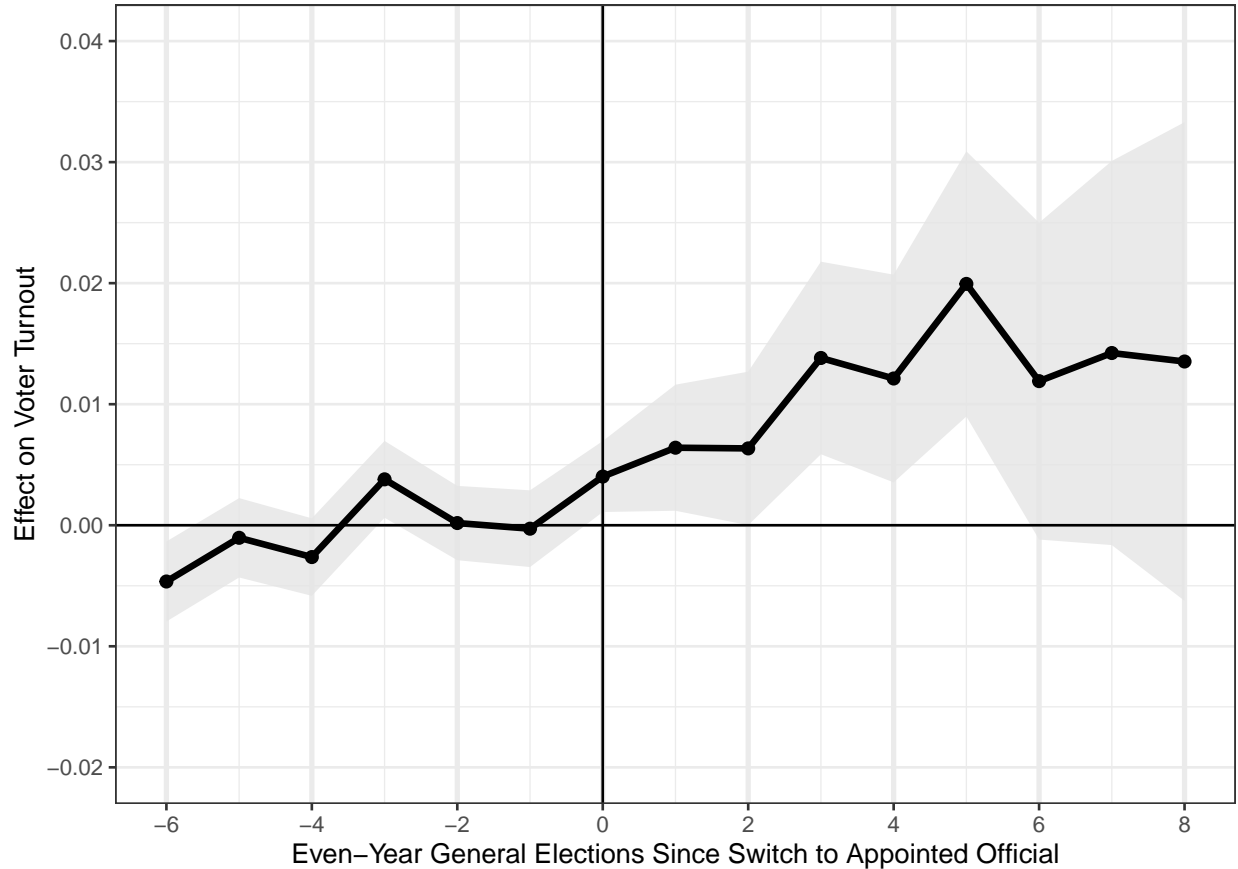
ing election officials are fundamentally different from those that remain elected on some unobserved characteristics, then this will undermine the causal validity of the regression specification. Figure A.14 in the online appendix investigates the validity of the parallel trends assumption using the Dube et al. (2022) local projections event studies estimator. It shows evidence that places that adopt appointments may be on different trajectories prior to reform.

I overcome this concern through the generalized synthetic control method. This estimation strategy rebalances the data sample by comparing treated and untreated units with similar pre-treatment voter turnout history. Figure 4 displays output from a Xu (2017) generalized synthetic control estimation. The line in the left-hand side of the figure is close to 0, showing that the strategy successfully compares treated and control counties with similar pre-treatment turnout trajectories. It becomes positive in the right-hand side of the figure and is statistically distinguishable from 0. This provides additional evidence that appointed election officials administer elections with higher turnout than their elected counterparts. As shown in Table A.17 in the online appendix, it produces a precisely estimated effect size of 0.8% on voter turnout, lower than the estimates shown in Table A.4 but still substantial for participation in federal general elections. In Section A.2.5 in the online appendix, I explore two possibilities for an increasing effect magnitude over time: delayed effects due to selection method reform triggering turnover, and a secular trend of declining availability in local news.

4.2.3 Randomization Inference

Randomization inference can be used to derive an alternative estimate of the likelihood of finding an effect as large or larger than the one observed by chance. I employ two different randomization permutations: in Figure A.18 in the online appendix, I randomly permute both which counties are treated and when they are treated, and in Figure 5, I randomly permute when treated counties receive treatment. Counties that switch from appointed to elected and counties that switch selection method multiple times are excluded. 1,000 per-

Figure 4: **Estimated ATT of Generalized Synthetic Control.** This graph displays a generalized synthetic control method of the two-way fixed effects regression estimating the effect of appointing local election officials on even-year general election voter turnout. The specification includes two-way additive county and year fixed effects, automated cross-validation to identify the optimal number of factors, and a parametric bootstrap 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.

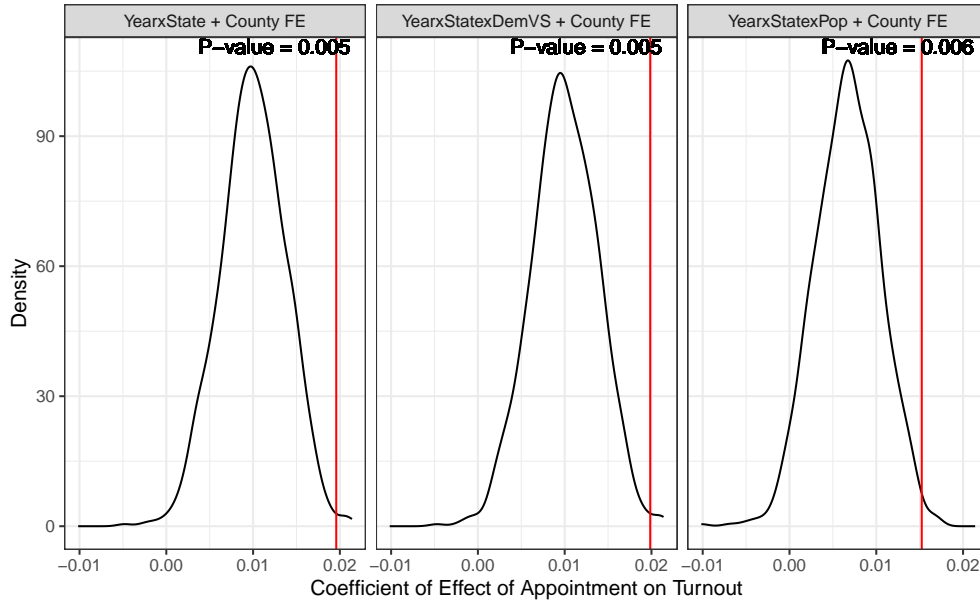


mutations are computed for each exercise. The three regressions shown in Table 1, columns 1-3 are replicated with the permuted data and the coefficient stored for each permutation. Finally, the actual coefficient derived is compared with the distribution of permuted coefficients. The p-value is the number of randomized coefficients that are greater than or equal to the actual effect size divided by the total number of iterations.

Figure A.18 shows that the likelihood of observing the actual result or a more extreme effect is close to 0, given randomized treatment and treatment timing and assuming the true effect is null. The more demanding inferential test is when the counties that switch

to appointments are preserved, but when they switch is scrambled. Figure 5 shows that random treatment timing of the treated units typically results in a positive relationship between appointments and voter turnout. This aligns with the evidence of pre-trending shown in Figure A.14 and corrected for by the generalized synthetic control method in Figure 4. However, it is still extremely unlikely to get an observed effect as large as was actually observed—only in about 1 out of 500 simulations does the effect size reach 2%. This provides additional validation that appointing election officials increases voter turnout.

Figure 5: Randomization Inference for Table 1, Columns 1-3 - Timing of Treated Counties. This graph displays the output of randomization inference for the main effects of appointed local election officials on voter turnout. Which counties receive treatment is preserved, but when they first switch to appointments is randomly permuted. The black distribution shows the resulting coefficients of 1,000 iterations. The red solid vertical line is the actual coefficient observed, and the p-value is the share of coefficients that are equal to or larger than the one estimated in the respective specification in Table 1.



4.3 Appointing Election Officials Boosts Registration Rates More when Their Duties Specifically Include Registration

I run a placebo test examining whether switching to appointed officials increases registration rates more in states where their duties specifically include registration. The results, found in Appendix A.3.6, show suggestive evidence that counties experience a larger boost in registration rates when the official directly in charge of registration duties switches from an elected to an appointed position.

4.4 Selection Method, Not Partisanship, Explains 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 in Table A.4 present a bundled treatment of both selection method and partisanship. The partisan nature of elected office could lead clerks to act in ways that differ from their nonpartisan appointed counterparts—for instance, by attempting to alter turnout to advantage co-partisans. Georgia, Montana, and Washington’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 A.19 provides strong evidence that selection method, and not the partisan nature of the office, drive the main results on voter turnout.

4.5 Selection Method Effects Are Largest in Small Jurisdictions

Previous research suggests that the population of a jurisdiction is a defining feature in how its elections are run (Burden et al. 2012; Kimball and Baybeck 2013). The vast majority of election jurisdictions serve a small number of people, with 94% of jurisdictions serving less than one-third of the population and the median jurisdiction serving only 2,000 individuals (Kimball and Baybeck 2013). In counties where local election officials have fewer deputies,

the actions of the chief official could have a greater impact on participation rates. Table 2 displays the results of difference-in-difference regressions testing the magnitude of the difference in effect between less and more populous counties. A “small county” is defined as ranking in the bottom half in population compared to other counties within the same state. The top row is the effect of switching to appointed election officials for populous counties, and the bottom row is the additional effect of switching to appointments for relatively less populous counties. It is apparent that the effects are largest in small counties. Appointed election officials in less populous jurisdictions produce turnout rates that are between 2.0 and 2.2 percentage points higher than their elected counterparts, compared with 0.7 to 0.9 percentage points higher in more populous jurisdictions. A similar pattern is found with registration rates, with effect sizes in smaller counties double those found in large counties.

Table 2: Appointing Local Election Officials Increases Citizen Participation Especially in Small Counties (Even-Year General Elections, 1968-2020)

	Voter Turnout			Registration Rate		
	(1)	(2)	(3)	(4)	(5)	(6)
Appointed	0.009 (0.003)	0.007 (0.003)	0.009 (0.003)	0.005 (0.004)	0.005 (0.004)	0.006 (0.005)
Appointed X Small County	0.013 (0.007)	0.013 (0.007)	0.013 (0.007)	0.006 (0.008)	0.005 (0.008)	0.003 (0.008)
Counties	1114	1114	1114	941	941	941
Elections	28	28	28	13	13	13
Observations	31104	31104	31104	12203	12203	12203
Outcome Mean	0.50	0.50	0.50	0.84	0.84	0.84
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year x State x Small FEs	Yes	No	No	Yes	No	No
Year x State x Dem vs x Small FEs	No	Yes	No	No	Yes	No
Year x State x Pop FEs	No	No	Yes	No	No	Yes

Small counties rank in the bottom half in population compared to other counties within the same state. Robust standard errors clustered by county in parentheses. The number of observations is smaller in columns 3-4 because Arizona and Georgia are excluded and because turnout data is available from 1968 but registration data is only available from 1996.

4.6 No Evidence That Appointed Local Election Officials Benefit Their Principals' Party Or That The Effect Is Different in Democratic Vs. Republican-Leaning Jurisdictions

I test whether appointed officials benefit the party of their supporters by compiling original data on counties in two states where partisan county commissioners appoint the local election official: Arizona and Pennsylvania. I use a difference-in-differences specification, testing what happens when the majority party of the election official's appointing body changes. The results, shown in Table A.20, reveal little evidence that election officials appointed by a newly Democratic body shift vote share or registrants in a Democratic direction. The regressions are precisely estimated, and no estimated effect is larger than three-tenths of a percentage point shift in a direction benefiting Democrats.

I also test whether the effect of appointments on participation is shared equally across jurisdictions, regardless of partisan lean, or concentrates in jurisdictions of a certain partisan balance. I show in Table A.21 that the benefits of switching to appointed election officials are similar across Democratic- and Republican-leaning jurisdictions.

5 Why Does Appointing Election Officials Increase Voter Participation?

What do appointed election officials do differently from elected officials that increases voter turnout and registration rates for their constituents? I show that appointed officials obtain additional election administration resources. Beyond this, I cannot definitively tell how appointed officials increase participation. However, I provide suggestive evidence that appointing election officials leads to higher election official salaries, a larger workforce, more robust communication with voters, and lower wait times, all consistent with activities that could increase participation. However, some findings are inconsistent with expectations. I

fail to find any differences between appointed and elected officials in polling places per 1,000 residents, share of provisional ballots cast, share of provisional or absentee ballots rejected, and share of registrants removed from the list.

5.1 Appointed Election Officials Obtain More Resources

Sufficiently funding elections is essential to ensuring high quality administration (Mohr et al. 2019, 2020; Kropf et al. 2020; McGowan et al. 2021). Previous scholarship has shown that increasing election administration resources can boost voter turnout (Grose 2022; but see Lal and Thompson 2024). Burden et al. (2013) argue that appointed officials are *less* able to advocate for more resources than their elected counterparts and therefore administer elections with fewer resources. However, Taylor, Swint, and Reilly (2024) find that appointed boards of election in Georgia spend 45% more on election administration than elected probate judges. Appointed officials might have better relationships with their principals and thus more sway over election funding. If the quality of selection is higher for appointed officials, they might be more proactive in securing additional resources. Alternatively, they might be more responsive to the interests of cost-conscious voters, as elected judges are more attentive voter’s desires to be efficient (Choi, Gulati, and Posner 2010). It is also 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.²⁶

I use jurisdiction election administration expenditure data from Mohr et al. (2018). This dataset includes estimated yearly expenditures for each county in Arizona, California, Georgia, Minnesota, Missouri, Nebraska, and Nevada starting from as early as 2002. This en-

²⁶Appointed officials’ sole job is to effectively administer elections. In comparison, most directly elected local election officials in the U.S. undertake additional responsibilities beyond election administration. County clerks have a variety of non-election duties such as maintaining legislative/judicial records and recording vital documents. Other 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 should only exist in the least populous counties, where sometimes only a single official administers elections. According to the 2020 EVIC Survey of Local Election Officials, 34 percent of jurisdictions have no full-time election administrators and 17 percent have exactly one FTE (<https://evic.reed.edu/leo-survey-summary/>). In all other jurisdictions, switching to an appointed official should not directly increase the amount of FTEs.

ables the use of a difference-in-differences regression design to test the effect of switching to appointed election officials on election expenditures. Following Taylor, Swint, and Reilly (2024), I use the natural log of total election expenditures per registered voter as my dependent variable. Table 3 displays the results (an event study plot, found in Section A.3.3, shows no evidence of pre-trending). The first three specifications test the overall effect of appointments on election expenditures and the latter three test whether less populous counties enjoy a larger boost in resources than more populous counties. All point estimates are large and statistically distinguishable from zero. The coefficient in column 1 means that when counties switch to an appointed election official, their election expenditures per registered voter increases by 28 percentage points on average. We can confidently rule out effects of less than 8 percentage points. The average county spends \$9.50 per registered voter on administering elections in even years, and appointed officials secure an additional \$2.66 per registrant.

The effects are, if anything, smaller in less populous jurisdictions. This means they are likely driven by the actions of the local election official rather than a result of creating an additional FTE election administration position. I further investigate the reason for this effect by examining whether appointed officials were more likely to apply for the Center for Tech and Civic Life’s 2020 COVID grants to election administrators. Using data on grant applications from Lal and Thompson (2024) and nationwide cross-sectional selection method data from Ferrer and Geyn (2023), I find in Table A.22 that appointed officials were 7 percentage points more likely to apply for the grants compared to elected officials in counties within the same state, even after controlling for a range of factors including population, partisanship, median income, urbanicity, non-Hispanic white share, and COVID severity. This provides suggestive evidence that the effect is due to a quality difference between elected and appointed officials rather than elected officials simply being more attentive to the desire of voters to minimize costs. Finally, I show in Table A.23 that increased election expenditures may lead to additional voter turnout, especially in smaller jurisdictions. A

Table 3: **Appointing Local Election Officials Increases Election Expenditures (Even-Year General Elections, 2004-2016)**

	Ln(Total Election Expenditures Per Registered Voter)					
	(1)	(2)	(3)	(4)	(5)	(6)
Appointed	0.280 (0.100)	0.270 (0.091)	0.310 (0.098)	0.323 (0.110)	0.367 (0.110)	0.357 (0.109)
Appointed X Small County				-0.100 (0.224)	-0.204 (0.209)	-0.122 (0.215)
Counties	434	434	434	432	432	432
Elections	6	6	6	6	6	6
Observations	1929	1929	1929	1920	1920	1920
Outcome Mean	2.25	2.25	2.25	2.25	2.25	2.25
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year x State FEs	Yes	No	No	No	No	No
Year x State x Dem vs FEs	No	Yes	No	No	No	No
Year x State x Pop FEs	No	No	Yes	No	No	Yes
Year x State x Small FEs	No	No	No	Yes	No	No
Year x State x Dem vs x Small FEs	No	No	No	No	Yes	No

Small counties rank in the bottom half in population compared to other counties within the same state. Robust standard errors clustered by county in parentheses. Data is from Mohr et al. (2018) and is available for Arizona, California, Georgia, Minnesota, Missouri, Nebraska, and Nevada. Elections are the average number of elections included for each state, rounded down to the nearest interger. Expenditure data is normalized to 2020 dollars.

doubling of expenditures per registrant increases voter turnout by 0.4 percentage points on average and by 0.7 percentage points in less populous counties. This is in line with previous findings linking election expenditures with higher turnout (Kropf and Pope 2020).

5.2 What Administrative Policies Do Appointed Election Officials Pursue Differently?

Given that appointed election officials obtain more resources, what might they do with these resources that could lead to higher participation? Using data from the 2020 EVIC Survey of Local Election Officials, I find suggestive evidence in Section A.5.1 that appointed officials serving in similarly populous jurisdictions within the same state make \$5,000 more

than elected officials and hire an additional 0.6 FTEs on average, although both results are imprecise.

Election officials could use additional funding to improve voter outreach. Clerks have significant discretion in their communication with voters. They can pursue a proactive strategy of providing additional information to the public and accurately responding to constituent questions. Or, they can provide the legally required minimum amount of information. More active election official communication strategies has been shown to increase the share of registered voters (Merivaki and Suttman-Lea 2023), improve voter confidence (Suttman-Lea and Merivaki 2023), and reduce the number of mail ballots that are rejected (Suttman-Lea and Merivaki 2022). In an audit study of election officials, White, Nathan, and Faller (2015) found that elected officials were 16% less responsive and 12% less accurate in their responses than appointed officials. Figure A.25 in the online appendix uses data from Thesalia Merivaki and Mara Suttman-Lea to test whether appointed officials are more likely to maintain official social media accounts than elected officials serving in similar jurisdictions. I find that appointed officials are twice as likely to have a Twitter account as elected officials, although I do not find differences in the usage of other platforms.

I use EAVS data to explore the possibility that more voter outreach reduces the usage of provisional ballots and the rejection of provisional and absentee ballots or additional resources leads appointed officials to open more polling places. The results, found in Section A.4.3, do not allow me to rule out that appointed and elected administrators run elections with similar provisional ballot usage, provisional rejection rates, and absentee ballot rejection rates, as well as numbers of polling places per 1,000 residents and registration removal rates.

Additional resources could be employed to improve the Election Day experience for voters in a number of additional ways, including hiring more poll workers, providing them with better pay and more rigorous trainings, and better provisioning polling places with poll booths and voting machines. While I cannot directly test these mechanisms, I use the 2020 CES to examine whether voter wait times decrease when counties switch to appointed adminis-

tration. I employ difference-in-difference regressions with county and year fixed effects and individual controls for gender, race, age, education, and party identification. The results are shown in Figure 4. While the regressions are relatively imprecise, the coefficients are all negative and the effect sizes are large in relation to the outcome means. Switching to appointed election officials reduces the percentage of voters that wait at least 10 minutes in line by 8 percentage points, an effect size of 0.3, and reduces the percentage of voters waiting in line for 30 minutes or more by 2 percentage points. Longer wait times have been found to depress future voter turnout, making this one plausible factor explaining why appointed officials boost participation (Pettigrew 2021).

Table 4: **Appointed Local Election Officials May Decrease Voter Wait Times (Even-Year General Elections, 2008-2022)**

	Any Wait (1)	> 10 min (2)	> 30 min (3)	> 1 hr (4)
Appointed	-0.006 (0.074)	-0.078 (0.049)	-0.020 (0.028)	-0.000 (0.013)
Counties	745	745	745	745
Elections	6	6	6	6
Observations	15902	15902	15902	15902
Outcome Mean	0.50	0.23	0.09	0.03
County FEs	Yes	Yes	Yes	Yes
Year x State FEs	Yes	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes	Yes

Robust standard errors clustered by county in parentheses. Individual controls are gender, race, age, education, and party identification.

6 Why Do Appointed Election Officials Outperform Elected Officials?

I explore two sets of mechanisms that could lead appointed election officials to produce better outcomes for constituents than elected officials: that the quality of selection is higher for appointed officials, and that the quality of sanctioning is higher for appointed officials.

For the former, I examine differences in education between elected and appointed clerks and the low contestation rates of clerk elections. For the latter, I investigate the information voters know about their election official, differences in turnover rates, and triple difference-in-difference estimates comparing the effect of selection method based on the presence of a local newspaper.

6.1 The Quality of Selection Is Higher For Appointed Election Officials

Are appointed local election officials more equipped for their job than elected administrators? This could be due to some failure in elections that prevent voters from selecting the most qualified individuals—because of a limited pool of viable candidates, lack of contested elections, aversion of experienced or well-educated administrators to elections, or the absence of high-quality information. It could also be due to geographic restrictions imposed by elections. In order for voters to choose quality candidates, they need to run in the first place. But voters rarely have a choice in election administrator at the ballot box. Ferrer, Geyn, and Thompson (2023) find that only 23% of general election races for local election official 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. Previous research shows that low contestation rates is a problem across local offices (Burden and Snyder 2021; Lappie and Marschall 2018; Marschall and Lappie 2018; Thompson 2020; Yntiso 2022).

I use the 2020 EVIC Survey of Local Election Officials to examine whether elected and appointed officials possess different levels of education, a common indicator of the quality of public officials (Dal Bó et al. 2017). Table 5 tests differences in education between elected and appointed officials. All specifications include state fixed effects and both log population and log population squared controls to ensure that comparisons 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.

Table 5: Appointed Local Election Officials Possess More Education Than Elected Officials

	Edu (1)	Degree (2)	Any College (3)	Edu (4)	Degree (5)	Any College (6)
Appointed	0.336 (0.209)	0.160 (0.115)	0.113 (0.044)	0.578 (0.232)	0.148 (0.089)	0.116 (0.056)
Appointed X Small County				-0.385 (0.316)	0.087 (0.168)	0.009 (0.100)
States	44	44	44	38	38	38
Observations	581	581	581	581	581	581
Outcome Mean	2.86	0.58	0.88	2.86	0.58	0.88
State FEs	Yes	Yes	Yes	Yes	Yes	Yes
Log Pop	Yes	Yes	Yes	Yes	Yes	Yes
Log Pop squared	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors clustered by state in parentheses. Data is from the 2020 EVIC 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. Columns 1 and 4 measure educational attainment on a 5-point scale: high school, some college, college, some graduate school, and graduate school. Columns 2 and 5 measure whether the official possess a college degree, and columns 3 and 6 measure any college education.

Appointed officials appear to possess more formal education than elected officials. Appointed officials are 16 percentage points more likely to hold a college degree than elected officials (column 2) and are 11 percentage points more likely to receive any college education (column 3), an effect statistically distinguishable from 0. Columns 4–6 test whether the difference in education between elected and appointed officials is larger in less populous jurisdictions. This should be the case if the quality difference is due to a limited pool of candidates or geographic restrictions, rather than the absence of adequate voter information or inherent aspects of elections that turn away more educated professionals. There is little evidence that the effect varies across less and more populous jurisdictions.

Table A.26 in the online appendix tests a number of additional indicators of quality between elected and appointed officials using the 2020 EVIC survey. I find that appointed

officials tend to possess less previous experience in election administration, may hold more professional memberships, are likelier to have served elsewhere and in a greater number of previous jurisdictions, are less likely to be over the age of 65 years, make approximately 10% more in salary than elected officials, and recruit an additional 0.5 FTE. These findings are in line with a recent survey of municipal clerks in New England which found that elected clerks are older, less educated, longer-tenured, and have less institutional capacity than appointed clerks (Marsh et al. 2024). I take this as evidence that appointed officials possess less election administration experience but are more professionalized than their elected counterparts.

6.2 The Quality of Sanctioning Is Higher For Appointed Election Officials

In this section, I present evidence that voters do not know much about their local election officials, that the effects of switching to appointments on voter turnout are largest in jurisdictions without the continuous presence of a local newspaper, and that appointed officials have higher turnover rates.

6.2.1 Voters Know Little About Their Local Election Official

I fielded a survey of 3,200 U.S. adults to test respondent knowledge of their local election official. The survey hypotheses and analysis was preregistered on OSF,²⁷ and technical details are provided in Section A.5.2 in the online appendix. I compiled a complete list of currently serving chief local election officials from government websites and linked respondents to their current election official using zip code. Correcting for the frequency of guesses, only 17.2% of respondents were able to correctly identify the title of their chief election official. Fewer than 10% of respondents knew whether their election official was elected or appointed. And only 7.9% of respondents correctly identified their election official out of a list of five names.

²⁷osf.io/k7hq2

If the vast majority of voters do not know the position responsible for administering their elections, whether it appears on their ballot, or the person in charge of running elections in their community, it is unlikely that voters are able to adequately monitor the performance of this official and sanction them for mediocre performance. This is in contrast to the local elites in charge of appointing election officials. By their very nature, all principals know who the election official is and are likely to have a better idea of the quality of their work.

6.2.2 The Performance Gap Between Elected And Appointed Officials Is Largest In Jurisdictions That Lack a Local Newspaper

If appointed officials perform better than elected officials because they are better monitored, then the difference in performance should be smaller in jurisdictions where voters have greater access to information about local politics. Previous scholarship has established a causal effect between the presence of a local newspaper and increased turnout in federal elections (Gentzkow, Shapiro, and Sinkinson 2014), increased electoral competition in local races (Rubado and Jennings 2020), and a stronger incumbency advantage (Lockhart 2021). Is the performance gap between appointed and elected clerks larger when the county lacks a local newspaper, thus depriving voters of the information necessary to hold the public official accountable?

I test the effects of the presence or lack of a local daily newspaper on the relationship between selection method and voter turnout using a triple difference-in-differences design and a combination of two datasets: (Gentzkow, Shapiro, and Sinkinson 2014), which contains newspaper data from 1960 to 2004, and data from Sean Ewing that updates this data through 2020. I sort counties into two categories: those that have continuously had at least one newspaper headquartered in its boundaries within the study period, and those that have not. Table 6 displays the results of this analysis. The first row shows the effect of counties switching from elected to appointed election officials when they lack the continuous presence of at least one local newspaper. Column 2 is the additive effect on switching for counties that

have a local newspaper presence. Nearly the entirety of the positive benefits to appointing election officials lie in counties that lack local news coverage. An alternative specification, introducing over-time variability in the presence of a local daily newspaper, shows results consistent with Table 6 and is found in Section A.5.3.

Table 6: Consistent Presence of a Daily Local Newspaper Attenuates the Effect of Appointing Local Election Officials on Citizen Participation (Even-Year General Elections, 1968-2020)

	Voter Turnout			Registration Rate		
	(1)	(2)	(3)	(4)	(5)	(6)
Appointed	0.025 (0.005)	0.024 (0.005)	0.021 (0.005)	0.011 (0.006)	0.011 (0.007)	0.007 (0.007)
Appointed X Newspaper	-0.021 (0.007)	-0.020 (0.007)	-0.013 (0.007)	0.001 (0.009)	0.002 (0.009)	0.007 (0.010)
Counties	979	979	979	824	824	824
Elections	14	14	14	6	6	6
Observations	13661	13661	13661	5751	5751	5751
Outcome Mean	0.58	0.58	0.58	0.85	0.85	0.85
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year x State x Newspaper FEs	Yes	No	No	Yes	No	No
Year x State x Dem vs x Newspaper FEs	No	Yes	No	No	Yes	No
Year x State x Pop x Newspaper FEs	No	No	Yes	No	No	Yes

Robust standard errors clustered by county in parentheses. All counties that switch between having and not having a daily newspaper over the period of analysis are dropped. The number of observations is smaller in columns 4-6 because Arizona and Georgia are excluded and because turnout data is available from 1968 but registration data is only available from 1996.

6.2.3 Appointed Election Officials Have Higher Turnover Rates Than Elected Officials

If appointed local election officials are monitored and sanctioned more than elected officials, then they should have shorter tenures in general. I test this using an original panel of the names and service tenures of chief local election officials across jurisdictions spanning 2000 to 2022, collected mainly from state and local administrative archives (Ferrer and Thompson 2024). Table 7 shows the results and Table A.16 in the appendix tests for pre-trending.

Table 7: **Appointed Local Elections Officials Turnover At Higher Rates Than Elected Officials (2004-2022)**

	Election Official Turnover (1)
Appointed	0.049 (0.025)
Counties	1113
Elections	3
Observations	10881
Outcome Mean	0.18
County FEs	Yes
Year x State FEs	Yes
Robust standard errors clustered by county in parentheses.	

Column 1 shows the effect of a switch in a county from elections to appointments on turnover of the election official. Because this switch causes turnover in most cases, I impute missing dependent variable values for the year each jurisdiction moves into treatment. It appears that appointed officials leave the position at higher rates than elected officials. Switching to an appointed official increases the probability of turnover over a 2-year period by 4.9 percentage points. Considering the average 2-year turnover rate of election officials in the dataset is 17%, this is a fairly substantial effect.

7 Conclusion

Across America’s history, democracy-minded reformers have tinkered with the selection method of government offices in an attempt to improve the accountability and performance of its public servants. In recent years, this practice has spread to local clerks, who are facing unprecedented attacks from former President Trump and his supporters and immense pressure to deliver free and fair elections. States are increasingly shaping the administrative structures of local jurisdictions for seemingly partisan ends, affecting who controls elections

for millions of Americans. These decisions could have significant consequences for the quality of elections and the timely and accurate certification of election results—something that came close to not happening in the 2020 presidential election.²⁸ Yet we have lacked the ability to effectively adjudicate between selection methods.

Using original data from 13 states, spanning 1,116 counties across 62 years, I show that when counties switch from electing to appointing their clerks voter participation rates increase substantially. The boost to voter turnout is on par with the most effective convenience reforms designed to raise participation such as implementing universal vote-by-mail (Thompson et al. 2020) and automatic voter registration (McGhee, Hill, and Romero 2021). It is several times the effect of get-out-the-vote interventions such as door-to-door knocking, mailings, and phone calls (Green, McGrath, and Aronow 2013). The findings are robust to alternate specifications including general synthetic control and randomization inference; hold across multiple time periods, states, offices, and reform mechanisms; and do not come at the expense of increased partisan manipulation of election results. Appointed officials appear to boost local expenditures on election administration, hire additional staff, increase communication with voters, and reduce voter wait times. I show evidence for stronger selection and sanctioning mechanisms to explain these effects. Appointed officials are more educated and more professionalized than elected officials. Most voters cannot identify their local election official from a list of names, appointed officials outperform elected officials most in jurisdictions with the least availability of local news, and appointed clerks have higher turnover rates.

These findings add to a growing literature on the limits of elections in ensuring accountable officeholders (Ashworth 2012; Rogers 2023). Elections are designed to achieve accountability between officeholders and the public. When voters 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

²⁸<https://www.politico.com/news/2020/11/17/wayne-county-michigan-election-certification-437181>

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. The findings are in line with studies that have found that appointing other local offices, such as municipal assessors, treasurers, and managers, leads to preferable policy outcomes (Hajnal and Trounstein 2014; Sances 2016; Whalley 2013). Elections for local offices can counter-intuitively 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 local officials grow more complex (Hale, Montjoy, and Brown 2015), and contestation rates remain low. In short, knowledge, information, and expertise matter—and sometimes democracy works best when it does not let voters make all the decisions.

It is worth noting that appointing public officials does not guarantee desirable outcomes and that elections play an important role in the democratic process. 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, including some filled with election deniers.²⁹ However, my results suggest that over a long period of time and across several states, appointed election officials have produced better outcomes for their constituents than elected officials.

Future work should consider other instances where elections fail to achieve their intended effects, with a goal of uncovering under what broader conditions appointed public produce better outcomes for their constituents. This analysis suggests that the information environment, competitiveness conditions, and technical requirements of the office shape the selection method trade-off. We also need better measures of accountability outcomes for public officials

²⁹<https://www.washingtonpost.com/nation/2022/03/14/georgia-elections-fraud-purge/>

(Carreri and Payson 2021). Scholars should work to distinguish between public responsiveness and conflicts in principals' goals. Are appointments beneficial only when the desires of voters and 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 are preferable to elections.

These findings also inform an ongoing public debate over the best form of election administration in the United States. Jurisdictions across the country continue to actively consider changes to how they select their local election officials. At a time when America's democracy has come under immense strain, it is more important than ever that the stewards of the democratic process are up to the task of administering our elections.

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Online Appendix

Intended for online publication only.

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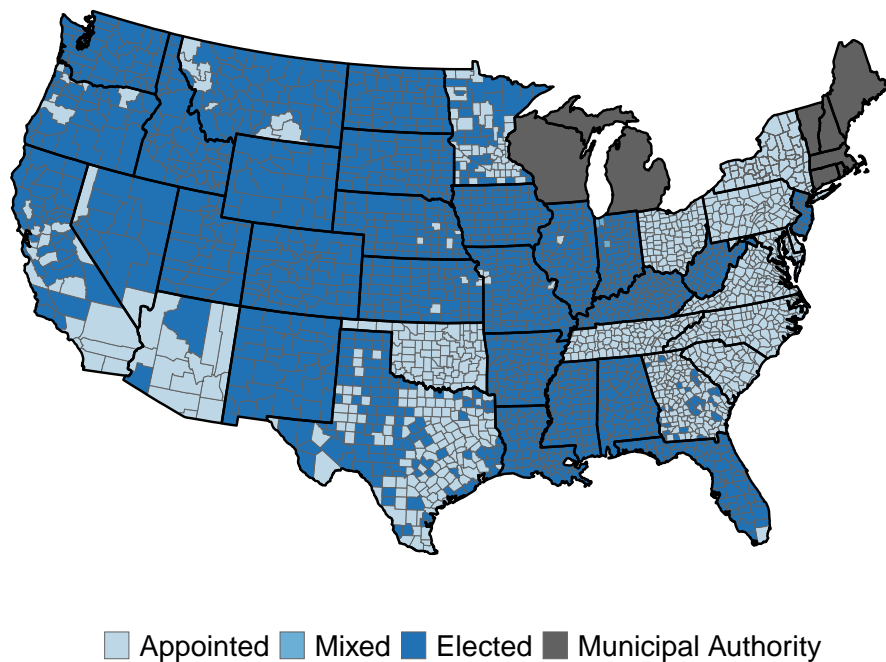
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A.1 Descriptive Appendices

A.1.1 Local Election Official Selection Method Map

Figure A.1 displays the current selection method of each main election authority for every jurisdiction in the United States where elections are administered at the county level.

Figure A.1: **Local Election Official Selection Method by County.** This map displays the selection method of the central election authority for each county in the United States where elections are administered at the county-level, as of 2022. In counties where municipal jurisdictions have separate administrators, the selection method for the county official is reflected. Data is from Ferrer and Geyn (2023). All election jurisdictions in Alaska use appointed officials and all counties in Hawaii use elected officials.



A.1.2 Descriptive Comparison of the Data Sample

Table A.1 compares counties within my sample of 13 states to counties in the 29 states that administer elections at the county level but that have not experienced any changes in selection method since 1960. I use population, racial/ethnic demographics, and region designation from the 2020 census and Democratic presidential vote share, voter turnout, and voter registration from Leip’s Election Atlas for the 2020 presidential election. Selection method data for the out-of-sample comparison is from Ferrer and Geyn (2023). Selection method for the in-sample data reflect administration for the 2020 general election.

Overall, there are few major differences between in-sample and out-of-sample counties. Counties within the sample look similar to other counties in the country in terms of population, partisanship, and voter participation. The sample is slightly more diverse than counties not in the sample, especially in terms of the share of Hispanics. It also consists of more Western and Midwestern states and no Northeastern states. Finally, counties in the sample are somewhat less likely to appoint their local election officials.

Table A.1: **Description of Counties In and Not In Sample**

	In Sample (1)	Not In Sample (2)
Population (Thousands)	110.83 (430.09)	103.00 (246.45)
Dem Pres Vote Share	0.32 (0.16)	0.35 (0.16)
Voter Turnout	0.62 (0.11)	0.64 (0.09)
Voter Registration	0.86 (0.10)	0.90 (0.11)
Share Non-Hispanic White	0.72 (0.21)	0.78 (0.19)
Share Black	0.096 (0.14)	0.100 (0.15)
Share Hispanic	0.13 (0.18)	0.07 (0.09)
Northeast	0.00	0.14
Midwest	0.38	0.27
South	0.45	0.49
West	0.17	0.10
Share Appointed	0.37	0.44
Num Counties	1117	2016

Standard deviations are reported in parentheses below group means. Counties for the 8 states with municipal-level election administration (CT, MA, ME, MI, NH, RI, VT, WI) are not included in the out-of-sample descriptive characteristics.

A.1.3 Local Election Official Selection Method Changes by State

Table A.2 displays additional data on the elected and appointed local election entities used in the analysis for each state, as well as counts of the number of counties in each state, the number always appointed, the number always elected, the number switching from elected to appointed, the number switching from appointed to elected, and the number undergoing multiple switches. These counts are a tabular form of Figure 1. The table also includes the first and last year a clerk selection method switched in each state. All of this data is in reference to the years of analysis, 1960 to 2022. Three columns are of particular importance: elected to appointed, appointed to elected, and multiple switchers. The counties falling in these three categories within each state power the difference-in-difference analysis. As shown in the table, the number of counties shifting to appointments far exceeds the number switching to elections. Ignoring those switching multiple times, 333 counties have switched to appointing their election official since 1960, compared with 3 counties that switched to electing theirs. In other words, 99.1% of all switches in selection method have been from elections to appointments. When counting each switch separately (including counties with multiple switches), 93% of all switches in selection method have been in the direction of appointments.

Table A.2: Local Election Official Selection Methods by State.

State	Elected Entity	Appointed Entity	Counties	Always Appointed	Always Elected	To Appointed	To Elected	Multiple Switches	First Switch	Last Switch
AZ	Election Administrator	Recorder	15	12	0	1	2	0	1997	2020
CA	Clerk	Registrar of Voters / Clerk	58	6	38	14	0	0	1970	2022
GA	Probate Judge	Board of Elections and Registration	159	0	28	129	0	2	1968	2022
IL	Clerk	Election Commission	102	0	93	0	0	1	1974	2016
IN	Clerk	Board of Election and Registration	92	0	89	3	0	0	1994	2020
MN	Auditor	Auditor	87	0	39	48	0	0	1968	2022
MO	Clerk	Election Commission / Director of Elections	115	3	110	1	0	0	1993	1994
MT	Clerk and Recorder	Election Administrator / Clerk and Recorder	56	0	47	8	1	0	1977	2022
NE	Clerk	Election Commissioner	93	2	86	2	0	3	1969	1996
NV	Clerk	Registrar of Voters	17	0	15	2	0	0	1966	1974
OR	Clerk	Elections Manager/Director	36	0	29	6	0	1	1964	1994
TX	Clerk / Tax Assessor	Elections Administrator	254	0	118	119	0	17	1980	2022
WA	Auditor	Elections Director	39	0	38	0	0	1	1969	2009
Totals	-	-	1123	23	730	333	3	23	-	-

Only primary local election authorities are listed under elected and appointed entities—those responsible for the majority of election duties in each county. In states with multiple primary election authorities, they are listed in order by frequency. Always appointed and always elected refer to counties that have maintained the same election official selection method since 1960. Multiple switches refers to counties that have both switched from elected to appointed and from appointed to elected. Not all county switch rows add up to the total number of counties in each state because some counties are excluded from analysis (i.e., those with municipal-level authorities in Illinois and Missouri).

A.1.4 Descriptive Comparison of Counties that Appoint vs. Elect Their Local Election Official

Table A.3 compares appointed and elected counties across the United States using the same data sources described in Section A.1.2 (Ferrer and Geyn 2023). Appointed counties are more than twice as populous on average as elected counties. They are also more Democratic, more racially and ethnically diverse, and more likely to be located in the Northeast and the South. Appointed counties have slightly lower voter turnout (62% vs. 63%) and voter registration rates (86% vs. 89%) than elected counties. This underscores the importance of using a credible research design to estimate causal effects from observational data.

Table A.3: **Description of Appointed and Elected Counties**

	Appointed (1)	Elected (2)
Population (Thousands)	162.35 (441.84)	62.85 (219.78)
Dem Pres Vote Share	0.37 (0.17)	0.30 (0.15)
Voter Turnout	0.62 (0.09)	0.63 (0.10)
Voter Registration	0.86 (0.09)	0.89 (0.11)
Share Non-Hispanic White	0.70 (0.21)	0.78 (0.20)
Share Black	0.14 (0.16)	0.08 (0.14)
Share Hispanic	0.10 (0.13)	0.10 (0.14)
Northeast	0.17	0.00
Midwest	0.11	0.41
South	0.66	0.39
West	0.06	0.20
Num Counties	1092	1816

Standard deviations are reported in parentheses below group means. Counties for the 8 states with municipal-level election administration (CT, MA, ME, MI, NH, RI, VT, WI) are not included.

A.2 Robustness Tests

A.2.1 Participation Effects Excluding Midterm Races

Table A.4 displays the results of a two-way fixed effects regression estimating the effects of directly electing a local election official on voter participation. These regressions only include data from presidential elections. The results are similar to those displayed in Table 1 in the main analysis, albeit slightly less precise.

Table A.4: **Appointing Local Election Officials Increases Citizen Participation (Presidential Elections, 1968-2020)**

	Voter Turnout			Registration Rate		
	(1)	(2)	(3)	(4)	(5)	(6)
Appointed	0.020 (0.004)	0.018 (0.004)	0.016 (0.004)	0.010 (0.004)	0.010 (0.005)	0.008 (0.005)
Counties	1116	1116	1116	942	942	942
Elections	14	14	14	6	7	6
Observations	15571	15571	15571	6577	6577	6577
Outcome Mean	0.57	0.57	0.57	0.85	0.85	0.85
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 Arizona and Georgia are excluded and because turnout data is available from 1968 but registration data is only available from 1996.

A.2.2 Participation Effects with Alternative Administrative Data

Conflicts arose between administrative and web scrapped data in Texas and the main results included some data imputations for missing cells. Table A.5 shows that the main finding that appointed election officials increase voter participation is robust to alternative coding decisions privileging documents provided by the Texas Secretary of State over archived Secretary of State web pages and removing all data imputations.

Table A.5: **Appointing Local Election Officials Increases Citizen Participation (Even-Year General Elections, 1968-2020, Public Information Act Preferred)**

	Voter Turnout			Registration Rate		
	(1)	(2)	(3)	(4)	(5)	(6)
Appointed	0.018 (0.003)	0.016 (0.003)	0.014 (0.003)	0.009 (0.004)	0.008 (0.004)	0.008 (0.004)
Counties	1116	1116	1116	942	942	942
Elections	28	28	28	13	13	13
Observations	31123	31123	31123	12213	12213	12213
Outcome Mean	0.50	0.50	0.50	0.84	0.84	0.84
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 Arizona and Georgia are excluded and because turnout data is available from 1968 but registration data is available from 1996.

A.2.3 Exploring State and Office Heterogeneity

This section shows evidence that the main result is generalizable across states and offices. Table A.6 estimates the effects of appointing election officials on voter participation separately for each of the four states with at least 10 counties that have changed their election official selection method since 1960. Those states are California, Georgia, Minnesota, and Texas. The results reveal precisely estimated and substantively meaningful effects for Georgia, Minnesota, and Texas. The magnitude of the effect on turnout is greater in Georgia and Minnesota than in Texas. The point estimate for CA is negative, although it is imprecisely estimated.

Table A.6: **Appointing Local Election Officials Increases Voter Turnout in Multiple States (Even-Year General Elections, 1968-2020)**

	Voter Turnout			
	(1)	(2)	(3)	(4)
Appointed	-0.008 (0.012)	0.022 (0.005)	0.027 (0.008)	0.016 (0.005)
Counties	58	159	87	253
Elections	28	28	28	28
Observations	1624	4452	2436	7084
Outcome Mean	0.49	0.40	0.64	0.43
State	CA	GA	MN	TX
County FEs	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes

Robust standard errors clustered by county in parentheses. States are included if at least 10 counties have switched between electing and appointing their local election official since 1960.

I also examine whether the effect holds across different statutory offices. Most directly elected election officials across the United States are county clerks. In my sample of 13 states, all elected election officials in Illinois, Indiana, Montana, Missouri, Nevada, and Oregon are clerks or hold clerk duties in addition to other titles. The same is true of almost all elected election officials in California and Texas. All elected election officials in Arizona are recorders,

which I group with clerks in this analysis due to their similar roles.³⁰ A few Texas counties use elected tax assessors as their election official. Auditor is also a fairly common position. All elected election officials in Minnesota and Washington are auditors, as well as a small number of counties in California. Finally, probate judges are the elected election officials in Georgia. Table A.7 shows that participation increases when appointed officials (the omitted category) replace elected auditors, clerks, and probate judges. The increase is larger when probate judges and auditors are replaced, and somewhat smaller when clerks are replaced. The point estimate for tax assessors is negative but imprecisely estimated, as it relies on a relatively small set of observations.

Table A.7: Switching from Elected Auditors, Clerks, and Probate Judges to Appointed Officials Increases Citizen Participation (Even-Year General Elections, 1968-2020)

	Turnout (1)	Registration (2)
Tax Assessor	-0.008 (0.012)	0.008 (0.012)
Auditor	0.022 (0.007)	0.047 (0.008)
Clerk	0.014 (0.005)	0.008 (0.005)
Probate Judge	0.022 (0.005)	0.006 (0.009)
Counties	1116	1116
Elections	28	13
Observations	31146	14478
Outcome Mean	0.50	0.82
County FEs	Yes	Yes
Year x State FEs	Yes	Yes

Robust standard errors clustered by county in parentheses. Point estimates are reversed for clarity, and thus show the effect of switching from each elected position to an appointed office on participation.

³⁰District & county clerks, found in smaller Texas counties, are also pooled with clerks for parsimony.

A.2.4 Results by Clerk Selection Method Reform Mechanism

One threat to causal inference is that reforms caused by some specific mechanism—state legislature, county legislature, and/or county referendum—are not exogenous to an increase in citizen participation. This seems most likely for referenda. Perhaps initial voter participation in a referendum that caused a change in clerk selection method spurs more turnout in future elections due to increased political efficacy. Or, perhaps the places with stronger cultures of direct democracy are more likely to have a referendum on the matter. Another scenario is that counties with local backing in the change are more likely to equip their newly appointed clerk with the tools to succeed or choose reform at the moment when it is most needed, compared with places where the state legislature initiates the reform. In Table A.8, I run regressions separating counties that have experienced a reform into three categories according to the reform initiator: county legislature, county referendum, and state legislature. Each regression also includes all counties that did not experience a move into or out of treatment throughout the dataset (“always elected” and “always appointed”).

Table A.8: **Appointing Local Election Officials Increases Citizen Participation Across Reform Mechanisms**

	Voter Turnout		
	(1)	(2)	(3)
Appointed	0.014 (0.004)	0.003 (0.015)	0.023 (0.005)
Counties	916	763	885
Elections	28	28	28
Observations	25621	21350	24766
Outcome Mean	0.51	0.53	0.51
Initiator	County Leg	County Referendum	State Leg
County FEs	Yes	Yes	Yes
Year x State FEs	Yes	Yes	Yes

Robust standard errors clustered by county in parentheses.

The results show that both county and state legislature-initiated reform mechanisms lead to a boost in turnout. Counties whose legislatures decide to switch from elected to appointed

clerks see 1.4 percentage points higher turnout in future presidential elections, on average. The effect is almost double—2.3 percentage points—when states initiate the reform. The result for county referendums is slightly positive but is imprecisely estimated. In short, the results hold across multiple reform mechanisms.

One related concern is that the reforms to clerk selection method that were initiated as part of a county charter suffer from similar endogeneity issues. The bundled treatment nature of these cases could also mean that the turnout effects are due to other changes in county governance that happened to coincide with the change to selection method. Table A.9 removes counties that changed their clerk selection method along with other amendments to their county charter. The results are similar to the main results shown in Table A.4. Virtually all other reforms concerned only the clerk selection method itself or, in rare cases, a reorganization of a few county departments, and thus the turnout effects cannot be attributed to other state or local policy changes.

Table A.9: Finding that Appointing Local Election Officials Increases Citizen Participation is Robust to Removing County Charter Changes

	Voter Turnout			Registration Rate		
	(1)	(2)	(3)	(4)	(5)	(6)
Appointed	0.018 (0.003)	0.016 (0.003)	0.014 (0.003)	0.010 (0.004)	0.010 (0.004)	0.008 (0.004)
Counties	1108	1108	1108	934	934	934
Elections	28	28	28	12	12	12
Observations	30922	30922	30922	12112	12112	12112
Outcome Mean	0.50	0.50	0.50	0.84	0.84	0.84
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. Counties that switched the selection method of clerk as part of a package of reforms to their county charter are removed.

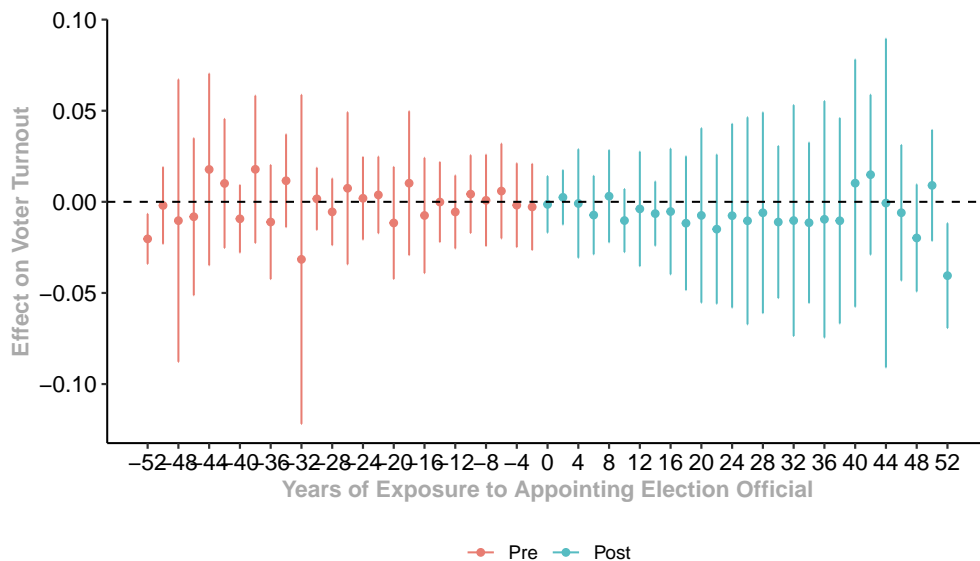
A.2.5 Examining Dynamic, Group, and Time Period Effects of Appointing Election Officials

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 clerks on presidential voter turnout. Because state-year fixed effects cannot be incorporated, I run separate estimates for each of the four states with at least 10 counties that have switched local election official selection methods since 1960: California, Georgia, Minnesota, and Texas. Dynamic effects for each of these states are visualized in Figures A.2, A.3, A.4, and A.5; cohort effects are visualized in Figures A.6, A.7, A.8, and A.9; and time period effects are visualized in Figures A.10, A.11, A.12, and A.13.

As seen in Figure 4 in the main analysis, there appears to be increasing effects on voter turnout over time for counties that switch to appointed administrators, relative to counties with elected officials. There are two potential explanations for this: appointed officials increasingly outperform elected officials as their tenure lengthens, or the value of appointed officials over elected ones has grown over time. In the former scenario, institutional learning effects and start-up costs of switching selection methods mean appointed officials need the practice of administering a few elections to realize their full potential compared to elected officials. Recent work has found that voter wait times may increase after the turnover of a local election official, although turnout rates do not dip when a change of leadership takes place (Ferrer and Thompson 2024). In the latter scenario, the declining ability of voters to adequately select and sanction elected officials combined with the increasing technical demands of the job and growing recruitment problems create a bigger gap between elected and appointed officials over time.³¹ The evidence I present regarding the differential effect of selection method by the presence of a local newspaper in Table 6 supports this theory.

³¹<https://www.inquirer.com/politics/election/spl/pennsylvania-election-2020-officials-retiring-nightmare-20201221.html>

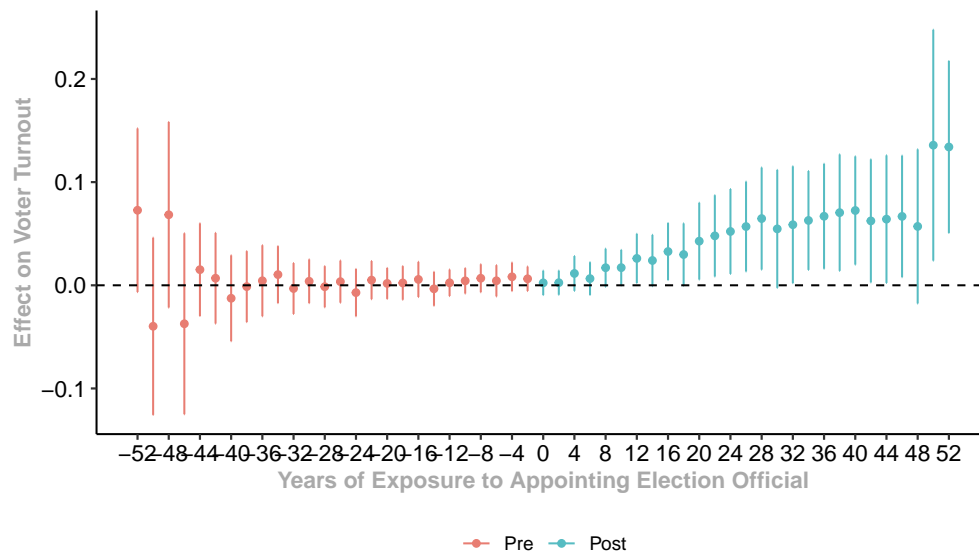
Figure A.2: **Average Effect of Appointed Election Officials on Voter Turnout by Length of Exposure to Appointing - California.** Year 0 is the even-year general election after a county's first switch from electing to appointing an election official. Each point is the estimated effect of appointing an election official on 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.



Figures A.3, A.4, and A.5 show fairly large increases in the effect size on turnout several elections after the initial switch. These estimators account for heterogeneous treatment effects, but they do not correct for pre-trending so should be interpreted cautiously. What is more plausible are the increases in effect size shown in the generalized synthetic control (Figure 4 in the main analysis), which are approximately one additional percentage point boost in turnout three elections after the switch to appointments.

Figures A.6, A.7, A.8, and A.9 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 officials have experienced stronger overall treatment effects than more recent adopters. Figures A.10, A.11, A.12, and A.13 display time period effects of switching to appointing election officials. They suggest that the greater effect of earlier

Figure A.3: **Average Effect of Appointed Election Officials on Voter Turnout by Length of Exposure to Appointing - Georgia.** Year 0 is the even-year general 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 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.



adopters is due to long-term accumulation rather than a diminishing instantaneous effect over time.

Figure A.4: **Average Effect of Appointed Election Officials on Voter Turnout by Length of Exposure to Appointing - Minnesota.** Year 0 is the even-year general 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 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.

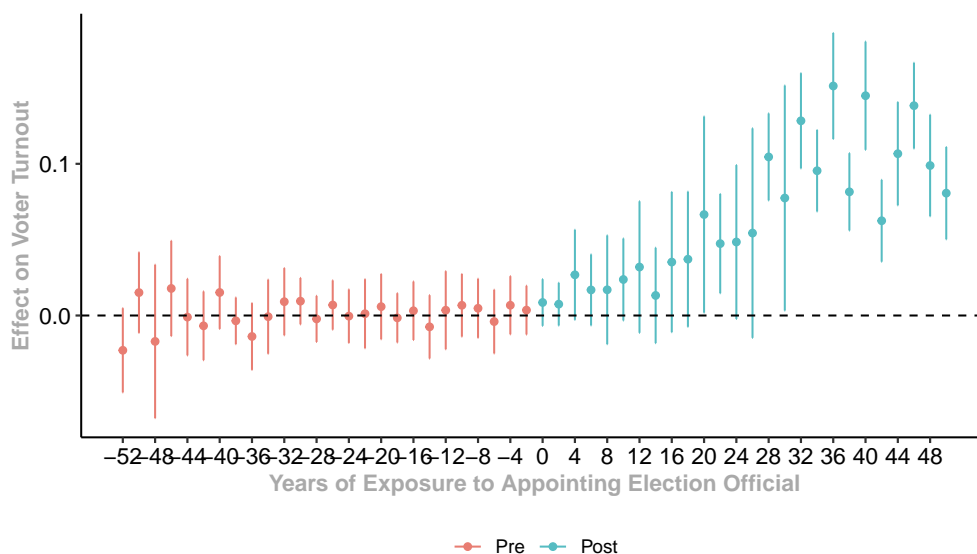


Figure A.5: **Average Effect of Appointed Election Officials on Voter Turnout by Length of Exposure to Appointing - Texas.** Year 0 is the even-year general election after a county's first switch from electing to appointing an election official. Each point is the estimated effect of appointing an election official on 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.

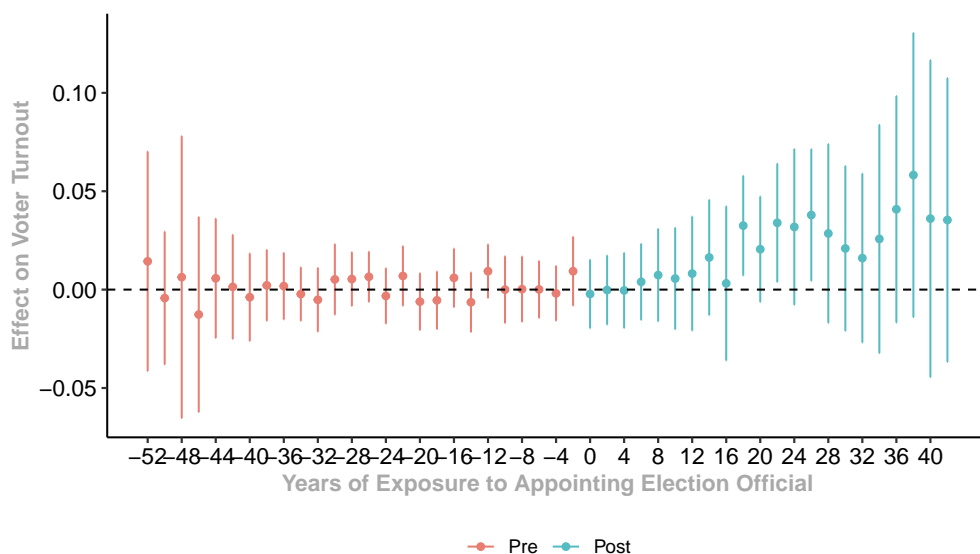


Figure A.6: **Average Effect of Appointed Election Officials on Voter Turnout by Cohort Group - California.** Each point is an estimate of the average group effect of appointing election officials on voter turnout for counties that switch in the given cohort year. The lines above and below each point 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.

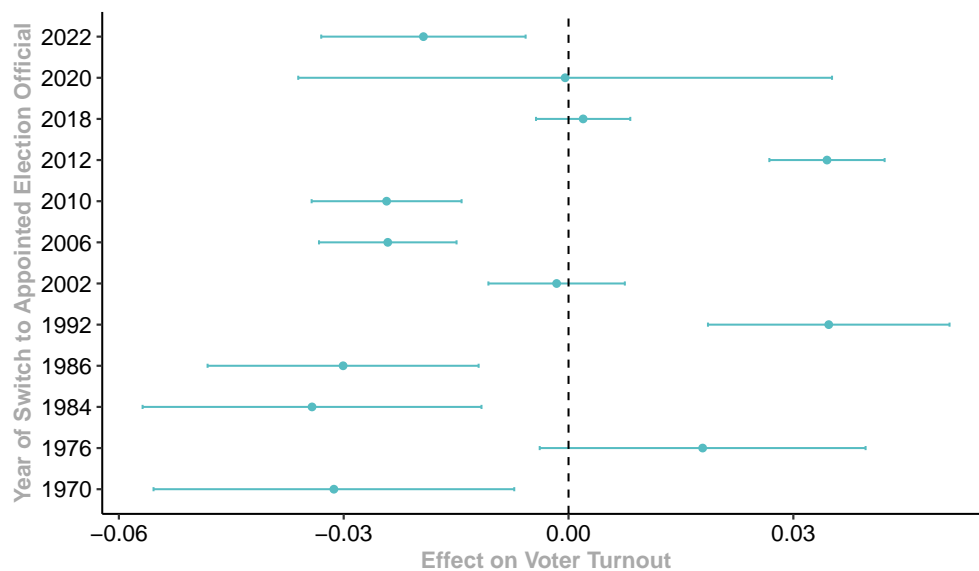


Figure A.7: **Average Effect of Appointed Election Officials on Voter Turnout by Cohort Group - Georgia.** Each point is an estimate of the average group effect of appointing election officials on voter turnout for counties that switch in the given cohort year. The lines above and below each point represent 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.

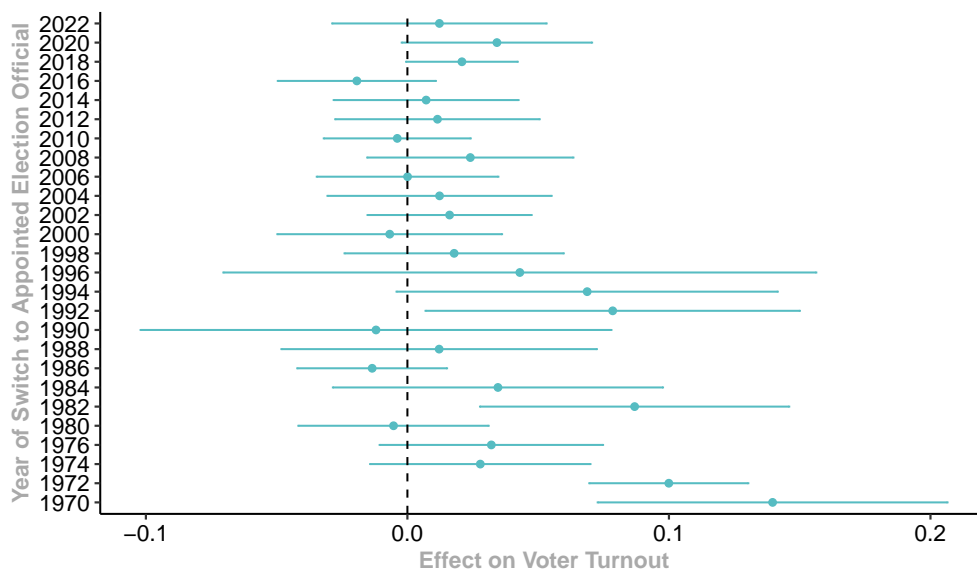


Figure A.8: **Average Effect of Appointed Election Officials on Voter Turnout by Cohort Group - Minnesota.** Each point is an estimate of the average group effect of appointing election officials on voter turnout for counties that switch in the given cohort year. The lines above and below each point 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.

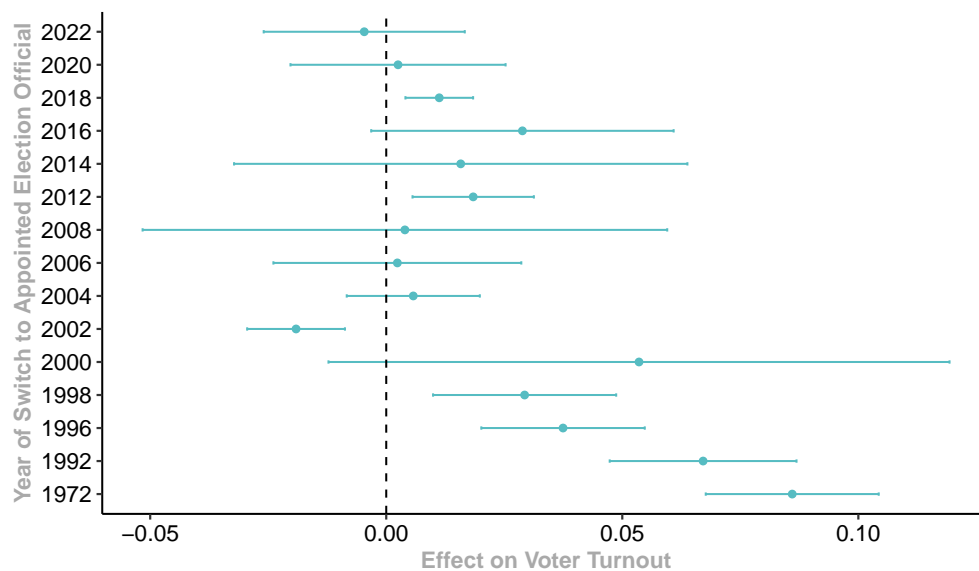


Figure A.9: **Average Effect of Appointed Election Officials on Voter Turnout by Cohort Group - Texas.** Each point is an estimate of the average group effect of appointing election officials on voter turnout for counties that switch in the given cohort year. The lines above and below each point represent 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.

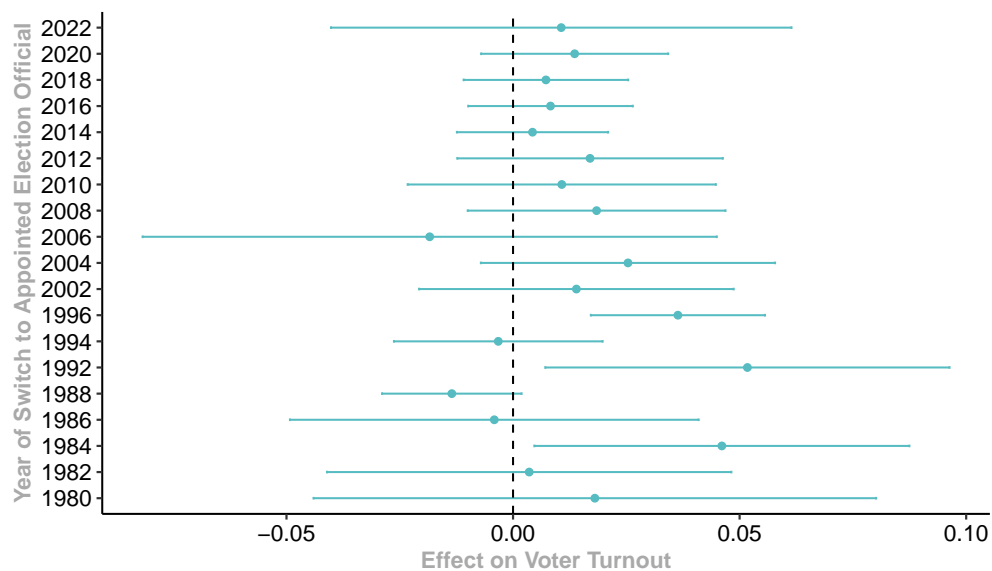


Figure A.10: **Average Effect of Appointed Election Officials on Voter Turnout by Time Period - California.** Each point is an estimate of the average time period effect of appointing election officials on voter turnout. The lines above and below each point represent 95-percent confidence intervals. 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.

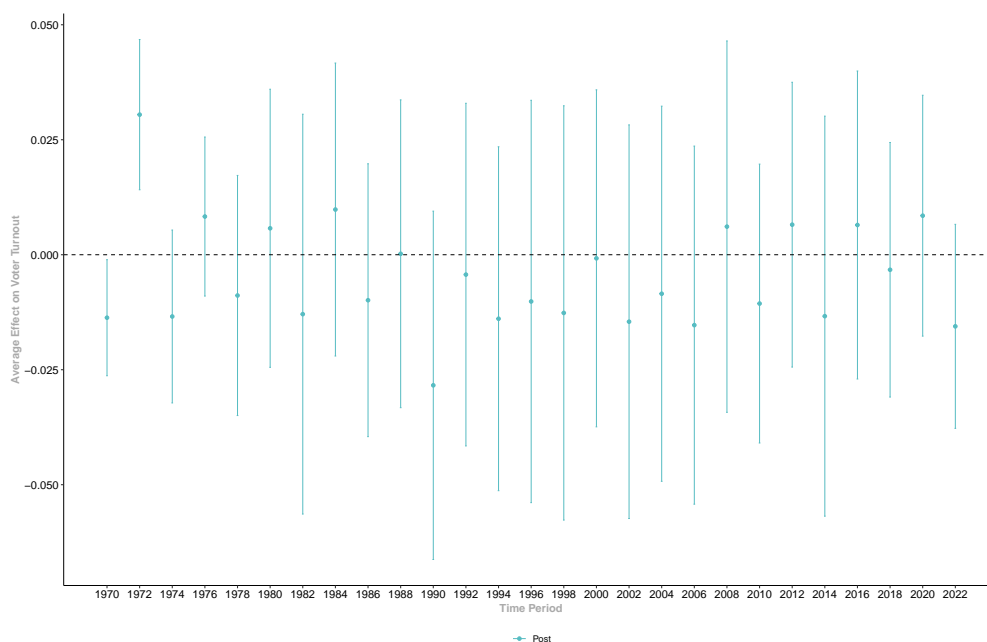


Figure A.11: **Average Effect of Appointed Election Officials on Voter Turnout by Time Period - Georgia.** Each point is an estimate of the average time period effect of appointing election officials on voter turnout. The lines above and below each point represent 95-percent confidence intervals. Estimates 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.

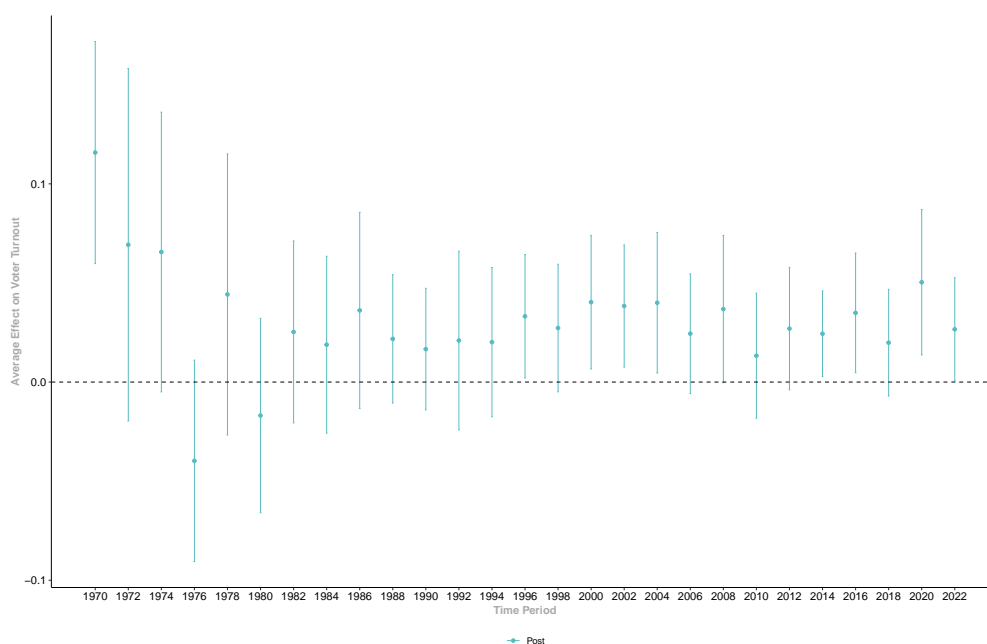


Figure A.12: **Average Effect of Appointed Election Officials on Voter Turnout by Time Period - Minnesota.** Each point is an estimate of the average time period effect of appointing election officials on voter turnout. The lines above and below each point represent 95-percent confidence intervals. Estimates 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.

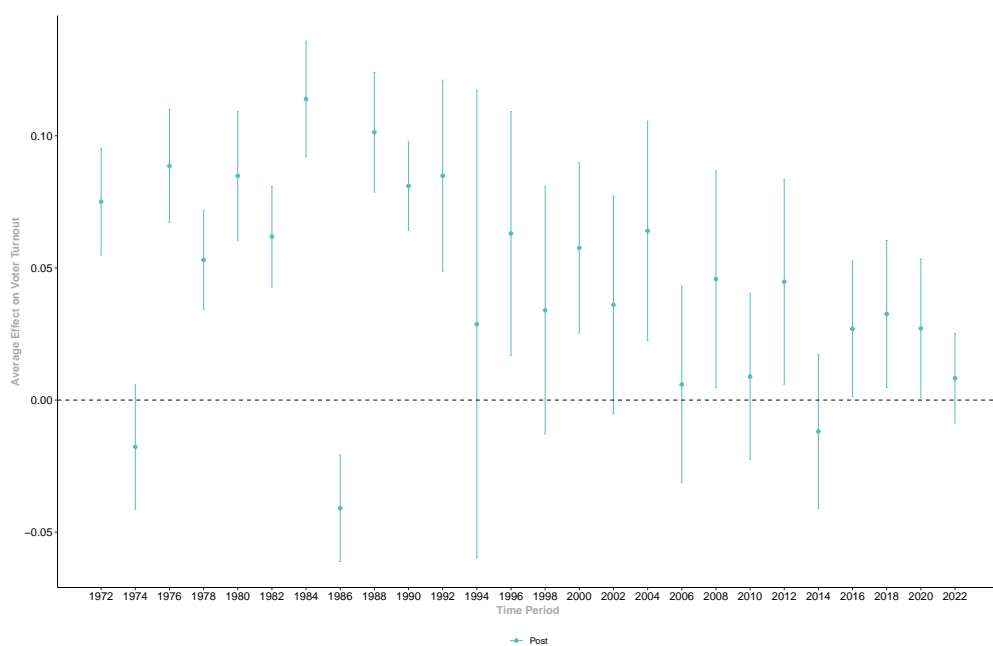
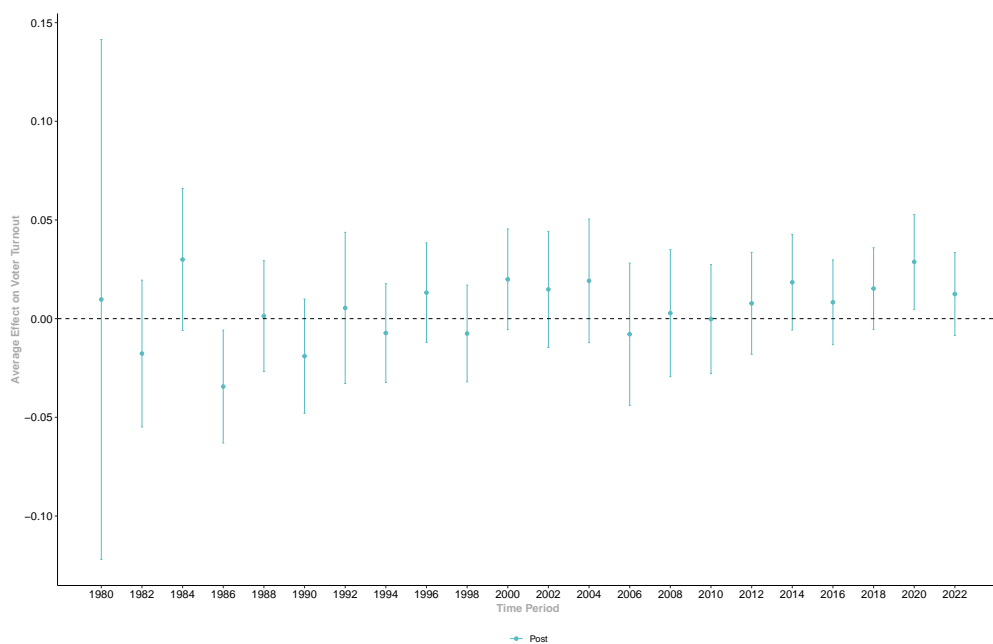


Figure A.13: **Average Effect of Appointed Election Officials on Voter Turnout by Time Period - Texas.** Each point is an estimate of the average time period effect of appointing election officials on voter turnout. The lines above and below each point represent 95-percent confidence intervals. Estimates 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.2.6 Are the Results an Artifact of the Jim Crow South?

One concern is that registration and turnout rates of African-Americans in Southern states 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 the South continued to trail behind those of white voters until many decades later (Fraga 2018). 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.10 displays three truncated cuts 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 1968 presidential election. Because registration data is only available from 1996, I focus on voter turnout here. The point estimates do grow similar when older data is discarded. However, in all specifications the point estimates are substantively large and statistically distinguishable from zero. In the most restrictive analysis, which only uses data from 2000 onwards, counties that switch to appointed clerks are estimated to boost turnout by half a percentage point.

Table A.10: **Finding that Appointing Local Election Officials Increases Citizen Participation is Robust to Alternative Year Cutoffs**

	Voter Turnout			
	(1)	(2)	(3)	(4)
Appointed	0.013 (0.003)	0.012 (0.003)	0.006 (0.002)	0.005 (0.002)
Counties	1116	1116	1116	1116
Elections	11	8	6	4
Observations	24481	18926	13365	7800
Outcome Mean	0.50	0.50	0.51	0.50
Year Cutoff	1980	1990	2000	2010
County FEs	Yes	Yes	Yes	Yes
Year x State FEs	Yes	Yes	Yes	Yes

Robust standard errors clustered by county in parentheses. Year cutoff indicates the first even-year general election included in the analysis.

A.3 Validation Exercises

A.3.1 Validating the Staggered Rollout Design with Alternative Estimators

Table A.11 displays results from additional estimators designed to help overcome the identification issues of the staggered adoption two-way fixed effects design. All estimators include county and year by state fixed effects. Column 1 is the same specification found in column 1 of Table 1. Column 2 excludes counties that switch from appointed to elected clerks, as they can be a source of bias. 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 estimation. The last two columns show the results of stacked difference-in-difference estimates (Cengiz et al. 2019). The point estimates are consistent and precisely estimated across all specifications.

I employ the (Imai, Kim, and Wang 2023) strategy of matching treated and control units. The key advantage of this procedure is that it allows me to match both on pre-treatment voter turnout trajectory and exact match on state, state by pre-treatment population, or state by Democratic vote share. I only include counties that either are elected throughout the dataset and those that start elected and switch to appointed. I do not include counties with multiple switches between elections and appointment. I match on eight elections of pre-treatment data, use the mahalanobis refinement method, and allow up to 10 control units to match with each treated unit. Table A.12 shows the results of this exercise.

The procedure produces 147 matches, leaving the estimates somewhat imprecise. However, the point estimates are in line with those found in Table 1.

Table A.11: **Finding that Appointing Local Election Officials Increases Citizen Participation Is Robust to Alternative Estimators (Even-Year General Elections, 1968-2020)**

	Voter Turnout				
	(1)	(2)	(3)	(4)	(5)
Appointed	0.018 (0.003)	0.020 (0.003)	0.020 (0.003)	0.028 (0.005)	0.024 (0.004)
Counties	1116	1085	1062	1062	853
Elections (avg)	28	28	28	28	28
Observations	31146	30366	29735	560421	41691
Outcome Mean	0.50	0.51	0.51	0.53	0.59
County FEs	Yes	Yes	Yes	Yes	Yes
Year x State FEs	Yes	Yes	Yes	Yes	Yes
App to Elect 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 is identical to the specification shown in column 1 of Table 1. Column 2 excludes 28 counties that switch from appointing to electing their clerks. Column 3 additionally excludes counties that have not elected their clerk since 1966. 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.

Table A.12: **Finding that Appointing Local Election Officials Increases Voter Turnout Is Robust to Imai et al. 2024 Matching Estimator (Even-Year General Elections, 1968-2020)**

	Voter Turnout		
	(1)	(2)	(3)
Appointed	0.015 (0.017)	0.015 (0.017)	0.015 (0.018)
Matches	147	147	147
County FEs	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes
Pre-treatment voter turnout matching	Yes	Yes	Yes
State exact matching	Yes	No	No
State x Dem vote share exact matching	No	Yes	No
State x Population exact matching	No	No	Yes

Bootstrapped standard errors with a degree-of-freedom adjustment in parentheses. All regressions use a pooled estimator that averages over the first 8 elections after treatment and matches over 8 elections prior to treatment. Matching is done using mahalanobis distance

A.3.2 Validating the Staggered Rollout Design with State-Specific Estimates

I run the de Chaisemartin and D’Haultfoeuille (2020) and the Callaway and Sant’Anna (2021) estimators separately for each state with at least 10 counties that have switched their election official selection method since 1960. This is to ensure that the results are not biased by differential trending between states. The results are displayed in Tables A.13, A.14, A.15, and A.16. The de Chaisemartin and D’Haultfoeuille (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 stacked difference-in-differences estimator displayed in column 4 of Table A.11. First, always treated units are removed from the dataset (i.e., counties that have appointed their election officials since at least 1960). 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 counties that switch from appointment to election are assigned to treatment even after their switch. Finally, those counties that are never treated (i.e., have always had elected election officials since 1960) are separated out as the “true control” by which each cohort can be compared with. Doing so avoids negative weights, thereby addressing the problems introduced by heterogeneous treatment and timing effects.

The point estimates produced by these analyses are generally in line with the main findings. All estimators for Georgia, Minnesota, and Texas return positive point estimates and are precisely estimated. The point estimates for California are slightly negative but are statistically indistinguishable from a null effect.

Table A.13: Main Finding that Appointing Local Election Officials Increases Voter Turnout is Robust to Alternate Specifications - California

	Voter Turnout		
	Two-Way FEs (1)	de Chaisemartin and D'Haultfoeuille (2)	Callaway and Sant'Anna (3)
Appointed	-0.008 (0.012)	-0.001 (0.002)	-0.006 (0.011)
Counties	58	58	52
Elections	28	28	28
Observations	1624	1291	1664
Outcome Mean	0.49	0.49	0.49
County FEs	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes

Robust standard errors clustered by county in parentheses.

Table A.14: 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	0.022 (0.005)	0.006 (0.004)	0.050 (0.011)
Counties	159	159	155
Elections	28	28	28
Outcome Mean	0.40	0.40	0.40
County FEs	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes

Robust standard errors clustered by county in parentheses.

Table A.15: Main Finding that Appointing Local Election Officials Increases Voter Turnout is Robust to Alternate Specifications - Minnesota

	Voter Turnout		
	Two-Way FEs (1)	de Chaisemartin and D'Haultfoeuille (2)	Callaway and Sant'Anna (3)
Appointed	0.027 (0.008)	0.011 (0.005)	0.066 (0.007)
Counties	87	87	86
Elections	28	28	28
Observations	2436	1704	2784
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.16: 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	0.016 (0.005)	0.001 (0.005)	0.021 (0.009)
Counties	253	253	236
Elections	28	28	28
Outcome Mean	0.43	0.43	0.43
County FEs	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes

Robust standard errors clustered by county in parentheses.

A.3.3 Testing the Parallel Trends Assumption with Event Studies Estimators

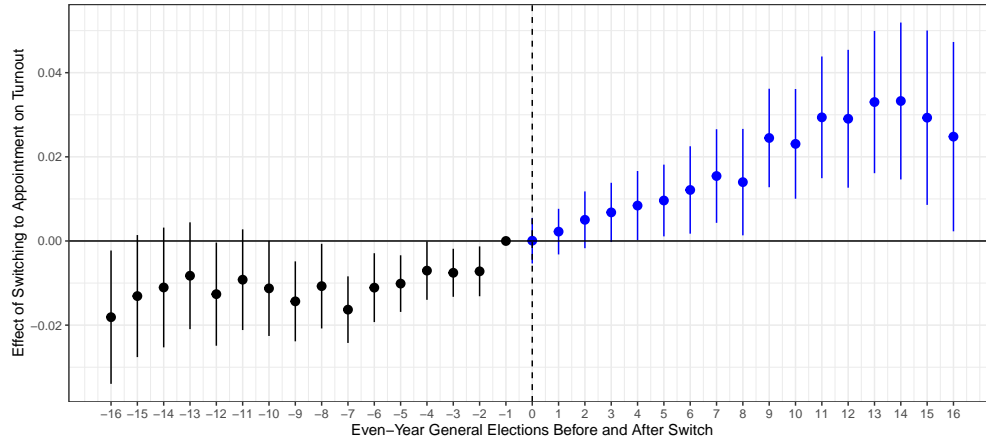
I investigate the validity of the parallel trends assumption for difference-in-difference specifications using the Dube et al. (2022) local projections event studies estimator. I make a series of pooled two-period two-group comparisons and estimate period-by-period effects, eliminating biases due to heterogeneous treatment effects. However, biases due to parallel trending remain a possibility. Figure A.14 plots the results for voter turnout. The x-axis marks the even-year general 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 federal elections before or after each county's actual switch. Negative coefficients in the left half of the graph suggest pre-trending. In other words, counties that switched to appointing clerks may already have been on a trajectory of higher turnout. The estimated effect becomes positive 1 even-year federal election after adoption of appointments. There also appears to be an increasing effect on turnout after counties switch their method of clerk selection, which I explore in Section A.2.5.

I examine the parallel trends assumption for the test on registration rates in A.15. There is some evidence of parallel trending, though a positive effect first appears in the first election after counties adopt appointments. Due to the more limited span of the registration data, I am unable to employ a general synthetic control design. Therefore, the main results for registration rates should be viewed with some degree of caution.

I examine the parallel trends assumption for the test on turnover rates in A.16. Point 0 shows an extremely large positive effect on turnover because in the vast majority of cases, switching to an appointed elections official forced turnover. Besides this expected aberration, I find little evidence of pre-trending.

Finally, Figure A.17 examines the validity of the parallel trends assumption for the effect of appointments on expenditures. I find no evidence of pre-trending and imprecisely esti-

Figure A.14: **Dube et al. (2022) Local Projections Difference-in-Differences Estimate of Effect of Appointing an Election Official on Voter Turnout.** Year 0 is the even-year general 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 voter turnout, at x federal 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.



mated but positive coefficients after a county switches to appointments. Additionally, the increase in expenditures is not instantaneous with the switch in selection method (period 0), but rather begins in the election after this switch. This is an indication that the increase in expenditures is caused by the appointed election official rather than some confounding factor causing both the selection method to change and election expenditures to increase.

Figure A.15: **Dube et al. (2022) Local Projections Difference-in-Differences Estimate of Effect of Appointing an Election Official on Registration.** Year 0 is the even-year general 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 voter registration, at x federal 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.

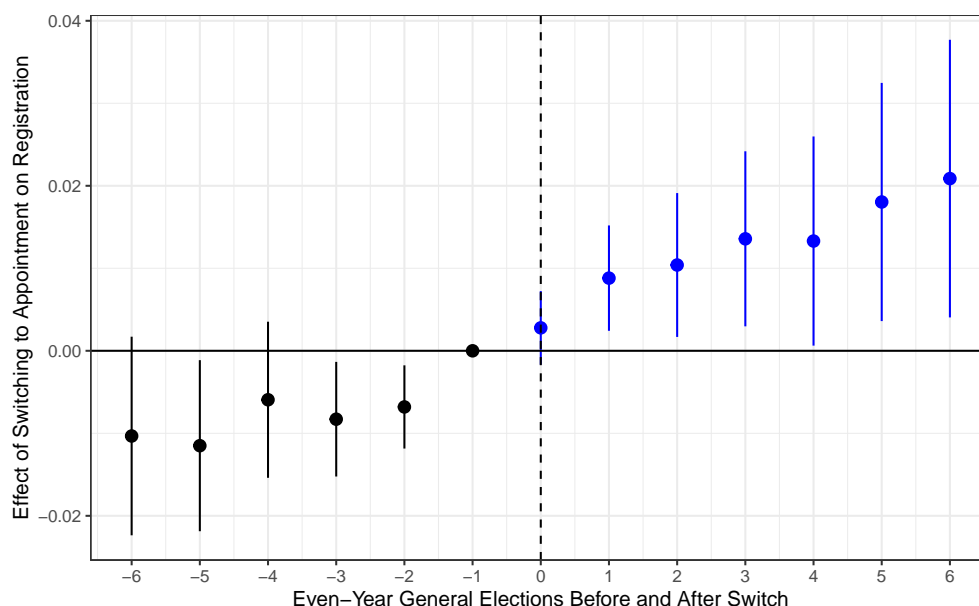


Figure A.16: **Dube et al. (2022) Local Projections Difference-in-Differences Estimate of Effect of Appointing an Election Official on Turnover.** Year 0 is the even-year general election after a county's first switch from electing to appointing an election official. Each point is the estimated effect of appointing an election official on the election official turnover rate over a two-year period, at x federal 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.

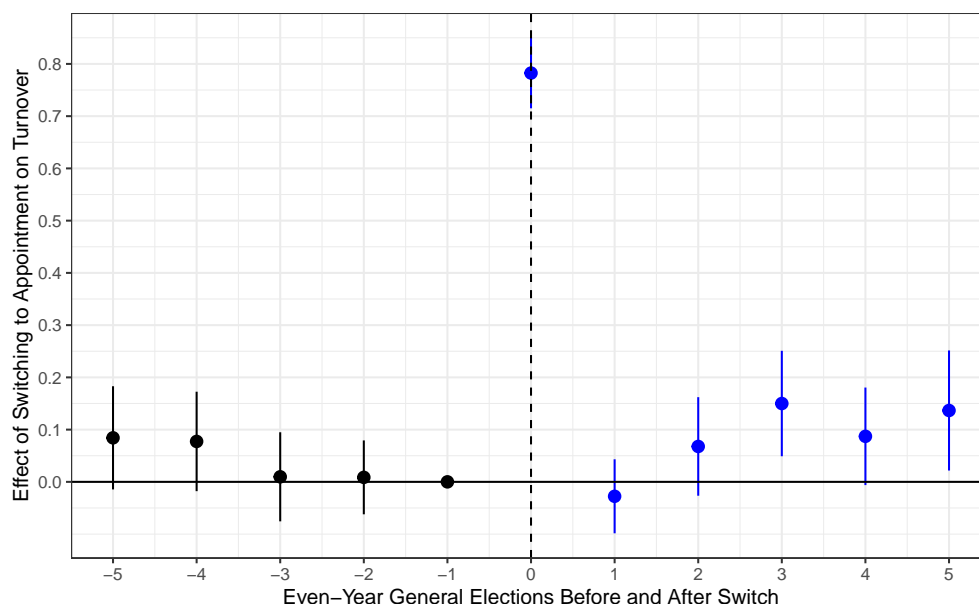
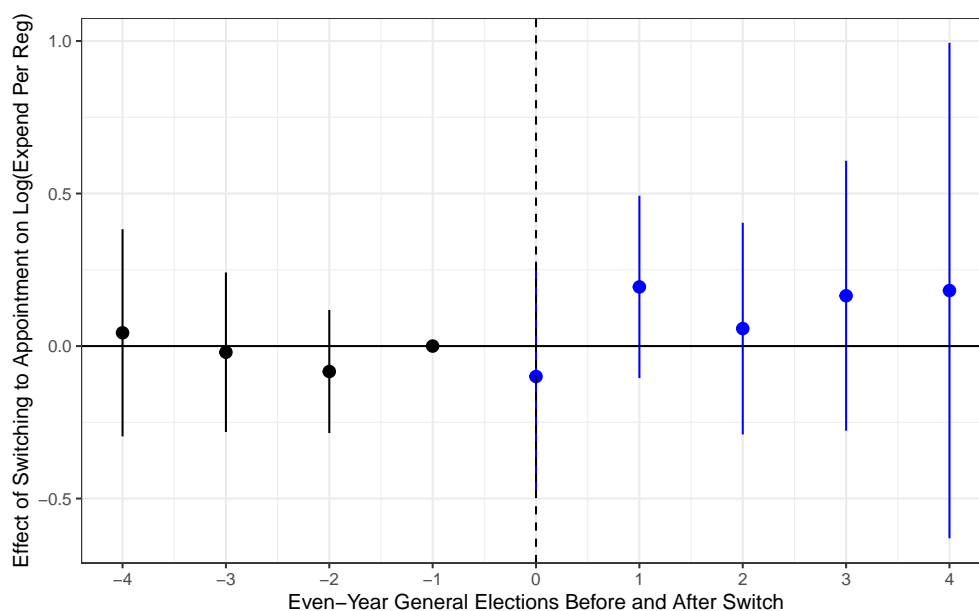


Figure A.17: **Dube et al. (2022) Local Projections Difference-in-Differences Estimate of Effect of Appointing an Election Official on Log(Expenditures Per Registrant)**. Year 0 is the even-year general election after a county's first switch from electing to appointing an election official. Each point is the estimated effect of appointing an election official on logged election expenditures per registered voters, at x 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.



A.3.4 Generalized Synthetic Control Regression Output

Table A.17 displays regression output from the Xu (2017) generalized synthetic control estimator, comparing treatment and control counties with similar pretreatment turnout histories. This method relies on strictly fewer assumptions than the difference-in-differences estimator and allows for a relaxation of the parallel trends assumption. The point estimate in Table A.17 is 0.8%. This is smaller and less precisely estimated than those found in Table A.4 in the main analysis, but it is still a substantively significant effect for even-year general elections.

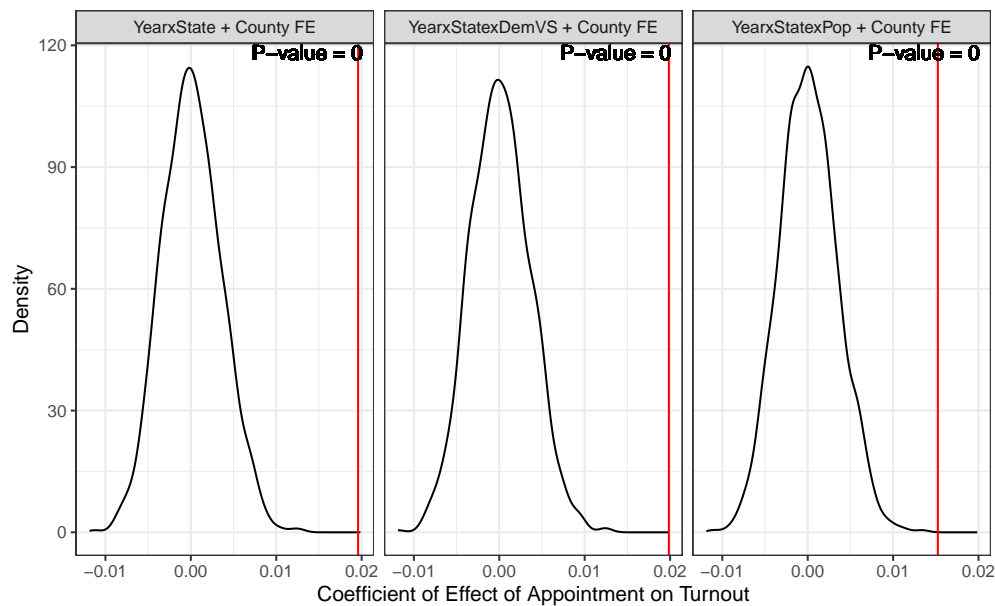
Table A.17: Main Finding that Appointing Local Election Officials Increases Voter Turnout is Robust to Generalized Synthetic Control Estimator

	Voter Turnout (1)
Appointed	0.008 (0.006)
Counties	1042
Elections	28
Observations	29176
Outcome Mean	0.50
Generalized synthetic control method matches treated and control counties on pretreatment voter turnout.	

A.3.5 Randomization Inference Additional Output

Figure A.18 shows the distribution of point estimates of the effect of appointments on voter turnout which counties switch to appointed local election officials and when they switch is randomly permuted. This procedure shows that it is extremely unlikely to observe an effect of appointments on voter turnout as large or larger than that observed by chance alone.

Figure A.18: Randomization Inference for Table 1, Columns 1-3 - Treatment and Timing. This graph displays the output of randomization inference for the main effects of appointed local election officials on voter turnout. Both which counties are treated and when counties are treated are randomly permuted. The black distribution shows the resulting coefficients of 1,000 iterations. The red solid vertical line is the actual coefficient observed, and the p-value is the share of coefficients that are equal to or larger than the one estimated in the respective specification in Table 1.



A.3.6 Appointing Election Officials Boosts Registration Rates More when Their Duties Specifically Include Registration

In most states, the switch from elected to appointed election officials involves both registration administration and voting administration duties. In Arizona and Georgia, the shift only impacts voting administration; registration duties are primarily carried out by separate appointed officials. It is possible that election administrators in these states impact registration rates by referring individuals to registration officials or providing a better overall voting experience. However, if appointed officials outperform their elected counterparts, we should expect to see a larger effect on registration rates when the official directly in charge of registration duties switches from elected to appointed. Table A.18 displays the results of this placebo test. The first four columns individually test registration rates for the four states with at least 10 counties that have switched between electing and appointing their local election official: California, Georgia, Minnesota, and Texas. We should observe greater effects of appointed administration on registration rates in California, Minnesota, and Texas than in Georgia. The point estimate is smallest for Georgia, although the magnitude of the effect is similar across Georgia, Minnesota, and Texas.

Column 5 pools results across states and uses a triple difference-in-differences design to test whether the effects of switching to appointed election officials on registration rates are smaller in states with separate registration systems. The effect on switching to appointed election officials on registration rates in counties where the registrar is always appointed is roughly half that found in counties where the reform switched registration duties from an elected to an appointed official. Overall, the evidence is suggestive that counties experience a larger boost to registration rates when the official directly in charge of registration duties switches from an elected to an appointed position.

Table A.18: **Appointing Election Officials Boosts Registration Rates More when Their Duties Specifically Include Registration (Even-Year General Elections, 1996-2020)**

	Registration Rate				
	(1)	(2)	(3)	(4)	(5)
Appointed	0.016 (0.012)	0.006 (0.009)	0.007 (0.005)	0.009 (0.005)	0.009 (0.004)
Appointed X Separate Reg					-0.004 (0.010)
Counties	58	159	87	253	1116
Elections	13	13	13	13	13
Observations	754	2067	1131	3289	14478
Outcome Mean	0.68	0.71	0.85	0.81	0.82
State	CA	GA	MN	TX	Pooled
County FEs	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	No
Year x State x Sep Reg FEs	No	No	No	No	Yes
Reg Switch	Yes	No	Yes	Yes	—

Robust standard errors clustered by county in parentheses. Individual regressions are run on states with at least 10 treated counties. “Sep Reg” is short for a separate registration dummy.

A.3.7 Selection Method, Not Partisanship, Explains the Results

Georgia, Missouri, and Texas’s long histories of race-based disenfranchisement, the strong association between race and partisanship (Abramowitz and McCoy 2019; Carmines and Stimson 1989), 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. I distinguish between the effects of selection method and partisanship by utilizing changes in Georgia, Montana, and Washington counties between partisan elections, nonpartisan elections, and appointments of election officials. Table A.19 displays estimates of voter turnout separating out the effects of appointments and partisan elections, with the omitted category elected nonpartisan elections. The results provide strong evidence that elections themselves, and not the partisan nature of the office, drive the main results on voter turnout. All of the estimated positive effect on turnout is observed for a switch from elected to appointed administration, whereas the effect of switching between partisan and nonpartisan administration is negative and indistinguishable from zero.

Table A.19: **Appointments, Rather than Partisanship, Drive the Effects on Voter Turnout (Even-Year General Elections, 1968-2020)**

	Voter Turnout		
	(1)	(2)	(3)
Appointed	0.015 (0.004)	0.014 (0.004)	0.011 (0.004)
Partisan Elected	-0.005 (0.005)	-0.004 (0.005)	-0.004 (0.005)
Counties	1116	1116	1116
Elections	28	28	28
Observations	31122	31122	31122
Outcome Mean	0.50	0.50	0.50
County FEs	Yes	Yes	Yes
Year x State FEs	Yes	No	No
Year x State x Dem vs FEs	No	Yes	No
Year x State x Pop FEs	No	No	Yes

Robust standard errors clustered by county in parentheses. The omitted category is selection through non-partisan elections.

A.3.8 Appointed Local Election Officials Do Not Benefit Their Principals' Party

If the quality of selection and sanctioning of local election officials is indeed higher for those that are appointed, this leads to the possibility that appointed clerks might be selected and/or more successfully pursue strategies that benefit a certain political party over another. This would significantly alter the normative implications of the paper's findings. Appointments could lead to better-administered elections and higher voter participation. But they could also lead to officials who try to skew election results in their party's favor. Recent scholarship has found that Democratic and Republican clerks administer elections in similar ways and produce similar partisan outcomes and voter turnout (Ferrer, Geyn, and Thompson 2023). Shepherd et al. (2021) find no evidence that the party of appointed clerks in North Carolina shapes their decisions on polling place allocation. Here I examine whether appointed local election officials act in ways that benefit the majority party of their principals.

To test whether appointed officials benefit the party of their appointers, I examine two states where election officials are appointed by county officials who run in partisan elections: Arizona and Pennsylvania. In Arizona, the Board of Supervisors appoints the election official, whereas in most Pennsylvania counties the County Commissioners have this authority. I collect original data on the majority party of each clerk's appointers between 2000 and 2022, using a combination of the American local government elections database (de Benedictis-Kessner et al. 2023), data from de Benedictis-Kessner and Warshaw (2020), and archival web searches. I combine this data with Democratic presidential and gubernatorial vote share from David Leip and Democratic share of registrants from administrative records in each state.

Table A.20 shows difference-in-differences regressions testing the effect of a switch to a Democratic party controlled appointing body on three outcomes: Democratic presidential 2-party vote share (columns 1-2), Democratic gubernatorial 2-party vote share (columns 3-4), and share of registrants that are Democrats (columns 5-6). All specifications include state-

by-year fixed effects to account for differential partisan trending in each state. The even columns also include county linear time trends, since counties that switch from Republican to Democratic local leadership are likely trending in a Democratic direction. This means that the outcomes are tested in excess of the underlying partisan trend specific to each county. There are concerns that including unit linear time trends in a two-way fixed effects analysis might absorb potential treatment effects, biasing the analysis downward (Wolfers 2006). However, in this dataset many counties switch back and forth between Democratic and Republican control, making this less of a concern .

Table A.20: **Appointed Local Election Officials Do Not Clearly Benefit The Party That Appoints Them (County-Level Shifts in AZ and PA, 2000-2022)**

	Dem pres vote share		Dem gov vote share		Dem reg share	
	(1)	(2)	(3)	(4)	(5)	(6)
Dem Appointer	0.023 (0.007)	-0.001 (0.003)	0.017 (0.006)	0.003 (0.007)	0.027 (0.008)	0.003 (0.003)
Counties	80	80	80	80	80	80
Elections	6	6	6	6	12	12
Observations	472	472	471	471	943	943
Outcome Mean	0.41	0.41	0.44	0.44	0.40	0.40
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year x State FEs	Yes	Yes	Yes	Yes	Yes	Yes
County Time Trend	No	Yes	No	Yes	No	Yes

Robust standard errors clustered by county in parentheses. Columns 1–4 use Leip’s Atlas of U.S. elections. Columns 5-6 use an original data collection from each state’s administrative records. Dem Appointer means that the appointing authority of the local election official has a Democratic majority.

Two pictures emerge from these results depending on if the county time trend is included. Columns 1, 3, and 5 indicate that switching from a Republican-controlled appointing body to a Democratic-controlled body increases Democratic presidential vote share by 2.3 percentage points, increases Democratic gubernatorial vote share by 1.7 percentage points, and increases Democratic share of registrants by 2.7 percentage points. However, the odd columns show this to be an artifact of pre-trending: counties that start electing Democrats majorities to their county legislature or a Democrat to their county chief executive also become more

favorable to Democratic state and national candidates. The largest effect size including unit-specific linear time trends (columns 2, 4, and 6) is three-tenths of a boost in Democratic gubernatorial vote share and Democratic share of registrants, but both fall well within a 95% confidence interval. The coefficient for Democratic presidential vote share is slightly negative. In sum, the evidence is consistent with no effect of appointed election officials benefiting their principals' majority party.

A.3.9 Appointed Local Election Officials Increase Participation Similarly in Democratic and Republican Counties

If conservative-leaning voters prefer less turnout and elected officials are more beholden to the voters, switching to appointed officials could increase participation more in Republican-leaning jurisdictions. On the other hand, if appointed officials are more responsive to voters because they are better monitored and sanctioned, we might expect the opposite effect: that appointed officials increase voter turnout *less* in Republican-leaning jurisdictions. These effects could cancel each other out.

Table A.21 examines whether switching from an elected to an appointed election official leads to a larger boost in participation in jurisdictions that are more Democratic. “Democratic” is measured as being in the top half of a state’s Democratic vote shares for the 1968 presidential election, the last pre-treatment election year. The top row shows the effect of switching to appointed election officials in more Republican-leaning jurisdictions. The bottom row shows the additive effect of switching to an appointed election official in Democratic-leaning counties.

Column 1 shows that voter turnout increases by 1.6 percentage points, on average, when a Republican-leaning county switches to an appointed election official. When the county is Democratic-leaning, the effect is 1.9 percentage points. The difference in effect sizes is statistically indistinguishable from zero. When comparing differences within counties of similar size, the estimated effect of appointing election officials on turnout in more Democratic jurisdictions is only 0.1 percentage points larger than the effect in Republican-leaning jurisdictions. Columns 3 and 4 show that the effect on increased registration rates are actually smaller in Democratic-leaning jurisdictions, although again the difference is not statistically distinguishable. In summary, both Democratic- and Republican-leaning counties see similar increases in voter turnout when switching to appointed election officials.

Table A.21: **Appointing Local Election Officials Has A Similar Effect In Democratic- and Republican-Leaning Counties (Even-Year General Elections, 1968-2020)**

	Voter Turnout			Registration Rate		
	(1)	(2)	(3)	(4)	(5)	(6)
Appointed	0.016 (0.004)	0.015 (0.004)	0.013 (0.004)	0.016 (0.005)	0.015 (0.005)	0.015 (0.005)
Appointed X Democratic County	0.003 (0.006)	0.003 (0.006)	0.001 (0.006)	-0.012 (0.007)	-0.012 (0.007)	-0.012 (0.008)
Counties	1861	1861	1861	1625	1625	1625
Elections	28	28	28	13	13	13
Observations	30964	30964	30964	12138	12138	12138
Outcome Mean	0.51	0.51	0.51	0.84	0.84	0.84
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year x State x Dem 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 x Dem FEs	No	No	Yes	No	No	Yes

Democratic counties rank in the top half in pre-treatment presidential Democratic vote share compared to other counties within the same state. Robust standard errors clustered by county in parentheses. The number of observations is smaller in columns 3-4 because Arizona and Georgia are excluded and because turnout data is available from 1968 but registration data is only available from 1996.

A.4 Mechanism Tests for Why Appointed Officials Increase Voter Participation

A.4.1 Appointed Election Officials Were More Likely To Apply For Private Grant Funding

Table A.22 tests whether appointed election officials were more likely to apply for the Center for Tech and Civic Life’s (CTCL) COVID-19 Response Grant program in September 2020.³² I obtain data on CTCL applications as well as population, income, metro, non-Hispanic white share, COVID death rate, social distancing share, and National Association of Counties membership covariate data from Lal and Thompson (2024) and follow Lal and Thompson’s specification strategy. I combine this data with 2020 election official selection methods across all counties ((Ferrer and Geyn 2023) and employ state fixed effects. In total, the data covers 37 states and over 2,600 counties.

A bivariate specification is shown in column 1, comparing the likelihood that appointed and elected counties within the same state applied for the CTCL grant. Counties with appointed election officials were 21 percentage points more likely to apply for the grant than counties that elect their election official. Column 2 controls for lagged Democratic presidential vote share to account for the fact that the grant was viewed with suspicion among some Republicans.³³ This attenuates the effect size to 16 percentage points. Column 3 adds controls for logged county population and logged county median income. Column 4 adds an indicator for urban and suburban counties and a control for the share of the county that is non-Hispanic white. Column 5 adds controls for COVID death rate and the share of respondents to the Nationscape survey who reported always complying with recommended social distancing in Fall of 2020. Column 6 adds an indicator for county membership in the National Association of Counties. The effect size is consistent at 7 percentage points across

³²<https://www.techandciviclelife.org/10-facts-about-ctcl-grants/>

³³<https://apnews.com/article/elections-facebook-mark-zuckerberg-d034c4c1f5a9fa3fb02aa9898493c708>

Table A.22: **Appointed Election Officials Were More Likely to Apply For CTCL Funding In 2020**

	Applied for CTCL Funding					
	(1)	(2)	(3)	(4)	(5)	(6)
Appointed	0.210 (0.031)	0.164 (0.034)	0.075 (0.034)	0.072 (0.032)	0.073 (0.032)	0.073 (0.032)
Lag dem vote share		0.532 (0.090)	0.331 (0.082)	0.450 (0.102)	0.432 (0.099)	0.431 (0.100)
Log(Population)			0.059 (0.007)	0.055 (0.007)	0.054 (0.007)	0.053 (0.007)
Log(Median income)			0.096 (0.038)	0.052 (0.043)	0.056 (0.042)	0.055 (0.042)
Metro				0.032 (0.019)	0.032 (0.019)	0.032 (0.019)
Non-Hisp white share				0.162 (0.117)	0.133 (0.123)	0.131 (0.124)
COVID death rate					-0.008 (0.021)	-0.008 (0.021)
Social distancing share					-0.023 (0.030)	-0.024 (0.030)
NACo						0.013 (0.021)
Observations	2644	2644	2644	2644	2589	2589
Outcome Mean	0.36	0.36	0.36	0.36	0.36	0.36
State FEs	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors clustered by state in parentheses. Center for Tech and Civil Life grant and covariate data is from Lal and Thompson (2024). Population is the voting-age population. Median Income is median household income measured with the 5-year ACS ending in 2019. Metro is an indicator for urban and suburban counties based on the Census nine-value urban–rural continuum. Non-Hisp White Share is the share of residents who are classified as non-Hispanic White in the 2020 census. COVID death rate is the number of deaths per 1,000 residents prior to September 1, 2020. Social Distancing Share is the share of Nationscape respondents in the county who report always complying with recommended social distancing in the early fall of 2020. NACo is an indicator for county membership in the National Association of Counties.

columns 3 through 6. This shows that appointed officials were more likely to take advantage of this alternative source of funding than their elected counterparts in similar counties.

A.4.2 Additional Expenditures on Election Administration May Boost Voter Turnout

Table A.23 presents the results of difference-in-difference regressions testing the effects of increased election expenditures on voter turnout. The first three specifications test the overall effects of an increase in expenditures on turnout and columns 4 through 6 test the additional effect of expenditures in small jurisdictions. The point estimates can be interpreted as the percentage change to voter turnout due to a doubling of election expenditures per registered voter. Column 1 shows that a doubling of election expenditures increases voter turnout by 0.4 percentage points. The effect appears concentrated in small counties.

Table A.23: **Additional Election Expenditures Increases Voter Turnout (Even-Year General Elections, 2004-2016)**

	Voter Turnout)					
	(1)	(2)	(3)	(4)	(5)	(6)
Ln expend per reg	0.004 (0.002)	0.004 (0.002)	0.002 (0.002)	-0.001 (0.002)	-0.003 (0.003)	-0.001 (0.003)
Ln expend per reg X Small County				0.006 (0.004)	0.008 (0.005)	0.005 (0.004)
Counties	434	434	434	432	432	432
Elections	6	6	6	6	6	6
Observations	1929	1929	1929	1920	1920	1920
Outcome Mean	0.50	0.50	0.50	0.50	0.50	0.50
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year x State FEs	Yes	No	No	No	No	No
Year x State x Dem vs FEs	No	Yes	No	No	No	No
Year x State x Pop FEs	No	No	Yes	No	No	Yes
Year x State x Small FEs	No	No	No	Yes	No	No
Year x State x Dem vs x Small FEs	No	No	No	No	Yes	No

Ln expend per reg is the natural log of total yearly election expenditures per registered voter. Small counties rank in the bottom half in population compared to other counties within the same state. Robust standard errors clustered by county in parentheses. Data is from Mohr et al. (2018) and is available for Arizona, California, Georgia, Minnesota, Missouri, Nebraska, and Nevada. Elections are the average number of elections included for each state, rounded down to the nearest interger. Expenditure data is normalized to 2020 dollars.

A.4.3 Using EAVS Data to Examine Administrative Outcomes

Following Ferrer, Geyn, and Thompson (2023), I use the U.S. Election Commission’s Elections And Voting Surveys to examine a number of election administration outcomes. I combine all past available surveys and extensively clean the data to correct for data irregularities and errors in the raw data (Stewart 2018). Table A.24 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, and registration removal rate. The point estimates for polling places, provisional rejection rates, and registration removal rates are all consistent with a positive effect on voter turnout, but the effect sizes are small and the coefficient for absentee rejection rate is in the opposite direction. In short, there is no strong indication that appointed officials site more polling places, run elections with fewer provisional ballots or fewer rejected absentee and provisional ballots, or remove more registrants from the voter roll.

Table A.24: **Comparison of Administrative Outcomes Between Appointed and Directly Elected Local Election Officials (Even-Year General, 2000-2022)**

	Polling Places (1)	Prov Share (2)	Prov Rejection (3)	Absentee Rejection (4)	Reg Removal (5)
Appointed	0.059 (0.065)	0.000 (0.001)	-0.024 (0.024)	0.004 (0.004)	-0.003 (0.002)
Counties	1037	1112	1013	1112	1112
Elections (avg)	6	7	7	9	9
Observations	7346	7736	6101	9809	9174
Outcome Mean	1.225	0.006	0.497	0.023	0.100
County FEs	Yes	Yes	Yes	Yes	Yes
Year x State	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.

A.4.4 Appointed Election Officials May Pursue More Constituent Communication

More active election official communication strategies has been shown to increase the share of registered voters (Merivaki and Suttman-Lea 2023), improve voter confidence (Suttman-Lea and Merivaki 2023), and reduce the number of mail ballots that are rejected (Suttman-Lea and Merivaki 2022). Figure A.25 uses data provided by Thessalia Merivaki and Mara Suttman-Lea to examine whether appointed local election officials are more likely to have official social media accounts than elected officials. Appointed officials serving jurisdictions in the same state and with similar populations as elected officials are more likely to have social media accounts, although the results are imprecisely estimated. Appointed officials are twice as likely to have Twitter/X social media accounts as elected officials serving similar jurisdictions.

Table A.25: **Appointed Local Election Officials May Be More Likely To Maintain Official Social Media Accounts**

	Has social media (1)	Has FB (2)	Has X (3)	Has Insta (4)	Has Tiktok (5)
Appointed	0.007 (0.022)	0.008 (0.028)	0.037 (0.017)	0.002 (0.014)	0.002 (0.010)
Counties	13	13	13	13	13
States	13	13	13	13	13
Observations	1115	1115	1115	1115	1115
Outcome Mean	0.336	0.296	0.072	0.030	0.006
County FEs	Yes	Yes	Yes	Yes	Yes
Year x State FEs	Yes	Yes	Yes	Yes	Yes

Robust standard errors clustered by county in parentheses. Election official social media data is provided by Thessalia Merivaki and Mara Suttman-Lea.

A.5 Mechanism Tests for Why Appointed Officials Produce Better Outcomes Than Elected Officials

A.5.1 Differences in the Experience, Age, and Professionalization of Appointed And Elected Local Election Officials

In the main text, I use the 2020 EVIC Survey of Local Election Officials to show that appointed clerks possess more formal education than elected clerks serving in similarly sized jurisdictions within the same state. Table A.26 shows the results of additional indicators of official quality. Column 1 tests whether appointed clerks possess greater previous experience in election administration than elected officials. I find that appointed officials actually possess 1.5 fewer years of election administration experience upon assuming their current position in the field. In column 2, I show that appointed officials are a member of marginally more professional election administration organizations than elected officials, but the difference is small and cannot be confidently distinguished from 0. Column 3 shows that appointed officials are slightly more likely than elected officials to have served as an election official in other jurisdictions. Among those who have served in elsewhere, appointed officials are much more likely to have served in multiple other jurisdictions (column 4). Appointed officials are half as likely as elected officials to be 65 years of age or older (column 5), and also make on average \$5,000 more a year than elected officials in the same state serving jurisdictions of a similar size. While this effect is statistically indistinguishable from zero, it represents an 8% salary premium. In column 6, I find that appointed officials hire an additional 0.6 FTEs, approximately 10% more than elected officials, although we cannot rule out that the finding arose by chance.

In sum, appointed officials are on average more educated and more professionalized than elected officials. However, they possess less election administration experience. This is

Table A.26: **Appointed and Elected Local Election Officials Possess Less Experience in Elections But Are More Professionalized**

	Previous Experience (1)	Professional Memberships (2)	Served Elsewhere (3)	Number Served (4)	Age >65+ (5)	Salary (6)	FTEs (7)
Appointed	-1.584 (0.745)	0.039 (0.085)	0.028 (0.043)	0.423 (0.233)	-0.152 (0.063)	5.077 (5.052)	0.565 (0.888)
States	44	44	44	28	44	44	44
Observations	587	699	664	97	584	556	669
Outcome Mean	7.40	1.17	0.15	1.71	0.16	59.74	5.87
State FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Log Pop	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Log Pop squared	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors clustered by state in parentheses. Data is from the 2020 EVIC 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 years of previous experience in election administration, calculated by subtracting current tenure length from total experience working in the field. Column 2 sums the number of professional 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 3 measures whether clerks have served as election officials in other jurisdictions and column 4 measures the total number of other jurisdictions served in. Column 5 is a binary for whether the election official is over the age of 65 or not. Column 6 measures salary in dollars, which is derived by taking the midpoint values of salary ranges. Column 7 measures full-time equivalents, which is derived by taking the midpoint values of FTE ranges.

potentially an artifact of higher turnover rates among appointed officials, which is examined in Section 6.2.3.

A.5.2 Voter Knowledge Survey Technical Appendix

I fielded the UCLA Representation Survey, a large-scale nationwide survey conducted between April 29 and May 5, 2024 using ResearchCloud Connect. I collected responses from 3,200 participants comprising a representative sample of Americans with over-samples of Blacks, Hispanics, and Asians. The survey received approval from the UCLA IRB Review Board prior to fielding. I employ post-stratification weights of sex, region, age, education, race/ethnicity, and the interaction of race and education using census data to ensure the sample is representative of the nationwide adult population. In addition to the knowledge questions analyzed in the paper, the survey included basic demographic and political questions and three experimental components related to voters' attitudes towards local election officials.

I collected nationwide cross-sectional data on the institutional position, selection method, and name of every chief local election official. (Ferrer and Geyn 2023; Ferrer and Thompson 2024; Ferrer, Thompson, and Orey 2024) I matched participants with their current election official based on the zip code they provide earlier in the survey. For zip codes that span multiple counties, the county with the majority of the zip code's area was chosen. While it is true that approximately 20% of zip codes cross county lines, in most cases the vast majority of the zip code lies in one county. I was unable to match respondents living in jurisdictions with municipal-administered election administration because zip code was the smallest geography provided by respondents. This excluded approximately 6% of the population.

A.5.3 Local Newspaper Analysis

Table 6 in the main analysis examined the differences in the effect of appointments on citizen participation based on whether jurisdictions continuously had a local newspaper between 1968 and 2020. Table A.27 allows counties to switch in and out of having a local newspaper. The results are in line with those found in Table 6.

Table A.27: Presence of a Daily Local Newspaper Attenuates the Effect of Appointing Local Election Officials on Citizen Participation (Even-Year General Elections, 1968-2020)

	Voter Turnout			Registration Rate		
	(1)	(2)	(3)	(4)	(5)	(6)
Appointed	0.026 (0.005)	0.024 (0.005)	0.020 (0.005)	0.011 (0.006)	0.011 (0.007)	0.006 (0.006)
Appointed X Newspaper	-0.021 (0.006)	-0.021 (0.007)	-0.014 (0.007)	0.000 (0.008)	0.001 (0.009)	0.007 (0.009)
Counties	1243	1243	1243	1011	1011	1011
Elections	14	14	14	7	7	7
Observations	15571	15571	15571	6577	6577	6577
Outcome Mean	0.57	0.57	0.57	0.85	0.85	0.85
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year x State x Newspaper FEs	Yes	No	No	Yes	No	No
Year x State x Dem vs x Newspaper FEs	No	Yes	No	No	Yes	No
Year x State x Pop x Newspaper 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 Arizona and Georgia are excluded and because turnout data is available from 1968 but registration data is only available from 1996.