

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 which has come under immense strain to deliver democratic elections and for which selection method is a live policy debate. Using an exhaustive 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 times 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

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.

⁷2020 Democracy Fund/Reed College Survey of Local Election Officials

Association of Election Officials.⁸ Finally, local election officials have the ability to influence participation rates given their far-ranging duties and discretion over administrative decisions (Burden et al. 2013; Ferrer, Geyn, and Thompson 2023).

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; and are largest in less populous jurisdictions. 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 is due to greater resource provision, 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 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.

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.

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

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; 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) and 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; Fourinaies and Hall 2022). 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 (Ferrer, Geyn, and Thompson 2023; Thompson 2020; Yntiso 2021). In the 2020 general election, 78% of all county-level races went uncontested.⁹ 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). 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 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 may be particularly acute, as there is poor data availability on the quality of elections and election officials have a portfolio of non-election responsibilities and unintuitive titles that further dilute the ability of voters to effectively monitor and sanction their performance.¹⁰ If public officials are acting

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

¹⁰Examples include probate judge in Alabama and Georgia; auditor in Iowa, South Dakota and Washington; and tax assessor in Texas.

rationality, we should expect them to shirk their duties in these circumstances, since their principals (the voters) lack the tools necessary to effectively monitor and sanction them. The information-poor environment voters face contrasts with the richer information environment that appointed officials' principals typically 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 may 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 prefer political outcomes achieved by reducing participation, then elected officials could be incentivized to concentrate costs on certain voters or discourage voting across the board. Appointments do not remove this possibility, but they may counterintuitively insulate officials from the pressures of responsiveness in ways that lead to socially desirable outcomes.

2.2 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.

In an audit study of constituent communication rates, elected and appointed election officials responded to correspondence at similar rates (White, Nathan, and Faller 2015). 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 structure 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,

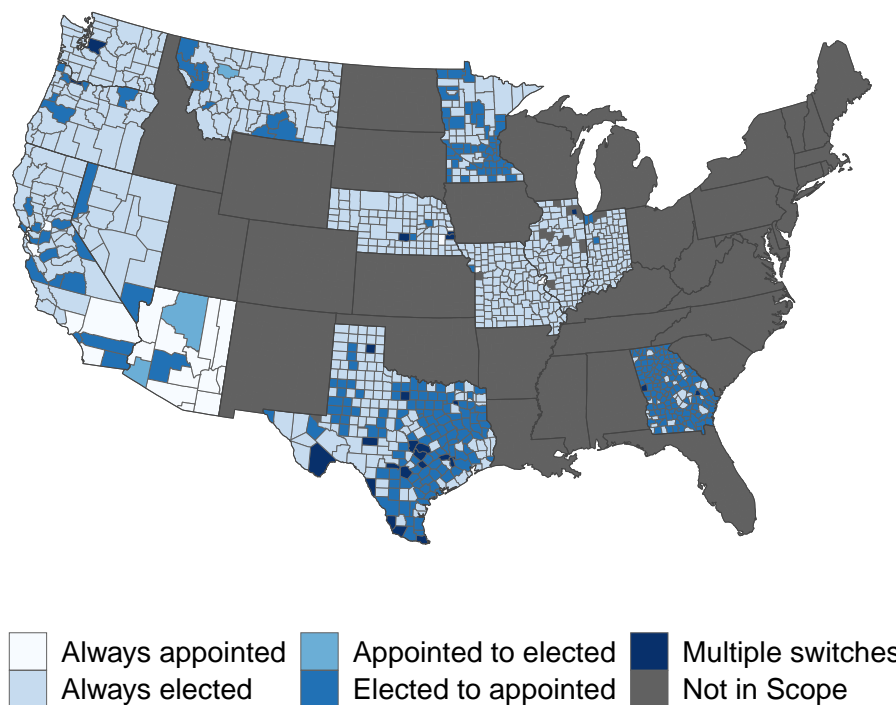
Nebraska, Nevada, Oregon, Texas, and Washington.¹¹ Figure A.1 in the online appendix shows the selection method of election officials in all county-administered jurisdictions. Table A.1 shows that counties in the dataset are similar to those that are excluded. Figure 1 shows which counties enter into 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 and irregular fashion over many decades, with switches accelerating in most states since 2000. Table A.1.3 in the online appendix details the specific election authority used for each state as 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.

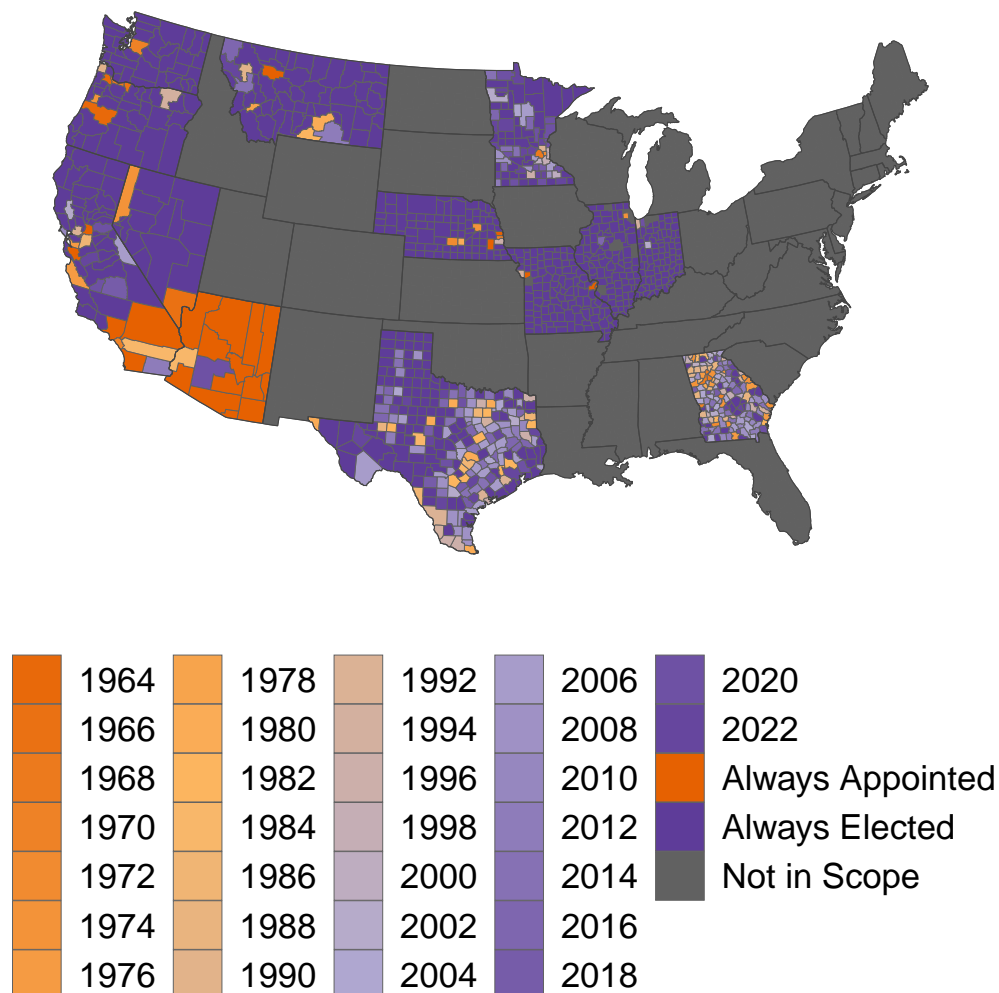
¹¹In states with multiple election authorities, I use the selection method for the authority with primary responsibility for administering elections on Election Day, as defined by Ferrer and Geyn (2023). I exclude five counties in Illinois and one in Missouri with nested municipal-level election administration.

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.



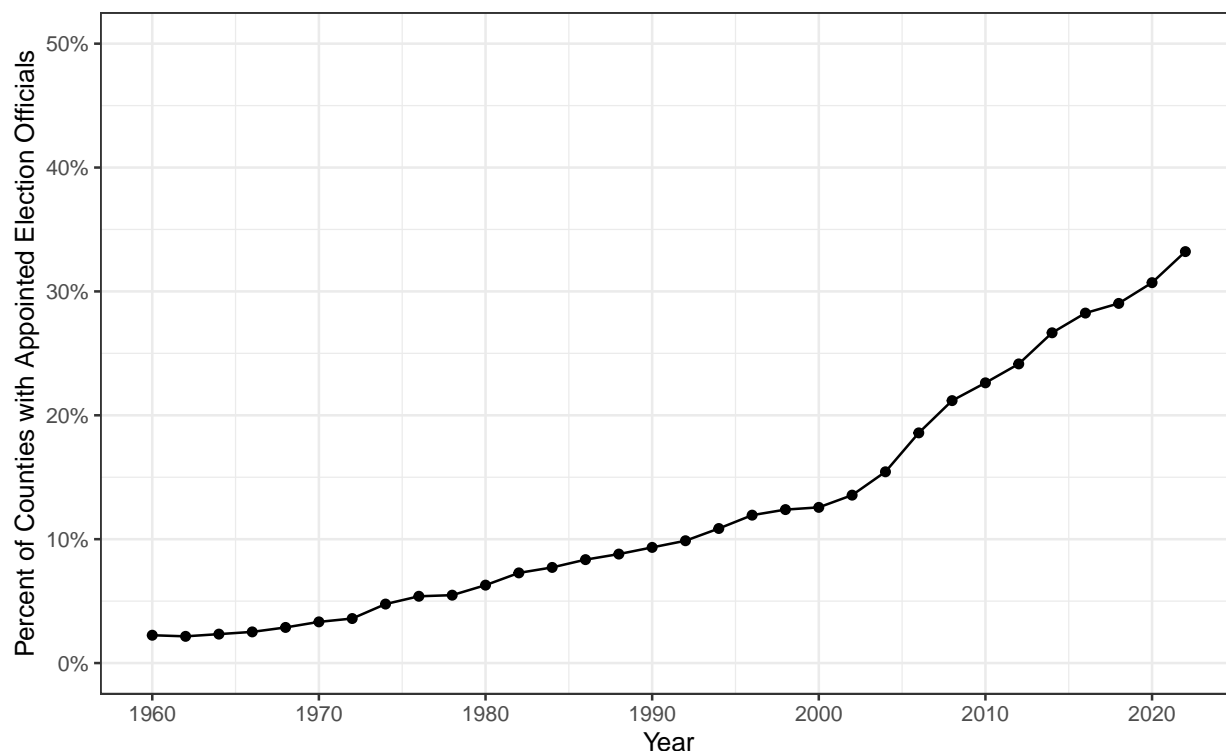
Some counties in California, Minnesota, Montana, and Washington hold binding referendums to initiate the reform, and several Midwestern states have population thresholds at which appointing their election official becomes possible or required. In most cases, the switch in selection method was not accompanied by any other substantive change to election policy or resource provision. For instance, in Georgia a state legislator that represented the affected county introduced 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 to administer elections. In Texas, the county commissioners enacted 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 did not even

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.



result in a change in leadership. I conduct a series of robustness tests isolating the effects of reform independent of other substantive policy changes.

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.



Reasons counties state for making the switch include difficulty finding qualified candidates for office,¹² a desire to professionalize the job, increasing efficiency and streamlining services,¹³ creating a dedicated position for election administration,¹⁴ or simply following in the footsteps of other counties in the state.¹⁵ Who receives appointing authority also varies, as well as whether they appoint an individual or a board (Ferrer and Geyn 2023).

Local election officials in all of these states are entrusted with broad statutory authority to conduct elections (Ferrer, Geyn, and Thompson 2023). For instance, probate judges in

¹²<https://www.fairmontsentinel.com/news/local-news/2023/07/19/faribault-county-looks-to-appoint-auditor-treasurer/>

¹³<https://maplelakemessenger.com/2020/12/wright-county-considers-changing-auditor-treasurer-from-elected-to-appointed/>

¹⁴<https://www.timesrecordnews.com/story/news/local/2023/06/05/wichita-county-to-hire-election-administrator/70289429007/>

¹⁵https://www.unionrecorder.com/news/commissioners-discuss-possibly-creating-a-board-of-elections/article_43508cfc-6718-11ee-a035-13c8d8908b19.html

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 municipal and state legislative databases, home rule charters, newspaper archives, web scraped internet archives, Blue Book directories, Open Records Requests, and email and phone 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 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 (Ferrer, Geyn, and Thompson 2023). 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 Democracy Fund/Reed College Survey of Local Election Officials, over 67% of local election officials agree that encouraging voter turnout is part of their job, compared with fewer than 10% who disagree. This is reflected in the National Association of Election Officials, which

lists increasing participation as one of main considerations for election officials.¹⁶ Third, participation rates are widely viewed as a key measure of election quality and, more broadly, an important indicator of the health of a nation’s democracy. MIT’s Election Performance Index uses both voter turnout and voter registration in comparing election administration performance across states,¹⁷ and Varieties of Democracy (V-Dem) uses electoral participation as one of its key indicators of democratic health.¹⁸ Voter turnout can be considered the ultimate effect of poor 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 both turnout and registration rates exist at the county level and, particularly in the case of voter turnout, are available going back many decades. This is not true of any other indicator of election quality, including voter confidence, voter wait times, polling place relocations, and constituent communication.

Data on county-level vote totals is from Congressional Quarterly and David Leip’s U.S. Election Atlas. It spans from 1968 to 2022.¹⁹ I use data on registration totals from Leip’s Election Atlas. This covers presidential elections from 1996 and gubernatorial elections from 2004. I measure voting age population, the denominator in turnout and registration measurements, using estimates from the National Cancer Institute’s Surveillance, Epidemiology, and End Results Program, which is available from 1970.²⁰ 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 2020. This survey measures outcomes in every even-year general election for each

¹⁶<https://www.electioncenter.org/about-us.php>

¹⁷<https://elections.mit.edu/#/data/map>

¹⁸<https://www.v-dem.net/static/website/img/refs/codebookv12.pdf>

¹⁹I exclude Loving county, Texas from the analysis because its population is too small to reliably estimate participation rates.

²⁰This 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/>. I extrapolate the estimate to 1968 and to 2022.

county. I use this survey to measure the number of polling places per 1,000 people, provisional ballots cast, provisional ballots rejected, absentee ballots rejected, and the number of registrants removed from the voter roll. Following Ferrer, Geyn, and Thompson (2023) and Pettigrew (2017), I use data from the Cooperative Congressional Election Study to measure the share of voters who had to wait at the polls for more than 30 minutes. This is available for general elections in 2006, 2008, and 2012–2018. I also use election official communication data, generously 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 Democracy Fund/Reed College Survey of Local Election Officials,²¹ 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 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

²¹<https://evic.reed.edu/leo-survey-summary/>

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 $Y_{it} = \alpha_i + \delta_t + \beta \textit{Appointed}_{it} + \epsilon_{it}$, 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 $\textit{Appointed}_{it}$ 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 more rapid rates 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 Year by Democratic vote share fixed effect compares within-county over time change to other counties with similar partisan makeup, whereas the Year by Population fixed effect compares within-county overtime change to other counties with similar populations. These account for the possibility that counties that switch their election administration may 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 from state to state, and measured pretreatment.²²

Even with these interacted fixed effects, it is still possible there are unobserved reasons why counties that switch to appointed clerks are on a different turnout trajectory than those that maintain elected offices. I address these concerns by conducting a generalized synthetic control matching 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 rather than electing clerks results in increased turnout and registration rates. I then validate these findings using a range of alternative estimators, conduct a placebo analysis using registration rates, distinguish between the effects of selection method and partisanship, and examine where the effect is largest.

²²I measure Democratic vote share as votes for the top-ticket Democratic candidate divided by votes for the top-ticket Democratic and Republican candidates.

4.1 Appointing Election Officials Increases Voter Participation

Table A.4 displays the results of a two-way fixed effects regression estimating the effects of appointing rather than electing a local election official on citizen participation. Columns 1 through 3 estimate the effects on votes per voting-age resident 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 county and year by state fixed effects. This means that comparisons are made on within-county changes in participation, relative to changes in other counties in the same state and year. This ensures that differential participation trends between states are not driving the results. I cluster robust standard errors by county.

Table 1: Appointing Local Election Officials Increases Citizen Participation (Presidential and Midterm Elections, 1968-2022)

	Voter Turnout			Registration Rate		
	(1)	(2)	(3)	(4)	(5)	(6)
Appointed	0.018 (0.003)	0.018 (0.003)	0.014 (0.003)	0.009 (0.004)	0.010 (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 presidential voter turnout of 1.8 percentage points, compared with counties that do not switch. The point estimate is precisely estimated, allowing us

to confidently rule out effects of less than 1.2 percentage points. It is also substantively meaningful. The effect size for turnout in presidential 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).

It could be the case that counties with similar partisan compositions were on the same participation trajectory prior to their shift in selection method. I introduce year by state by Democratic vote share fixed effects to alleviate this concern. In column 2, differences in within-county turnout shifts are only made between counties in the same state, year, and partisan makeup. The result is similar under this estimation strategy. The inclusion of year by state by population fixed effects in column 3 makes comparisons between counties of similar sizes within the same state, and yields similar results.

Appointed election administrators also 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.

These estimations provide strong evidence that appointed clerks increase voter participation in presidential elections, 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). One concern is that the effect only holds for certain elected offices. Table A.7 in the online appendix shows that switching from elected probate judges, auditors, and clerks to appointments increases voter turnout. An additional concern is that the boost to turnout is an artifact of the way the reform in selection method was initiated and therefore not exogenous. 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. I also run a series of Callaway and Sant’Anna (2021) regressions to estimate the dynamic effects of switching from an elected to an appointed election official. These results, found in Section A.2.5 of the online appendix, show that the positive effects of appointments on voter turnout appear over time and across multiple county cohorts and time periods.²³ 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 when only examining more recent elections, with some attenuation of the effect size.

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

²³This 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.

estimators and employ a generalized synthetic control method which relaxes the assumptions needed for causal inference. These estimators show the results to be robust to a range of specifications.

4.2.1 Validating the Staggered Rollout Design

Recent scholarship has identified potential problems with the standard 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. This is because units that switch early on from control to treatment are treated as controls in some comparisons and subtracted from the difference-in-difference estimator, even if they continue to experience dynamic treatment effects.

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. I also 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 and are found in Section A.3.2 in the online appendix.

4.2.2 Generalized Synthetic Control

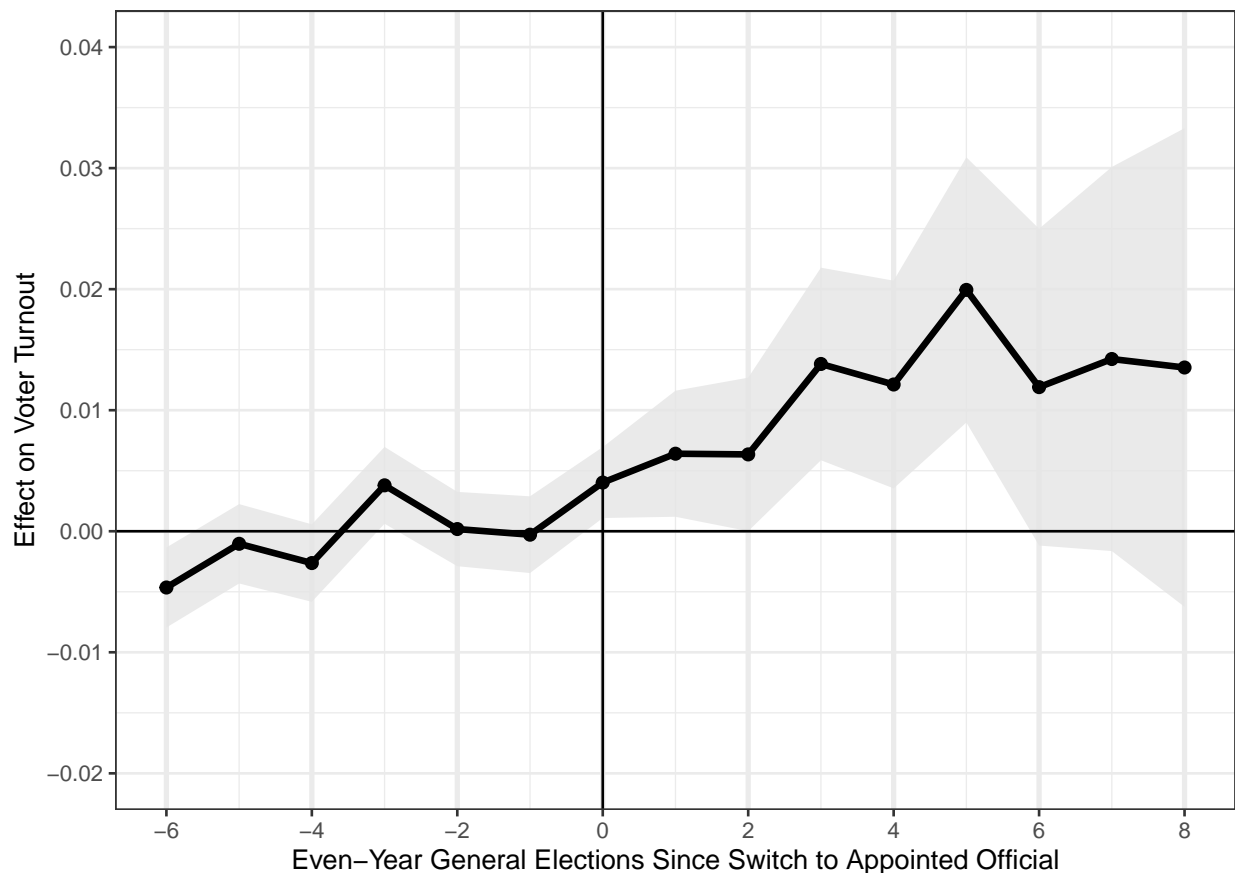
An underlying concern of the difference-in-difference estimation strategy is that treated and control units do not look like one another. If the places that switch from electing to appoint-

ing election officials are fundamentally different 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 difference-in-differences event studies estimator and reveals modest evidence of pre-trending. One way to overcome this concern and relax the parallel trends assumption is through the generalized synthetic control method. This estimation re-balances the data sample by matching treated and untreated units to ensure that treated units look like control units pre-treatment. Figure 4 displays output from the Xu (2017) generalized synthetic control estimation. The line in the left-hand side of the figure is close to 0, showing that the matching strategy was successful. 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 main estimates found in Table A.4 but still significant for participation in federal general elections.

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.15 in the online appendix, both which counties are treated and when they are treated are randomly permuted, and in Figure 5, when treated counties receive treatment is randomly permuted (treatment meaning a switch to appointed clerks). In each case, counties that switch from appointed to elected and counties that switch selection method multiple times are excluded. 1,000 permutations are computed for each exercise. The same three regressions showed in Table 1 columns 1-3 are replicated with the permuted data, and the coefficient stored. Finally, the actual coefficient derived is compared with the 1,000 coefficients found through random permutation; the p-value is the number

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.

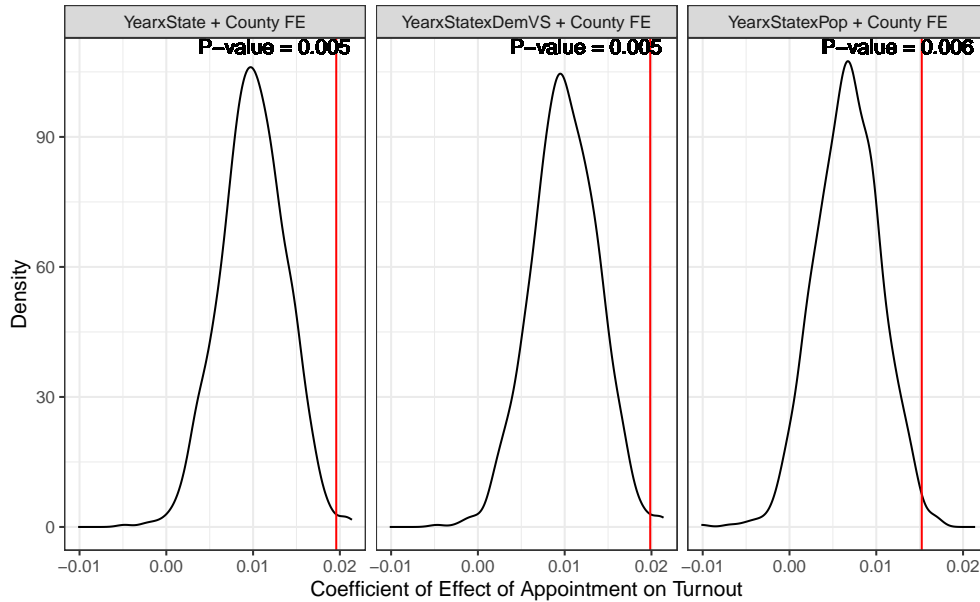


of randomized coefficients that are greater than or equal to the actual effect size divided by the total number of iterations.

Figure A.15 shows that the likelihood of getting the observed result, given randomized treatment and treatment timing and assuming the true effect is null, is close to 0. 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 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 result 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 shows 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 1.5 and 2.2 percentage points higher than their elected counterparts, compared with 0.8 to 0.9 percentage points higher in more populous jurisdictions. A similar pattern is found with registration rates, with effect sizes in smaller counties roughly 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)	0.008 (0.004)	0.009 (0.003)	0.005 (0.004)	0.004 (0.004)	0.006 (0.005)
Appointed X Small County	0.013 (0.007)	0.017 (0.007)	0.013 (0.007)	0.006 (0.008)	0.009 (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 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

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 4-6 because Arizona and Georgia are excluded and because turnout data is available from 1968 but registration data is only available from 1996.

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 examine whether appointed officials

obtain additional resources, pursue different election administration policies, are more likely to communicate with voters, and oversee elections with lower voter wait times. I find substantial evidence that appointed officials increase the amount of resources devoted to election administration. I also find modest evidence that election officials more actively communicate with voters and reduce voter wait times in elections.

5.1 Appointed Election Officials Obtain More Resources

One explanation for the observed effect is that switching to appointed officials boosts election administration resources, and that this leads to increased turnout. Sufficiently funding elections is essential to ensuring high quality administration (Mohr et al. 2019, 2020; Kropf et al. 2020; McGowan et al. 2021). 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. Additionally, if the quality of selection is higher for appointed officials, then they might be better able to secure additional resources. Finally, it is possible that in smaller jurisdictions switching to a dedicated appointed local election official increases the amount of full-time equivalent (FTE) employees who work in election administration. Appointed officials' sole job is to effectively administer elections. In comparison, most directly elected local election officials in the 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 Democracy Fund/Reed College Survey of Local Election Officials, 34 percent of jurisdictions have no full-time election administrators and 17 percent have exactly one FTE.²⁴ In all other jurisdictions, switching to an appointed official should also not directly increase the amount of FTEs.

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)	0.272 (0.101)	0.310 (0.098)	0.323 (0.110)	0.304 (0.117)	0.357 (0.109)
Appointed X Small County				-0.100 (0.224)	-0.135 (0.227)	-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 FEs	Yes	No	No	Yes	No	No
Year x Dem vs FEs	No	Yes	No	No	Yes	No
Year x Pop FEs	No	No	Yes	No	No	Yes

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 integer. Expenditure data is normalized to 2020 dollars.

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, although there is significant missingness and high within-county variance. This enables the use of a difference-in-differences regression design to credibly estimate the effect of switching to appointed election officials on the natural log of total election expenditures. Following Taylor,

²⁴<https://evic.reed.edu/leo-survey-summary/>

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. 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. The 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 that they are likely driven by the actions of the local election official rather than a result of creating an additional FTE election administration position. Finally, I show in Table A.20 in the online appendix that increased election expenditures may lead to additional voter turnout, especially in smaller jurisdictions. A doubling of expenditures per registrant increases voter turnout overall by 0.4 percentage points and increases turnout in less populous jurisdictions by 0.7 percentage points.

5.2 Appointed Election Officials Pursue Similar Administrative Policies

I use EAVS data to explore whether appointed election officials pursue different election administration policies. The results, found in Section A.4.2 of the online appendix, do not allow me to rule out the possibility that appointed and elected administrators run elections with similar numbers of polling places per 1,000 residents, provisional ballot usage, provisional rejection rates, absentee ballot rejection rates, and registration removal rates. The point estimate is positive for polling places and negative for provisional rejection rates and

registration removals, all suggestive of policy decisions that would increase voter turnout, but the effect sizes are small and statistically indistinguishable from zero.

5.3 Appointed Election Officials May Be More Likely To Communicate With Voters

One way that appointed and elected officials may differ that could affect participation rates is their communication strategies. Clerks have significant discretion in their communication with voters. Election officials 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 (Table SI.13 in their Supplemental Information). Figure A.22 in the online appendix uses data from Thessalia 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 observe significant differences in other measures of social media presence.

5.4 Appointed Election Officials Oversee Elections With Lower Wait Times

I use CES to examine whether voter wait times decrease when counties switch to appointed administration. I employ difference-in-difference regressions with county and year fixed ef-

fects, as well as 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. Switching to appointed officials reduces the percentage of voters waiting in line for 30 minutes or more by 2 percentage points, an effect size of 0.16. There is no evidence to suggest that appointments affect the percentage of voters who have any wait time or a wait time in excess of 1 hour. Longer wait times have been found to depress future voter turnout, making this one plausible factor explaining the main effect (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 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 estimations 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. This problem is common across local offices, with other research finding low rates of contested elections for sheriffs and district attorneys (Thompson 2020; Yntiso 2021).

I use the 2020 EVIC Survey of Local Election Officials, a nationwide poll of 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 (5)
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 X Small 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 Democracy Fund/Reed College Survey of Local Election Officials and is filtered to only include chief local election officials. County is imputed from zip code to calculate population controls. Observations are weighted to be representative of the population of local election officials. 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 we can confidently distinguish 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.23 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, and make approximately 10% more in salary than elected officials. 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 in 2024 to test respondent’s knowledge of their local election official. The survey hypotheses and analysis was preregistered on OSF.²⁵ 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

²⁵osf.io/k7hq2

appointed. And only 7.9% of respondents correctly identified their election official out of a list of five names.

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 poor 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 more 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 shows 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)	0.027 (0.006)	0.021 (0.005)	0.011 (0.006)	0.012 (0.007)	0.007 (0.007)
Appointed X Newspaper	-0.021 (0.007)	-0.026 (0.008)	-0.013 (0.007)	0.001 (0.009)	-0.003 (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	NA	NA	NA	NA	NA	NA
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 all chief local election officials across all jurisdictions

that administer federal elections at the county level, collected mainly from state and local administrative archives (Ferrer and Thompson 2024). Table 7 shows the results. Columns 1 and 2 include only year by state fixed effects and should be interpreted as the correlation between selection method and turnover. Appointed election officials are 17 percentage points more likely to leave over a four year period than elected officials in the same state.

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

	Election Official Turnover			
	(1)	(2)	(3)	(4)
Appointed	0.167 (0.016)	0.080 (0.010)	0.274 (0.036)	0.049 (0.025)
Counties	2831	2831	2831	2831
Elections	10	10	10	10
Observations	28631	28631	28631	28631
Outcome Mean	0.32	0.17	0.32	0.17
Turnover yrs def	4	2	4	2
County FEs	No	No	Yes	Yes
Year x State FEs	Yes	Yes	Yes	Yes

Robust standard errors clustered by county in parentheses.

Columns 3 and 4 include by county and year by state fixed effects. The interpretation is therefore 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. Again, it appears that appointed officials leave the position at much higher rates than elected officials. Switching to an appointed official increases the probability of turnover over a 4-year period by 27.4 percentage points. Considering the average 4-year turnover rate of election officials in the dataset is 32%, this is a very 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.²⁶ 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 and hold across multiple time periods, states, offices, and reform mechanisms. Appointed officials appear to boost local expenditures on election administration, increase communication with voters, 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

²⁶<https://www.politico.com/news/2020/11/17/wayne-county-michigan-election-certification-437181>

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). 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 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.²⁷ However, my results suggest that over a long period of time and across many

²⁷<https://www.washingtonpost.com/nation/2022/03/14/georgia-elections-fraud-purge/>

states, appointed election officials have produced better outcomes for their constituents than elected officials.

Future work should consider other instances where elections may 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 shapes the selection method trade-off. We also need better measures of accountability outcomes for public officials (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 ongoing 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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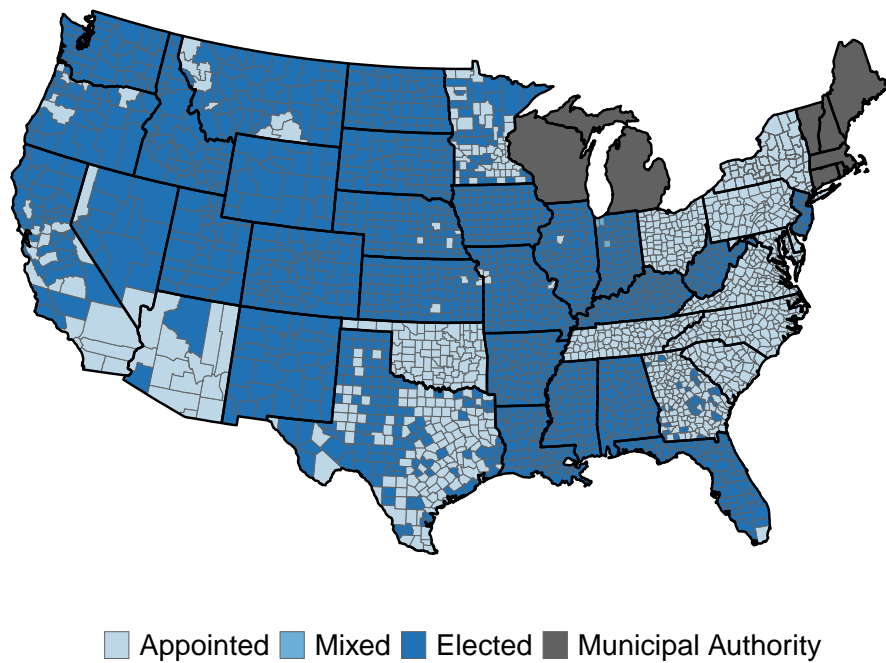
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A.1 Descriptive Appendices

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).



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. Population, racial/ethnic demographics, and region are from the 2020 census. Democratic presidential vote share, voter turnout, and voter registration are 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 slightly less likely to appoint their clerks.

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. Together these account for less than 10 percent of the US population.

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	Elected to Appointed	Appointed to Elected	Multiple Switches	Year First Switch	Year Last Switch
Arizona	Election Administrator	Recorder	15	12	0	1	2	0	1997	2020
California	Clerk	Registrar of Voters / Clerk / Elections Commission	58	6	38	14	0	0	1970	2022
Georgia	Probate Judge	Board of Elections and Registration	159	0	28	129	0	2	1968	2022
Illinois	Clerk	Election Commission	102	0	93	0	0	1	1974	2016
Indiana	Clerk	Board of Election and Registration	92	0	89	3	0	0	1994	2020
Minnesota	Auditor	Auditor	87	0	39	48	0	0	1968	2022
Missouri	Clerk	Board of Election Commissioners / Director of Elections	115	3	110	1	0	0	1993	1994
Montana	Clerk and Recorder	Election Administrator / Clerk and Recorder	56	0	47	8	1	0	1977	2022
Nebraska	Clerk	Election Commissioner	93	2	86	2	0	3	1969	1996
Nevada	Clerk	Registrar of Voters	17	0	15	2	0	0	1966	1974
Oregon	Clerk	Elections Manager/Director	36	0	29	6	0	1	1964	1994
Texas	Clerk / District Clerk / Tax Assessor	Elections Administrator	254	0	118	119	0	17	1980	2022
Washington	Auditor	Elections Director	39	0	38	0	0	1	1969	2009

Only primary local election authorities are listed under elected and appointed entities—those responsible for the majority of election duties in each county, especially voter administration on Election Day. 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 (see Ferrer and Geyn (2023) for a more in-depth comparison). 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. Importantly, there are few differences in participation between counties that appoint and those that elect their clerk. Taken at face value, 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 1 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)	0.020 (0.004)	0.016 (0.004)	0.010 (0.004)	0.011 (0.005)	0.008 (0.005)
Counties	1116	1116	1116	942	942	942
Elections	14	14	14	6	6	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 Public Information Act documents provided by the Texas Secretary of State over archival Secretary of State data web scrapped from the WayBackMachine 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)	0.018 (0.003)	0.014 (0.003)	0.009 (0.004)	0.010 (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 state with at least 10 counties that have switched clerk selection methods since 1960. Those states are California, Georgia, Minnesota, and Texas. The results reveal statistically significant, precisely estimated, and substantively meaningful effects for Georgia, Minnesota, and Texas. The effect size is much larger in Georgia and Minnesota than in Texas.

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.²⁸ A few Texas counties use elected tax assessors as their election official. Auditor is also a fairly common position for elected election officials. All elected election officials in Minnesota and Washington are auditors, as well as a small number of counties in California. Finally, in Georgia elected election officials are probate judges. Table A.7 shows that participation increases when appointed officials (the omitted category) replace elected auditors, clerks, and probate judges. The increase is largest when probate judges are replaced, and smallest when clerks are replaced. The point estimate for tax assessors is negative but is imprecisely estimated and 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.

²⁸District & 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 to clerk selection methods 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 spurred more voter participation in future elections due to increases in political efficacy among the populace. 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 is slightly positive for county referendums 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 the implementation or amendments to 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)	0.018 (0.003)	0.014 (0.003)	0.010 (0.004)	0.011 (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

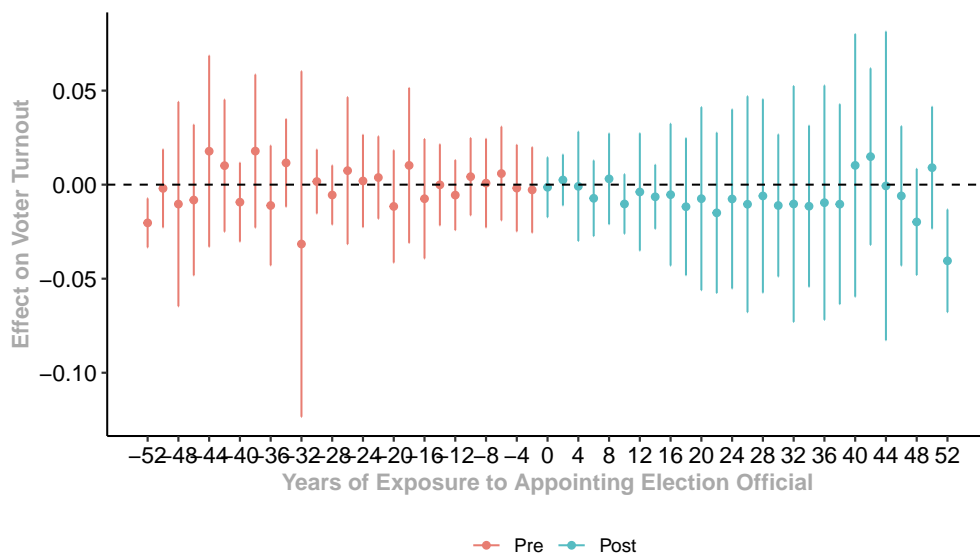
I use specifications from the Callaway and Sant’Anna (2021) estimator to examine dynamic, cohort, and time period effects of switching from elected to appointed clerks on presidential voter turnout. Because state-year fixed effects cannot be incorporated, I run separate estimations for each of the four 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 A.14 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. Figures A.2, A.3, A.4, and A.5 provide additional evidence for the dynamic effects of appointments on voter participation. There are two potential explanations for this: appointed officials increasingly outperform elected officials as their tenure increases, 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. 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.²⁹

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

²⁹<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.



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 adopters is due to long-term accumulation rather than a diminishing instantaneous effect over time. In fact, in more recent decades the positive effects of appointed election officials on turnout has, if anything, increased. This could be interpreted as evidence of long-run dynamic gains in having appointed rather than elected officials run elections, as well as the idea that the gap in turnout produced by appointed and elected officials has grown.

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.

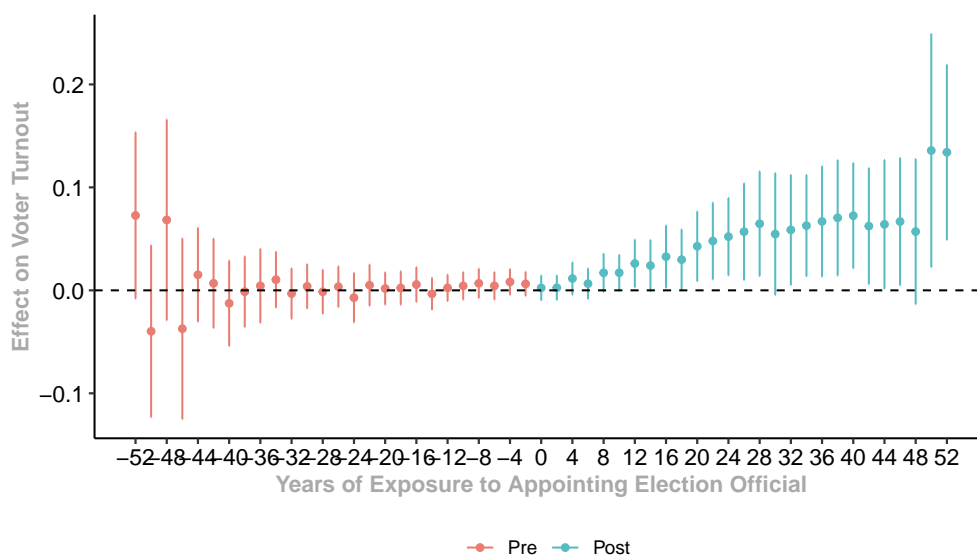


Figure A.4: **Average Effect of Appointed Election Officials on Voter Turnout by Length of Exposure to Appointing - Minnesota.** Year 0 is the even-year general election after a county's first switch from electing to appointing an election official. Each point is the estimated effect of appointing an 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.

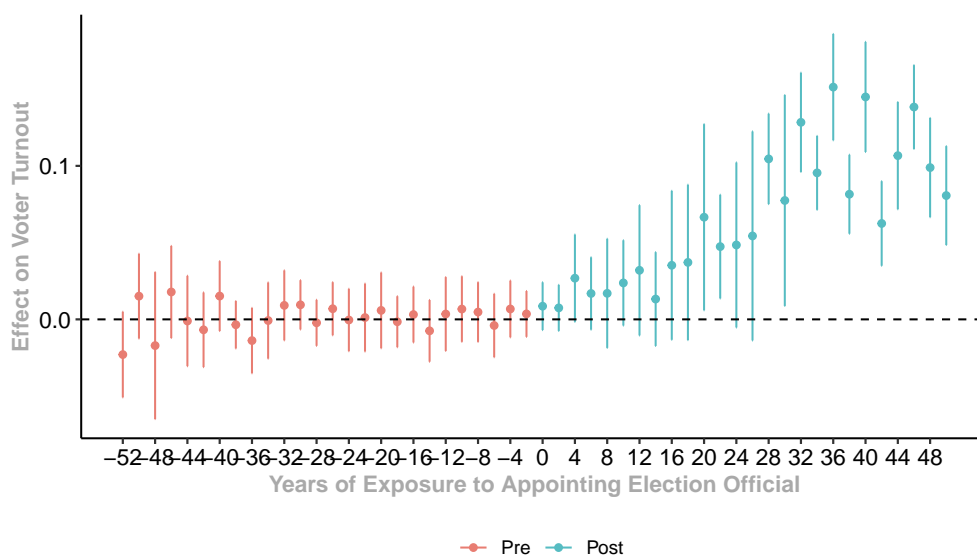


Figure A.5: **Average Effect of Appointed Election Officials on Voter Turnout by Length of Exposure to Appointing - Texas.** Year 0 is the even-year general election after a county's first switch from electing to appointing an election official. Each point is the estimated effect of appointing an election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

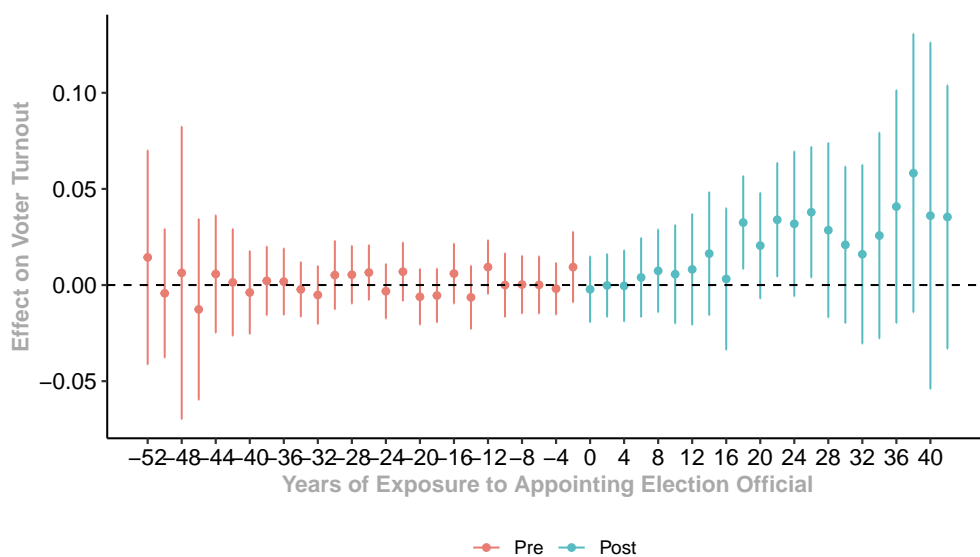


Figure A.6: **Average Effect of Appointed Election Officials on Voter Turnout by Cohort Group - California.** Each point is an estimate of the average group effect of appointing election officials on voter turnout for counties that switch in the given cohort year. The lines above and below each point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

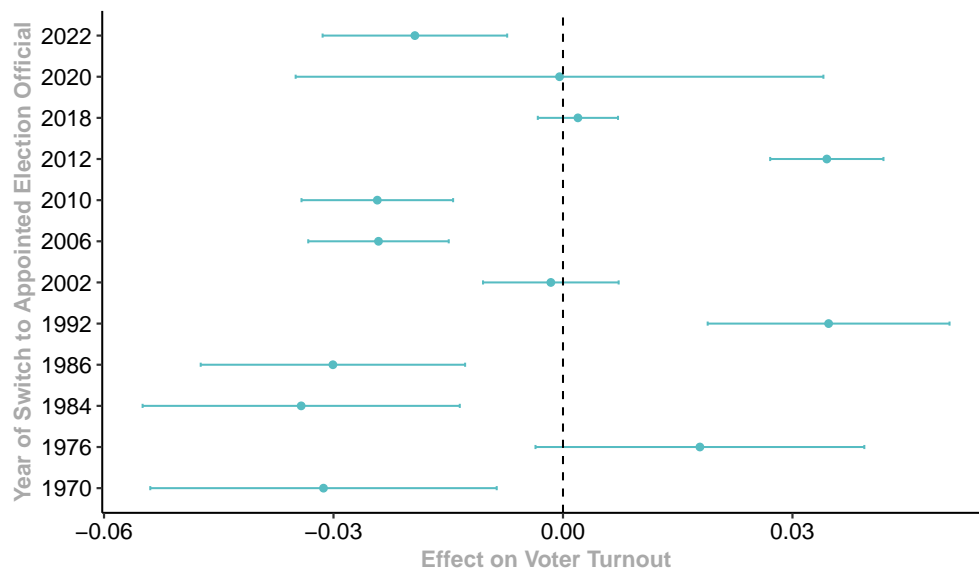


Figure A.7: **Average Effect of Appointed Election Officials on Voter Turnout by Cohort Group - Georgia.** Each point is an estimate of the average group effect of appointing election officials on voter turnout for counties that switch in the given cohort year. The lines above and below each point represent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

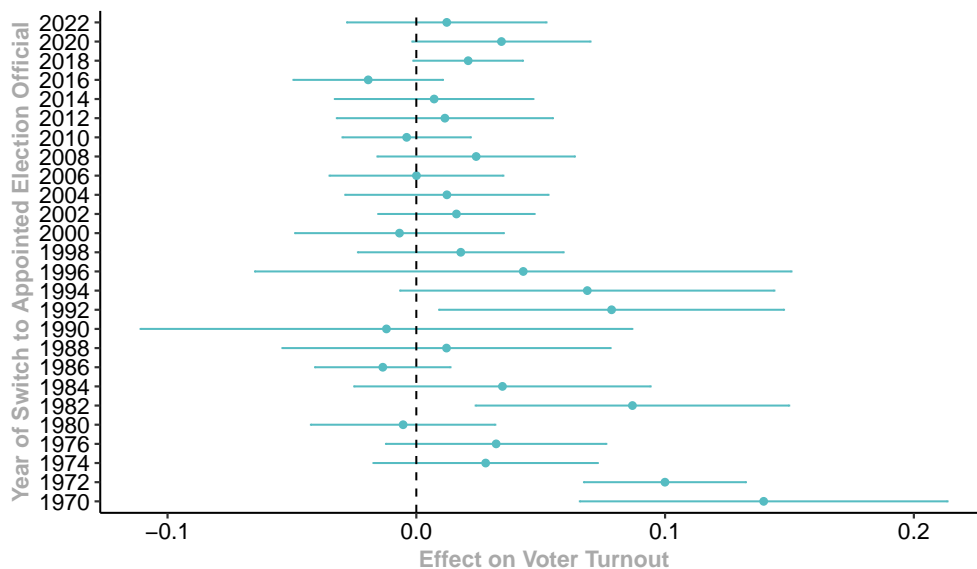


Figure A.8: **Average Effect of Appointed Election Officials on Voter Turnout by Cohort Group - Minnesota.** Each point is an estimate of the average group effect of appointing election officials on voter turnout for counties that switch in the given cohort year. The lines above and below each point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

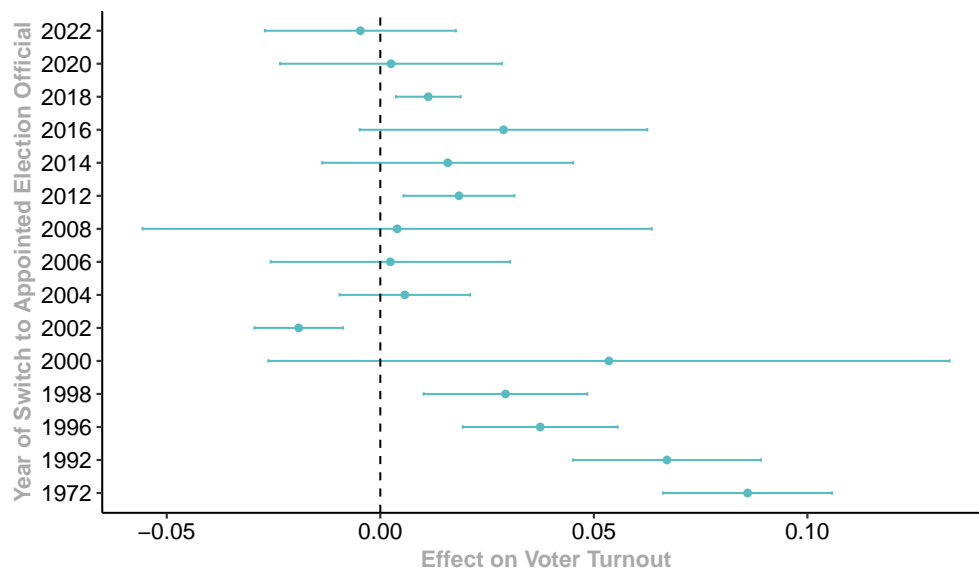


Figure A.9: **Average Effect of Appointed Election Officials on Voter Turnout by Cohort Group - Texas.** Each point is an estimate of the average group effect of appointing election officials on voter turnout for counties that switch in the given cohort year. The lines above and below each point represent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

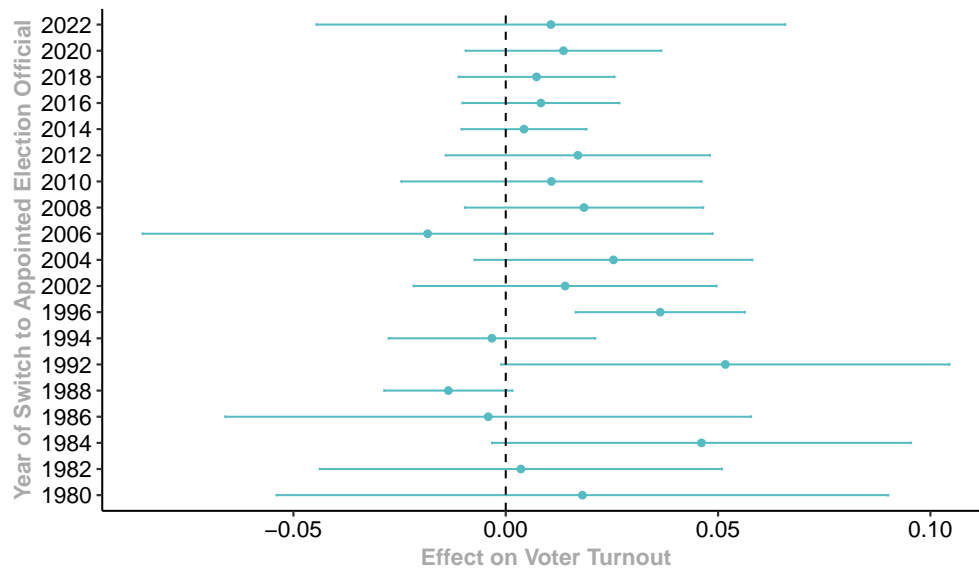


Figure A.10: **Average Effect of Appointed Election Officials on Voter Turnout by Time Period - California.** Each point is an estimate of the average time period effect of appointing election officials on voter turnout. The lines above and below each point represent 95-percent confidence intervals.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

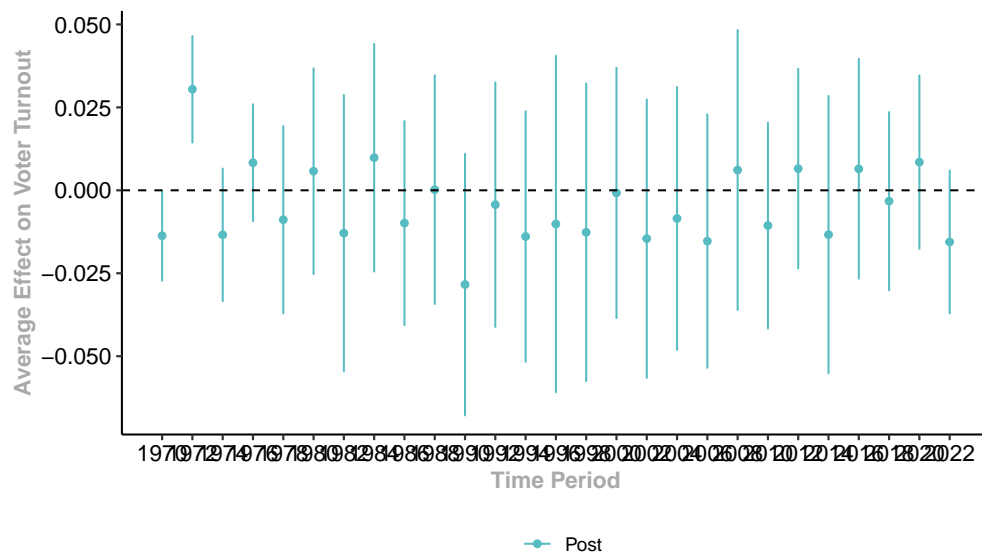


Figure A.11: **Average Effect of Appointed Election Officials on Voter Turnout by Time Period - Georgia.** Each point is an estimate of the average time period effect of appointing election officials on voter turnout. The lines above and below each point represent 95-percent confidence intervals. Estimates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

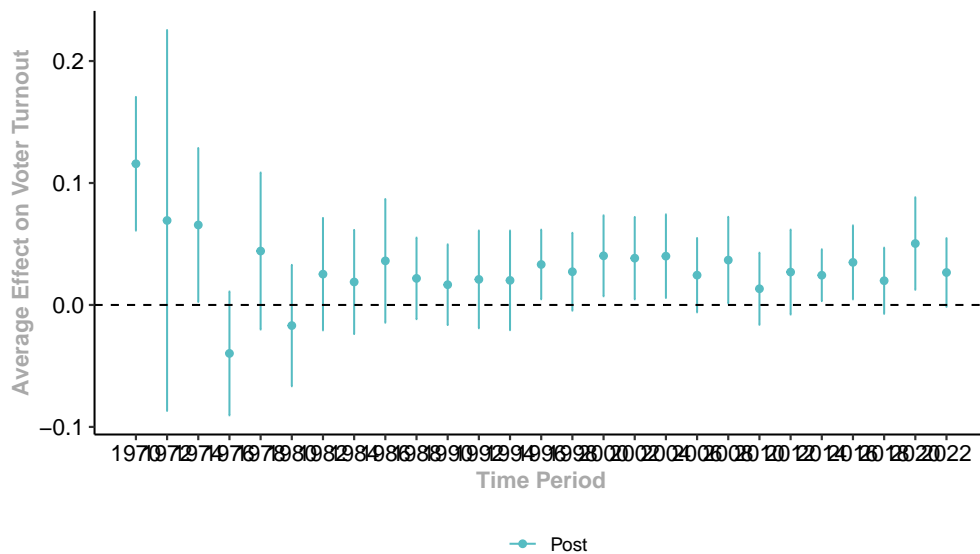


Figure A.12: **Average Effect of Appointed Election Officials on Voter Turnout by Time Period - Minnesota.** Each point is an estimate of the average time period effect of appointing election officials on voter turnout. The lines above and below each point represent 95-percent confidence intervals. Estimates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

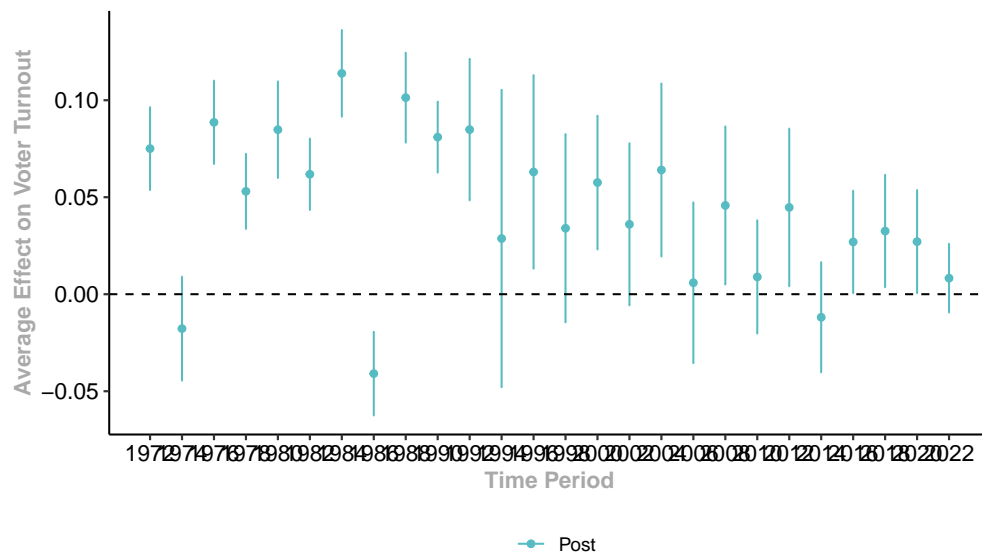
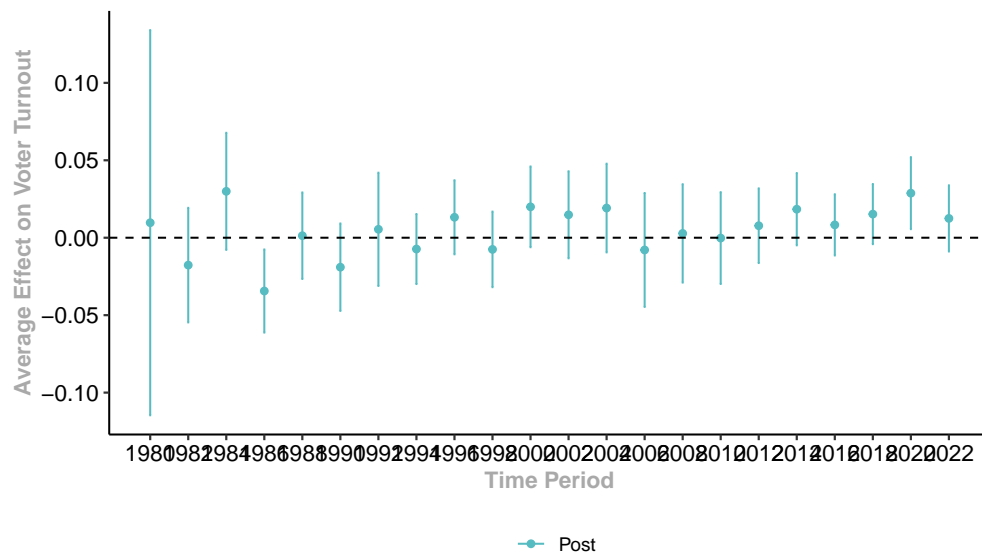


Figure A.13: **Average Effect of Appointed Election Officials on Voter Turnout by Time Period - Texas.** Each point is an estimate of the average time period effect of appointing election officials on voter turnout. The lines above and below each point represent 95-percent confidence intervals. Estimates 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 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 A.4 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 results show some evidence of diminishing effect sizes when only more recent data is considered. However, in all estimations the point estimates are substantively large and statistically distinguishable from zero. In the most restrictive analysis, considering data from 2000 onwards, counties that switch to appointed clerks are estimated to boost turnout by 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 methodological 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 A.4. 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 estimations (Cengiz et al. 2019). This is one technique that has been developed to eliminate biases in the two-way fixed effects estimator. 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 it allows me to match both on pre-treatment voter turnout trajectory and exact match on state or state and pre-treatment population or democratic vote share. I only include counties that start and remain elected throughout the dataset and those that switch from elected 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 result of this exercise.

The procedure produces only 147 matches, leaving the estimations somewhat imprecise. However, the point of a 1.5 percentage point effect is similar to the results 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’Haultfoeulle (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’Haultfoeulle (2020) estimator employs dynamic effects with placebos. The Callaway and Sant’Anna (2021) estimator employs dynamic effects after aggregating counties into cohorts that begin treatment at the same time. This estimator is very similar to the 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 weighting problems of the simple two-way fixed effects estimator.

These tables further validate 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 cannot be statistically distinguished from a null effect. In summary, these results validate the main finding that appointed local election officials increase participation.

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
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
Observations	4452	3305	5088
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
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

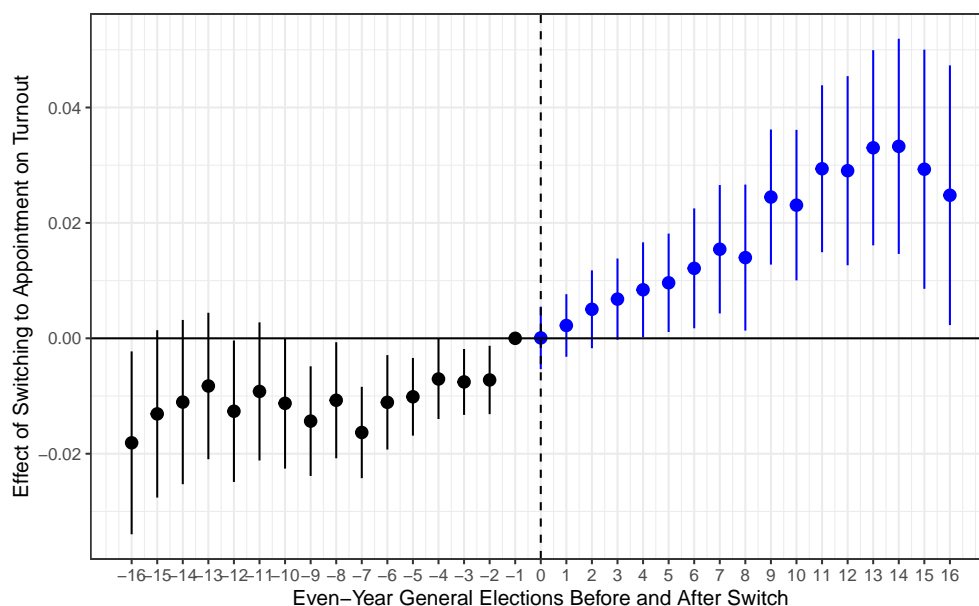
	Voter Turnout		
	Two-Way FEs (1)	de Chaisemartin and D'Haultfoeuille (2)	Callaway and Sant'Anna (3)
Appointed	0.016 (0.005)	0.001 (0.005)	0.021 (0.009)
Counties	253	253	236
Elections	28	28	28
Observations	7084	4929	8096
Outcome Mean	0.43	0.43	0.43
County FEs	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes

Robust standard errors clustered by county in parentheses.

A.3.3 Testing the Parallel Trends Assumption with an Event Studies Estimator

I investigate the validity of the parallel trends assumption using the Dube et al. (2022) local projections difference-in-differences event studies estimator. This estimator makes a series of pooled two-period two-group comparisons, estimating period-by-period effects and eliminating biases due to heterogeneous treatment effects. Figure A.14 plots the results. The x-axis marks the presidential elections before and after a switch in local administration, with 0 marking the first election under an appointed clerk. Each point estimate is the difference in the change in turnout from the previous election of counties with appointed election officials rather than elected ones, at x presidential elections before or after each county's actual switch. Negative coefficients in the left half of the graph suggest some pre-trending. In other words, it appears that counties that switched to appointing clerks may already have been on a trajectory of higher turnout. There is a substantial and statistically significant jump in voter turnout in the first presidential election after a county switches to appointments (0 on the x-axis). There also appears to be a strong dynamic effect on turnout after counties switch their method of clerk selection, evidenced by an increasing trend in the right half of the graph. I explore explanations for this in Section A.2.5.

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 presidential election after a county's first switch from electing to appointing an election official. Each point is the estimated effect of appointing an election official on presidential voter turnout, at x presidential elections of exposure since first selecting the official via appointment. The bar lines above and below each point represent 95-percent confidence intervals. Estimates use the Dube et al. (2022) local projections difference-in-differences estimator for dynamic heterogeneous-robust difference-in-difference designs, which corrects for bias due to heterogeneity in year and county treatment effects.



A.3.4 Generalized Synthetic Control Regression Output

Table A.17 displays regression output from the Xu (2017) generalized synthetic control estimator, matching treatment and control counties with similar pretreatment turnout histories to create balance. 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% and precisely estimated. This is smaller than those found in Table A.4 in the main analysis but is still a substantively significant effect of even-year general elections.

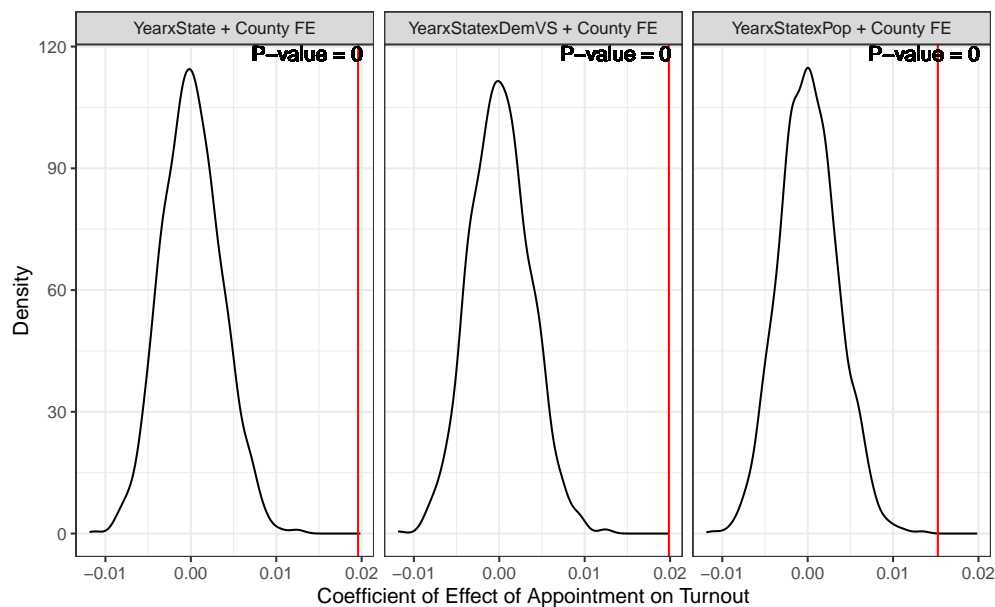
Table A.17: Main Finding that Appointing Local Election Officials Increases Voter Turnout is Robust to Generalized Synthetic Control Estimator

	Voter Turnout (1)
Appointed	0.008 (0.004)
Counties	1042
Elections	28
Observations	29176
Outcome Mean	0.57
Generalized synthetic control method matches treated and control counties on pretreatment voter turnout.	

A.3.5 Randomization Inference Additional Output

As described in the paper, Figure A.15 shows that it is extremely unlikely to observe an effect of appointment on voter turnout as large as that observed by chance alone given which counties switch to appointed local election officials and when they switch is randomized.

Figure A.15: 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 tests whether states with separate registration systems have diminished effects on registration upon switching to appointed clerks. The effect on registration rates is smaller for counties with separate registration system, although this difference is imprecisely estimated. 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 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.

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 estimations of voter turnout separating out the effects of appointments and partisan elections, with the omitted category elected nonpartisan officials. 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)	-0.006 (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 FEs	Yes	No	No
Year x Dem vs FEs	No	Yes	No
Year x Pop FEs	No	No	Yes

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

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

A.4.1 Additional Expenditures on Election Administration May Boost Voter Turnout

Table A.20 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, although the effect diminishes when comparisons are only made between counties in jurisdictions with similar pre-treatment population and partisan makeup. All of the benefit to increased expenditures on turnout appears in small counties.

Table A.20: **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)	0.002 (0.002)	0.002 (0.002)	-0.002 (0.003)	-0.002 (0.003)	-0.001 (0.003)
Ln expend per reg X Small County				0.009 (0.003)	0.008 (0.004)	0.005 (0.004)
Counties	434	434	434	432	432	432
Elections	6	6	6	3	3	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 FEs	Yes	No	No	Yes	No	No
Year x Dem vs FEs	No	Yes	No	No	Yes	No
Year x Pop FEs	No	No	Yes	No	No	Yes

'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.4.2 Do Appointed Election Officials Follow Different Election Administration Policies?

Perhaps directly electing election officials curtails voter participation because the type of officers who run for office pursue different election administration policies than those who are appointed. Following Ferrer, Geyn, and Thompson (2023), I use the EAVS dataset to examine a number of election administration policies. I extensively clean the data to correct for data irregularities and errors. Table A.21 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 the registration removal rate. While the nature of the data should caution against any definitive takeaways, there is no indication that appointed officials pursue election administrative policies that differ from those of directly elected officials. 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.

Table A.21: **Appointed and Directly Elected Local Election Officials Pursue the Same Election Administration Policies (Presidential and Midterm Elections, 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.4.3 Appointed Election Officials May Pursue More Constituent Communication

Figure A.22 examines whether appointed local election officials are more likely to have official social media accounts than elected officials, using data provided by Thessalia Merivaki and Mara Suttman-Lea. 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. 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).

Table A.22: **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 serving possess more formal education than elected clerks serving in similarly sized jurisdictions within the same state. Table A.23 shows the results of additional indicators of official quality using the same specifications as in Table 5. 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 null, it represents an 8% salary premium.

In sum, appointed officials are more educated and more professionalized than elected officials, on average. However, they possess less election administration experience. This is potentially an artifact of higher turnover rates among appointed officials, which is examined in Section 6.2.3.

Table A.23: **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)
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)
States	44	44	44	28	44	44
Observations	587	699	664	97	584	556
Outcome Mean	7.40	1.17	0.15	1.71	0.16	59.74
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 Democracy Fund/Reed College Survey of Local Election Officials and is filtered to only include chief local election officials. County is imputed from zip code to calculate population controls. Observations are weighted to be representative of the population of local election officials. Column 1 measures 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.

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 besides 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; Ferer, 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.24 allows counties to switch in and out of having a local newspaper. The results are almost identical to those found in Table 6.

Table A.24: 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)	0.027 (0.005)	0.020 (0.005)	0.011 (0.006)	0.012 (0.007)	0.006 (0.006)
Appointed X Newspaper	-0.021 (0.006)	-0.023 (0.007)	-0.014 (0.007)	0.000 (0.008)	-0.004 (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.