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Making Elections Happen: Accountability, Diversity, and Partisanship in U.S. Election Administration

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Making Elections Happen:
Accountability, Diversity, and Partisanship in U.S. Election Administration

A dissertation submitted in partial satisfaction of the

requirements for the degree Doctor of Philosophy

in Political Science

by

Joshua Micah Ferrer

2025

ABSTRACT OF THE DISSERTATION

Making Elections Happen:

Accountability, Diversity, and Partisanship in U.S. Election Administration

by

Joshua Micah Ferrer

Doctor of Philosophy in Political Science

University of California, Los Angeles, 2025

Professor Daniel Thompson, Chair

This dissertation examines emerging challenges to election administration including partisan polarization, the limits of local accountability, underrepresentation of racial minorities, and heightened turnover in the profession. Chapter 1 examines whether voters or local elites more effectively monitor and sanction local officials. I find that appointed election officials outperform their directly elected counterparts. Chapter 2 presents evidence that election leaders have become more diverse, that minority leadership does not alleviate racial disparities in turnout or election administration, and that minority voters place more trust in election officials of color. Chapter 3 studies the extent to which increasing polarization affects the way that Democratic and Republican election officials run elections. Chapter 4 examines whether increasing turnover among election officials negatively affects the quality of elections.

The dissertation of Joshua Micah Ferrer is approved.

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2025

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PUBLISHED BOOK CHAPTERS

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Chapter 1: Introduction

Election administration in the US is fragmented into thousands of individual offices that are tasked with running elections for their community. My dissertation studies these people, the institutions that select them, and their behavior in office. Using comprehensive original data on elections, modern techniques for causal inference, and survey experiments, I examine emerging challenges to election administration in the U.S. and its resiliency in the face of partisan polarization, limited accountability, underrepresentation of racial minorities, and heightened turnover.

Chapter 1 investigates whether local election officials are effectively monitored and sanctioned by voters when they are elected rather than appointed. Elections should improve representation by providing a direct link to voters. However, citizens may have too little information to select good leaders and hold them accountable, especially at the local level. I assess these conflicting predictions by examining the performance of local election officials, an office that has come under immense strain to deliver democratic elections and for which selection method is a live policy debate. Over the past half-century, 30% of jurisdictions across 13 states have taken control of elections out of the hands of elected officials and put it in the hands of appointed ones. I use original data on election administration structures in 1,116 counties over 62 years and a difference-in-differences design. I find that appointed officials out-perform their elected counterparts, increasing voter turnout by one to two percentage points and raising registration rates. 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 democratic ideals.

Despite making up a large and growing share of the population, racial and ethnic minorities lead vanishingly few local election offices in the US. Chapter 2 studies whether increasing the share of racial and ethnic minority leaders alleviates persistent disparities in voter participation and gives minorities more confidence that the election was fair. I collect a massive new dataset on the race of local election officials in every jurisdiction in the US from 2000 to 2024. Using a combination of subjective and statistical race imputation methods, I find that the share of local election officials from minority racial groups is growing faster than their share in the population, although minorities remain underrepresented. Linking my panel data on election officials with voter registration files and employing a difference-in-differences design, I find that having a minority group member administer the election does not generally alleviate racial and ethnic disparities in participation. I field a large-scale survey of U.S. adults to investigate the empowerment effects of descriptive representation. In vignette and conjoint experiments, racial minorities report trusting coethnic officials more with their vote and care more about election officials' race than white respondents do. However, in a novel information provision experiment, minority respondents do not report significantly higher levels of voter confidence when they learn that their election official is coethnic. These findings shed light on the importance of representation in local offices.

In the U.S., elections are often administered by directly elected local officials who run as members of a political party. Has increasing partisanship and the nationalization of local politics led Democratic and Republican officials to administer elections differently? In Chapter 3, coauthored with Igor Geyn and Dan Thompson, we use a collection of 5,900 clerk election results across 1,313 counties and a close-election regression discontinuity design to compare counties that narrowly elect a Democratic clerk to those that narrowly elect a Republican. We find that Democratic and Republican officials oversee similar election results, turnout rates, and policies, and are therefore not noticeably advantaging their preferred party. This chapter presents an important counterpoint to claims that local politics are fully nationalized.

Amid a toxic partisan environment over elections and reports of harassment, there has been a surge in departures of many local officials who conduct elections. This has led to concerns that these departures have degraded the ability of election officials to do their job. Chapter 4, coauthored with Dan Thompson, investigates how disruptive leadership changes are in local government. We build an original dataset containing the names and service tenures of chief local election officials in all 50 states from 2000 to 2024, encompassing more than 18,000 officials and 6,000 jurisdictions. Using a variety of panel analyses, we find that a change in leadership prior to an election does not affect participation or other observable indicators of performance. Despite concerns that turnover degrades the quality of election administration, election performance is remarkably resilient in the face of leadership changes.

Chapter 2: To Elect or Appoint? Evidence from Local Election Administration

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 the selection method of local bureaucratic offices 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 President Trump alleged the 2020 presidential election was stolen. Some officials were pressured to refuse to certify the election results,[1] and many have received 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 2024). 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 voter referendum in 2018.[5]

Over 300 jurisdictions–nearly 1 out of 4 counties across 13 U.S. states that comprise nearly 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

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

[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-aheadof-potential-state-takeover/

[5] https://www.miamiherald.com/news/politics-government/election/article215034905.html

appointed local bureaucratic officials produce better outcomes for their constituents.[6] Within-jurisdiction variation in selection method over time allows me to identify a precise effect on differences in election outcomes.

I use measures of voter participation such as turnout and registration rates as my primary outcome. Voter participation is one of the few reliable measures of election quality available over a large span of time. It is also an important one, frequently used in election quality indices such as the MIT Election Performance Index and the Varieties of Democracy Project. More than two-thirds of election officials consider increasing participation a central component of their job,[7] as does their chief professional organization, the National Association of Election Officials.[8] Finally, local election officials likely have the ability to influence participation rates given their far-ranging duties and discretion over administrative decisions (Burden et al. 2013; Kimball and Kropf 2006). Election administrator decisions over communication strategies (Merivaki and Suttmann-Lea 2023), election expenditures (Grose 2022), and polling places (Yoder 2018) have all been show to affect participation, as well as their indirect ability to shape voter wait times (Pettigrew 2017).

I find that when counties switch from electing to appointing their local election official, voter turnout in presidential elections increases by between 1 and 2 percentage points and registration rates seem to increase as well. These findings are robust to a variety of different estimators; hold across multiple states, offices, years, and reform mechanisms; and do not differ by jurisdiction partisanship or appear to come at the expense of increased partisan manipulation of elections. They are also

[6] I occasionally refer to local election officials as clerks in shorthand. While clerks are the most common county election officials, the position title varies widely across states and counties.

[7] https://evic.reed.edu/wp-content/uploads/2021/04/leo2020codebook.pdf

[8] https://www.electioncenter.org/about-us.php

substantively significant. A 2 percentage point boost to voter turnout in federal elections is equivalent to or larger than the effect of universal vote-by-mail (Thompson et al. 2020), automatic voter registration (McGhee, Hill, and Romero 2021), 10 additional days of early voting (Kaplan and Yuan 2020), or a door-to-door canvassing campaign (Green, McGrath, and Aronow 2013). I find suggestive evidence that part of the effect may be due to greater resource provision, contributing to additional staff, more active communication with constituents, and shorter wait times at the polls.

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

My findings speak to the challenges in designing local institutions that advance and protect democratic ideals—especially for bureaucratic offices operating in low-information environments. 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 2024), this paper also informs ongoing debates over who should run elections in the U.S.

2 Selecting Public Officials

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

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

According to political economy theories of governance, elections improve representation by allowing voters to select higher-quality politicians and ensuring their accountability to the electorate through the sanctioning mechanism of reelection (Besley 2006; Besley and Case 2003; Besley and Coate 2003; Fearon 1999). In some empirical contexts, it appears that elections do achieve these goals, producing officials who are more competent than the constituents they represent (Dal Bó et al. 2017), who work harder when they have the incentive of being reelected (Alt, Bueno de Mesquita, and Rose 2011; Christensen and Ejdemyr 2018; Ferraz and Finan 2011; Fouirnaies and Hall 2022), and who better represent voters (Besley and Coate 2003). For technical jobs and low-salience offices, 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. This can prove a significant restricting factor for less populous jurisdictions. Additionally, technological advancements and population growth have led many local public duties to require greater expertise, including election administration (Hale, Montjoy, and Brown 2015). Local elections are rarely contested (Burden and Snyder 2021; Lappie and Marschall 2018; Marschall and Lappie 2018). In the 2020 general election, 78% of all county-level races went uncontested,[9] and half of all elections for partisan office went uncontested in 2022.[10] Whereas long tenures and few challengers could be a sign of voter contentment with the officeholder, it could alternatively mean a breakdown of the accountability mechanism that is essential to ensuring good performance (Besley 2006). If only one candidate is willing to run, this severely limits the ability of voters to select the highest quality candidate and punish them once in office.

Second, low-information and low-salience environments can prevent voters from using elections to effectively monitor officials and sanction them for poor performance (Ashworth and Bueno de Mesquita 2008; Berry and Howell 2007; Besley 2006; Lim and Snyder 2010; Rogers 2023). In theory, elections should provide voters with a more direct accountability mechanism than appointments (Burden et al. 2013). In the absence of sufficient information, however, voters may be unable to select good candidates in the first place, distinguish between highly and poorly performing election officials, or select on quality rather than ideology or other characteristics (Franchino and Zucchini 2015). Local media has been on the 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

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

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

it all the way to the bottom of the ballot where local offices are typically found (Augenblick and Nicholson 2015). Voters might be especially poor judges of performance in complex and technical policy areas (Whalley 2013). The voter information gap for election officials is particularly acute because they have a portfolio of non-election responsibilities and unintuitive titles that dilute the ability of voters to effectively monitor and sanction their performance.[11] If public officials are acting rationally, we should expect them to shirk their duties in these circumstances because their principals (the voters) lack the information necessary to effectively monitor and sanction them. The information-poor environment voters face contrasts with the richer information environment that appointed officials' principals possess. Appointments for election administrators are typically made by boards of local elites and can include county officers, local party chairs, judges, and county supervisors.

Finally, elections can create adverse incentives for officeholders to make politically motivated decisions that are normatively undesirable (Canes-Wrone, Herron, and Shotts 2001). Electing rather than appointing assessors in New York exacerbates economic inequalities (Sances 2016), electing rather than appointing city managers skews economic policies towards the wealthy (Lubell, Feiock, and De La Cruz 2009), and electing rather than appointing municipal assessors in California leads to the adoption of more costly policies and higher borrowing costs (Whalley 2013). If a majority of the voting electorate prefers political outcomes achieved by reducing participation, then elected officials could be incentivized to concentrate costs on certain voters or discourage voting across the board. Likewise, elected officials have won office with the present electorate, so they might be disinclined to pursue actions to expand the electorate. Appointments do not remove this possibility, but they may counterintuitively insulate officials from the pressures of responsiveness in ways that lead

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

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 Trounstine 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 bureaucrats has been found to improve policy outcomes in some cases. In a study of California treasurers, Whalley (2013) finds that municipalities that switched from elected to appointed treasurers enjoyed lower borrowing costs. He concludes that voters may be poorly equipped to judge performance, especially in complex policy areas.

A cross-sectional study of Wisconsin election officials finds that elected clerks produce higher turnout, although it relies on the assumption that elected and appointed clerks are assigned as-if randomly in the state (Burden et al. 2013). The authors theorize that appointed officials are more insulated from public opinion than elected clerks, and thus pursue their own personal goals or the goals of the county officials who appoint them rather than the goals of the public. Because voters prefer that clerks make voting convenient whereas the appointing officials prefer minimizing costs, appointed clerks should oversee elections with lower turnout.

3 Data and Methods

3.1 Measuring the Selection Method of Local Election Officials

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

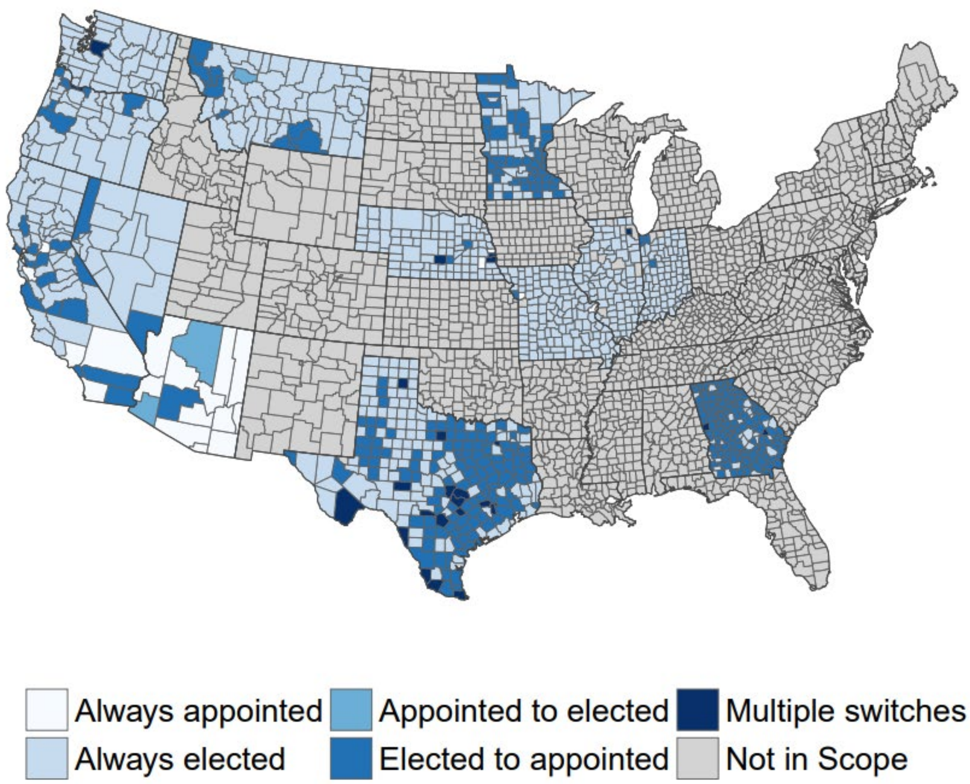
My sample consists of every state in the United States with at least one county-level change between appointing and electing clerks since 1960. These 13 states are a subset of the 42 states in the country where elections are primarily administered at the county level (Ferrer and Geyn 2024) and combined cover nearly 40% of the nation's population. The states included are Arizona, California, Georgia, Illinois, Indiana, Minnesota, Missouri, Montana, Nebraska, Nevada, Oregon, Texas, and Washington.[12] Figure A.1 in the appendix shows the selection method of election officials in all county-administered jurisdictions across the United States. Table A.1 shows that counties in the dataset are similar to those that are excluded. Figure 1 shows which counties enter into the dataset as well as whether they are always appointed, always elected, switch from elections to appointments, switch from appointments to elections, or have undergone multiple changes in selection method. The vast majority of counties that have switched since 1960 have moved from electing to appointing their clerks. In fact, 99.1% of counties switching their selection method have adopted appointments, and 93% of all singular switches have been in the direction of appointments. Four states in particular stand out for the number of switches: California, Georgia, Minnesota, and Texas. Figure 2 shows when each switch in selection method occurred. Counties have changed their clerk selection method in a staggered fashion over many decades, with switches accelerating since 2000. Table A.1.3 in the 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

[12] In 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 (2024). I exclude five counties in Illinois and one in Missouri with nested municipal-level election administration.

the first and last year a change occurred.

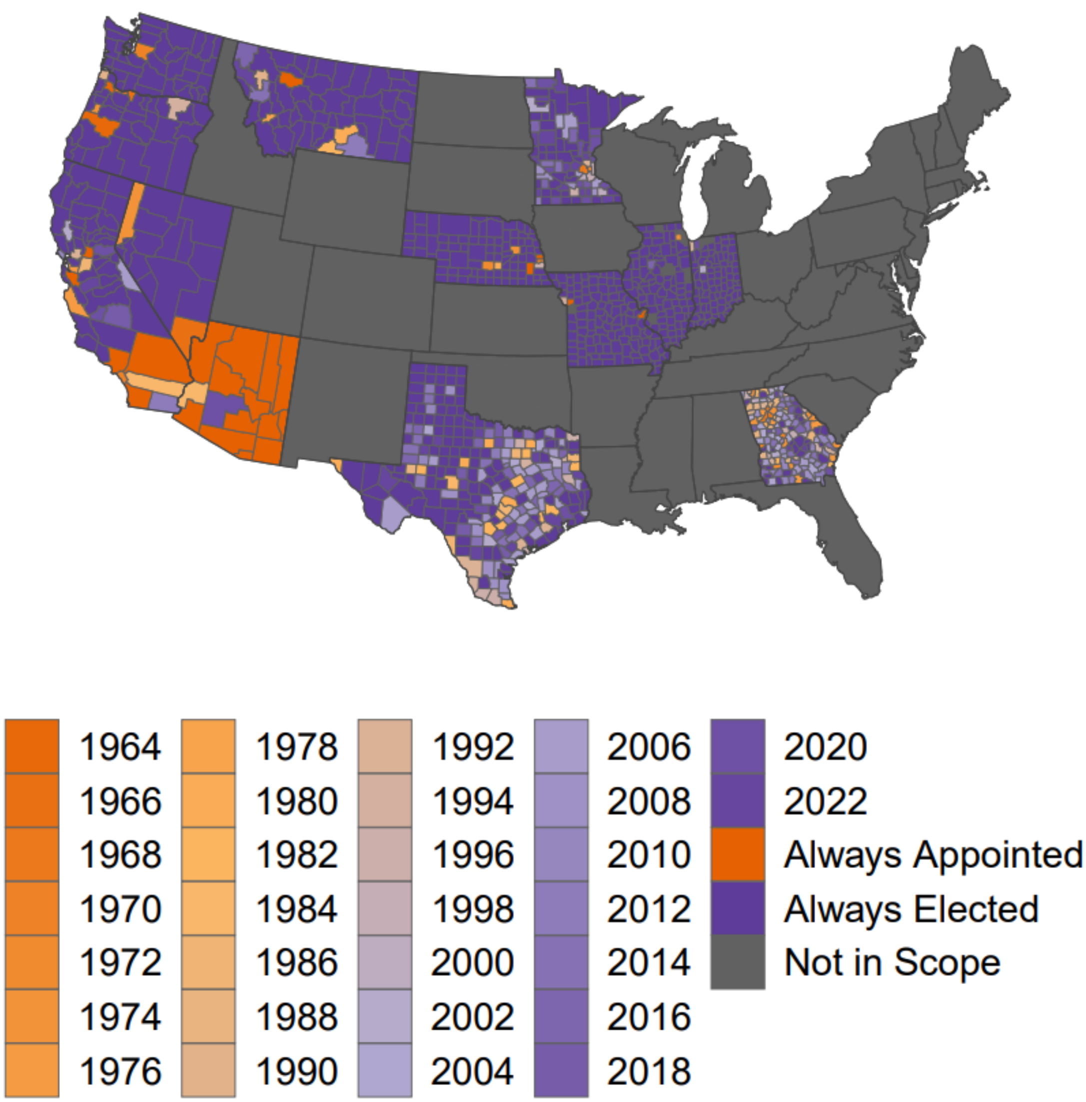
Finally, Figure 3 graphs the extraordinary shift in selection method over time across these 13 states. The percentage of counties that appoint their election officials has grown from 2% in 1960 to 33% in 2022. The balance continues to shift towards appointments, with the trend accelerating over the past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

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.



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

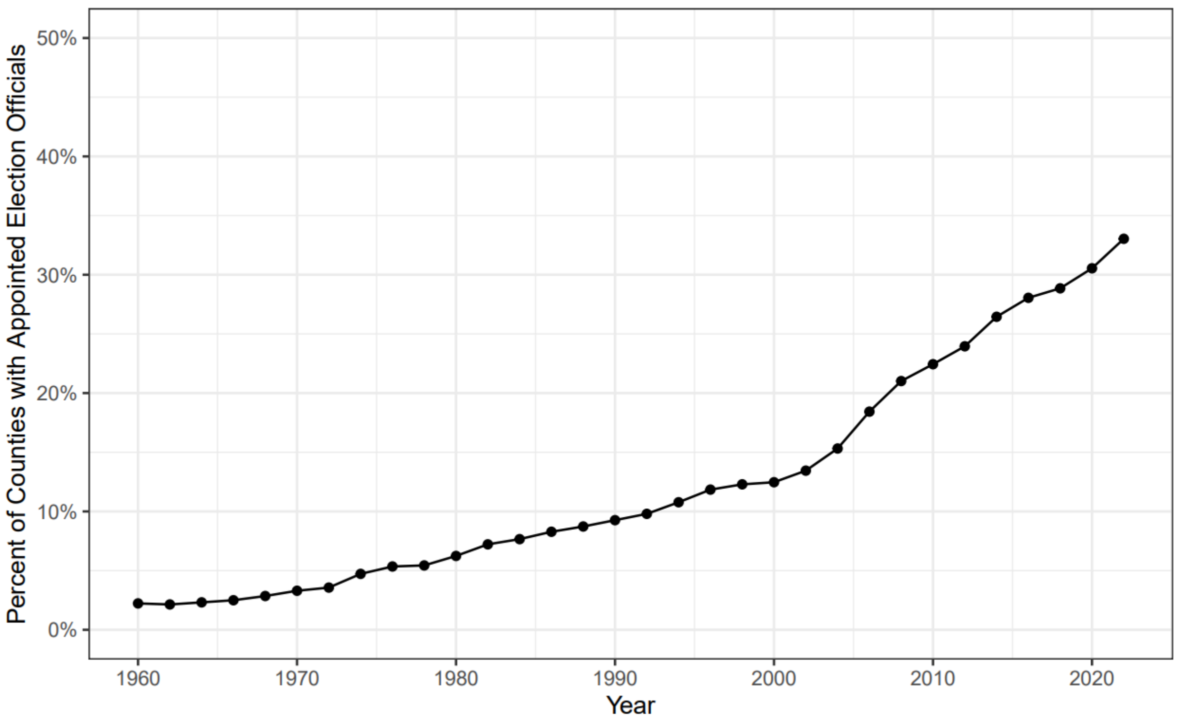
Figure 2: **Local Election Administration Selection Cohorts, 1960-2022**. This graph displays the year county-level switches occurred between electing and appointing local election officials since 1960. In most cases, this switch is from electing to appointing the local election official. In counties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



Some counties in California, Minnesota, Montana, and Washington hold binding referendums to initiate the reform, and several Midwestern states have population thresholds at which appointing their election official becomes possible or mandatory. In most cases, the switch in selection method is not accompanied by any other substantive change to election policy or resource provision. For instance, in Georgia a state legislator that represents the affected county introduces a law to the legislature transferring election administration authority from the elected probate judge to an appointed board of elections that then selects an elections director. In Texas, the county commissioners enact the transfer of authority from an elected clerk or tax assessor to an appointed elections administrator. Minnesota presents a particularly minimal case of change, with county officials agreeing to a switch from election to appointment of the county auditor. In some cases, this does not even result in a change in leadership. I conduct a series of robustness tests isolating the effects of reform independent of other substantive policy changes.

Local election officials are entrusted with broad statutory authority to conduct elections (Ferrer, Geyn, and Thompson 2024). For instance, probate judges in Georgia determine precinct divisions, handle nomination petitions of candidates, publish notices and advertisements of elections, select and equip polling places, purchase and maintain election equipment, conduct early in-person voting, appoint and train poll officers, inspect the conduct of elections, receive and certify election results, prepare a budget estimate and appropriations request, conduct hearings to determine the eligibility of candidates, and administer photo ID provisions. Most clerks also handle registration administration and voter list maintenance duties, although these responsibilities are divided in Arizona, Georgia, and parts of Texas.

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.



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

3.2 Data

I use presidential and midterm participation rates as my primary outcome measure. I focus on turnout and registration rates for four reasons: local election officials have the ability to influence participation levels, they view increasing participation as part of the job, voter participation is a key component of election quality metrics and the ultimate outcome of election quality, and I have access to high-quality data on participation rates. First, election officials typically have far-ranging duties and a significant degree of discretion in carrying out these duties (Kimball and Kropf 2006). Some studies have found that clerks of different parties influence turnout rates (Bassi, Morton, and Trounstine 2009; Burden et al. 2013; but see Ferrer, Geyn, and Thompson 2024). Second, according to the 2020 EVIC Survey of Local Election Officials, over 67% of local election officials agree that encouraging voter turnout is part of their job, compared with fewer than 10% who disagree. This is reflected in the National Association of Election Officials, which lists increasing participation as one of the main considerations for election officials.[13] Third, participation rates are widely viewed as a key measure of election quality. MIT's Election Performance Index uses both voter turnout and voter registration in comparing election administration performance across states,[14] and Varieties of Democracy (V-Dem) uses electoral participation as one of its indicators of democratic health.[15] Voter turnout can be considered the ultimate effect of the quality of election administration. If voters have a poor voting experience or are not readily or proactively provisioned with the information necessary to vote, then they are less likely to participate. Finally, high-quality data for turnout exists at the county level and is available going back many decades. This is not true of any other indicator of election quality, including voter confidence, voter wait times, number of polling places, and constituent communication. Data on county-level vote totals is from Congressional Quarterly and David Leip's U.S. Election Atlas and spans 1968 to 2022.[16] I use data on registration totals from Leip's Election Atlas. This covers presidential elections from 1996 and gubernatorial elections from 2004. I measure voting age population using estimates from the National Cancer

[13] https://www.electioncenter.org/about-us.php

[14] https://elections.mit.edu/#/data/map

[15] https://www.v-dem.net/static/website/img/refs/codebookv12.pdf

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

Institute's Surveillance, Epidemiology, and End Results Program.[17] I measure registration rate by dividing total registrants by the voting age population.

I assemble a set of county-level indicators of election administration policy using the U.S. Election Assistance Commission's Election Administration and Voting Surveys (EAVS) from 2004 to 2022, including the number of polling places per 1,000 people, provisional ballot rate, provisional ballot rejection rate, absentee ballot rejection rate, and the registration removal rate. Following Ferrer, Geyn, and Thompson (2024) and Pettigrew (2017), I use data from the Survey on the Performance of American Elections (SPAE) to measure the share of voters who had to wait at the polls for certain lengths of time.[18] This is available for general elections in 2008, 2012–2016, 2020, and 2022. I also use election official communication data provided by Thessalia Merivaki and Mara Suttmann-Lea.

I probe mechanisms using data on election administration expenditures from Mohr et al. (2018), data on the prior experience of local election officials from the 2020 EVIC Survey of Local Election Officials,[19] 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

[17] This 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 because few people decide where to live based solely on the selection method of a county's local election official. The data I use is available at https://seer.cancer.gov/popdata/. It is available from 1970 to 2020. I extrapolate the estimates to 1968 and to 2022.

[18] https://electionlab.mit.edu/research/projects/survey-performance-american-elections

[19] https://evic.reed.edu/leo-survey-summary/

the 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 2024). They also tend to have lower participation rates (Leighley and Nagler 2017). Similarly, counties in Western states tend to elect their officials and also tend to have higher turnout rates than counties in other regions (Springer 2014). Given these correlations, a simple cross-sectional analysis of counties would result in a relationship between appointed officials and lower turnout—but this would not be evidence that appointing officials *causes* lower turnout. Even if all of these obvious differences are controlled for, unobservable factors likely exist 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 $Y_{it} = \alpha_i + \delta_t + \beta Appointed_{it} + \epsilon_{it}$, where Y_{it} is a measure of voter turnout or registration in county i at election year t, α_i and δ_t are county and year fixed effects, respectively, and $Appointed_{it}$ is a dummy variable taking 1 when counties appoint their local election official and 0 when counties elect their local election official. β is the causal effect of an appointed election official on voter turnout.

The causal interpretation of the difference-in-differences design rests on the parallel trends assumption. This means that counties that switch to appointed clerks are on similar turnout trajectories to those that do not switch, prior to the reform. It is possible to imagine that counties that switch to appointed officials are growing at a more rapid rate than those that stay with elected officials, and that turnout is trending down as a result. In this case, appointed officials might be viewed as a way to professionalize the county's election administration. Similarly, selection method might become a partisan issue. If more Democratic counties start to adopt appointed clerks, and Democrats reduce or increase their turnout relative to Republicans, then this would also result in the spurious appearance of a causal relationship between appointments and turnout. Table A.4 reveals differences in population, participation rates, and demographics between counties that switch to appointed officials and counties that stay with elected officials.

All regressions include at the minimum Year by State fixed effects. This ensures that comparisons are only made between counties in the same state, addressing the possibility that states may be on different turnout trajectories. I further address parallel trending concerns by incorporating two additional sets of interacted fixed effects: Year by State by Democratic vote share and Year by State by Population fixed effects. The former compares within-county over time change to other counties with similar partisan makeup, whereas the latter compares within-county overtime change to other counties with similar populations. These account for the possibility that counties that switch their election administration may also happen to shift either population or partisan trends in ways that are systematically related to turnout. Democratic vote share and population are divided into quartiles for each state, allowing the grouping cut points to vary by state, and measured pretreatment.[20]

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

4 Results

In this section, I present evidence that appointing local election officials results in increased participation rates. I then validate this finding with a range of alternative estimators, conduct a placebo analysis using registration rates, distinguish between the effects of selection method and partisanship, and examine whether appointed officials benefit their principals' party or if the effect differs by jurisdiction partisanship.

4.1 Appointing Election Officials Increases Voter Participation

Table 1 displays the results of a two-way fixed effects regression estimating the effects of appointing a local election official on citizen participation. Columns 1 through 3 estimate the effects on votes per voting-age resident and columns 4 through 6 estimate the effects on registrants per voting-age resident. Both are measured as proportions out of 1. The coefficients are the average percentage point difference in turnout and registration rates when counties switch from elected to appointed clerks. All six regressions include, at minimum, county and year by state fixed effects.

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

Column 1 shows that counties switching from directly elected to appointed election officials see an average increase in voter turnout of 1.8 percentage points in even-year general elections, compared with counties that do not switch. The point estimate is precisely estimated, allowing us to confidently rule out effects smaller than 1.2 percentage points at the 95% confidence level. It is also substantively meaningful. The estimated effect on turnout in even-year general elections is on par or larger than those generated by the most significant modern policy interventions designed to boost voter participation. It is equivalent to implementing universal vote-by-mail (Thompson et al. 2020) or adding 10 days of early voting (Kaplan and Yuan 2020). It is also double the turnout boost caused by implementing automatic voter registration (McGhee, Hill, and Romero 2021). This effect is also significant compared to get-out-the-vote interventions. It is twice the average turnout effect of door-to-door canvassing, three times that of a direct mailing, and five times that of a phone call campaign (Green, McGrath, and Aronow 2013).

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

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

	Voter Turnout			Registration Rate		
	(1)	(2)	(3)	(4)	(5)	(6)
Appointed	0.018	0.016	0.014	0.009	0.009	0.008
	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(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.

Appointed election administrators also appear to oversee elections with higher registration rates. Arizona and Georgia are excluded from these specifications because registration duties are always undertaken by appointed registration boards. The coefficients range from 0.8 to 0.9 percentage points in magnitude, and a null of no difference can be confidently ruled out in all three estimators. An event study plot of the effect of appointment on registration, shown in Section A.3.3, reveals some evidence of pre-trending, so this effect should be viewed with caution, though the point estimates are replicated in a matching analysis specification shown in A.20.

These estimates provide strong evidence that appointed clerks increase voter participation, relative to their directly elected counterparts. Regressions excluding midterm contests are found in Section A.2.1 and yield substantively similar findings. Table A.6 in the appendix shows the results are also robust to the use of different criteria in constructing the panel data of election official selection methods, and

Table A.7 shows the results are robust to the inclusion of county linear time trends, albeit attenuated.

The results hold in multiple states, across multiple offices, for multiple reform mechanisms, and over multiple years and date ranges. In Table A.13 in the appendix, I show that switching to an appointed election administrator increases voter turnout in three of the four states with at least 10 counties experiencing switches (Georgia, Minnesota, and Texas), and is imprecisely estimated in the fourth case (California). Table A.14 in the appendix shows that switching from elected probate judges, auditors, and clerks to appointments increases voter turnout. In Section A.2.5 in the 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 is packaged with unrelated reforms. This alleviates concerns that the boost to turnout is an artifact of the way the reform in selection method is initiated. I also run a series of Callaway and Sant'Anna (2021) regressions in Section A.2.6 to estimate the dynamic effects of switching from an elected to an appointed election official. The positive effects of appointments on voter turnout appear over time and across multiple county cohorts and time periods.[21] 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.17 in the appendix that the results hold using only more recent elections, with some attenuation in effect magnitude.

4.2 Validating the Effect of Appointing Election Officials on Voter Turnout

In this section, I validate my main finding that appointed local election officials produce higher voter turnout than directly elected officials. I utilize alternative difference-in-difference estimators and

[21] This 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.

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

4.2.1 Validating the Staggered Rollout Design

Recent scholarship has identified potential problems with the standard two-way fixed effects estimator when used in staggered adoption designs (Baker, Larcker, and Wang 2022; Borusyak and Jaravel 2018; de Chaisemartin and D'Haultfœuille 2020; Callaway and Sant'Anna 2021). These issues stem from heterogeneous treatment effects. If treatment effects vary across time or units, the estimate will be biased due to the assignment of negative weights to some comparison groups.

To validate my main findings, I test a range of alternative specifications in Table A.18 in the appendix, including removing counties that switch from appointments to elections, removing counties that use appointments throughout the dataset, and using stacked difference-in-difference estimators. All specifications result in precisely estimated effects on turnout between 2.1 and 3.5 percentage points. In Section A.3.2, I employ the de Chaisemartin and D'Haultfœuille (2020) estimator and the Callaway and Sant'Anna (2021) dynamic effects estimator for states with at least 10 treated counties. The results are consistent with those shown in Section A.2.4.

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 appointing election officials are fundamentally different from those that remain elected on some unobserved characteristics, then this undermines the causal validity of the regression specification. Figure A.5 in the appendix investigates the validity of the parallel trends assumption using the Dube et al. (2022) local projections event studies estimator. It shows evidence that places that adopt appointments may be on different trajectories prior to reform.

I overcome this concern through the generalized synthetic control method. This estimation strategy rebalances the data sample by comparing treated and untreated units with similar pre-treatment voter turnout history. Figure 4 displays output from a Xu (2017) generalized synthetic control estimation. The line in the left-hand side of the figure is close to 0, showing that the strategy successfully compares treated and control counties with similar pre-treatment turnout trajectories. It becomes positive in the right-hand side of the figure and is statistically distinguishable from 0. This provides additional evidence that appointed election officials administer elections with higher turnout than their elected counterparts. As shown in Table A.26 in the appendix, it produces a precisely estimated effect of 0.8% on voter turnout, lower than the estimates shown in Table 1 but still substantial for participation in federal general elections. In Section A.2.6 in the appendix, I explore two possibilities for an increasing effect magnitude over time: delayed effects due to selection method reform triggering turnover, and a secular trend of declining availability in local news.

4.2.3 Randomization Inference

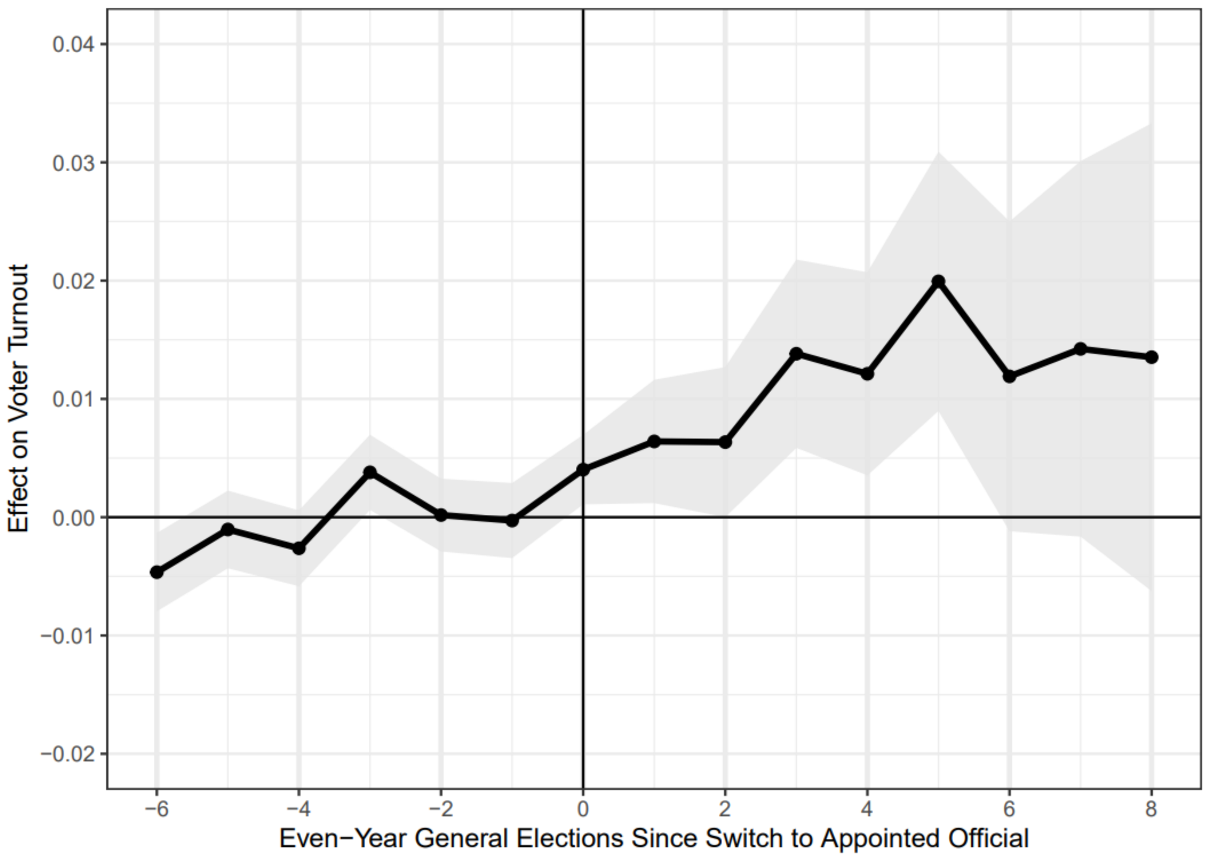
Randomization inference can be used to derive an alternative estimate of the likelihood of finding an effect as large or larger than the one observed by chance. I employ two different randomization permutations: in Figure A.9 in the appendix, I randomly permute both which counties are treated and when they are treated, and in Figure 5, I randomly permute when treated counties receive treatment. Counties that switch from appointed to elected and counties that switch selection method multiple times are excluded. 1,000 permutations are computed for each exercise. The three regressions shown in Table 1, columns 1-3 are replicated with the permuted data and the coefficient stored for each permutation. Finally, the actual coefficient derived is compared with the distribution of permuted coefficients. The p-value is the number of randomized coefficients that are greater than or equal to the actual estimated effect divided by the total number of iterations.

Figure A.9 shows that the likelihood of observing the actual result or a more extreme effect is close to 0, given randomized treatment and treatment timing and assuming the true effect is null. The more demanding inferential test is when the counties that switch to appointments are preserved, but when they switch is scrambled. Figure 5 shows that random treatment timing of the treated units typically results in a positive relationship between appointments and voter turnout. This aligns with the evidence of pre-trending shown in Figure A.5 and corrected for by the generalized synthetic control method in Figure 4. However, it is still extremely unlikely to get an observed effect as large as that actually observed–only in about 1 out of 200 simulations does the effect reach 2%. This provides additional validation that appointing election officials increases voter turnout.

4.3 Selection Method, Not Partisanship, Explains the Results

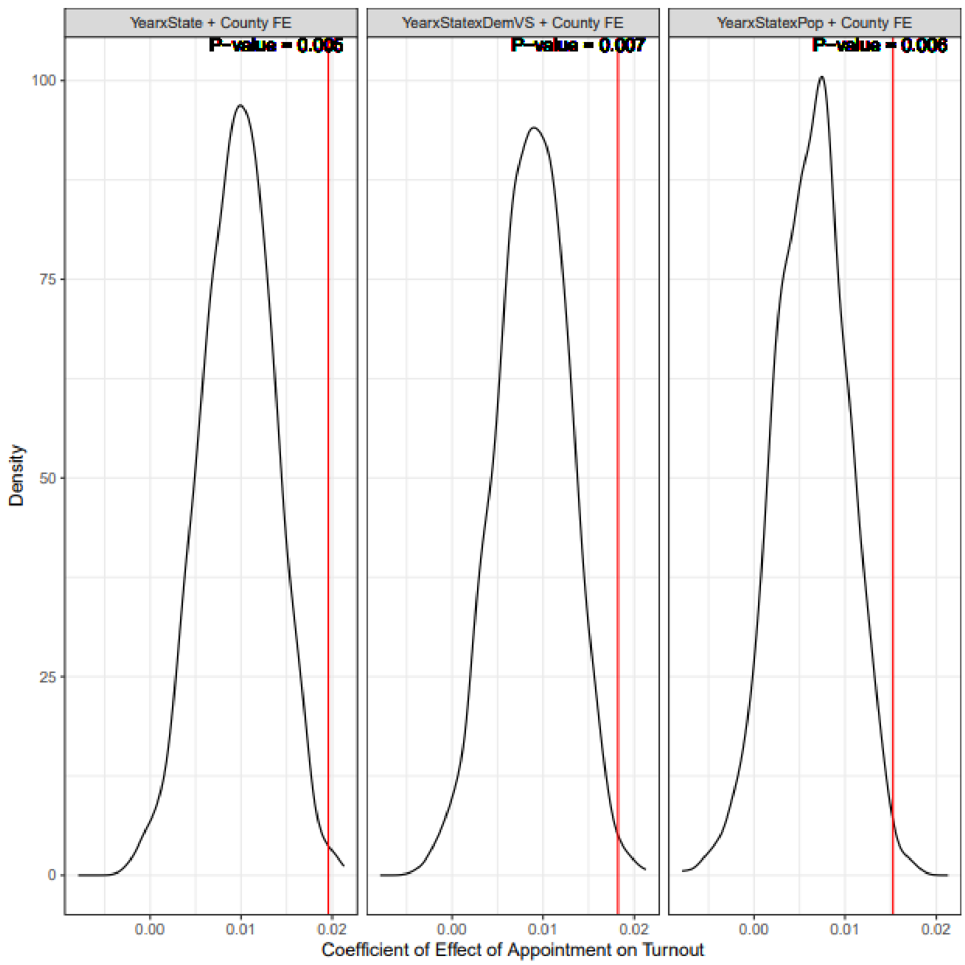
Are the observed effects the result of a switch from elected to appointed clerks, or are they due to the switch from an openly partisan office to an ostensibly nonpartisan position? The results in Table 1 present a bundled treatment of both selection method and partisanship. The partisan nature of elected 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.28 provides strong evidence that selection method, and not the partisan nature of the office, drive the main results on voter turnout.

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.



I also test whether the effect of appointments on participation is shared equally across jurisdictions, regardless of partisan lean, or concentrates in jurisdictions of a certain partisan balance. I show in Table A.30 that the benefits of switching to appointed election officials are similar across Democratic- and Republican-leaning jurisdictions.

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.



5 Why Does Appointing Election Officials Increase Voter Participation?

What do appointed local election officials do differently from elected officials that increases voter participation for their constituents? I show that appointed officials obtain additional election administration resources. Beyond this, I cannot definitively tell how appointed officials increase participation. However, I provide suggestive evidence that appointing election officials leads to higher election official salaries, a larger workforce, more robust communication with voters, and may lower wait times, all consistent with activities that could increase participation. However, some findings are inconsistent with expectations. I fail to find any differences between appointed and elected officials in number of polling places per 1,000 residents, share of provisional ballots cast, share of provisional or absentee ballots rejected, and share of registrants removed from the list.

5.1 Appointed Election Officials Obtain More Resources

Sufficiently funding elections is essential to ensuring high quality administration (Mohr et al. 2019, 2020; Kropf et al. 2020; McGowan et al. 2021). Previous scholarship has shown that increasing election administration resources can boost voter turnout (Grose 2022; but see Lal and Thompson 2024). Burden et al. (2013) argue that appointed officials are *less* able to advocate for more resources than their elected counterparts and therefore administer elections with fewer resources. However, Taylor, Swint, and Reilly (2024) find that appointed boards of election in Georgia spend 45% more on election administration than elected probate judges. Appointed officials might have better relationships with their principals and thus more sway over election funding. If the quality of selection is higher for appointed officials, they might be more proactive in securing additional resources. Alternatively, they might be more responsive to the interests of cost-conscious voters because elected judges are more attentive voter's desires to be efficient (Choi, Gulati, and Posner 2010). It is also possible that in smaller jurisdictions, switching to a dedicated appointed local election official increases the amount of full-time equivalent (FTE) employees who work in election administration.[22]

[22] 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

I use jurisdiction election administration expenditure data from Mohr et al. (2018). This dataset includes estimated yearly expenditures for each county in Arizona, California, Georgia, Minnesota, Missouri, Nebraska, and Nevada starting from as early as 2002. This enables the use of a difference-in-differences regression design to test the effect of switching to appointed election officials on election expenditures. Following Taylor, Swint, and Reilly (2024), I use the natural log of total election expenditures per registered voter as my dependent variable. Table 2 displays the results (an event study plot, found in Section A.3.3, shows no evidence of pre-trending, and the inclusion of county time trends in Section A.2.3 shows similar 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, defined as counties below each state's median county population. All point estimates are large and statistically distinguishable from zero. The coefficient in column 1 means that when counties switch to an appointed election official, their election expenditures per registered voter increases by 28 percentage points on average. We can confidently rule out effects of less than 7.5 percentage points at the 95% confidence level. The average county spends \$9.50 per registered voter on administering elections in even years, and appointed officials secure an additional \$3.06 per registrant.

The effects are, if anything, smaller in less populous jurisdictions. This means they are likely driven by the actions of the local election official rather than a result of creating an additional FTE election administration position. An alternative explanation is that counties that become more concerned about the quality of election administration both switch selection methods and increase election expenditures at the same time. I further investigate the reason for this effect by examining whether appointed officials were more likely to apply for the Center for Tech and Civic Life's 2020 COVID

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counties, where sometimes only a single official administers elections. According to the 2020 EVIC Survey of Local Election Officials, 34 percent of jurisdictions have no full-time election administrators and 17 percent have exactly one FTE (https://evic.reed.edu/leo-survey-summary/). In all other jurisdictions, switching to an appointed official should not directly increase the amount of FTEs.

grants to election administrators, a decision made directly by the election official rather than county supervisors. Using data on grant applications from Lal and Thompson (2024) and nationwide cross-sectional selection method data from Ferrer and Geyn (2024), I find in Table A.31 that appointed officials were 7 percentage points more likely to apply for the grants compared to elected officials in counties within the same state, even after controlling for a range of factors including population, partisanship, median income, urbanicity, non-Hispanic white share, and COVID severity. This provides suggestive evidence that the effect is due to a quality difference between elected and appointed officials rather than elected officials simply being more attentive to the desire of voters to minimize costs or because a switch to appointments happens at the same time counties pour more funds into election administration. Finally, I show in Table A.32 that increased election expenditures may lead to additional voter turnout, especially in smaller jurisdictions. A doubling of expenditures per registrant increases voter turnout by 0.27 percentage points on average, and 0.39 percentage points in smaller counties. This is in line with previous findings linking election expenditures with higher turnout (Kropf and Pope 2020).

5.2 What Administrative Policies Do Appointed Election Officials Pursue Differently?

Given that appointed election officials obtain more resources, what might they do with these resources that could lead to higher participation? Using data from the 2020 EVIC Survey of Local Election Officials, I find suggestive evidence in Section A.5.1 that appointed officials serving in similarly populous jurisdictions within the same state make \$5,000 more than elected officials and hire an additional 0.6 FTEs on average, although both results are imprecise.

Table 2: **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	0.270	0.310	0.323	0.367	0.357
	(0.100)	(0.091)	(0.098)	(0.110)	(0.110)	(0.109)
Appointed X Small County				-0.100	-0.204	-0.122
				(0.224)	(0.209)	(0.215)
Counties	434	434	434	432	432	432
Elections	6	6	6	6	6	6
Observations	1929	1929	1929	1920	1920	1920
Outcome Mean	2.25	2.25	2.25	2.25	2.25	2.25
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year x State FEs	Yes	No	No	No	No	No
Year x State x Dem vs FEs	No	Yes	No	No	No	No
Year x State x Pop FEs	No	No	Yes	No	No	Yes
Year x State x Small FEs	No	No	No	Yes	No	No
Year x State x Dem vs x Small FEs	No	No	No	No	Yes	No

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

Election officials could use additional funding to improve voter outreach. Clerks have significant discretion in their communication with voters. They can pursue a proactive strategy of providing additional information to the public and accurately responding to constituent questions. Or, they can provide the legally required minimum amount of information. More active election official communication strategies has been shown to increase the share of registered voters (Merivaki and Suttmann-Lea 2023), improve voter confidence (Suttmann-Lea and Merivaki 2023), and reduce the number of mail ballots that are rejected (Suttmann-Lea and Merivaki 2022). In an audit study of election officials, White, Nathan, and Faller (2015) found that elected officials were 16% less responsive and 12% less accurate in their responses than appointed officials. Figure A.34 in the 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 find differences in the usage of other platforms.

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

Additional resources could be employed to improve the Election Day experience for voters in a number of additional ways, including hiring more poll workers, providing them with better pay and more rigorous trainings, and better provisioning polling places with poll booths and voting machines. While I cannot directly test these mechanisms, I use data from the 2008, 2012, 2014, 2016, 2020, and 2022 Survey of the Performance of American Elections (SPAE) to examine whether voter wait times decrease when counties switch to appointed administration. I employ difference-in-difference regressions with county and state-by-year fixed effects and individual controls for gender, race, age, education, and party identification. The results are shown in Figure 3. While the regressions are relatively imprecise, the coefficients are all negative and the effect sizes are substantively meaningful. Switching to appointed election officials reduces the average voter's self-reported wait time by roughly half a minute on average. It reduces the percentage of voters that wait at least 10 minutes in line by 3 percentage points and reduces the percentage of voters waiting in line for 30 minutes or more by 1 percentage point. Longer wait times have been found to depress future voter turnout (Pettigrew 2021), making this one plausible factor explaining why appointed officials boost participation.[23]

Table 3: **Appointed Local Election Officials May Decrease Voter Wait Times (Even-Year General Elections, 2008-2022)**

	Min Waited (1)	> 10 min (2)	> 30 min (3)	> 1 hr (4)
Appointed	-0.387 (0.774)	-0.031 (0.031)	-0.012 (0.021)	-0.006 (0.010)
Counties	798	798	798	798
Respondents	9169	9169	9169	9169
Elections	6	6	6	6
Observations	9169	9169	9169	9169
Outcome Mean	8.43	0.29	0.11	0.04
County FEs	Yes	Yes	Yes	Yes
Year x State FEs	Yes	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes	Yes

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

6 Why Do Appointed Election Officials Outperform Elected Officials?

I explore two sets of mechanisms that could lead appointed local 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, the low contestation rates of clerk elections, and differential effect of appointments in small and large jurisdictions. For the latter, I investigate the information voters know about their election official, differences in turnover rates, and triple difference-in-difference estimates comparing the effect of selection method based

[23] Table A.9 in the appendix shows these results are somewhat robust to the inclusion of county time trends.

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 (2024) find that only 23% of general election races for local election official feature a contest between a Democrat and a Republican, and only 12% of all contests result in a race with a margin of victory of less than 20 percentage points. Previous research shows that low contestation rates is a problem across local offices (Burden and Snyder 2021; Lappie and Marschall 2018; Marschall and Lappie 2018; Thompson 2020; Yntiso 2022).

I use the 2020 EVIC Survey of Local Election Officials to examine whether elected and appointed officials possess different levels of education, a common indicator of the quality of public officials (Dal Bó et al. 2017). Table 4 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.

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

Table 4: **Appointed Local Election Officials Possess More Education Than Elected Officials**

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

Robust standard errors clustered by state in parentheses. Data is from the 2020 EVIC Survey of Local Election Officials and is filtered to only include chief local election officials. County is imputed from zip code to calculate population controls. Observations are weighted to be representative of the population of local election officials. Columns 1 and 4 measure educational attainment on a 5-point scale: high school, some college, college, some graduate school, and graduate school. Columns 2 and 5 measure whether the official possess a college degree, and columns 3 and 6 measure any college education.

Table A.35 in the appendix tests a number of additional indicators of quality between elected and appointed officials using the 2020 EVIC survey. I find that appointed officials tend to possess less previous experience in election administration, may hold more professional memberships, are likelier to have served elsewhere and in a greater number of previous jurisdictions, are less likely to be over the age of 65 years, make approximately 10% more in salary than elected officials, and recruit an additional 0.5 FTE. These findings are in line with a recent survey of municipal clerks in New England which found that elected clerks are older, less educated, longer-tenured, and have less institutional capacity than appointed clerks (Marsh et al. 2024). I take this as evidence that appointed officials possess less election administration experience but are more professionalized than their elected counterparts.

6.2 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. The gap in selection quality between elected and appointed officials is likely to be greatest in less populous jurisdictions. This is because elected officials typically must live in the jurisdiction, whereas appointed officials can be hired from elsewhere. Table 5 displays the results of difference-in-difference regressions testing the magnitude of the difference in effect between less and more populous counties. A "small county" is defined as ranking in the bottom half in population compared to other counties within the same state. The top row is the effect of switching to appointed election officials for populous counties, and the bottom row is the additional effect of switching to appointments for relatively less populous counties. It is apparent that the effects are largest in small counties. Appointed election officials in less populous jurisdictions produce turnout rates that are between 2.0 and 2.2 percentage points higher than their elected counterparts, compared with 0.7 to 0.9 percentage points higher in more populous jurisdictions. A similar pattern is found with registration rates, with point estimates in smaller counties double those found in large counties. This evidence is consistent with selection mechanisms explaining the difference in performance between elected and appointed officials.[24]

Table 5: **Appointing Local Election Officials Increases Citizen Participation Especially in Small Counties (Even-Year General Elections, 1968-2022)**

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

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

6.3 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 official, 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 may have higher turnover rates.

6.3.1 Voters Know Little About Their Local Election Official

I fielded a survey of 3,200 U.S. adults to test respondent knowledge of their local election official.

[24] Table A.10 in the appendix shows that the voter turnout results are robust to the inclusion of county time trends but the registration results are not.

The survey hypotheses and analysis are preregistered on OSF,[25] and technical details are provided in Section A.5.2 in the 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 guessing, only 17.2% of respondents were able to correctly identify the title of their chief election official. Only 5.2% of respondents knew whether their election official is elected or appointed. And fewer than 8% 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 mediocre performance. This is in contrast to the local elites in charge of appointing election officials. By their very nature, all principals know who the election official is and are likely to have a better idea of the quality of their work.

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

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

[25] osf.io/k7hq2

the information necessary to hold the public official accountable?

I test the effects of the presence or lack of a local daily newspaper on the relationship between selection method and voter turnout using a triple difference-in-differences design and a combination of two datasets: (Gentzkow, Shapiro, and Sinkinson 2014), which contains newspaper data from 1960 to 2004, and data from Sean Ewing that updates this data through 2020. I sort counties into two categories: those that have continuously had at least one newspaper headquartered in its boundaries within the study period, and those that have not. Table 6 displays the results of this analysis. The first row shows the effect of counties switching from elected to appointed election officials when they lack the continuous presence of at least one local newspaper. Column 2 is the additive effect on switching for counties that have a local newspaper presence. Nearly the entirety of the positive benefits to appointing election officials lie in counties that lack local news coverage. An alternative specification, introducing over-time variability in the presence of a local daily newspaper, shows results consistent with Table 6 and is found in Section A.5.3. The inclusion of county time trends, found in Section A.2.3, shows the same general finding for voter turnout but not for registration rates.

6.3.3 Appointed Election Officials May Have Higher Turnover Rates Than Elected Officials

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

Table 6: **Consistent Presence of a Daily Local Newspaper Attenuates the Effect of Appointing Local Election Officials on Citizen Participation (Even-Year General Elections, 1968-2022)**

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

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

Column 1 shows the effect of a switch in a county from elections to appointments on turnover of the election official. Because this switch causes turnover in most cases, I impute missing dependent variable values for the year each jurisdiction moves into treatment. It appears that appointed officials leave the position at higher rates than elected officials. Switching to an appointed official increases the probability of turnover over a 2-year period by 4.9 percentage points. Considering the average 2-year turnover rate of election officials in the dataset is 18%, this is a fairly substantial effect.

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

	Election Official Turnover (1)
Appointed	0.049 (0.025)
Counties	1113
Elections	3
Observations	10881
Outcome Mean	0.18
County FEs	Yes
Year x State FEs	Yes

Robust standard errors clustered by county in parentheses.

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 election officials, who are facing intense scrutiny from political elites 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

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

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

These findings add to a growing literature on the limits of elections in ensuring accountable officeholders (Ashworth 2012; Rogers 2023). Elections are designed to achieve accountability between officeholders and the public. When voters have access to high-quality information, can make a choice between multiple candidates, and are able to effectively sanction an officeholder who shirks their duty, agents are 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, they have little ability 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 Trounstine 2014; Sances 2016; Whalley 2013). Elections for local bureaucratic 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 in local politics 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 local public offices 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 partisan election boards, including some filled with election deniers.[27] However, my results suggest that over a long period of time and across several states, appointed election officials have produced better outcomes for their constituents than elected officials.

Future work should consider other instances where elections fail to achieve their intended effects, with the goal of uncovering under what conditions appointed public officials produce better outcomes for their constituents. This analysis suggests that the information environment, competition, and technical requirements of the office shape the selection method trade-off. We also need better measures of objective accountability outcomes for public officials (Carreri and Payson 2021).

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

Finally, scholars should work to distinguish between public responsiveness and conflicts in principals' goals. Appointments are likely only to be beneficial when the desires of the general public and political elites align. Measuring which issues and to what degree elites and voters have differing preferences could go a long way to clarifying the contexts where appointments are preferable to elections.

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

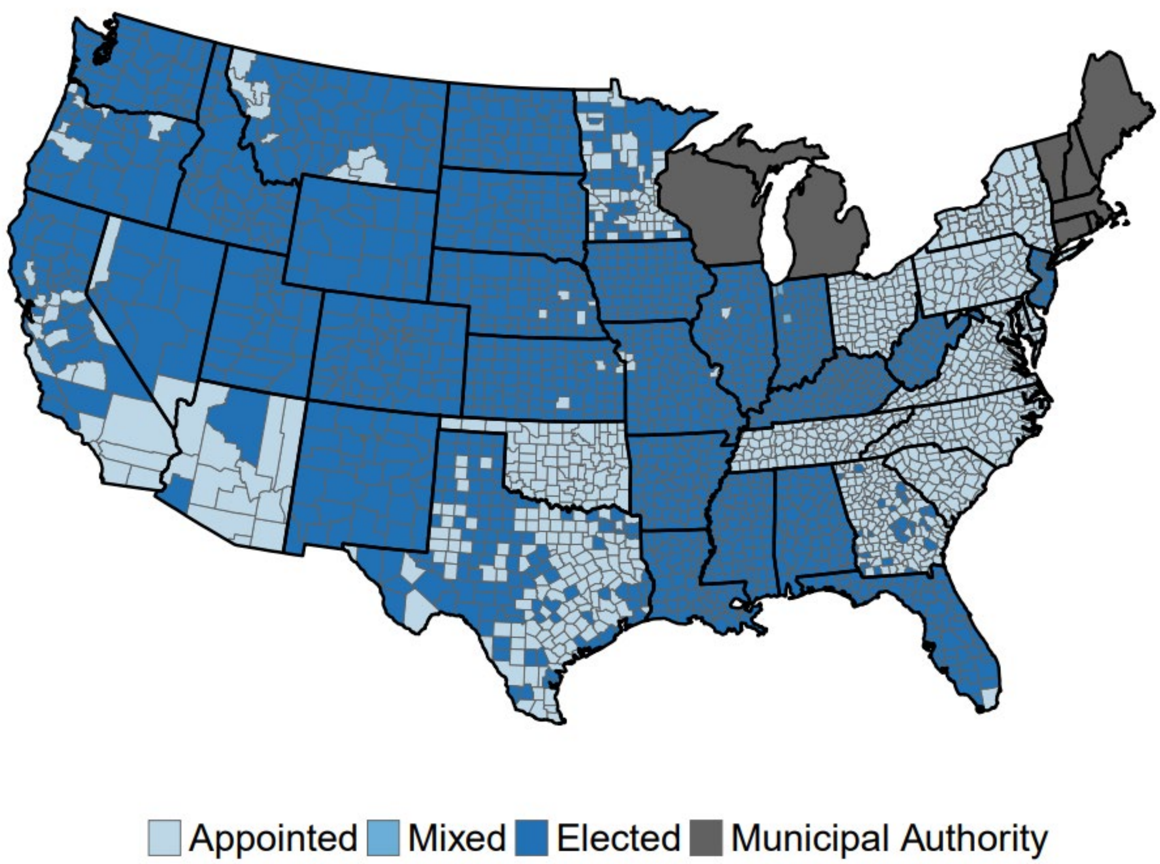
Chapter 2 Appendix

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 (2024). All election jurisdictions in Alaska use appointed officials and all counties in Hawaii use elected officials.



A.1.2 Descriptive Comparison of the Data Sample

Table A.1 compares counties within my sample of 13 states to counties in the 29 states that administer elections at the county level but that have not experienced any changes in selection method since 1960. I use population, racial/ethnic demographics, and region designation from the 2020 census and Democratic presidential vote share, voter turnout, and voter registration from Leip's Election Atlas for the 2020 presidential election. Selection method data for the out-of-sample comparison is from Ferrer and Geyn (2024). Selection method for the in-sample data reflect administration for the 2020 general election.

Overall, few major differences exist between in-sample and out-of-sample counties. Counties within the sample are slightly more populous, less Democratic, and have larger Hispanic populations than counties not in the sample. The sample consists of more Western and Midwestern states and no Northeastern states. Finally, counties in the sample are somewhat less likely to appoint their local election officials.

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

	In Sample (1)	Not In Sample (2)
Population (Thousdands)	113.42	99.25
	(449.41)	(243.57)
Dem Pres Vote Share	0.31	0.35
	(0.16)	(0.16)
Voter Turnout	0.63	0.64
	(0.11)	(0.10)
Voter Registration	0.86	0.90
	(0.10)	(0.11)
Share Non-Hispanic White	0.74	0.77
	(0.21)	(0.19)
Share Black	0.076	0.102
	(0.12)	(0.15)
Share Hispanic	0.14	0.07
	(0.18)	(0.09)
Northeast	0.00	0.11
Midwest	0.42	0.28
South	0.39	0.50
West	0.19	0.11
Share Appointed	0.32	0.42
Num Counties	1117	2016

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

A.1.3 Local Election Official Selection Method Changes by State

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

Table A.2: **Local Election Offical Selection Methods by State.**

State	Elected Entity	Appointed Entity	Counties	Always Appointed	Always Elected	To Appointed	To Elected	Multiple Switches	First Switch	Last Switch
AZ	Election Administrator	Recorder	15	12	0	1	2	0	1984	2020
CA	Clerk	Registrar of Voters / Clerk	58	6	38	14	0	0	1970	2022
GA	Probate Judge	Board of Elections and Registration	159	0	28	129	0	2	1968	2022
IL	Clerk	Election Commission	102	0	93	0	0	1	1974	2016
IN	Clerk	Board of Election and Registration	92	0	89	3	0	0	1994	2020
MN	Auditor	Auditor	87	0	39	48	0	0	1968	2022
MO	Clerk	Election Commission / Director of Elections	115	3	110	1	0	0	1994	1994
MT	Clerk and Recorder	Election Administrator / Clerk and Recorder	56	0	47	8	1	0	1978	2022
NE	Clerk	Election Commissioner	93	2	86	2	0	3	1970	1996
NV	Clerk	Registrar of Voters	17	0	15	2	0	0	1966	1974
OR	Clerk	Elections Manager/Director	36	0	29	6	0	1	1964	1994
TX	Clerk / Tax Assessor	Elections Administrator	254	0	118	119	0	17	1980	2022
WA	Auditor	Elections Director	39	0	38	0	0	1	1970	2009
Totals	-	-	1123	23	730	333	3	25	-	-

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

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

Table A.3 compares appointed and elected counties across the United States using the same data sources described in Section A.1.2 (Ferrer and Geyn 2024). Appointed counties are more than twice as populous on average as elected counties. They are also more Democratic, more racially diverse, and more likely to be located in the Northeast and the South. Appointed counties have slightly lower voter turnout (62% vs. 63%) and voter registration rates (86% vs. 89%) than elected counties. This underscores the importance of using a credible research design to estimate causal effects from observational data.

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

	Appointed (1)	Elected (2)
Population (Thousands)	164.89	63.44
	(465.11)	(221.15)
Dem Pres Vote Share	0.37	0.30
	(0.16)	(0.15)
Voter Turnout	0.62	0.63
	(0.10)	(0.10)
Voter Registration	0.86	0.89
	(0.09)	(0.11)
Share Non-Hispanic White	0.70	0.78
	(0.21)	(0.19)
Share Black	0.13	0.08
	(0.15)	(0.14)
Share Hispanic	0.10	0.10
	(0.14)	(0.14)
Northeast	0.13	0.00
Midwest	0.13	0.41
South	0.67	0.39
West	0.07	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.1.5 Descriptive Comparison of Counties that Switched from Elected to Appointed vs. Always Elected Their Local Election Official

Table A.4 compares "control" counties in the sample–those that always elect their local election officials–to "treated" counties that switch from electing to appointing their election official. Counties that switch from elections to appointments are on average 3.4 times more populous than those that stay elected. They are also more Democratic, tend to have lower turnout and registration rates, are much more racially and ethnically diverse, and are mostly found in South and to a lesser degree the Midwest.

Table A.4: **Description of Elected To Appointed and Always Elected Counties**

	Elected to Appointed (1)	Always Elected (2)
Population (Thousdands)	173.28	51.51
	(499.67)	(122.30)
Dem Pres Vote Share	0.35	0.28
	(0.16)	(0.14)
Voter Turnout	0.60	0.64
	(0.12)	(0.10)
Voter Registration	0.85	0.87
	(0.11)	(0.09)
Share Non-Hispanic White	0.63	0.80
	(0.22)	(0.18)
Share Black	0.15	0.04
	(0.16)	(0.08)
Share Hispanic	0.18	0.12
	(0.21)	(0.16)
Northeast	0.00	0.00
Midwest	0.15	0.57
South	0.76	0.21
West	0.09	0.23
Num Counties	358	730

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

A.2 Robustness Tests

A.2.1 Participation Effects Excluding Midterm Races

Table A.5 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.5: **Appointing Local Election Officials Increases Citizen Participation (Presidential Elections, 1968-2020)**

	Voter Turnout			Registration Rate		
	(1)	(2)	(3)	(4)	(5)	(6)
Appointed	0.020	0.018	0.016	0.010	0.010	0.008
	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)
Counties	1116	1116	1116	942	942	942
Elections	14	14	14	6	7	6
Observations	15571	15571	15571	6577	6577	6577
Outcome Mean	0.57	0.57	0.57	0.85	0.85	0.85
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year x State FEs	Yes	No	No	Yes	No	No
Year x State x Dem vote share FEs	No	Yes	No	No	Yes	No
Year x State x Population FEs	No	No	Yes	No	No	Yes

Robust standard errors clustered by county in parentheses. Voter turnout and registration rate are measured as proportions out of 1. The number of observations is smaller in columns 4-6 because Arizona and Georgia are excluded and because turnout data is available from 1968 but registration data is only available from 1996.

A.2.2 Participation Effects with Alternative Administrative Data

Conflicts arose between administrative and web scrapped data in Texas and the main results included some data imputations for missing cells. Table A.6 shows that the main finding that appointed election officials increase voter participation is robust to alternative coding decisions privileging documents provided by the Texas Secretary of State over archived Secretary of State web pages and removing all data imputations.

Table A.6: **Appointing Local Election Officials Increases Citizen Participation (Even-Year General Elections, 1968-2022, Public Information Act Preferenced)**

	Voter Turnout			Registration Rate		
	(1)	(2)	(3)	(4)	(5)	(6)
Appointed	0.018	0.016	0.014	0.009	0.008	0.008
	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(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 Inclusion of County Time Trends

One way to assuage concerns of pre-trending in event study designs is to incorporate unit-specific linear time trends. County-specific trends allow each county to be on a different linear turnout trajectory, which helps rule out the possibility that treatment and control counties were on different turnout trajectories prior to any switches to appointments. However, unit-specific time trends might also absorb part of the actual treatment effect, especially if switching to appointments causes delayed or increasing turnout benefits (Borusyak and Jaravel 2018; Meer and West 2016; Strezhnev 2024; Wolfers 2006). This could lead to negative weighting and under-identification of the treatment effect.

Table A.7 replicates the specifications in Table 1 in the main analysis with the inclusion of county-specific linear time trends. The effect of appointments on voter turnout remains positive and statistically significant, although substantially attenuated, and the effects on registration rates are now indistinguishable from zero. On average, switching to appointed election officials increases voter turnout by roughly half a percentage point.

The attenuation of effect magnitude is unsurprising considering that Section A.2.6 reveals evidence of dynamic treatment effects, which would lead the inclusion of county time trends to result in underestimation of the true effect.

Table A.8 replicates the expenditure analysis shown in Table 2 but includes county time trends. Here, the results are mostly in line with those shown in the main analysis, though again somewhat attenuated.

Table A.9 replicates the SPAE wait time analysis shown in Table 3 but includes county time trends. Here, the results are less consistent but generally replicate the main findings.

Table A.10 replicates the analysis of differential participation effects between small and large jurisdictions shown in Table 5. The voter turnout results appear robust to the inclusion of unit-specific time trends, but the registration results are not.

Table A.10 replicates the analysis of differential participation effects between jurisdictions with and without newspapers shown in Table 5. As with jurisdiction size, the voter turnout results are somewhat robust in this specification but that registration results are not.

Table A.12 replicates the analysis of the effects of appointments on turnover shown in Table 7. Here, the results are substantially noisier and do not provide evidence that appointments lead to a higher probability of turnover in subsequent years.

Table A.7: **Finding that Appointing Local Election Officials Increases Voter Turnout is Robust to Including County Time Trends**

	Voter Turnout			Registration Rate		
	(1)	(2)	(3)	(4)	(5)	(6)
Appointed	0.006	0.005	0.005	0.000	-0.000	0.001
	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)
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
County Time Trend	Yes	Yes	Yes	Yes	Yes	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.

Table A.8: **Appointing Local Election Officials Increases Election Expenditures (Even-Year General Elections with Time Trends, 2004-2016)**

	Ln(Total Election Expenditures Per Registered Voter)					
	(1)	(2)	(3)	(4)	(5)	(6)
Appointed	0.113	0.107	0.132	0.279	0.298	0.309
	(0.154)	(0.147)	(0.164)	(0.086)	(0.123)	(0.084)
Appointed X Small County				-0.486	-0.584	-0.501
				(0.376)	(0.384)	(0.409)
Counties	434	434	434	432	432	432
Elections	6	6	6	6	6	6
Observations	1929	1929	1929	1920	1920	1920
Outcome Mean	2.25	2.25	2.25	2.25	2.25	2.25
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year x State FEs	Yes	No	No	No	No	No
Year x State x Dem vs FEs	No	Yes	No	No	No	No
Year x State x Pop FEs	No	No	Yes	No	No	Yes
Year x State x Small FEs	No	No	No	Yes	No	No
Year x State x Dem vs x Small FEs	No	No	No	No	Yes	No
County Time Trend	Yes	Yes	Yes	Yes	Yes	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.

Table A.9: **Appointed Local Election Officials May Decrease Voter Wait Times (Even-Year General Elections with Time Trends, 2008-2022)**

	Min Waited	> 10 min	> 30 min	> 1 hr
	(1)	(2)	(3)	(4)
Appointed	-2.842	-0.022	-0.062	0.024
	(1.693)	(0.065)	(0.033)	(0.021)
Counties	798	798	798	798
Respondents	9169	9169	9169	9169
Elections	6	6	6	6
Observations	9169	9169	9169	9169
Outcome Mean	8.43	0.29	0.11	0.04
County FEs	Yes	Yes	Yes	Yes
Year x State FEs	Yes	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes	Yes
County Time Trend	Yes	Yes	Yes	Yes

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

Table A.10: **Appointing Local Election Officials Increases Citizen Participation Especially in Small Counties (Even-Year General Elections with Time Trends, 1968-2022)**

	Voter Turnout			Registration Rate		
	(1)	(2)	(3)	(4)	(5)	(6)
Appointed	0.003	0.001	0.003	0.004	0.003	0.004
	(0.002)	(0.003)	(0.002)	(0.003)	(0.003)	(0.003)
Appointed X Small County	0.006	0.007	0.006	-0.006	-0.008	-0.007
	(0.005)	(0.005)	(0.005)	(0.007)	(0.007)	(0.007)
Counties	1114	1114	1114	941	941	941
Elections	28	28	28	13	13	13
Observations	31104	31104	31104	12203	12203	12203
Outcome Mean	0.50	0.50	0.50	0.84	0.84	0.84
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year x State x Small FEs	Yes	No	No	Yes	No	No
Year x State x Dem vs x Small FEs	No	Yes	No	No	Yes	No
Year x State x Pop FEs	No	No	Yes	No	No	Yes
County Time Trend	Yes	Yes	Yes	Yes	Yes	Yes

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

Table A.11: **Consistent Presence of a Daily Local Newspaper Attenuates the Effect of Appointing Local Election Officials on Citizen Participation (Even-Year General Elections with County Time Trends, 1968-2022)**

	Voter Turnout			Registration Rate		
	(1)	(2)	(3)	(4)	(5)	(6)
Appointed	0.006	0.006	0.005	-0.004	-0.005	-0.005
	(0.004)	(0.004)	(0.004)	(0.005)	(0.006)	(0.006)
Appointed X Newspaper	-0.005	-0.007	-0.003	0.012	0.016	0.012
	(0.006)	(0.006)	(0.006)	(0.008)	(0.008)	(0.008)
Counties	979	979	979	824	824	824
Elections	14	14	14	6	6	6
Observations	13661	13661	13661	5751	5751	5751
Outcome Mean	0.58	0.58	0.58	0.85	0.85	0.85
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year x State x Newspaper FEs	Yes	No	No	Yes	No	No
Year x State x Dem vs x Newspaper FEs	No	Yes	No	No	Yes	No
Year x State x Pop x Newspaper FEs	No	No	Yes	No	No	Yes
County Time Trend	Yes	Yes	Yes	Yes	Yes	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.

Table A.12: **Effect of Appointing Local Elections Officials on Turnover (with County Time Trends, 2004-2022)**

	Election Official Turnover
	(1)
Appointed	-0.022
	(0.043)
Counties	1113
Elections	3
Observations	10881
Outcome Mean	0.18
County FEs	Yes
Year x State FEs	Yes
County Time Trend	Yes

Robust standard errors clustered by county in parentheses.

A.2.4 Exploring State and Office Heterogeneity

This section shows evidence that the main result holds across states and offices. Table A.13 estimates the effects of appointing election officials on voter participation separately for each of the four states with at least 10 counties that have changed their election official selection method since 1960. Those states are California, Georgia, Minnesota, and Texas. The results reveal precisely estimated and substantively meaningful effects for Georgia, Minnesota, and Texas. The magnitude of the effect on turnout is greater in Georgia and Minnesota than in Texas. The point estimate for CA is negative, although it is imprecisely estimated.

Table A.13: **Appointing Local Election Officials Increases Voter Turnout in Multiple States (Even-Year General Elections, 1968-2022)**

	Voter Turnout			
	(1)	(2)	(3)	(4)
Appointed	-0.008	0.022	0.027	0.016
	(0.012)	(0.005)	(0.008)	(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. A few Texas counties use elected tax assessors as their election official. Auditor is also a fairly common position. All elected election officials in Minnesota and Washington are auditors, as well as a small number of counties in California. Finally, probate judges are the elected election officials in Georgia. Table A.14 shows that participation increases when appointed officials (the omitted category) replace elected auditors, clerks, and probate judges. The increase is larger when probate judges and auditors are replaced, and somewhat smaller when clerks are replaced. The point estimate for tax assessors is negative but imprecisely estimated, as it relies on a relatively small set of observations.

Table A.14: **Switching from Elected Auditors, Clerks, and Probate Judges to Appointed Officials Increases Citizen Participation (Even-Year General Elections, 1968-2022)**

	Turnout (1)	Registration (2)
Tax Assessor	-0.008	0.008
	(0.012)	(0.012)
Auditor	0.022	0.047
	(0.007)	(0.008)
Clerk	0.014	0.008
	(0.005)	(0.005)
Probate Judge	0.022	0.006
	(0.005)	(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.

A.2.5 Results by Clerk Selection Method Reform Mechanism

One threat to causal inference is that reforms caused by some specific mechanism—state legislature, county legislature, and/or county referendum—are not exogenous to an increase in citizen participation. This seems most likely for referenda. Perhaps initial voter participation in a referendum that caused a change in clerk selection method spurs more turnout in future elections due to increased political efficacy. Or, perhaps the places with stronger cultures of direct democracy are more likely to have a referendum on the matter. Another scenario is that counties with local backing in the change are more likely to equip their newly appointed clerk with the tools to succeed or choose reform at the moment when it is most needed, compared with places where the state legislature initiates the reform. In Table A.15, 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.15: **Appointing Local Election Officials Increases Citizen Participation Across Reform Mechanisms**

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

Robust standard errors clustered by county in parentheses.

The results show that both county and state legislature-initiated reform mechanisms lead to a boost in turnout. Counties whose legislatures decide to switch from elected to appointed clerks see 1.4 percentage points higher turnout in future presidential elections, on average. The effect is almost double—2.3 percentage points—when states initiate the reform. The result for county referendums is slightly positive but is imprecisely estimated. In short, the results hold across multiple reform mechanisms.

One related concern is that the reforms to clerk selection method that were initiated as part of a county charter suffer from similar endogeneity issues. The bundled treatment nature of these cases could also mean that the turnout effects are due to other changes in county governance that happened to coincide with the change to selection method. Table A.16 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 1. 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.16: **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	0.016	0.014	0.009	0.008	0.008
	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(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.2.6 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. State dummies are used as covariates in these estimates to correct for state-specific trending in voter turnout. Dynamic effects are visualized in Figure A.2, cohort effects are visualized in Figure A.3, and time period effects are visualized in Figure A.4.

As seen in Figure 4 in the main analysis, the effect of appointments on voter turnout appears to increase over time for counties that switch to appointed administrators, relative to counties with elected officials. Two potential explanations exist for this: appointed officials increasingly outperform elected officials as their tenure lengthens, or the value of appointed officials over elected ones has grown over time. In the former scenario, institutional learning effects and start-up costs of switching selection methods mean appointed officials need the practice of administering a few elections to realize their full potential compared to elected officials. Recent work has found that voter wait times may increase after the turnover of a local election official, although turnout rates do not dip when a change of leadership takes place (Ferrer and Thompson 2024). In the latter scenario, the declining ability of voters to adequately select and sanction elected officials combined with the increasing technical demands of the job and growing recruitment problems create a bigger gap between elected and appointed officials over time.[28] The evidence I present regarding the differential effect of selection method by the presence of a local newspaper in Table 6 supports this theory.

[28] 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**. 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 and with state dummy covariates. 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.

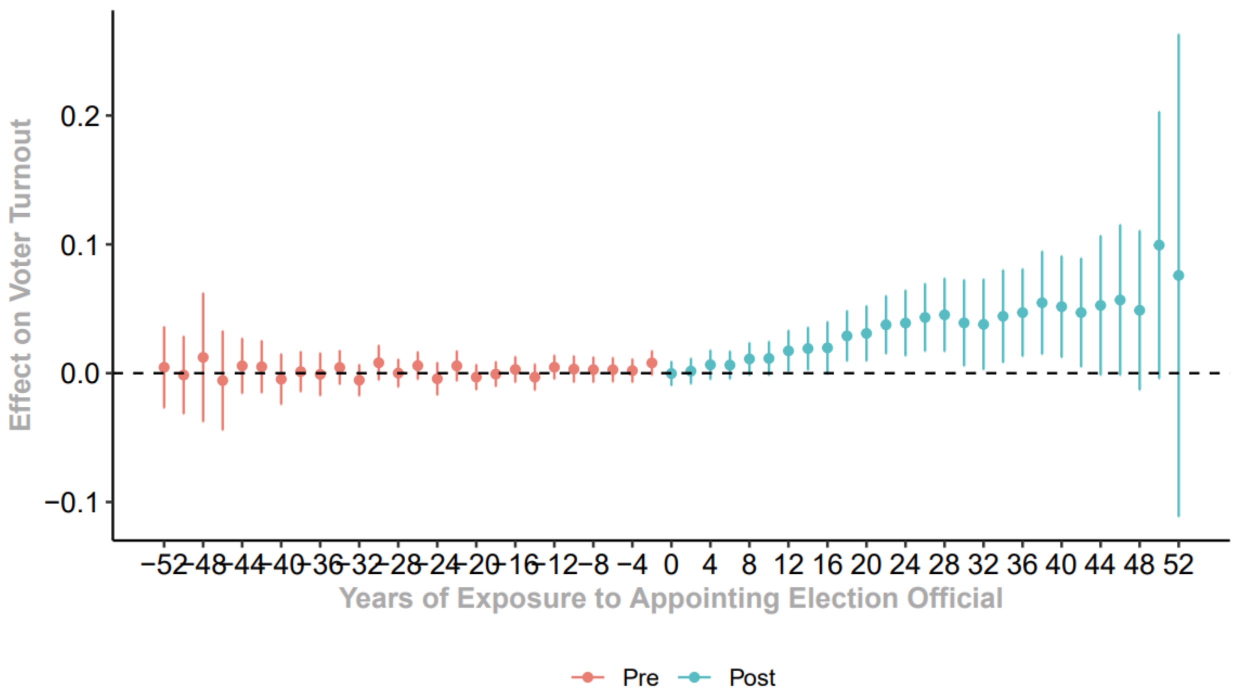


Figure A.2 shows a fairly large increase in the effect on turnout several elections far after the initial switch, to about 5 percentage points. This estimator accounts for heterogeneous treatment effects but does not correct for pre-trending so should be interpreted cautiously. What is more plausible is the increase in effect magnitude shown in the generalized synthetic control (Figure 4 in the main analysis), which is an approximately one additional percentage point boost in turnout three elections after the switch to appointments.

Figure A.3 displays cohort treatment effects of the Callaway and Sant'Anna (2021) estimator. Although the estimates are noisy, they suggest that earlier adopters of appointed election officials have experienced stronger overall treatment effects than more recent adopters. Figure A.4 displays time period effects of switching to appointing election officials. The greater effect of earlier adopters appears to be mostly due to long-term accumulation rather than a diminishing instantaneous effect over time.

Figure A.3: **Average Effect of Appointed Election Officials on Voter Turnout by Cohort Group**. Each point is an estimate of the average group effect of appointing election officials on voter turnout for counties that switch in the given cohort year, with state dummy covariates. The lines above and below each point represent 95-percent confidence intervals. Estimates are from the Callaway and Sant'Anna (2021) estimator for dynamic two-way fixed effects designs, which corrects for bias due to heterogeneous treatment effects.

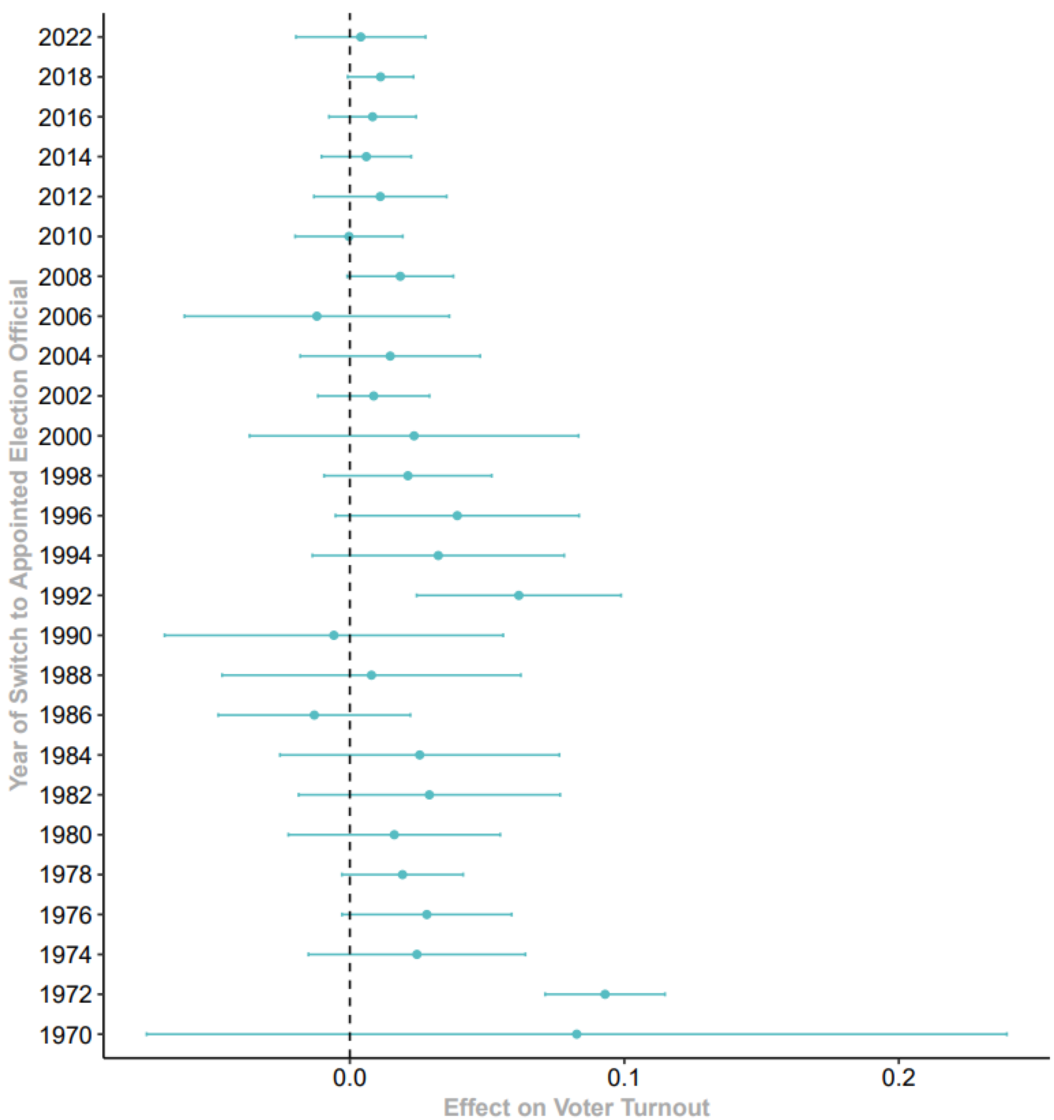
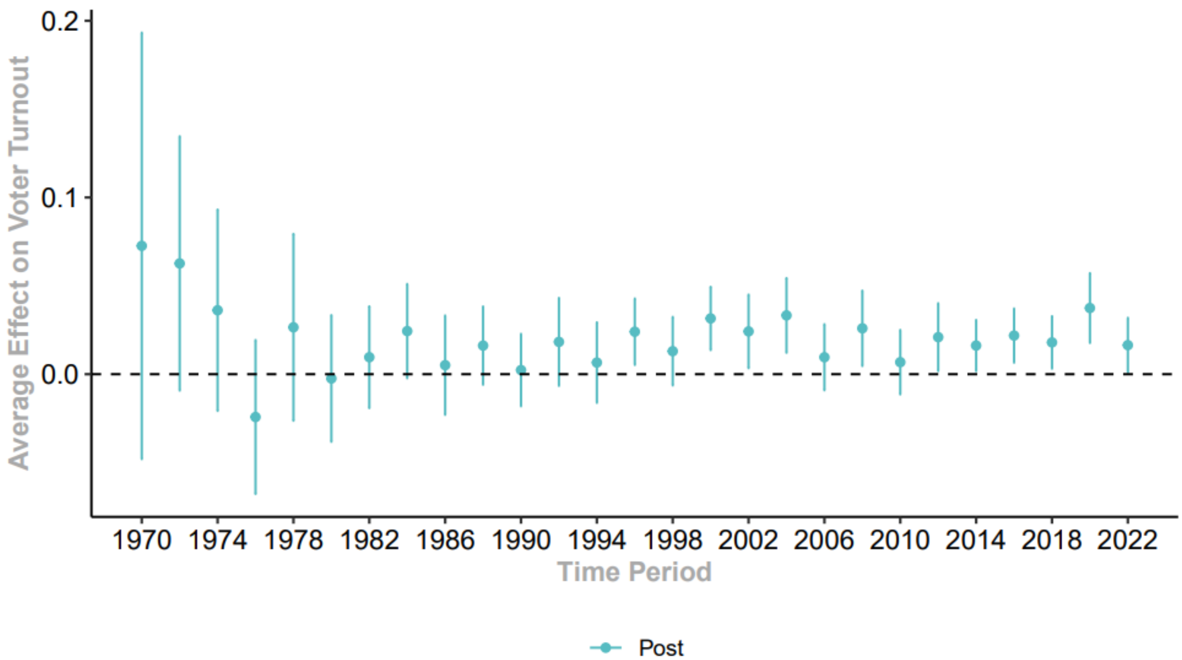


Figure A.4: **Average Effect of Appointed Election Officials on Voter Turnout by Time Period**. Each point is an estimate of the average time period effect of appointing election officials on voter turnout, with state dummy covariates. The lines above and below each point represent 95-percent confidence 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.2.7 Are the Results an Artifact of the Jim Crow South?

One concern is that registration and turnout rates of African-Americans in Southern states were artificially low in the earlier periods of the dataset due to the lingering effects of racially targeted barriers to the ballot box. Even though the Voting Rights Act passed in 1965, African-American registration rates in the South continued to trail behind those of white voters until many decades later (Fraga 2018). For instance, African American and white registration rates in Louisiana did not achieve parity until 2000 (Keele, Cubbison, and White 2021). If counties that switch to appointments are more likely to have large African-American populations (Komisarchik 2018), then the inclusion of these earlier years in the dataset could confound the relationship between appointments and voter turnout. Table A.17 displays three truncated cuts of the data: starting with the 1980 presidential election, the 1992 presidential election, and the 2000 presidential election. The main analysis displayed in Table 1 relies on turnout data beginning with the 1968 presidential election. Because registration data is only available from 1996, I focus on voter turnout here. The point estimates do grow similar when older data is discarded. However, in all specifications the point estimates are substantively large and statistically distinguishable from zero. In the most restrictive analysis, which only uses data from 2000 onwards, counties that switch to appointed clerks are estimated to boost turnout by half a percentage point.

Table A.17: **Finding that Appointing Local Election Officials Increases Citizen Participation is Robust to Alternative Year Cutoffs**

	Voter Turnout			
	(1)	(2)	(3)	(4)
Appointed	0.013	0.012	0.006	0.005
	(0.003)	(0.003)	(0.002)	(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.18 displays results from additional estimators designed to help overcome the identification issues of the staggered adoption two-way fixed effects design. All estimators include county and year by state fixed effects. Column 1 is the same specification found in column 1 of Table 1. Column 2 excludes counties that switch from appointed to elected clerks, as they can be a source of bias. In the third specification, counties that are always "treated"—in this case, those that use appointments from the beginning of data availability—are excluded to avoid problematic comparisons in the estimation. The last two columns show the results of stacked difference-in-difference estimates (Cengiz et al. 2019). The point estimates are consistent and precisely estimated across all specifications.

I employ the (Imai, Kim, and Wang 2023) strategy of matching treated and control units. The key advantage of this procedure is that it allows me to match both on pre-treatment voter turnout trajectory and exact match on state, state by pre-treatment population, or state by Democratic vote share. I only include counties that either are elected throughout the dataset and those that start elected and switch to appointed. I do not include counties with multiple switches between elections and appointment. I match on eight elections of pre-treatment data, use the mahalanobis refinement method, and allow up to 10 control units to match with each treated unit. Table A.19 shows the results of this exercise.

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

I also use the (Imai, Kim, and Wang 2023) matching strategy for two other tests: voter registration rate in Table A.20 and election administration budget expenditures in Table A.21. The matching results for registration rates are also in line with those found in the main analysis. The matching results for the expenditure analysis is uninformative due to the small number of matches made.

Table A.18: **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	0.020	0.020	0.028	0.017
	(0.003)	(0.003)	(0.003)	(0.005)	(0.004)
Counties	1116	1085	1062	1062	873
Elections (avg)	28	28	28	28	28
Observations	31146	30366	29735	560421	153503
Outcome Mean	0.50	0.51	0.51	0.53	0.53
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.19: **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	0.013	0.015
	(0.017)	(0.017)	(0.017)
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.

Table A.20: **Finding that Appointing Local Election Officials Increases Voter Registration Is Robust to Imai et al. 2024 Matching Estimator (Even-Year General Elections, 1996-2020)**

	Registration Rate		
	(1)	(2)	(3)
Appointed	0.011	0.010	0.009
	(0.009)	(0.009)	(0.009)
Matches	124	123	124
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 4 elections after treatment and matches over 4 elections prior to treatment. Matching is done using mahalanobis distance.

Table A.21: **Effect of Appointing Local Election Officials on Election Expenditures using Imai et al. 2024 Matching Estimator (Even-Year General Elections, 2002-2020)**

	Ln expend per reg		
	(1)	(2)	(3)
Appointed	-0.027	0.096	-0.063
	(0.443)	(0.437)	(0.462)
Matches	10	10	9
County FEs	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes
Pre-treatment election expenditure 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 2 elections after treatment and matches over 2 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'Haultfœuille (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. The results are displayed in Tables A.22, A.23, A.24, and A.25. The de Chaisemartin and D'Haultfœuille (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.18. First, always treated units are removed from the dataset (i.e., counties that have appointed their election officials since at least 1960). This eliminates a handful of counties that were extremely early adopters of appointed election administrators. Next, each county's time period of first treatment is identified. The counties that switch from appointment to election are assigned to treatment even after their switch. Finally, those counties that are never treated (i.e., have always had elected election officials since 1960) are separated out as the "true control" by which each cohort can be compared with. Doing so avoids negative weights, thereby addressing the problems introduced by heterogeneous treatment and timing effects.

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

Table A.22: **Main Finding that Appointing Local Election Officials Increases Voter Turnout is Robust to Alternate Specifications - California**

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

Robust standard errors clustered by county in parentheses.

Table A.23: **Main Finding that Appointing Local Election Officials Increases Voter Turnout is Robust to Alternate Specifications - Georgia**

	Voter Turnout		
	Two-Way FEs	de Chaisemartin and D'Haultfoeuille	Callaway and Sant'Anna
	(1)	(2)	(3)
Appointed	0.022	0.006	0.050
	(0.005)	(0.004)	(0.012)
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.24: **Main Finding that Appointing Local Election Officials Increases Voter Turnout is Robust to Alternate Specifications - Minnesota**

	Voter Turnout		
	Two-Way FEs	de Chaisemartin and D'Haultfoeuille	Callaway and Sant'Anna
	(1)	(2)	(3)
Appointed	0.027	0.011	0.066
	(0.008)	(0.005)	(0.007)
Counties	87	87	86
Elections	28	28	28
Observations	2436	1704	2784
Outcome Mean	0.64	0.64	0.64
County FEs	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes

Robust standard errors clustered by county in parentheses.

Table A.25: **Main Finding that Appointing Local Election Officials Increases Voter Turnout is Robust to Alternate Specifications - Texas**

	Voter Turnout		
	Two-Way FEs	de Chaisemartin and D'Haultfoeuille	Callaway and Sant'Anna
	(1)	(2)	(3)
Appointed	0.016	0.001	0.021
	(0.005)	(0.005)	(0.009)
Counties	253	253	236
Elections	28	28	28
Observations	7084	4929	8128
Outcome Mean	0.43	0.43	0.43
County FEs	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes

Robust standard errors clustered by county in parentheses.

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

I investigate the validity of the parallel trends assumption for difference-in-difference specifications using the Dube et al. (2022) local projections event studies estimator. I make a series of pooled two-period two-group comparisons and estimate period-by-period effects, eliminating biases due to heterogeneous treatment effects. However, biases due to parallel trending remain a possibility. Figure A.5 plots the results for voter turnout. The x-axis marks the even-year general elections before and after a switch in local administration, with 0 marking the first election under an appointed clerk. Each point estimate is the difference in the change in turnout from the previous election of counties with appointed election officials rather than elected ones, at x federal elections before or after each county's actual switch. Negative coefficients in the left half of the graph suggest pre-trending. In other words, counties that switch to appointing clerks may already have been on a trajectory of higher turnout. The estimated effect becomes positive one even-year federal election after adoption of appointments. The effect on turnout appears to increase after counties switch their method of clerk selection, a phenomenon I explore in Section A.2.6.

I examine the parallel trends assumption for the test on registration rates in A.6. Some evidence of parallel trending exists, though a positive effect first appears in the first election after counties adopt appointments. Due to the more limited span of the registration data, I am unable to employ a general synthetic control design. Therefore, the main results for registration rates should be viewed with some degree of caution.

I examine the parallel trends assumption for the test on turnover rates in A.7. Point 0 shows an extremely large positive effect on turnover because in the vast majority of cases, switching to an appointed elections official forced turnover. Besides this expected aberration, I find little evidence of pre-trending.

Finally, Figure A.8 examines the validity of the parallel trends assumption for the effect of appointments on expenditures. I find no evidence of pre-trending and imprecisely estimated but positive coefficients after a county switches to appointments. Additionally, the increase in expenditures is not instantaneous with the switch in selection method (period 0), but rather begins in the election after this switch. This is an indication that the increase in expenditures is caused by the appointed election official rather than some confounding factor causing both the selection method to change and election expenditures to increase.

Figure A.5: **Dube et al. (2022) Local Projections Difference-in-Differences Estimate of Effect of Appointing an Election Official on Voter Turnout**. Year 0 is the even-year general election after a county's first switch from electing to appointing an election official. Each point is the estimated effect of appointing an election official on voter turnout, at x federal elections of exposure since first selecting the official via appointment. The bar lines above and below each point represent 95-percent confidence intervals. Estimates use the Dube et al. (2022) local projections difference-in-differences estimator for dynamic heterogeneous-robust difference-in-difference designs, which corrects for bias due to heterogeneity in year and county treatment effects.

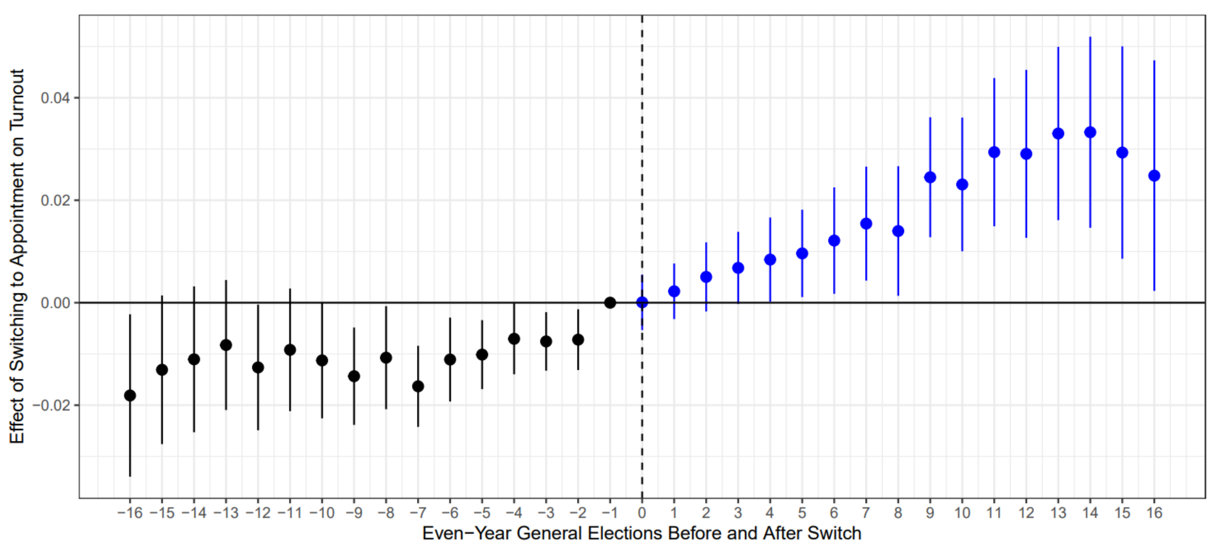


Figure A.6: **Dube et al. (2022) Local Projections Difference-in-Differences Estimate of Effect of Appointing an Election Official on Registration**. Year 0 is the even-year general election after a county's first switch from electing to appointing an election official. Each point is the estimated effect of appointing an election official on voter registration, at x federal elections of exposure since first selecting the official via appointment. The bar lines above and below each point represent 95-percent confidence intervals. Estimates use the Dube et al. (2022) local projections difference-in-differences estimator for dynamic heterogeneous-robust difference-in-difference designs, which corrects for bias due to heterogeneity in year and county treatment effects.

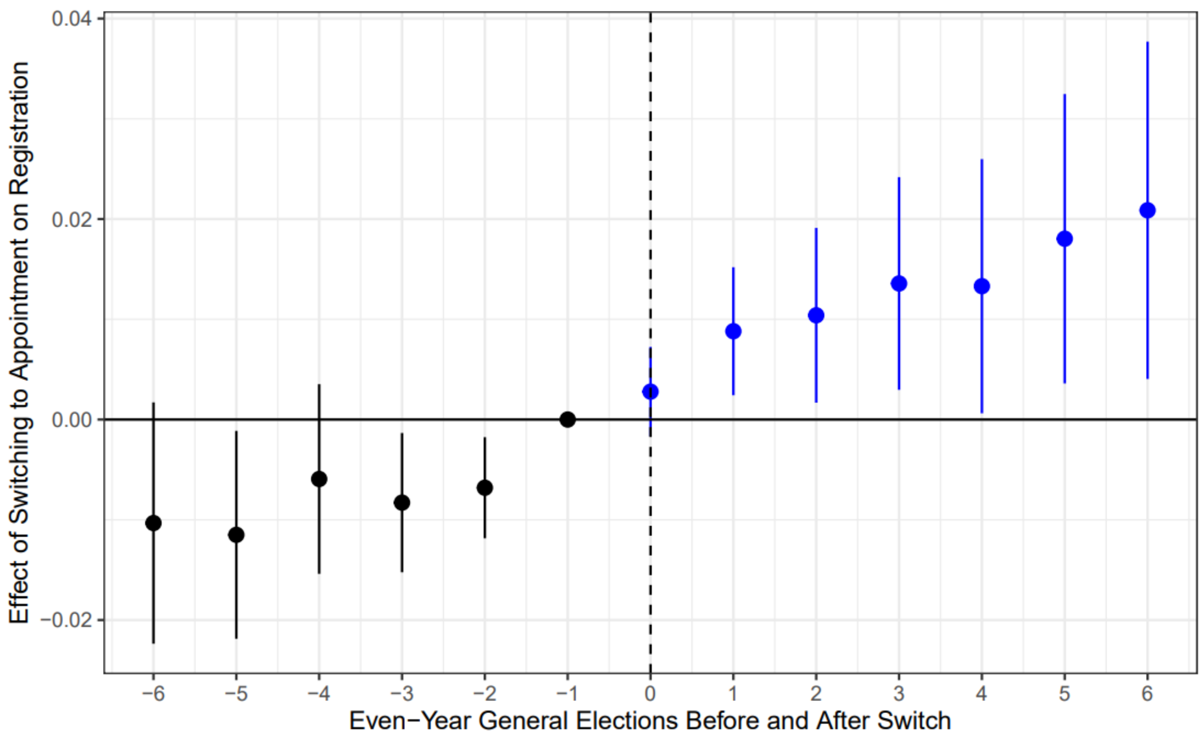


Figure A.7: **Dube et al. (2022) Local Projections Difference-in-Differences Estimate of Effect of Appointing an Election Official on Turnover**. Year 0 is the even-year general election after a county's first switch from electing to appointing an election official. Each point is the estimated effect of appointing an election official on the election official turnover rate over a two-year period, at x federal elections of exposure since first selecting the official via appointment. The bar lines above and below each point represent 95-percent confidence intervals. Estimates use the Dube et al. (2022) local projections difference-in-differences estimator for dynamic heterogeneous-robust difference-in-difference designs, which corrects for bias due to heterogeneity in year and county treatment effects.

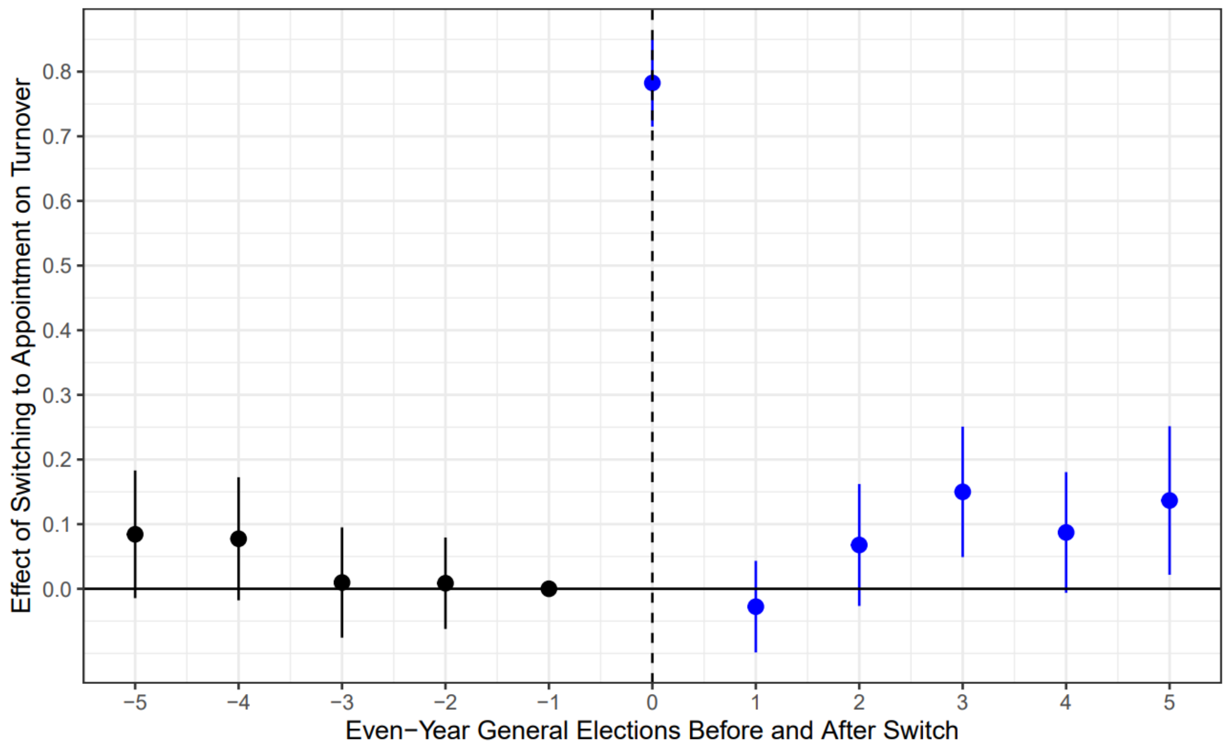
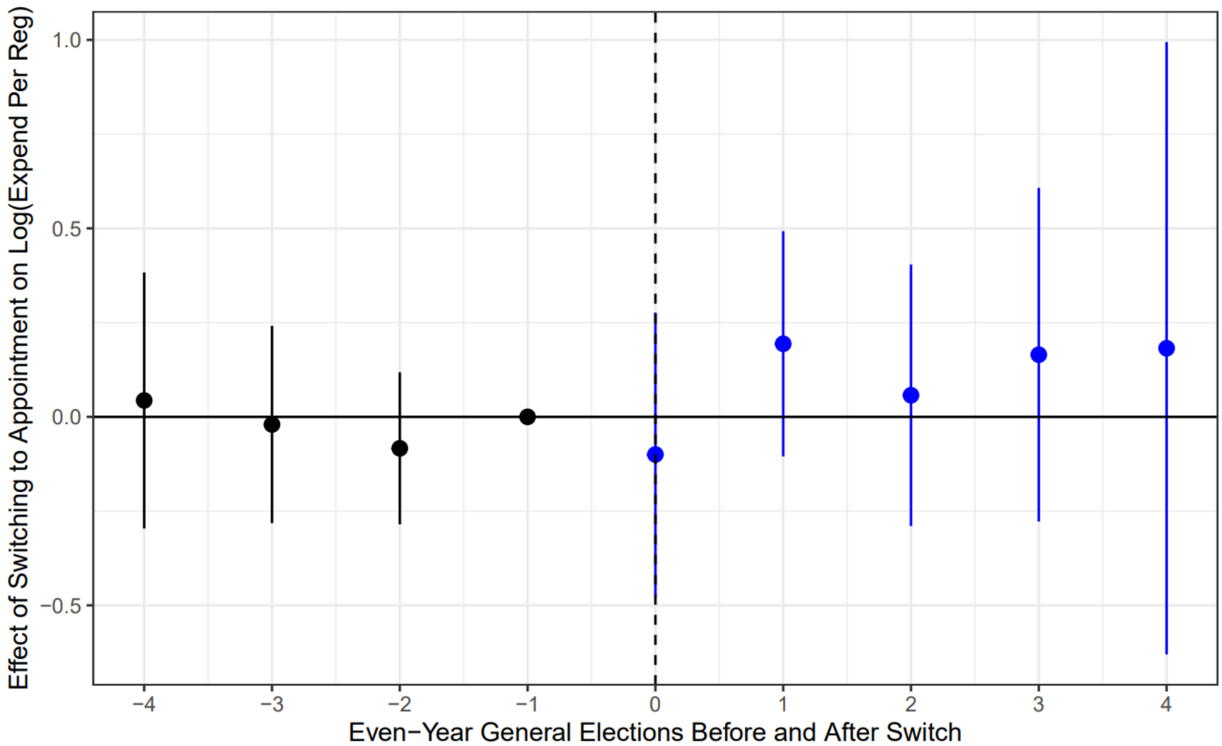


Figure A.8: **Dube et al. (2022) Local Projections Difference-in-Differences Estimate of Effect of Appointing an Election Official on Logged Expenditures Per Registrant**. Year 0 is the even-year general election after a county's first switch from electing to appointing an election official. Each point is the estimated effect of appointing an election official on logged election expenditures per registered voters, at x elections of exposure since first selecting the official via appointment. The bar lines above and below each point represent 95-percent confidence intervals. Estimates use the Dube et al. (2022) local projections difference-in-differences estimator for dynamic heterogeneous-robust difference-in-difference designs, which corrects for bias due to heterogeneity in year and county treatment effects.



A.3.4 Generalized Synthetic Control Regression Output

Table A.26 displays regression output from the Xu (2017) generalized synthetic control estimator, comparing treatment and control counties with similar pretreatment turnout histories. This method relies on strictly fewer assumptions than the difference-in-differences estimator and allows for a relaxation of the parallel trends assumption. The point estimate in Table A.26 is 0.8%. This is smaller and less precisely estimated than those found in Table 1 in the main analysis, but it is still a substantively significant effect for even-year general elections.

Table A.26: **Main Finding that Appointing Local Election Officials Increases Voter Turnout is Robust to Generalized Synthetic Control Estimator**

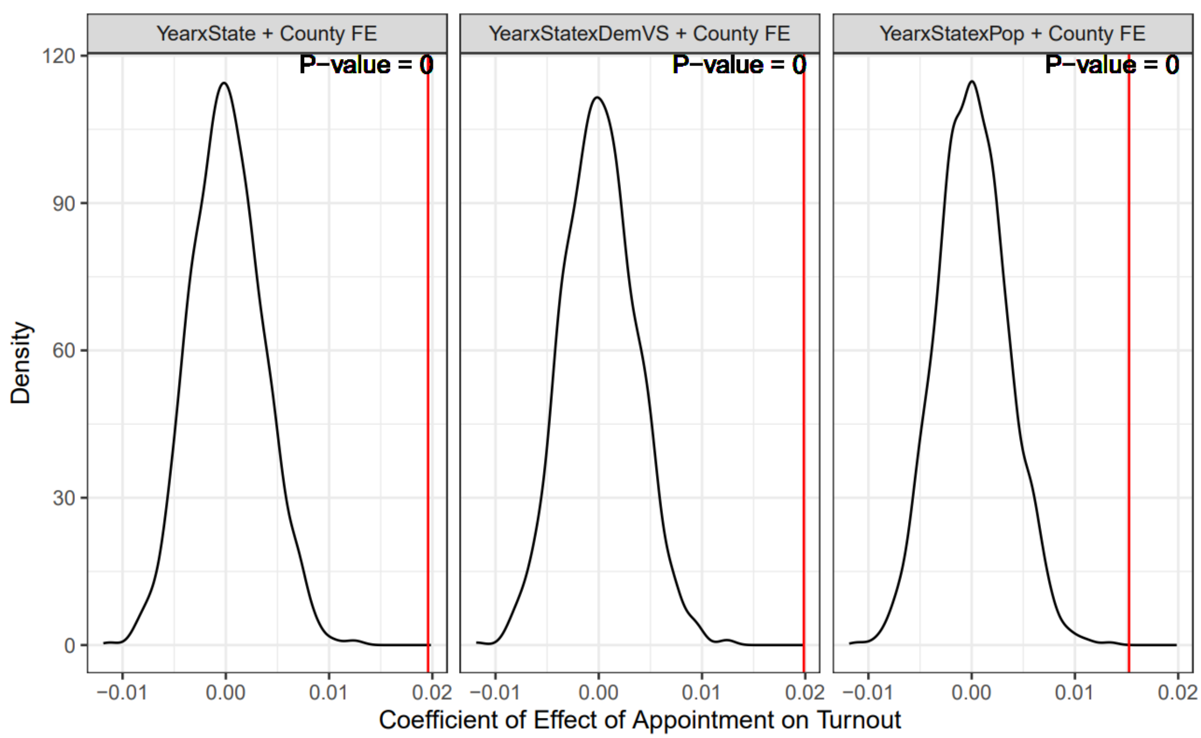
	Voter Turnout (1)
Appointed	0.008 (0.006)
Counties	1042
Elections	28
Observations	29176
Outcome Mean	0.50

Generalized synthetic control method matches treated and control counties on pretreatment voter turnout.

A.3.5 Randomization Inference Additional Output

Figure A.9 shows the distribution of point estimates of the effect of appointments on voter turnout which counties switch to appointed local election officials and when they switch is randomly permuted. This procedure shows that it is extremely unlikely to observe an effect of appointments on voter turnout as large or larger than that observed by chance alone.

Figure A.9: **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.27 displays the results of this placebo test. The first four columns individually test registration rates for the four states with at least 10 counties that have switched between electing and appointing their local election official: California, Georgia, Minnesota, and Texas. We should observe greater effects of appointed administration on registration rates in California, Minnesota, and Texas than in Georgia. The point estimate is smallest for Georgia, although the magnitude of the effect is similar across Georgia, Minnesota, and Texas.

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

Table A.27: **Appointing Election Officials Boosts Registration Rates More when Their Duties Specifically Include Registration (Even-Year General Elections, 1996-2022)**

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

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

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

Georgia, Missouri, and Texas's long histories of race-based disenfranchisement, the strong association between race and partisanship (Abramowitz and McCoy 2019; Carmines and Stimson 1989), and the present efforts of Republican politicians to increase barriers to the ballot box all contribute to the possibility that adverse policy responsiveness rather than quality differences could explain the divergence between appointed and elected election officials. I distinguish between the effects of selection method and partisanship by utilizing changes in Georgia, Montana, and Washington counties between partisan elections, nonpartisan elections, and appointments of election officials. Table A.28 displays estimates of voter turnout separating out the effects of appointments and partisan elections, with the omitted category elected nonpartisan elections. The results provide strong evidence that elections themselves, and not the partisan nature of the office, drive the main results on voter turnout. All of the estimated positive effect on turnout is observed for a switch from elected to appointed administration, whereas the effect of switching between partisan and nonpartisan administration is negative and indistinguishable from zero.

Table A.28: **Appointments, Rather than Partisanship, Drive the Effects on Voter Turnout (Even-Year General Elections, 1968-2022)**

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

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

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

If the quality of selection and sanctioning of local election officials is indeed higher for those that are appointed, this leads to the possibility that appointed clerks might be selected and/or more successfully pursue strategies that benefit a certain political party over another. This would significantly alter the normative implications of the paper's findings. Appointments could lead to better-administered elections and higher voter participation. But they could also lead to officials who try to skew election results in their party's favor. Recent scholarship has found that Democratic and Republican clerks administer elections in similar ways and produce similar partisan outcomes and voter turnout (Ferrer, Geyn, and Thompson 2024). Shepherd et al. (2021) find no evidence that the party of appointed clerks in North Carolina shapes their decisions on polling place allocation. Here I examine whether appointed local election officials act in ways that benefit the majority party of their principals.

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

Table A.29 shows difference-in-differences regressions testing the effect of a switch to a Democratic party controlled appointing body on three outcomes: Democratic presidential 2-party vote share (columns 1-2), Democratic gubernatorial 2-party vote share (columns 3-4), and share of registrants that are Democrats (columns 5-6). All specifications include state-by-year fixed effects to account for differential partisan trending in each state. The even columns also include county linear time trends, because counties that switch from Republican to Democratic local leadership are likely trending in a Democratic direction. In other words, parallel trend concerns are particularly acute in this analysis. The inclusion of unit-specific time trends means that the outcomes are tested in excess of the underlying partisan trend specific to each county. Concerns exist that including unit linear time trends in a two-way fixed effects analysis might absorb potential treatment effects, biasing the analysis downward (Borusyak and Jaravel 2018; Wolfers 2006)—concerns I more fully explore in Section A.2.3. However, in this dataset many counties switch back and forth between Democratic and Republican control, making this a less pressing worry.

Table A.29: **Appointed Local Election Officials Do Not Clearly Benefit The Party That Appoints Them (County-Level Shifts in AZ and PA, 2000-2022)**

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

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

Two pictures emerge from these results depending on if the county time trend is included. Columns 1, 3, and 5 indicate that switching from a Republican-controlled appointing body to a Democratic-controlled body increases Democratic presidential vote share by 2.3 percentage points, increases Democratic gubernatorial vote share by 1.7 percentage points, and increases Democratic share of registrants by 2.7 percentage points. However, the odd columns show this to be an artifact of pre-trending: counties that start electing Democrats majorities to their county legislature or a Democrat to their county chief executive also become more favorable to Democratic state and national candidates. The largest point estimate including unit-specific linear time trends (columns 2, 4, and 6) is three-tenths of a boost in Democratic gubernatorial vote share and Democratic share of registrants, but both fall well within a 95% confidence interval. The coefficient for Democratic presidential vote share is slightly negative. In sum, the evidence is consistent with no effect of appointed election officials benefiting their principals' majority party but not dispositive.

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

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

Table A.30 examines whether switching from an elected to an appointed election official leads to a larger boost in participation in jurisdictions that are more Democratic. "Democratic" is measured as being in the top half of a state's Democratic vote shares for the 1968 presidential election, the last pre-treatment election year. The top row shows the effect of switching to appointed election officials in more Republican-leaning jurisdictions. The bottom row shows the additive effect of switching to an appointed election official in Democratic-leaning counties.

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

Table A.30: **Appointing Local Election Officials Has A Similar Effect In Democratic- and Republican-Leaning Counties (Even-Year General Elections, 1968-2022)**

	Voter Turnout			Registration Rate		
	(1)	(2)	(3)	(4)	(5)	(6)
Appointed	0.016	0.015	0.013	0.016	0.015	0.015
	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.006)
Appointed X Democratic County	0.003	0.003	0.001	-0.012	-0.012	-0.012
	(0.006)	(0.006)	(0.006)	(0.008)	(0.008)	(0.008)
Counties	1109	1109	1109	936	936	936
Elections	28	28	28	13	13	13
Observations	30964	30964	30964	12138	12138	12138
Outcome Mean	0.51	0.51	0.51	0.84	0.84	0.84
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year x State x Dem FEs	Yes	No	No	Yes	No	No
Year x State x Dem vs FEs	No	Yes	No	No	Yes	No
Year x State x Pop x Dem FEs	No	No	Yes	No	No	Yes

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

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

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

Table A.31 tests whether appointed election officials were more likely to apply for the Center for Tech and Civic Life's (CTCL) COVID-19 Response Grant program in September 2020.[29] I obtain data on CTCL applications as well as population, income, metro, non-Hispanic white share, COVID death rate, social distancing share, and National Association of Counties membership covariate data from Lal and Thompson (2024) and follow Lal and Thompson's specification strategy. I combine this data with 2020 election official selection methods across all counties ((Ferrer and Geyn 2024) and employ state fixed effects. In total, the data covers 37 states and over 2,600 counties.

A bivariate specification is shown in column 1, comparing the likelihood that appointed and elected counties within the same state applied for the CTCL grant. Counties with appointed election officials were 21 percentage points more likely to apply for the grant than counties that elect their election official. Column 2 controls for lagged Democratic presidential vote share to account for skepticism toward the grant among some Republicans.[30] This attenuates the estimated effect to 16 percentage points. Column 3 adds controls for logged county population and logged county median income. Column 4 adds an indicator for urban and suburban counties and a control for the share of the county that is non-Hispanic white. Column 5 adds controls for COVID death rate and the share of respondents to the Nationscape survey who reported always complying with recommended social

[29] https://www.techandciviclife.org/10-facts-about-ctcl-grants/

[30] https://apnews.com/article/elections-facebook-mark-zuckerberg-d034c4c1f5a9fa3fb02aa9898493c708

distancing in Fall of 2020. Column 6 adds an indicator for county membership in the National Association of Counties. The effect magnitude is consistent at 7 percentage points across columns 3 through 6. This shows that appointed officials were more likely to take advantage of this alternative source of funding that their elected counterparts in similar counties.

Table A.31: **Appointed Election Officials Were More Likely to Apply For CTCL Funding In 2020**

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

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

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

Table A.32 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 0.27 percentage points on average. Column 4 shows that the effect appears concentrated in small counties, where a doubling of election expenditures increases voter turnout by 0.39 percentage points on average. There does not appear to be any relationship between election expenditures and turnout in populous jurisdictions.

Table A.32: **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	0.004	0.002	-0.001	-0.003	-0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
Ln expend per reg X Small County				0.006	0.008	0.005
				(0.004)	(0.005)	(0.004)
Counties	434	434	434	432	432	432
Elections	6	6	6	6	6	6
Observations	1929	1929	1929	1920	1920	1920
Outcome Mean	0.50	0.50	0.50	0.50	0.50	0.50
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year x State FEs	Yes	No	No	No	No	No
Year x State x Dem vs FEs	No	Yes	No	No	No	No
Year x State x Pop FEs	No	No	Yes	No	No	Yes
Year x State x Small FEs	No	No	No	Yes	No	No
Year x State x Dem vs x Small FEs	No	No	No	No	Yes	No

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

A.4.3 Using EAVS Data to Examine Administrative Outcomes

Following Ferrer, Geyn, and Thompson (2024), I use the U.S. Election Commission's Election Administration and Voting Surveys to examine a number of election administration outcomes. I combine all past available surveys and extensively clean the data to correct for data irregularities and errors in the raw data (Stewart 2018). Table A.33 displays the results of a two-way fixed effects regression of appointing election officials on the following county-level variables: number of polling places per 1,000 residents, provisional votes share, provisional rejection rate, absentee rejection rate, and registration removal rate. The point estimates for polling places, provisional rejection rates, and registration removal rates are all consistent with a positive effect on voter turnout, but the point estimates are small and the coefficient for absentee rejection rate is in the opposite direction. In short, no strong evidence suggests that appointed officials site more polling places, run elections with fewer provisional ballots or fewer rejected absentee and provisional ballots, or remove more registrants from the voter roll.

Table A.33: **Comparison of Administrative Outcomes Between Appointed and Directly Elected Local Election Officials (Even-Year General, 2000-2022)**

	Polling Places (1)	Prov Share (2)	Prov Rejection (3)	Absentee Rejection (4)	Reg Removal (5)
Appointed	-0.001	0.000	-0.024	0.004	-0.003
	(0.027)	(0.001)	(0.024)	(0.004)	(0.002)
Counties	1036	1112	1012	1111	1111
Elections (avg)	6	7	7	9	9
Observations	7340	7736	6100	9802	9167
Outcome Mean	1.177	0.006	0.497	0.023	0.100
County FEs	Yes	Yes	Yes	Yes	Yes
Year x State	Yes	Yes	Yes	Yes	Yes

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

A.4.4 Appointed Election Officials May Pursue More Constituent Communication

More active election official communication strategies has been shown to increase the share of registered voters (Merivaki and Suttmann-Lea 2023), improve voter confidence (Suttmann-Lea and Merivaki 2023), and reduce the number of mail ballots that are rejected (Suttmann-Lea and Merivaki 2022). Figure A.34 uses data provided by Thessalia Merivaki and Mara Suttmann-Lea to examine whether appointed local election officials are more likely to have official social media accounts than elected officials. Appointed officials serving jurisdictions in the same state and with similar populations as elected officials are more likely to have social media accounts, although the results are imprecisely estimated. Appointed officials are twice as likely to have Twitter/X social media accounts as elected officials serving similar jurisdictions.

Table A.34: **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	0.008	0.037	0.002	0.002
	(0.022)	(0.028)	(0.017)	(0.014)	(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 Suttmann-Lea.

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

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

In the main text, I use the 2020 EVIC Survey of Local Election Officials to show that appointed clerks possess more formal education than elected clerks serving in similarly sized jurisdictions within the same state. Table A.35 shows the results of additional indicators of official quality. Column 1 tests whether appointed clerks possess greater previous experience in election administration than elected officials. I find that appointed officials actually possess 1.6 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 15 percentage points less likely to be 65 years of age or older (column 5) and make $5,000 more a year on average than elected officials in the same state serving jurisdictions of a similar size. While this effect is statistically indistinguishable from zero, it represents an 8% salary premium. In column 6, I find that appointed officials hire an additional 0.6 FTEs, approximately 10% more than elected officials, although we cannot rule out that the finding arose by chance.

In sum, appointed officials are on average more educated and more professionalized than elected officials. However, they possess less election administration experience. This is potentially an artifact of higher turnover rates among appointed officials, which is examined in Section 6.3.3.

Table A.35: **Appointed and Elected Local Election Officials Possess Less Experience in Elections But Are More Professionalized**

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

Robust standard errors clustered by state in parentheses. Data is from the 2020 EVIC Survey of Local Election Officials and is filtered to only include chief local election officials. County is imputed from zip code to calculate population controls. Observations are weighted to be representative of the population of local election officials. Column 1 measures years of previous experience in election administration, calculated by subtracting current tenure length from total experience working in the field. Column 2 sums the number of professional memberships among the following four organizations: state association of local election officials, regional and/or local association of election officials, the Election Center (National Association of Election Officials), and the International Association of Government Officials (iGO). Column 3 measures whether clerks have served as election officials in other jurisdictions and column 4 measures the total number of other jurisdictions served in. Column 5 is a binary for whether the election official is over the age of 65 or not. Column 6 measures salary in dollars, which is derived by taking the midpoint values of salary ranges. Column 7 measures full-time equivalents, which is derived by taking the midpoint values of FTE ranges.

A.5.2 Voter Knowledge Survey Technical Appendix

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

I collect nationwide cross-sectional data on the institutional position, selection method, and name of every chief local election official. (Ferrer and Geyn 2024; Ferrer and Thompson 2024; Ferrer, Thompson, and Orey 2024) I match 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 is 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 am unable to match respondents living in jurisdictions with municipal-administered election administration because zip code is the smallest geography provided by respondents. This excludes 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.36 allows counties to switch in and out of having a local newspaper. The results are in line with those found in Table 6.

Table A.36: **Presence of a Daily Local Newspaper Attenuates the Effect of Appointing Local Election Officials on Citizen Participation (Even-Year General Elections, 1968-2022)**

	Voter Turnout			Registration Rate		
	(1)	(2)	(3)	(4)	(5)	(6)
Appointed	0.026	0.024	0.020	0.011	0.011	0.006
	(0.005)	(0.005)	(0.005)	(0.006)	(0.007)	(0.006)
Appointed X Newspaper	-0.021	-0.021	-0.014	0.000	0.001	0.007
	(0.006)	(0.007)	(0.007)	(0.008)	(0.009)	(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.

Chapter 2 References

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Chapter 3: Racial and Ethnic Representation In Local Election Offices

1 Introduction

Unlike any other Western democracy, the US relies on a large number of autonomous local officials to conduct our elections (Hale, Montjoy, and Brown 2015). These officials have varying levels of discretion to carry out a wide range of election duties, including registering voters, maintaining registration lists, siting polling places, conducting early and Election Day voting, hiring and training poll workers, selecting and maintaining voting equipment, processing provisional and absentee ballots, and tabulating and certifying election results. According to the 2022 Democracy Fund/Reed College Local Election Official Survey, two-thirds of election officials consider increasing voter turnout to be an important component of their jobs, and more than one in three agree that they should work to reduce demographic disparities in voter turnout.[31]

Beyond a long history of de jure and de facto racial discrimination in elections (Keyssar 2000) and a series of new voting laws targeted at suppressing minority participation (Bentele and O'Brien 2013), a growing body of literature shows that racial and ethnic minorities continue to experience inequities in election administration. Local election officials respond to Black and Hispanic voters at lower rates than white voters (Hughes et al. 2020; White, Nathan, and Faller 2015), are assigned lower quality polling locations (Barreto, Cohen-Marks, and Woods 2009), experience significantly longer wait times at the polls (Ansolabehere 2009; Chen et al. 2020; Klain et al. 2020; Stein et al. 2020; Pettigrew 2017), have lower quality interactions with poll workers (Hall, Monson, and Patterson 2009), are more

[31] https://evic.reed.edu/wp-content/uploads/2022/12/crosstabs.html

likely to be asked to show photo identification (Atkeson et al. 2010; Cobb, Greiner, and Quinn 2012), and are more likely to have their absentee (Baringer, Herron, and Smith 2020; Shino, Suttmann-Lea, and Smith 2021) and provisional (Merivaki and Smith 2020) ballots rejected. This environment contributes to lower levels of voter confidence among racial and ethnic minorities (Bowler et al. 2015; Bergeron-Boutin et al. 2023; Uribe et al. 2024), as well as ongoing disparities in voting participation rates (Fraga 2018). Turnout disparities are most acute for Latinos and Asians.

I examine the extent of racial/ethnic diversity among election officials over time and whether representation leads to improved voter participation and election administration. In order to do so, I combine an original panel of election officials across all 50 states, over 6,000 local election jurisdictions, and 25 years with large-scale administrative. I find that election administrators from minority groups has grown faster than their share in the population, from nearly all white in 2000 to about 12% identifying as Black, Latino, or Asian today. Utilizing a difference-in-differences design, I find that having a minority group member run the election office does not generally alleviate racial and ethnic disparities in voter registration and turnout rates, and minority clerks pursue similar election administration policies as white clerks. I field a large-scale survey experiment to examine the empowerment benefits of coethnic election official representation. Vignette and conjoint experiments show that racial and ethnic minority group members are trusted more by minority residents to lead elections fairly and white residents are equally trusting of minority and white election officials. Additionally, minority respondents who learn that their election official is also a racial minority report higher levels of voter confidence. These findings are encouraging in terms of descriptive representation, but also suggest representation is only one part of the solution to erasing long-standing racial disparities in the administration of elections.

2 Representation and Local Election Officials

Descriptive representation can lead to both positive behavioral/attitudinal changes among voters and to altered policy outputs. First, the well-established minority empowerment hypothesis posits that when racial and ethnic minorities see themselves represented in government, this leads to increased political efficacy, trust in political institutions, and political participation (Bobo and Gilliam 1990; Gay 2001; Tate 2003). Most of this literature examines federal and state offices, but some has extended to looking at street-level bureaucrats such as school teachers (Stewart, Meier, and England 1989) and police officers (Theobald and Haider-Markel 2009). One recent study found that descriptively representative poll workers increases general confidence in election administration for African American and Hispanic voters (King and Barnes 2018).

Second, descriptive representation may lead policy makers and government bureaucrats to act in the interests of the minorities they represent, improving policy outputs. In other words, descriptive representation can improve substantive representation. Some studies have examined the substantive impact of descriptive representation on local offices such as city councils and police officers (Ba et al. 2021; Farris and Holman 2017). However, no literature has studied the effects of minority representation on policy outcomes in local election administration.

Minority election officials could affect both attitudinal change and policy outputs. Descriptively representative election officials could increase voter confidence among traditionally excluded minorities, indirectly leading to increased participation. They could also make policy decisions designed to reduce racial disparities in the quality of election administration, directly boosting turnout and indirectly improving voter confidence.

According to the 2022 Democracy Fund/Reed College survey data, over 90% of local election officials are white.[32] It appears slightly more appointed officials are non-white, but the numbers are extremely low for both elected and appointed officials. Using data from the 2020 Democracy Fund/Reed College survey data, Ferrer and Geyn (2022) find that only 2.7% of appointed and 1.7% of elected officials are Black, 5.1% of appointed and 4.7% of elected officials are Latino, and 0.6% of appointed and 0.1% of elected officials are Asian. While this is discouraging from a descriptive representation perspective, it does mean that any positive effects of more minority election officials could potentially have a large impact when scaled to the population of administrators.

3 Data and Methods

3.1 Data

There are four major data components for the observational analysis in this paper: panel data of local election officials, data on the race/ethnicity of local election officials, county-level turnout and registration figures by race, and other election administration outcome data. I collect a large-scale panel dataset of chief local election officials across 50 states that administered each even-year general election between 2000 and 2024.[33] For states with multiple election authorities at the local level, I use the official with primary responsibility for administering elections on Election Day, as defined by Ferrer and Geyn (2022) which builds on Kimball and Kropf (2006). For states with election boards, I code the official who handles the day-to-day responsibilities of running elections.[34] Appendix A.1 includes more details on how the data was collected and a table of election officials

[32] https://evic.reed.edu/wp-content/uploads/2022/12/crosstabs.html

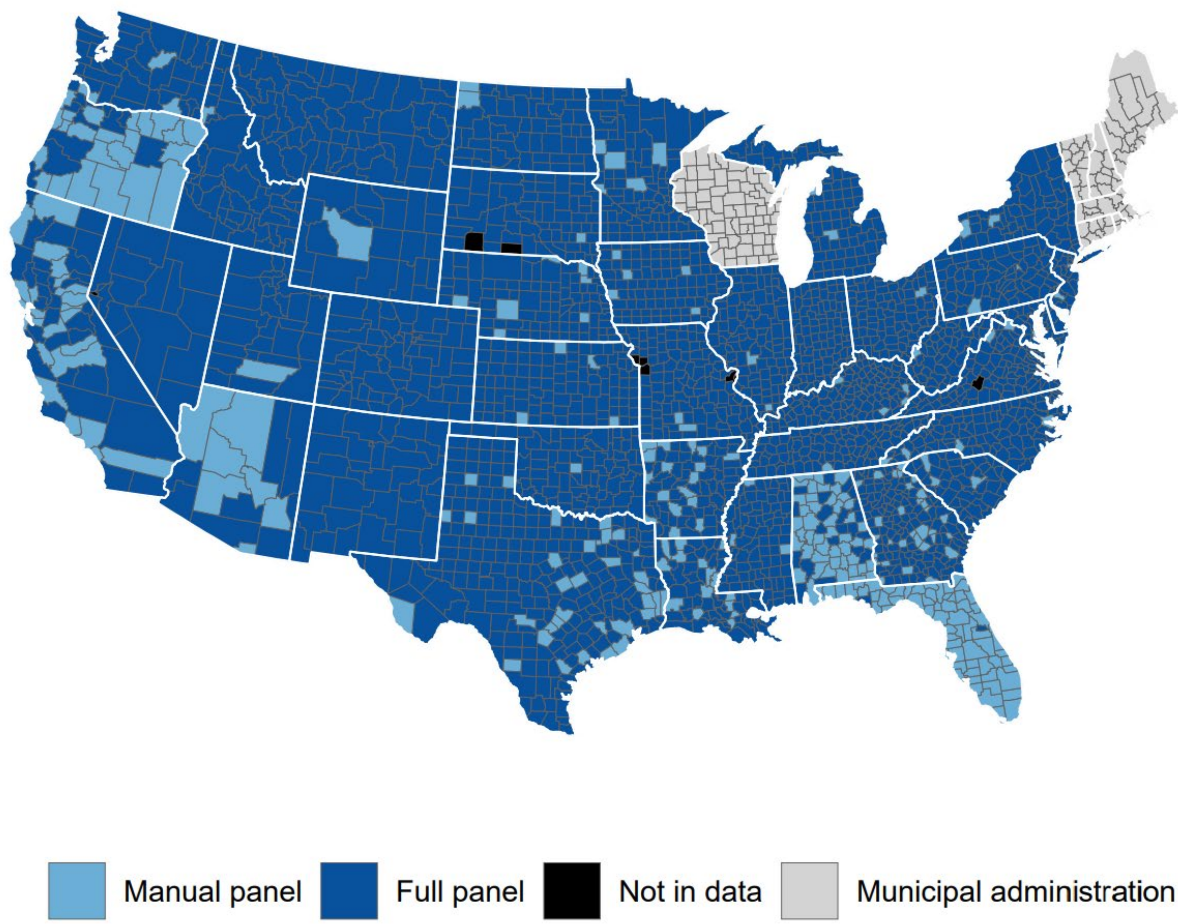
[33] 2024 data is collected in January of that year.

[34] I could not identify a single individual in each election jurisdiction in New York in charge of running elections. Instead, I code both the Democratic and Republican co-chairs of the county election boards.

coded in each state. I use the full dataset for my analysis of changes in descriptive representation among election officials over time. In total, this dataset comprises 18,882 unique local election officials across 6,276 jurisdictions and all 50 states, and spans 2000 to 2024.

I use two methods for determining the race/ethnicity of these officials: subjective and geocoding. I led a team of research assistants in searching for photos and biographic information of the election officials. In total, we were able to capture subjective hand-coded race data for 3,318 unique local election officials, or about 1 out of every 6 election officials. This is not a random sample of jurisdictions, but coverage is better in larger jurisdictions, in later years, and in more racially and ethnically diverse states. The second method is Bayesian Improved Surname Geocoding (BISG) analysis. This is a statistical method that combines census data on racial/ethnic composition of jurisdictions and the distribution of surnames by race/ethnicity. Imputing election officials' names and locations, BISG produces a posterior probability that the election official is a certain race/ethnicity. I am able to capture geocoded race data for virtually every election official in the dataset, and code the race/ethnicity that is of highest probability (out of white, Black, Asian, Hispanic, and Other). Figure 1 shows data availability of election official race by county.

Figure 1: **Map of Local Election Official Racial Data Availability, 2000-2024**. This graph displays the best panel data of local election official/race ethnicity available in each county. Counties in light blue have complete subjective researcher-collected data between 2000 and 2024. Counties in dark blue do not have complete subjective race data, but do have BISG-derived estimates of election official race/ethnicity between 2000 and 2024. States with counties in grey administer elections at the municipal level; virtually all municipalities in these states have panel BISG data but do not have subjective researcher data available. Finally, counties in black are not in the data. Alaska's jurisdictions are not in data. Kauai county in Hawaii has a full manual panel, and the rest have full geocoded panel data.



In general, the subjective data is higher quality than the BISG data. A validation exercise comparing election officials for whom both types are captured reveals that in 87.5% of cases, the predicted race matches the hand-coded race. However, this probability is inflated due to the fact that the population of election officials is overwhelmingly white. The likelihood that the BISG predicted race matches the subjective researcher-coded race is 97.9% for BISG-predicted whites, but is 46.5% for BISG-predicted, Hispanics, 32.9% for BISG-predicted Asians, 32.4% for BISG-predicted Blacks, and 6.9% for BISG-predicted Others. On the other hand, the likelihood that the subjectively coded race matches the BISG prediction is 88.8% for subjectively-coded whites, 75% for subjectively-coded Blacks, 70% for subjectively-coded Others, and 55.6% for subjectively-coded Asians. In short, BISG overestimates the likelihood that election officials are racial minorities. This is due to the fact that it makes predictions based on the overall population racial distribution, whereas the population of election officials and other leaders skews white.

I use the L2 nationwide voter file for county-level turnout and registration numerators by race. This data is available for even-year elections taking place between 2014 and 2020, and derives from a BISG-like calculation of each voter's race/ethnicity. This data encompasses billions of observations and captures the actual record of registrants and votes. In Appendix A.5, I conduct additional robustness tests of the main results using voter file data from Alabama, Florida, and Georgia, the three states with both race/ethnicity data on the voter file and where a single election official wields primary authority at the county-level.

For the denominator, I use both turnout and registration race shares (solely relaying on numerator data) and turnout/registration rates using county Citizen Voting-Age Population (CVAP) data from the ACS 5-year reports.[35] These reports are available for 2000 and 2009-2022. I linearly interpolate between 2000 and 2009 to create a full county-level CVAP panel for Black, Latino, Asian, and white voters. I then remove county-level race/ethnicity populations with fewer than 100 estimated values

[35] https://www.census.gov/programs-surveys/decennial-census/about/voting-rights/cvap.html

to reduce noisy low-sample participation rates.[36] This is used as the denominator in calculations of race-specific turnout and registration rates.[37]

I assemble a set of county-level indicators of election administration policy using the US Election Assistance Commission's Election Administration and Voting Surveys (EAVS) from 2004 to 2022.[38] This survey measures county-level outcomes in every even-year general election. I 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 (2024) and Pettigrew (2017), I use data from the Congressional Election Study to measure the share of voters who had to wait at the polls for more than 30 minutes. This is available for general elections in 2006-2022 except for 2010.

3.2 Research Design

I limit causal analysis to county election jurisdictions where the election official captured has primary authority to administer elections. Figure A.1 maps data availability across counties used in the observational analysis.

I employ a difference-in-differences design, leveraging changes in the race/ethnicity of local election officials to measure the effects of switching between white and minority officials on voter turnout, registration, and election administration policies. This design overcomes confounding due to spurious connections between election official race and voter turnout, including fixed factors

[36] Fraga (2018) chooses a similarly arbitrary but more conservative population threshold, removing county-level race-specific population estimates of fewer than 1000. Using this threshold yields similar results.

[37] Even with this cutoff, the turnout and registration rates using CVAP estimates remain noisy, especially in combination with poorly maintained registration files. In line with Morris and Shoub (2024), I cap all CVAP turnout and registration rates greater than 1 at 1.

[38] https://www.eac.gov/research-and-data/datasets-codebooks-and-surveys

(population, density, racial and political composition) and common time-varying factors (candidates on the ballot, public mood). The key assumption is that jurisdictions that experience a switch are on similar voter participation and election administration trajectories.

I estimate a series of regressions of the form $Y_{it} = \alpha_i + \delta_t + \beta Minority_{it} + \epsilon_{it}$, where Y_{it} is a measure of voter turnout, registration, or election administration outcome in county i at election year t, α_i and δ_t are county and year fixed effects, respectively, and $Minority_{it}$ is a dummy variable taking 1 when counties have a racial/ethnic minority as their local election official and 0 when counties have a white official. β is the causal effect of a minority election official on voter participation and election administration outcomes.

All main regression specifications 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 three additional sets of interacted fixed effects: Year by State by Non-Hispanic white population share, Year by State by Population, and Year by State by Democratic vote share fixed effects. The Year by Non-Hispanic white population fixed effect compares within-county over time change to other counties with similar racial demographics, whereas the Year by Democratic vote share fixed effect compares counties with similar partisan makeup and the Year by Population fixed effect compares counties with similar populations. These account for the possibility that counties that switch to a minority election official may also happen to shift demographics, population, or partisan trends in ways that are systematically related to turnout. All three interacted fixed effects are divided into quartiles and measured pre-treatment for each state.[39]

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

4 Descriptive Results

In this section, I present evidence that the number of minority local election officials across the country has increased over time. Existing surveys show that the population of local election officials are overwhelmingly white. However, all surveys to date have been cross-sectional samples and are therefore unable to clearly answer whether the descriptive representation of racial and ethnic minorities has increased. Survey samples also may produce noisy estimates of the population of election officials, and may also induce bias due to sampling and response rates. My panel data on administrator race overcomes these hurdles, conveying information on whether descriptive representation has increased over time without introducing any sampling or bias response issues.

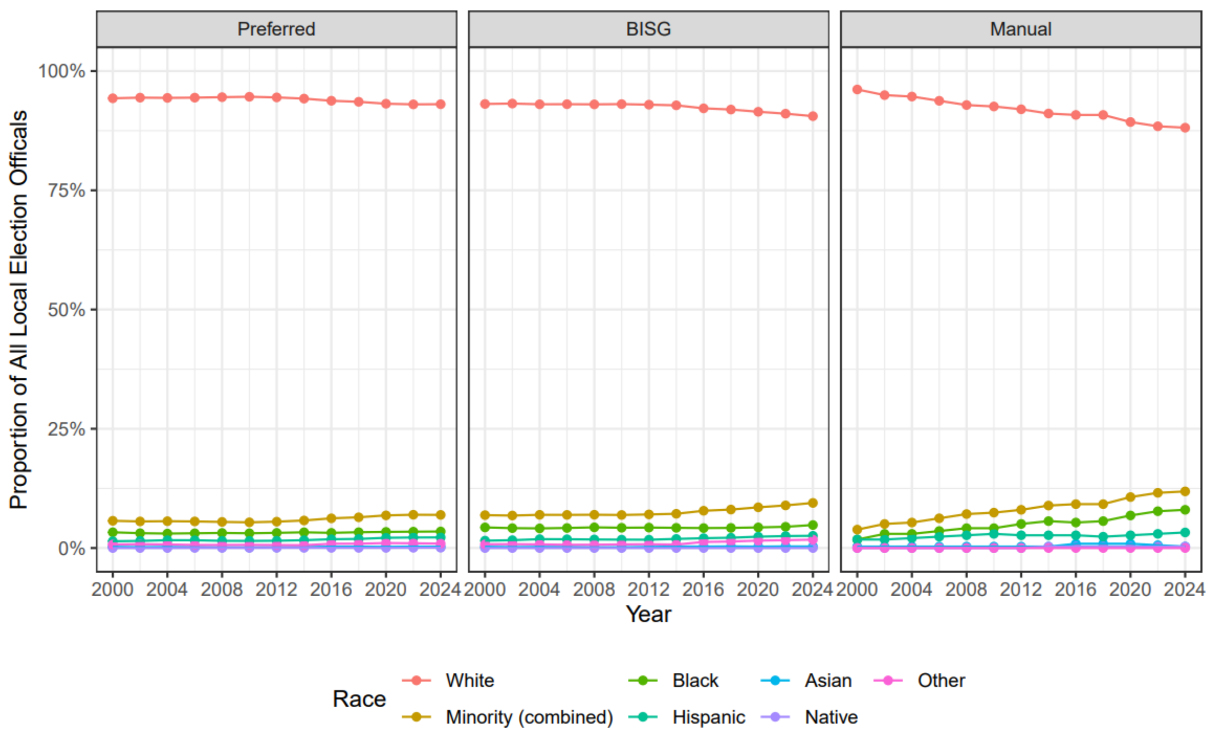
Figure 2 displays the percentage of Black, Latino, Asian, and White election officials that administered each even-year general election between 2000 and 2024. The left panel uses the best available data for determining election official race–researcher coded where possible, geocoded otherwise. The middle panel uses only BISG coding. The right panel uses only jurisdictions with complete panel researcher-coded data. All three panels show a similar story. In the early 2000s, approximately 95% of local election officials were white. This has slowly changed over the past 25 years, although how much is dependent on the data analyzed. Within the manually coded panel, minorities now make up 12% of all local election officials. According to the preferred and BISG panels, the figure is instead between 7 and 10 percent. Some of the reason for the conservative outlook with the Preferred panel could be due to the BISG's overestimation of minorities in general. Therefore, as more jurisdictions in the dataset switch to manually-coded data over time, this could mask a more significant diversification trend. In the apples-to-apples BISG and Manual data comparisons, the diversifying trend is stronger.

Almost all representational change has been fueled by growth in the proportion of election officials that are Black. Only 2-4% of election officials in 2000 were Black; today, that figure has roughly doubled. Unfortunately, there has been less improvement in the representation of Latinos, Asians, and Native Americans. According to the BISG data, Hispanics now make up 2.6% of all local election officials, up from 1.5% in 2000. Asian representation has grown from 0.2% to 0.3%. There are few indigenous election officials.

Figure 3 visualizes the racial and ethnic makeup of election officials across jurisdictions over time. It shows the same slow but steady trend towards increasing racial and ethnic diversity in the profession pictured in Figure 2, especially in the South and West. There has been little change in ethnoracial diversity in the East or Midwest. Appendix A.3 includes maps visualizing manual and preferred data across the US, as well as state-specific graphs. The underlying trends discussed here remain the same.

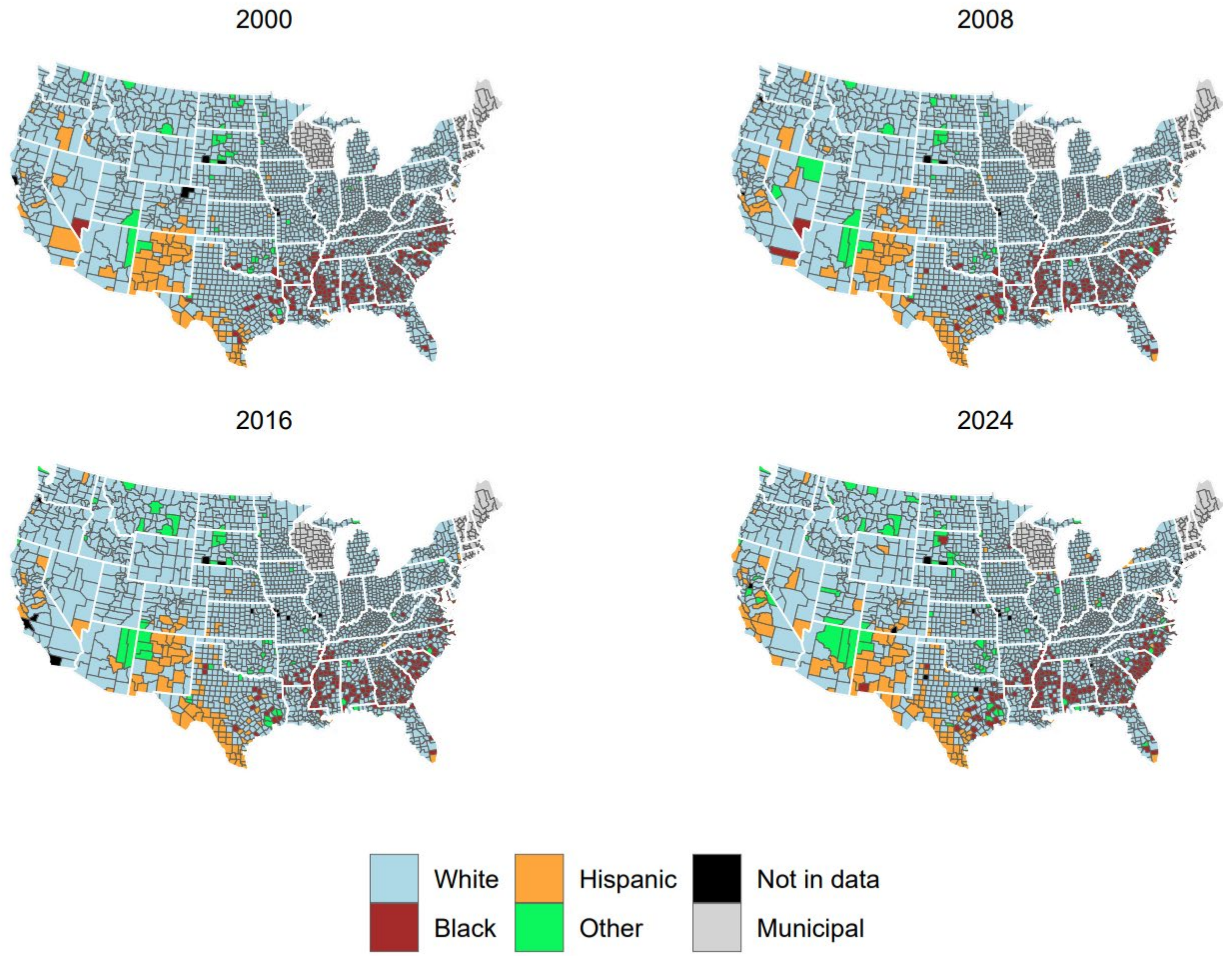
In summary, this descriptive evidence shows a positive trend in representation of racial minorities in election administration. Across the U.S., those tasked with running America's elections are starting to look more like the voters they work for than they did a few decades ago. However, there remains a large disparity between the racial makeup of these states and the racial makeup of the pool of local election officials. According to data from the 2020 Census, the country's population is 57.8% non-Hispanic white, 18.7% Hispanic or Latino, 12.1% Black, 3.8% Asian American, 8.6% Native American, and 10.2% two or more races. Additionally, racial minorities are not simply concentrated in a handful of populous jurisdictions. Over 400 counties are majority non-white across the country, over 700 are at least one-third minority, and one in three counties are at least 25% nonwhite.[40] Very few states reach parity between their election official pool and their population, and Latinos, Asians, and Native Americans remain particularly underrepresented. These results align with survey findings from Civic Pulse, which has found a substantial increase in racial and ethnic representation of local government officials between 2013 and 2024.[41] I turn next to whether minority officials make different administrative decisions or empower voters of color to participate at higher rates.

Figure 2: **Local Election Administrator Race, 2000-2024**. This graph displays over time change in the race of local election officials over the past 25 years. All panels include jurisdictions only with full panel data available between 2000 and 2024. "Preferred" uses subjective researcher-coded race data where available and BISG otherwise. "BISG" uses Bayesian Improved Surname Geocoding for race imputation. "Manual" uses jurisdictions for which there is a complete panel of hand-coded race data. Proportions are relative to the total number of jurisdictions in each dataset–5,920 in both the Preferred and BISG panels, and 337 jurisdictions in the Manual panel.



40 https://www.axios.com/2021/08/15/diversity-majority-minority-white-american-census
41 https://www.civicpulse.org/diversity-representation

Figure 3: **Local Election Administrator Race Map, 2000-2024**. This figure displays over time change in the race/ethnicity of local election officials over the past 25 years using Bayesian-Improved Surname Geocoding imputation. "Other" includes Native Americans.



5 Statistical Results

In this section, I present evidence that minority and white local election officials produce similar levels of minority participation rates and pursue similar election administration policies.

5.1 Minority and White Officials Produce Similar Levels of Minority Voter

Participation

Does descriptive representation improve participation for racial minorities? I test minority voter turnout and registration in this section using a combination of original data on local election officials and L2 voter files combined with a series of difference-in-difference estimates. All regressions include, at the minimum, both county and Year by State fixed effects.[42] This ensures that differential participation trends between states are not driving the results and that comparisons are only made of turnout differences between white- and minority-administered counties within the same state and election year. All estimates include robust standard errors clustered by county.

Table 1 displays difference-in-differences specifications testing the effects of minority election administration on Black voter participation, Table 2 shows the effects on Latino participation, Table 3 shows the effects on Asian participation, and Table 4 shows the effects on white participation. Across all specifications and all tables in the main analysis, a combination of manual and BISG-coded races are used, manual where available and BISG otherwise. Black, Asian, and Latino election officials are pooled together as minority officials to maximize statistical power. Since the vast majority of minority election officials are Black, the estimates are mostly powered by a switch between white and Black election officials. While Black officials might provide some representational benefits to other minorities due to a shared "people of color" racial affiliation (Pérez 2021), I expect point estimates to be largest for Black voter participation.

In all four tables, column 1 tests the effects of a switch to a minority election official on the CVAP turnout rate of that race, column 2 tests the effects on the share of voters of that race among all

[42] I also run specifications that include three additional interacted fixed effects: Year by State by Nonwhite quartile, Year by State by Population quartile, and Year by State by Democratic Vote Share quartile. The results are substantively similar.

participants in the jurisdiction, column 3 tests the effects on CVAP registration rates, and column 4 tests the effects on the share of registrants of that race. All regressions include county and year by state fixed effects. Observations are smaller for turnout and registration rates among minorities than for turnout shares because counties with fewer than 100 CVAP residents of that race are excluded.

Table 1: Minority Election Officials Do Not Affect Black Participation Rates (Manual + Geocoded Races, 2014-2020)

	Black Voter Turnout	Black Turnout Share	Black Reg	Black Reg Share
	(1)	(2)	(3)	(4)
Minority	0.001	−0.001	−0.002	−0.001
	(0.007)	(0.002)	(0.012)	(0.002)
County FEs	Yes	Yes	Yes	Yes
Year x State FEs	Yes	Yes	Yes	Yes
Observations	4,889	8,132	4,889	8,132

Table 2: Minority Election Officials Do Not Affect Latino Participation Rates (Manual + Geocoded Races, 2014-2020)

	Latino Voter Turnout	Latino Turnout Share	Latino Reg	Latino Reg Share
	(1)	(2)	(3)	(4)
Minority	0.006	0.003	0.003	0.001
	(0.005)	(0.003)	(0.007)	(0.001)
County FEs	Yes	Yes	Yes	Yes
Year x State FEs	Yes	Yes	Yes	Yes
Observations	6,202	8,132	6,202	8,132

All specifications for Blacks, Latinos, and whites result in near-zero point estimates that are relatively precisely estimated. For instance, the point estimate in column 1 of Table 1 means that a county switch from a white to a non-white local election official results in an average boost to Black voter turnout by 0.1 percentage points. Effects larger than 1.5 percentage points can be confidently ruled out. Point estimates for Asian voter turnout and Asian registration rates are slightly higher in Table 3, but these estimates are much less precise. Additionally, Asian turnout and registration shares do raise slightly, on average, and do reach statistical thresholds of significance. However, the effects are small. Column two indicates that a switch from a white to a minority election office boosts the share of voters that are Asian by 0.1 percentage points, compared with an average of 0.9% of voters being Asian. No other point estimates in these tables can be confidently distinguished from a null effect.

Table 3: Minority Election Officials Do Not Affect Asian Participation Rates (Manual + Geocoded Races, 2014-2020)

	Asian Voter Turnout	Asian Turnout Share	Asian Reg	Asian Reg Share
	(1)	(2)	(3)	(4)
Minority	0.014	0.001	0.014	0.0004
	(0.012)	(0.0004)	(0.011)	(0.0002)
County FEs	Yes	Yes	Yes	Yes
Year x State FEs	Yes	Yes	Yes	Yes
Observations	2,830	8,132	2,830	8,132

Table 4: Minority Election Officials Do Not Affect White Participation Rates (Manual + Geocoded Races, 2014-2020)

	White Voter Turnout	White Turnout Share	White Reg	White Reg Share
	(1)	(2)	(3)	(4)
Minority	0.006	−0.002	0.003	0.0003
	(0.004)	(0.003)	(0.004)	(0.002)
County FEs	Yes	Yes	Yes	Yes
Year x State FEs	Yes	Yes	Yes	Yes
Observations	8,132	8,132	8,132	8,132

These null results carry over to difference-in-difference tests of overall registration and turnout rates. Section A.7 in the Online Appendix shows that minority election officials do not significantly improve voter participation rates, but rather oversee elections with similar levels of participation as white election officials. Additionally, the results hold when zeroing in on changes between white and Black or Latino election officials on coethnic voter participation. These regressions, displayed in Section A.6 in the Online Appendix, show that the main results are not simply due to a lack of solidarity between racial minorities. The findings hold across a range of additional data specifications, including using only manually coded data and limiting the analysis to jurisdictions where the individual election official has particularly strong or complete authority to run elections. These are found in Appendix A.4.

5.2 Minority and White Officials Administer Elections Similarly

I use EAVS and CCES data to explore whether minority and white election officials pursue different election administration policies. The results, found in Table 5, suggest that minority and white administrators run elections with similar numbers of polling places per 1,000 residents, provisional ballot usage, provisional rejection rates, absentee ballot rejection rates, registration removal rates, and share of voters waiting longer than 30 minutes to vote. No point estimate achieves conventional levels of statistical significance. The results hold when using the full dataset (2004-2022) rather than the 2014-2020 data in line with the main analysis (Section A.6.1 in the online appendix). Taken together, there does not appear to be significant systematic differences in the election administration policies pursued by minority and white election officials.

6 Experimental Results

The minority empowerment hypothesis (Bobo and Gilliam 1990; Gay 2001; Tate 2003) suggests that descriptively representative officials should increase confidence among minorities and lead them to be more trusting in government. In the case of election administration, descriptively representative officials could increase voter confidence among traditionally excluded minorities and make them feel that voting is worthwhile. I conducted a pilot survey experiment module in the 2023 UCLA REPS Lab Omnibus Survey to test whether minority voters trust coethnic election officials to fairly administer elections more than they do white officials. The survey revealed positive empowerment benefits to coethnic representation among election officials (details and results of this study are in Section A.8 in the online appendix).

Table 5: Minority Election Officials Pursue Similar Administration Policies (Manual + Geocoded Races, 2014-2020)

	Polling Places (1)	Prov Share (2)	Prov Rejection (3)	Absentee Rejection (4)	Reg Removal (5)	Wait Share (6)
Minority	-0.019	0.003	-0.016	-0.011	-0.002	0.010
	(0.047)	(0.002)	(0.026)	(0.008)	(0.004)	(0.016)
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year x State FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,545	7,050	5,270	7,570	7,681	4,925

Building on the pilot study, I fielded the UCLA Representation Survey, a largescale 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 oversamples of Blacks, Hispanics, and Asians. The survey hypotheses and analysis was preregistered on OSF.[43] In addition to basic demographic and political questions, the survey contained three experimental components: a vignette, a conjoint, and an information provision

[43] osf.io/k7hq2

experiment. I conduct a vignette experiment to discern whether respondents favor coethnic election officials, a conjoint experiment to uncover how much the race of election officials matter in relation to other characteristics, and an information provision experiment to understand the real-world implications of Americans learning more about their local election officials. Finally, the survey included factual questions about respondents' local election official to measure knowledge about the position. I describe the main results of each component in the subsections below and leave additional analysis, technical details of the survey, and the survey instrument to Section A.9 in the online appendix. All regressions include post-stratification weights to ensure the sample is representative of the nationwide adult population.

6.1 Vignette Experiment

Respondents read a short vignette of a person described as potentially taking charge of elections in their county for the 2024 presidential election. The official was described with fixed job experience in election administration, political identity, age, and views on voter identification and absentee voting. They were also described as either white (control) or of the same race/ethnicity as the respondent (treatment). Respondents were then asked to rate how much they trust this official to conduct their elections fairly and how likely they would be to vote if this official became their election administrator. Using difference-in-means estimation, I compare the average response for same-race respondents in the treatment condition with the average response for same-race respondents in the control condition. I hypothesize that racial minorities will express greater trust in their election official and report a higher likelihood of voting if their local election official is described as coethnic rather than as white.

Figure 4: **Vignette Survey Experiment Difference-In-Means**. This figure visualizes difference-in-means regressions comparing Black, Latino, and Asian respondents' trust in their election official and reported likelihood of voting given a coethnic official rather than a white official. Each outcome is measured on a five-point Likert response scale.

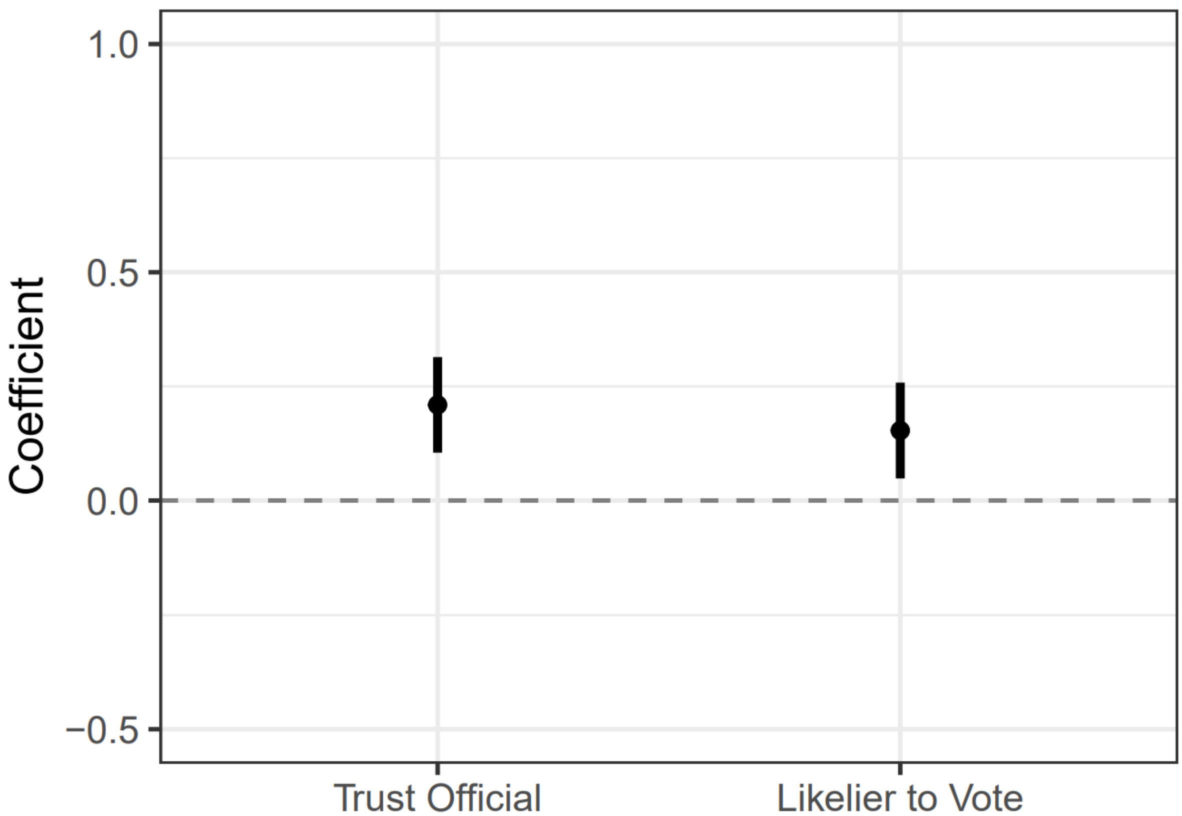


Figure 4 shows the output of difference-in-means tests comparing the responses given a coethnic election official to the responses given a white election official for minority respondents. The results show a modest but statistically significant positive effect for both outcomes. Respondents presented with a coethnic election official rather than a white official are express 0.2 points higher confidence in that election official to fairly count their vote (out of a five-point scale), and express being 0.15 points more likely to vote if the potential election official runs their election.

The results are robust to the inclusion of controls for a wide range of political, economic, and demographic factors (A.40). They also hold across racial groups, although the results are less precisely estimated (A.41). It appears that Hispanics are the most responsive to the coethnic treatment, followed by Black respondents.

This experiment isolates the effects of race in the presence of a great deal of additional political and demographic information about the election official. Voters are unlikely to know this much information about their local election official. As such, the vignette experiment trades off some degree of external validity for high internal validity. Little is left to the imagination of respondents–they cannot assume that simply because someone is a racial minority, they are a Democrat and support liberal election policies.

6.2 Conjoint Experiment

I conducted a paired-choice conjoint analysis (Hainmueller, Hangartner, and Yamamoto 2015). This experiment tests the revealed preferences of respondents when they are forced to choose between two candidates with different bundles of attributes (Hainmueller, Hopkins, and Yamamoto 2014). Previous studies have conducted similar analyses to estimate public support for descriptive representation among federal positions such as Supreme Court nominees (Kaslovsky, Rogowski, and Stone 2021; Sen 2017) and congressional candidates (Costa 2021; Lemi 2021), as well as candidates for an unspecified office (Kirkland and Coppock 2018). However, few studies for local officials exist. Sung (2023) tests revealed preferences for local prosecutor candidates, Stauffer, Miller, and Keiser (2023) examine mayoral candidates, and Crowder-Meyer, Gadarian, and Trounstine (2021) study mayoral and city council candidates. This is one of the first studies to test revealed preferences for any local office and the first to test revealed preferences for election officials.

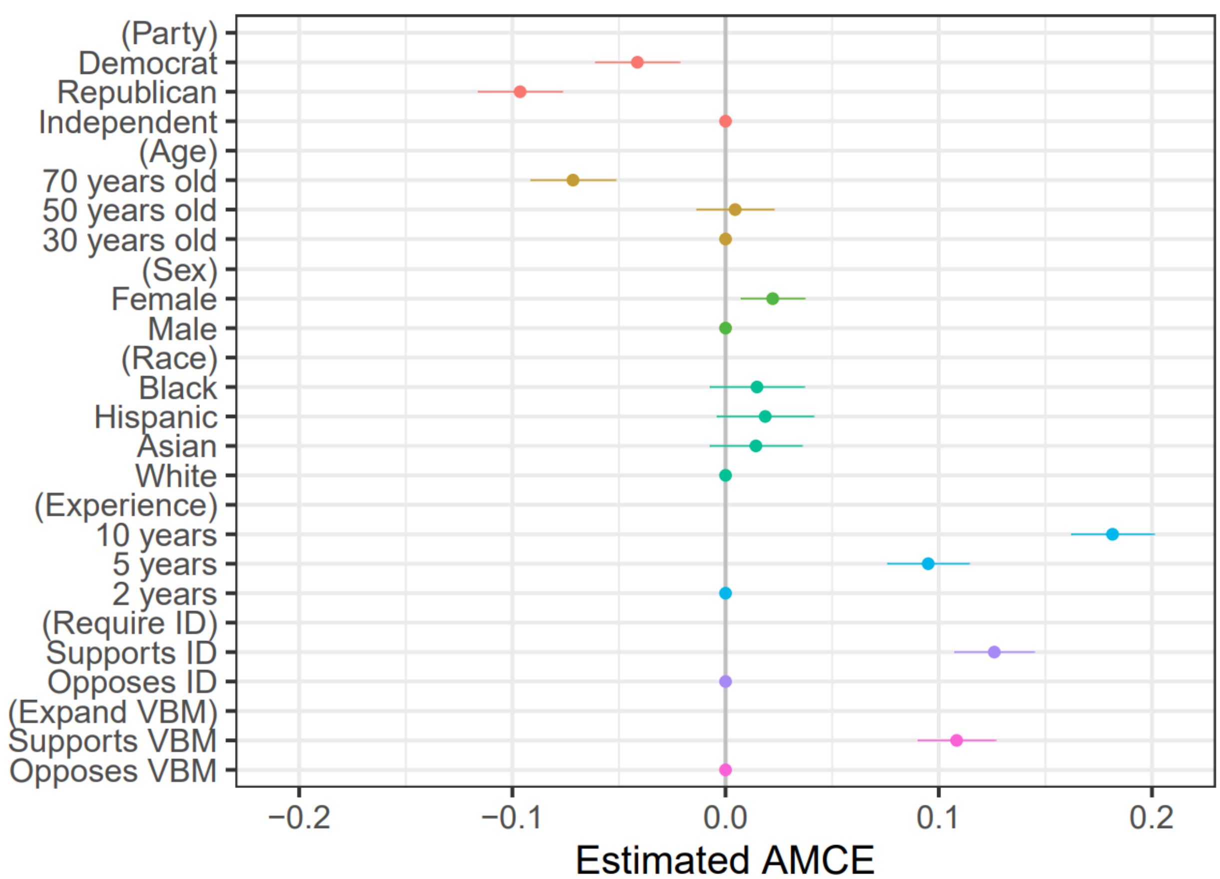
Respondents were presented with two candidates running to be the person in charge of administering elections in their county. Each candidate's party affiliation (Democrat, Republican, or Independent), age (30, 50, or 70), gender (male or female), race/ethnicity (white, Hispanic, Black, or Asian), years of experience in election administration (2 years, 10 years, or 20 years), support for voter ID laws (support/oppose), and support for expansive absentee voting provisions (support/oppose) were randomly chosen. Respondents were then asked which candidate they "trust more to do the job well", and were forced to choose between one of the two candidates. Each respondent completed four iterations of the randomized conjoint experiment. I use these tests to compute average marginal component effects (AMCE) for each attribute level, or the causal effect of the attribute on preferring that candidate. This experiment reveals how much respondents care about each attribute, and therefore whether participants care about the race of their election official relative to their party affiliation, age, gender, experience, and election administration policy preferences. I hypothesize that minority respondents will have a greater propensity to select coethnic candidates and will care about the race of these hypothetical candidates relative to other attributes than whites will.

Figure 5 shows the main results (Table A.43 in the online appendix displays regression output). The x-axis measures the average marginal component effect of each attribute level, or the probability that a respondent will pick a candidate with that characteristic rather than the reference attribute level. For instance, the first point estimate, Democrat, means that respondents were about 4% less likely to select candidates that were Democrats than were Independents, all else equal.

The biggest factors in a respondent's selection are, in decreasing order of importance, years of experience, election administration policy positions, party, and age. Respondents placed extremely high value on candidates who possessed 10 years of relevant experience, and also preferred those with five years of experience to those with only two years. Respondents were more than 10% likelier to select an election official who supports voter ID laws and and supports expanding vote-by-mail. Respondents also preferred Independents to candidates with a major party affiliation. Finally, candidates tended to dislike candidates who were 70 years old, but did not have a preference between 30-year-olds and 50-year-olds. All of these findings are in line with my preregistered expectations for the conjoint.

Respondents had a small but statistically significant preference for female candidates over male candidates (AMCE of 2.2%). Respondents also tended to prefer racial minorities over whites, although these differences do not attain traditional thresholds of significance.

Figure 5: **Conjoint Survey Experiment AMCEs**. This figure visualizes average marginal component effects of the local election official conjoint experiment conducted as part of the 2024 UCLA Representation Survey. Attributes are grouped together by color. 95% confidence intervals are illustrated. Point estimates of 0 without confidence intervals are the reference level for each attribute.



I more closely examine the racial preferences of whites and minorities in Figure 6 by breaking down respondents by race. Overall, this figure shows similar relative preferences in election officials across racial groups. Regardless of race, respondents highly value experience, popular election policy beliefs, Independent party affiliation, and youth. However, this graph does reveal small but meaningful differences in the ways racial minorities evaluate coethnic and co-POC candidates. Black respondents were 7% more likely to choose Black candidates over white candidates, all else equal, and were 4% more likely to pick a Hispanic candidate over a white one. Asians exhibited similarly strong coethnic affinity, and Hispanics exhibited strong affinity to by Asian and Hispanic candidates. In contrast, white respondents were indifferent to candidate race. While the overall effects are still modest, they are roughly equivalent with the preference for five years of experience over two years, and for candidates that support voter ID laws over those that do not.

Figure 7 directly compares the AMCEs of minority respondents to those of Whites. In the top panel, the deviation of the race AMCEs from zero for Asian, Black, and Hispanic respondents, compared with the lack of deviation among White respondents, again indicates that racial minorities indeed have stronger preferences about the race of their local election official. The bottom panel shows the relative preferences of Asians, Blacks, and Hispanics compared to Whites. In each case, one of the larger deviations is towards coethnics. In other words, while the absolute value of race is modest for racial minorities when compared with other candidate attributes, racial minorities care a lot more that local election officials match their race than whites do. This is one of the largest distinguishing characteristics between the preferences of racial minorities and whites, on par or larger than their differences in preferences for a specific party or policy position.

Tabular regression output for Figures 6 and 7 can be found in Section A.9.2 in the Online Appendix. I also run a robustness test examining the preferences of Nonwhites versus whites, which yields similar results.

In sum, the conjoint analysis reveals a modest but meaningful preference among racial minorities for coethnic and co-POC local election officials. Additionally, I have shown that racial minorities care more about election officials' race than white do, and that race is one of the biggest points of preference divergence between minorities and whites.

6.3 Information Provision Experiment

The final experiment is a novel information provision that tests whether learning about a participant's local election official improves their trust in U.S. elections. Providing factual information to respondents is a common practice in economics (Haaland, Roth, and Wohlfart 2023). In political science, it has been used to test how respondents update their (biased) beliefs about the world (Hill 2017). Providing tailored information specific to each respondent is much rarer, especially beyond provisioning general politician characteristics such as party identification and ideology (Kendall, Nannicini, and Trebbi 2015; Prina and Royer 2014; Roth, Settele, and Wohlfart 2022). Providing tailored information about political officials to respondents has been done in developing countries (Arias et al. 2019; Banerjee et al. 2011; Pande 2011), but my study will be one of the first to do this in the United States.

Figure 6: **Conjoint Survey Experiment AMCEs - By Respondent Race**. This figure visualizes average marginal component effects of the local election official conjoint experiment conducted as part of the 2024 UCLA Representation Survey, with effects separated by race. 95% confidence intervals are illustrated. Point estimates of 0 without confidence intervals are the reference level for each attribute.

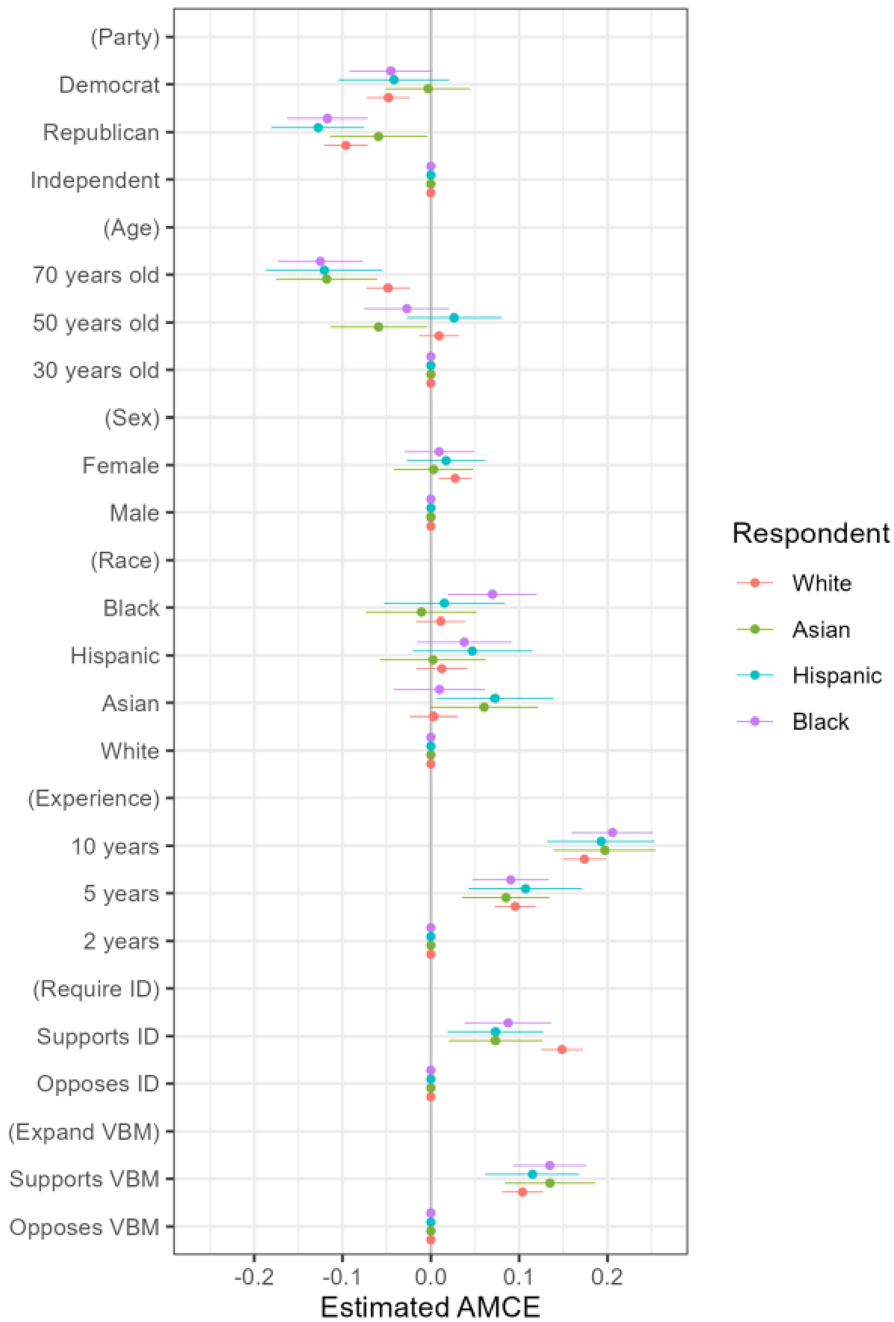
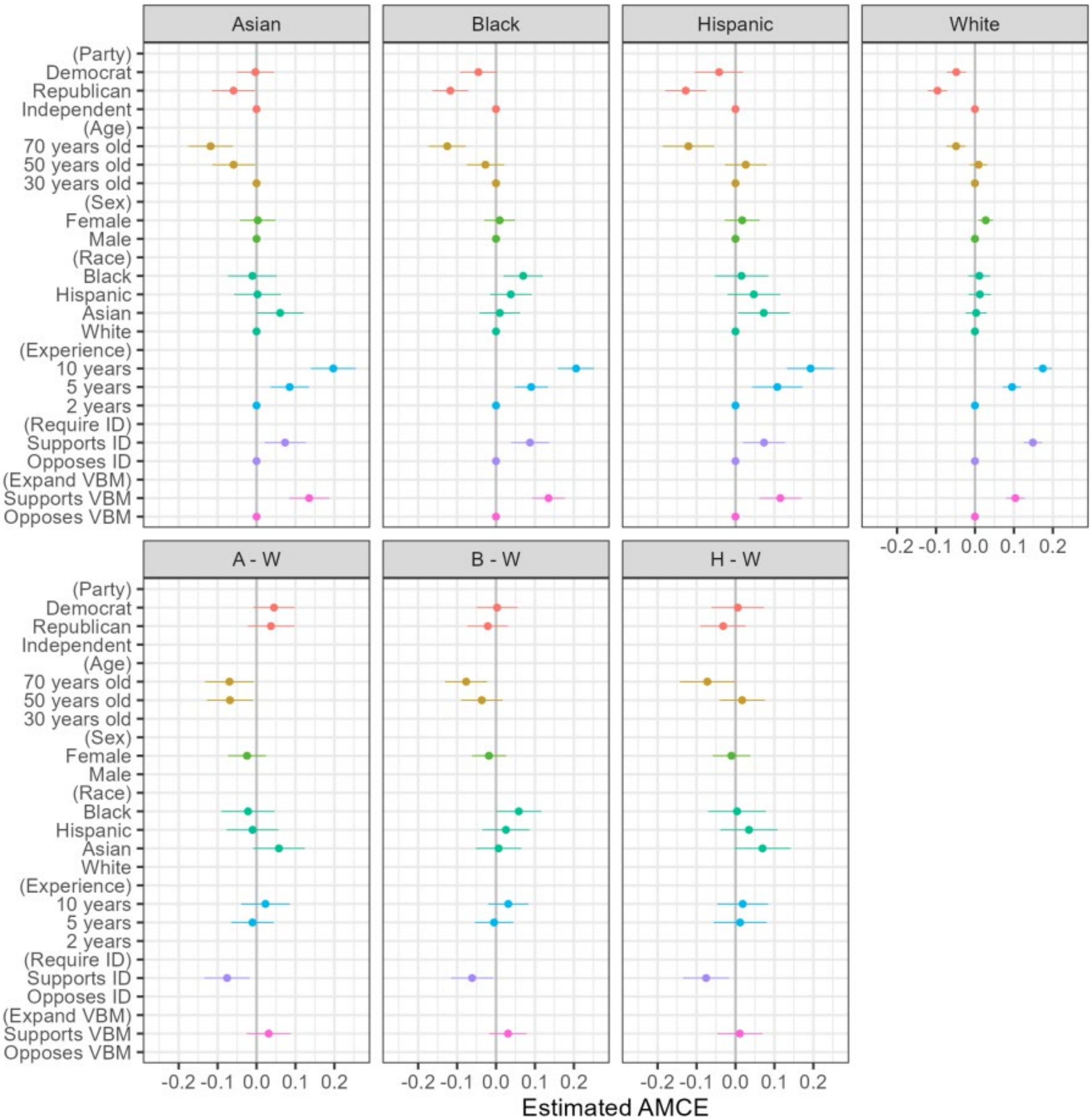


Figure 7: **Conjoint Survey Experiment AMCEs - Differences By Respondent Race**. This figure visualizes differences between racial minorities and whites in average marginal component effects of the local election official conjoint experiment. 95% confidence intervals are illustrated. Point estimates of 0 without confidence intervals are the reference level for each attribute. “A-W” means “Asian-White” AMCEs, “B-W” means “Black-White” AMCEs, and “H-W” means “Hispanic-White” AMCEs.



This experiment utilizes my original largescale data collection of the name of every chief local election official across the thousands of separate election jurisdictions in the U.S. (Hale, Montjoy, and Brown 2015). It also uses my original data on the race/ethnicity of each election official, as well as their gender and the institutional position, selection method, and tenure length of each official (Ferrer and Geyn 2022; Ferrer, Thompson, and Orey 2024).[44] This allows me to match participants with their current election official based on the zip code they provide earlier in the survey.[45] Participants were block-randomized by their racial identity into three conditions. In the control, participants were informed about their county of residence and the number of registered voters in that county. In the first treatment condition, they received the name, gender, position, and selection method of their local election official in addition to the number of registered voters. In the second treatment condition, they received all of the information in the first treatment in addition to the race/ethnicity of their election official. Respondents were then asked a series of post-treatment questions concerning their confidence that their vote is counted as intended and that their election official impartially administers elections. Respondents were assigned treatments based on an unequal probability distribution, with half of respondents assigned to the information + race treatment and a quarter each assigned to control and the information without race treatment. This was done to ensure sufficient power for tests of coethnic race reveal. As with the vignette experiment, I use difference-in-means estimation, comparing the average response for same-race respondents in each treatment condition with the average response for same-race respondents in the control condition, as well as the pooled difference-in-means across treatment conditions. I hypothesize that providing respondents information about

[44] Gender was imputed using election official first name and the 'gender' package in R. More thorough explanations of the other data sources can be found in the cited articles.

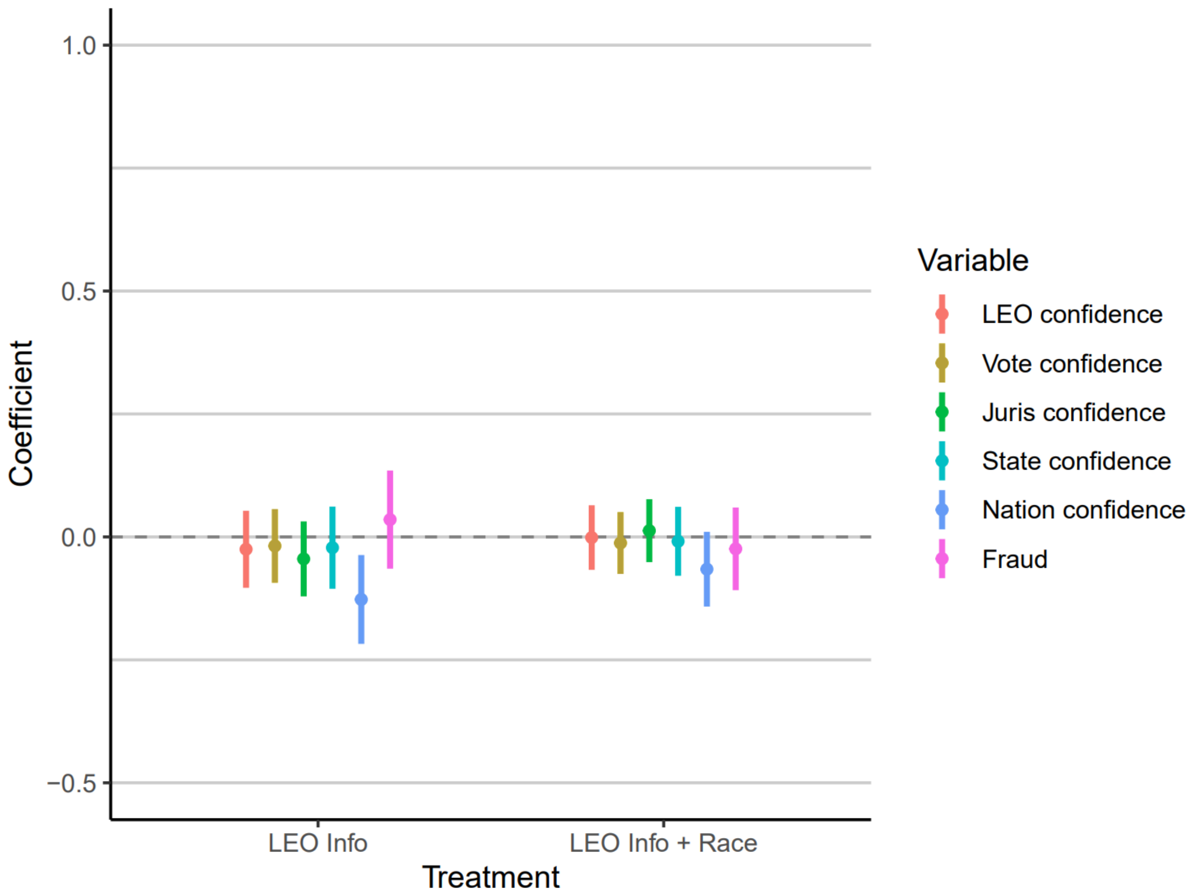
[45] For zip codes that span multiple counties, the county with the majority of the zip code's area was chosen. While it is true that approximately 20% of zip codes cross county lines, in most cases the vast majority of the zip code lies in one county.

their election official will improve voter confidence and that minority respondents who learn that their local election official looks like them will have higher confidence in the integrity of elections.

Figure 8 shows the respondent effects of learning about their local election official on confidence in the integrity of elections. Election confidence is measured as confidence in respondent's local election official, confidence that their vote is counted accurate, confidence that the vote in their jurisdiction, state, and nation are counted accurately, and belief in widespread voter fraud marring the 2020 presidential election results. Almost all point estimates are close to zero and all are precisely estimated. Additionally, the only point estimate statistically distinguishable from zero is negative: respondents reported *less* confidence that votes nationwide will be counted as voters intend in the 2024 presidential election, after learning about their local election official. On their face, these results suggest there is no relationship between knowledge about one's election official and confidence in the integrity of the election.

Does learning that a respondent's local election official is coethnic or co-POC increase voter confidence? Simply sub-setting to racial minorities and conducting a difference-in-means test between treatment groups will not answer this question, as whether or not a respondent has a coethnic election official is not randomly distributed. Rather, it is possible (and likely) that the revealed race of the respondent's election official will be white, and therefore the treatment effect of revealed race will be null or negative as a result. Subsetting to racial minorities and comparing those who learn their election official is co-POC vs. those who learn their election official is white also fails to produce a causal estimated.

Figure 8: **Information Provision Experiment Difference-In-Means**. This figure visualizes differences-in-means regressions comparing the effects of learning nothing about your local election official (control) to learning their name, position, selection method, years of experience, and gender ("LEO Info" treatment) and to additionally learning their race ("LEO Info + Race" treatment). Point estimates are grouped by treatment and each represent a separate post-treatment question measuring respondent voter confidence. Dependent variables are measured on a 4-point scale with a "I don't know" option. Regressions include demographic and political controls. 95% confidence intervals are shown.



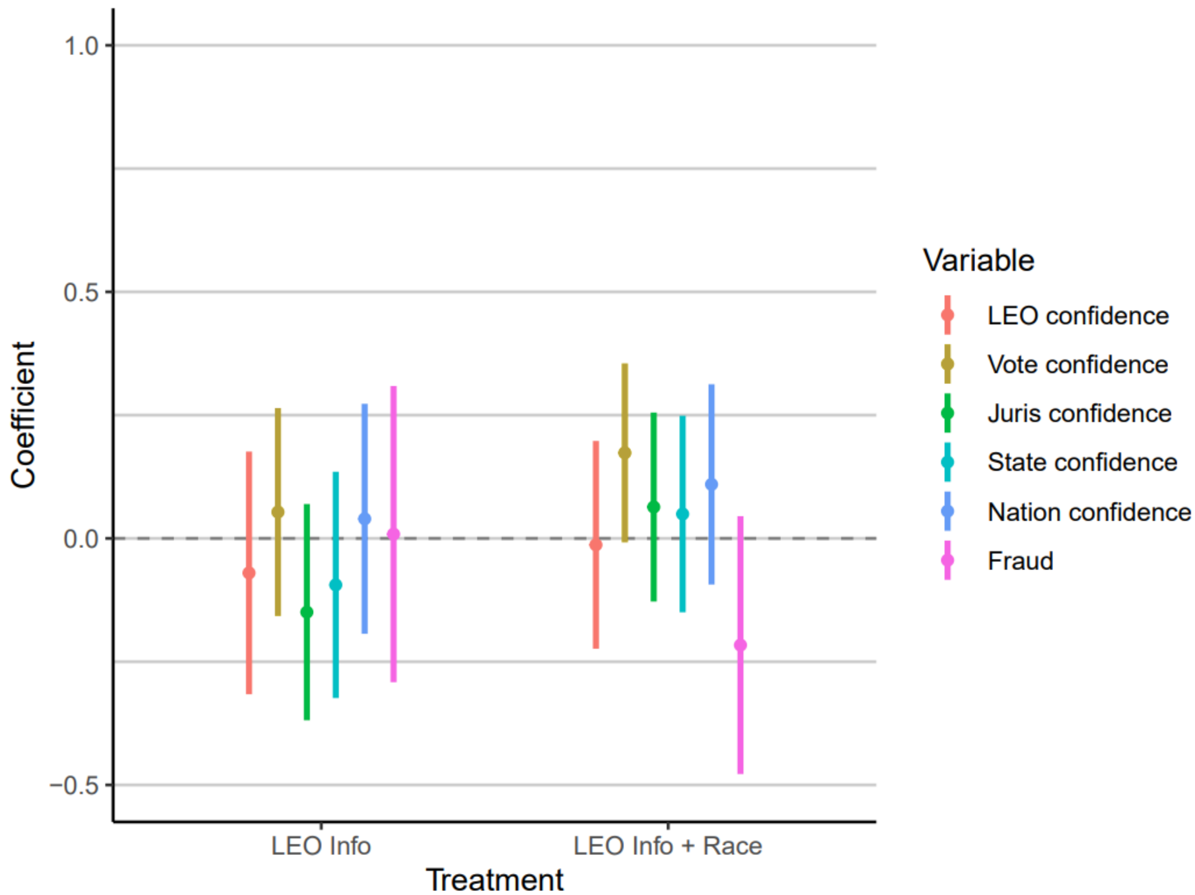
Therefore, I filter to minority respondents who have a minority election official. This ensures that the comparison between treatment and control measures the effect of learning that a respondent's election official is coethnic against a counterfactual where the respondent potentially remains unaware of this fact. The results, shown in Figure 9, shows modest but detectable positive effects for certain types of voter confidence. When minorities learn that their election official is also a racial minority, they report higher levels of confidence that their vote will be counted accurately in the 2024 presidential election. The effect is 0.17 on a four-point scale and is statistically distinguishable from zero. Additionally, minority respondents report higher levels of confidence in the national vote and lower levels of fraud in the 2020 election, compared to respondents with co-POC election officials in the control condition. Respondents report no greater confidence in their election official to be impartial, nor greater confidence that their jurisdiction or state's vote count was accurate. Tabular regression output and additional robustness tests of the information provision experiment can be found in Appendix A.9.3.

In summary, providing respondents information about their election officials failed to increase their voter confidence. However, minority respondents who learn that their election official is co-POC appear to have greater confidence in the integrity of elections.

7 Why Do Minority and White Officials Administer Elections Similarly?

Given the evidence in previous literature for the minority empowerment hypothesis, the null effects of descriptive representation on policy outputs, and the rather modest effects found in survey experiments, why have I failed here to find a stronger link between descriptive representation and improved administrative outcomes? I explore first why minority election officials might fail to affect behavioral or attitudinal change in their constituents and second why minority officials may pursue similar policies to white officials. I then test whether minority election officials see altered election expenditure environments.

Figure 9: **Information Provision Experiment Difference-In-Means - Among Minority Respondents with POC Election Officials**. This figure visualizes differences-in-means regressions comparing the effects of learning nothing about your local election official (control) to learning their name, position, selection method, years of experience, and gender ("LEO Info" treatment) and to additionally learning their race ("LEO Info + Race" treatment). Respondents are filtered to POCs who have POC election officials. Point estimates are grouped by treatment and each represent a separate post-treatment question measuring respondent voter confidence. Dependent variables are measured on a 4-point scale with a "I don't know" option. Regressions include demographic and political controls. 95% confidence intervals are shown.



Minority empowerment depends on the visibility of the official and interactions between the official and their constituents. Election officials tend to have minimal visibility and only interact with a small percentage of their constituents. Unlike President, Senator, or even Mayor, election administration is not a high-profile job. Additionally, election officials in most states bear multiple responsibilities and may have unintuitive titles. In both Alabama and Georgia, election officials are probate judges and also carry out the responsibilities of judge. A decline in local news coverage has generally led to less informed citizens (Rubado and Jennings 2020). Finally, voters typically interact with poll workers who volunteer to work on Election Day rather than the actual chief election officer for their jurisdiction. All of these factors reduce the ability of minority election officials to increase non-white turnout through empowerment.

What about policy outcomes? Recent literature suggests partisan differences between election officials are smaller than conventional wisdom suggests. Ferrer, Geyn, and Thompson (2024) find that Democratic and Republican election officials produce similar levels of presidential Democratic vote share and turnout rates. They also administer elections similarly across the broad range of policy indicators tested in Table 5. If Democrats and Republicans administer elections similarly, it makes sense that white and Black officials do so as well.

Ferrer, Geyn, and Thompson (2024) examined four explanations for their finding of minimal partisanship: the reelection incentive forces election officials to moderate in order to win, officials face a collective action problem in altering election outcomes, election officials are less polarized in their election policy views than the general public, and administration policies do not make much of an impact of outcomes such as registration and turnout rates. The evidence led them to support the latter two conclusions. Election officials appear to be self-selecting and truly seek to do the best job possible given limited resources and technical demands. In this case, this means both minority and white officials may seek to boost voter participation rates and alleviate racial disparities in turnout. This would result in the null results observed. Additionally, it is likely that election administration policies may not have that big of an effect on turnout. (Clinton et al. 2020) This means that observed differences in election administration policies will not clearly translate into differences in race-specific participation rates.

7.1 Do Restrictive Administrative Environments Explain the Results?

I test one additional explanation: that minority election officials face hostile political environments when they come into office. Perhaps these officials sincerely seek to increase resources for their minority constituents, providing more polling places, better election equipment, and more staffing. However, final decisions on expenditures usually rest with other county bodies such as the County Executive or County Supervisors. If these election officials are starved of resources by other actors, it could also explain the null results observed. I test this using election administration expenditure data from Mohr et al. (2018). This dataset includes estimated yearly election administration costs for half of all states spanning from the 2000s to 2016, though there is significant missingness and high within-county variance. Table 6 displays the results of a difference-in-differences regression testing the effects of switching to a minority election official on logged total county election expenditures.

Table 6: Minority Local Election Officials Do Not Affect Election Expenditures (Manual + Geocoded Races, 2000-2016)

	Log Total Election Expenditures			
	(1)	(2)	(3)	(4)
Minority	-0.048	0.007	-0.021	-0.011
	(0.061)	(0.059)	(0.059)	(0.063)
County FEs	Yes	Yes	Yes	Yes
Year x State FEs	Yes	No	No	No
Year x State x Nonwhite FEs	No	Yes	No	No
Year x State x Pop FEs	No	No	Yes	No
Year x State x Dem VS FEs	No	No	No	Yes
Observations	2,778	2,778	2,778	2,778

While the results are fairly imprecise, there is no clear pattern of increased or decreased election expenditures once non-white election officials assume office. The point estimate in column 1 implies that switching from a white to a minority election official decreases total election expenditures in that jurisdiction by 4.7%. However, the effect is not statistically distinguishable from zero, and is attenuated when jurisdictions are only compared with those with similar pretreatment demographic, population, or partisan makeups.

In sum, it appears that minority election officials do not see their budgets significantly reduced, nor are they able to appreciably grow their budgets more than white officials.

8 Conclusion

Local election officials are the front-line workers of America's democracy. But do they represent their voters? Using original largescale administrative data and a causally credible research design, I show that racial minorities make up a small but growing share of leadership positions in local election offices. However, having a minority group member in charge does not generally alleviate racial and ethnic disparities in voting. Minority and white chief local election officials oversee elections with similar levels of registration and turnout rates among Blacks, Latinos, Asians, and whites and with similar administrative outcomes. Using an original survey experiment that is nationally representative of the U.S. adult population, I find evidence that minority officials are more trusted by coethnic residents to run elections fairly and that white residents are equally trusting of minority and white election officials. Minority participants responded positively to vignettes that described the potential for their election official to match their race. Respondents also preferred fictitious candidates for chief local election official who matched their race or ethnicity. When provided true information that respondents' election official matched their minority racial status, participants became slightly more trusting of the system.

It is normatively desirable that a diversifying America is starting to be reflected in those tasked with running its democracy. Twenty years ago, virtually all election officials were white. Today, that is no longer the case. Additionally, it is reassuring that the reality of an unrepresentative class of election administrators does not obviously translate into inferior election quality outcomes. However, these results are discouraging for eliminating long-standing racial and ethnic disparities in voter participation and the quality of election administration. Electing more Black and Brown officials is an important step to ensuring equity in the voting experience, but it is only one piece of the solution. Minority voters prefer coethnic officials in charge and report trusting these officials more to manage elections fairly. But minority election officials are unable to reduce the racial turnout gap (Fraga 2018).

Minority election officials differ from white officials on many dimensions beyond simply their skin color and ancestry. They are more likely to belong to the Democratic than the Republican party, probably hold more liberal election policy views, may be younger on average, and are more likely to be appointed than white officials (Ferrer and Geyn 2022).

Racial treatment effects bundle all of these differences together. This is not necessarily a bad thing. In a series of survey experiments, I isolate the effects of race beyond some of the obvious characteristics that may otherwise be inferred. However, it is still possible that respondents made assumptions about an election official given their race, even when provided with evidence contradicting those assumptions.

Future research should leverage variation in selection method to test whether certain institutional mechanisms such as direct elections, consolidated authority, or partisan labels on the ballot moderate the effects of descriptive representation. Certain forms of election administration are artifacts of a dark history of racism. For instance, 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). Most counties in states such as Alabama, Georgia, and Texas maintain separate registration and election administration officers which were originally instituted in order to prevent African Americans from registering to vote. Recently, legislators in Georgia have pushed through changes to election official selection, some of which have shifted power from longstanding Black officials to partisan-minded white appointees.[46]

An increasingly polarized and dangerous national environment for elections may spillover into the local level, and has made it ever more important that the local officials responsible for running America's elections in a professional and nonpartisan manner are up to the task. It also makes it more important that these officials descriptively and substantively represent their constituents and gain their trust in the endeavor of preserving our shared democracy.

[46] https://www.washingtonpost.com/nation/2022/03/14/georgia-elections-fraud-purge/

Chapter 3 Appendix

A.1 Collection and Coding of Local Election Officials

I collect the majority of the data from state government websites either through election results for elected officials—building on Ferrer, Geyn, and Thompson (2024)—or from directories of these officials. I acquire the lists from a mix of archived websites, state election publications, and public information requests. Where state-level data is not available, I search one county at a time, collecting data from past election results, archived website pages, or via direct communication with county offices. More details of the data collection can be found in Ferrer, Thompson, and Orey (2024).

Table A.1 displays data on the selected local election officials for each state, as well as the number of jurisdictions in the state, the number of jurisdictions with a full panel of data, the level of geography captured, the selection method of the officials, whether the modal official captured in each state is the sole and/or primary election authority, the data sources used, and the start and end year of the data collected.

Table A.1: **Local Election Officials Captured in the Dataset**

State	Jurisdictions	Jurisdictions Used	Geography	Election Official	Selection Method	Sole Authority	Primary Authority	Data Source	Data Start	Data End
Alabama	67	67	County	Probate Judge	Elected	No	Yes	Elections and State	1996	2024
Alaska	5	4	Region	Regional Election Supervisor	Appointed	Yes	Yes	State	2000	2024
Arizona	15	15	County	County Election Administrator / County Recorder	Mixed	No	Yes	State	2000	2024
Arkansas	75	75	County	Clerk	Elected	No	Yes	State	2000	2024
California	58	58	County	Clerk / Registrar of Voters / Auditor / Director of Elections	Mixed	Yes	Yes	State	1996	2024
Colorado	64	63	County	Clerk and Recorder	Mixed	Yes	Yes	Elections and State	1998	2024
Connecticut	178	171	Municipal	Clerk	Mixed	No	No	State	2000	2024
Delaware	3	3	County	Director of Elections	Appointed	No	No	State	1996	2024
Florida	67	67	County	Supervisor of Elections	Mixed	No	Yes	Elections and State	1998	2024
Georgia	159	159	County	Elections Director / Probate Judge	Mixed	No	No	Elections and State	1996	2024
Hawaii	5	4	County	Clerk	Appointed	Yes	Yes	State	2000	2024
Idaho	44	44	County	Clerk	Elected	Yes	Yes	Elections	2000	2024
Illinois	102	102	County	Clerk / Executive Director	Mixed	Yes	Yes	Elections and State	2000	2024
Indiana	92	92	County	Clerk	Elected	No	Yes	Elections and State	1998	2024
Iowa	99	99	County	Auditor	Elected	Yes	Yes	Elections and State	2000	2024
Kansas	105	105	County	Clerk	Elected	Yes	Yes	State	2000	2024
Kentucky	120	120	County	Clerk	Elected	No	Yes	Elections and State	1998	2024
Louisiana	64	64	Parish	Clerk of Court	Elected	No	Yes	State	1998	2024
Maine	504	502	Municipal	Clerk	Mixed	No	Yes	State	2000	2024
Maryland	24	24	County	Election Director	Appointed	No	No	State	2000	2024
Massachusetts	351	0	Municipal	Clerk / Elections Commissioner	Mixed	No	Yes	Verified Voting	2012	2024
Michigan	83	83	County	Clerk	Elected	No	No	State and NGO	2000	2024
Minnesota	87	87	County	Auditor / Election Director	Mixed	No	Yes	State	2000	2024
Mississippi	82	82	County	Circuit Clerk	Elected	No	No	State	2000	2024
Missouri	115	110	County	Clerk / Director of Elections	Elected	Yes	Yes	State	2000	2024
Montana	56	56	County	Clerk and Recorder / Election Administrator	Mixed	Yes	Yes	Elections and State	1996	2024
Nebraska	93	93	County	Clerk / Election Commissioner	Mixed	Yes	Yes	Elections and State	2000	2024
Nevada	17	17	County	Clerk / Registrar of Voters	Mixed	Yes	Yes	Elections and State	2000	2024
New Hampshire	234	234	Municipal	Clerk	Mixed	No	No	State and NGO	2000	2024
New Jersey	21	21	County	Clerk	Elected	No	No	State	2000	2024
New Mexico	33	33	County	Clerk	Elected	No	Yes	Elections and State	2000	2024
New York	62	58	County	Election Commissioner	Appointed	No	No	State	2000	2024
North Carolina	100	100	County	Election Director	Appointed	No	No	State	2000	2024
North Dakota	53	53	County	Auditor	Elected	Yes	Yes	State	2000	2024
Ohio	88	88	County	County Election Director	Appointed	No	No	State and Local	2000	2024
Oklahoma	77	77	County	Election Board Secretary	Appointed	No	No	State	1996	2024
Oregon	36	36	County	Clerk / Elections Director	Mixed	Yes	Yes	State	2000	2024
Pennsylvania	67	67	County	Director of Elections	Appointed	No	Yes	State	2000	2024
Rhode Island	39	39	Municipal	Clerk / Registrar / Election Director	Mixed	No	Yes	State and Local	2000	2024
South Carolina	46	46	County	Director of Voter Registration and Elections	Appointed	No	No	State	2000	2024
South Dakota	66	64	County	Auditor	Mixed	Yes	Yes	Elections and State	2000	2024
Tennessee	95	95	County	Administrator of Elections	Appointed	No	No	State	2000	2024
Texas	254	254	County	Elections Administrator / Clerk / Tax Assessor	Mixed	No	Yes	State	2000	2024
Utah	29	29	County	Clerk	Elected	Yes	Yes	Elections and State	1998	2024
Vermont	246	246	Municipal	Clerk	Mixed	No	Yes	State	2000	2024
Virginia	133	133	County	General Registrar	Appointed	No	Yes	State and Local	1998	2024
Washington	39	39	County	Auditor / Elections Director	Elected	Yes	Yes	Elections, State, and NGO	2000	2024
West Virginia	55	55	County	Clerk / Elections Coordinator	Mixed	No	Yes	Elections and State	2000	2024
Wisconsin	1851	1779	Municipal	Clerk	Mixed	No	Yes	State	2000	2024
Wyoming	23	23	County	Clerk	Elected	Yes	Yes	Elections and State	1998	2024

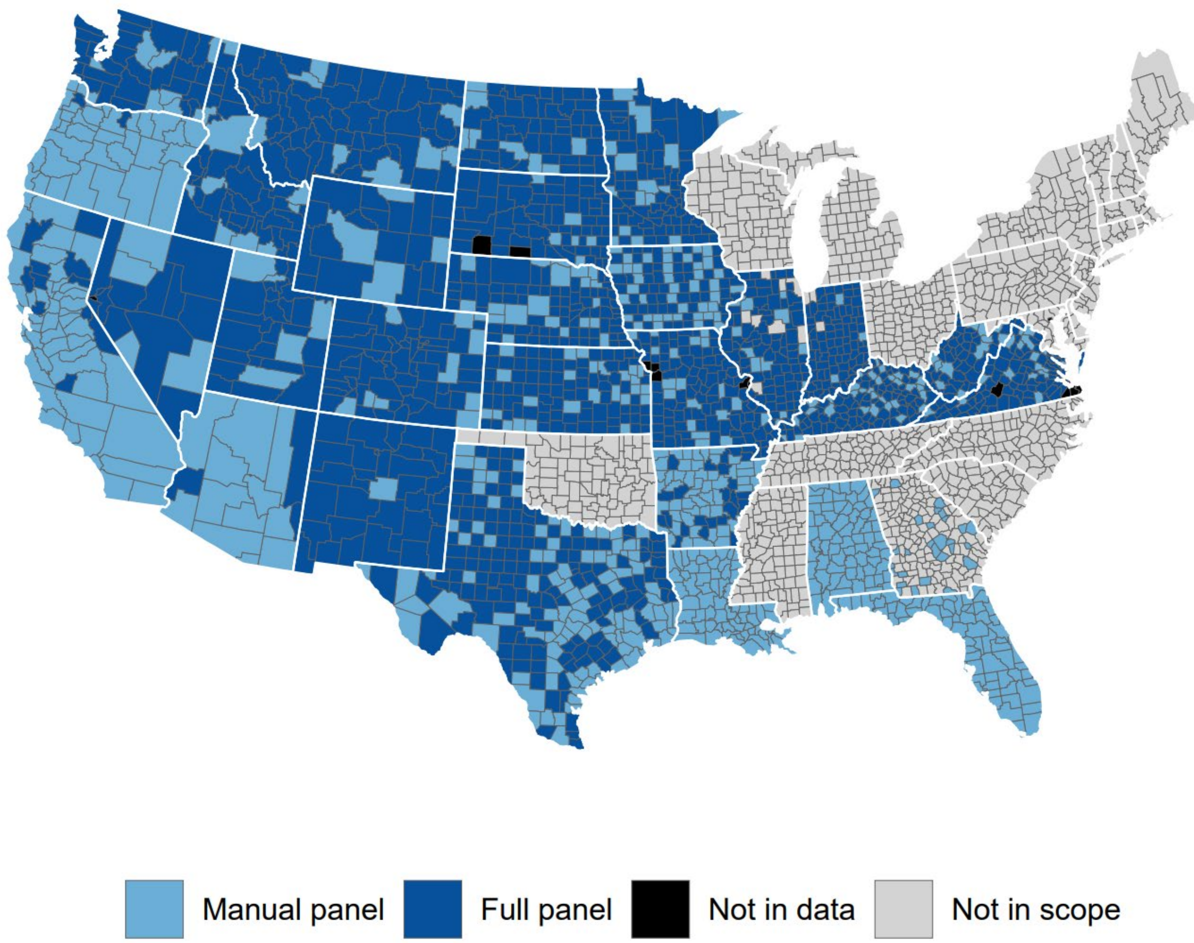
Number of jurisdictions are total number of jurisdictions in that state. Jurisdictions Used are the number of jurisdictions with a full panel of data between 2000 and 2024 and used in the main analysis. In states where multiple officials are coded, a '/' separates each distinct official and they are listed in order by frequency. I aim to code the official in each jurisdiction with primary authority to administer elections, especially those who oversee voting administration on Election Day. In jurisdictions with boards, I identify the single official with the most responsibility in running elections. In New York, no single individual could be identified so I code the two election commissioners in each jurisdiction. I exclude jurisdictions in other states where no single individual could be identified. I was unable to collect municipal-level data in Michigan, so instead code the most important county-level official. I was unable to collect data on municipal moderators in New Hampshire, so instead code the municipal clerk. Selection method indicates whether all officials coded in each state are elected, appointed, or a mix of both. Sole authority designates whether the official is the only election authority in that jurisdiction, excepting local legislative bodies that determine election administration budgets and appointing bodies whose sole purpose is to select a chief election official. Primary authority indicates whether the official coded is in charge of the majority of election administration responsibilities in the jurisdiction. For both columns, the modal coded official in the state is classified. For election source, 'State' indicates the data derives from the state election authority, 'Elections' indicates the data derives from election results, and 'NGO' indicates the data derives from a state-level independent organization, typically a state association of election officials. The date ranges indicate the maximal amount of data captured for each state, although only data from 2000 onward is used in the analysis.

A.2 Data Map of Jurisdictions Used in Observational Analysis

Observational data analysis of the effects of minority election administration is limited to county jurisdictions where a single individual has primary responsibility to run elections. This is to ensure that the analysis focuses on places where we should expect to find an effect, if one exists. Because the L2 data is available for all even-year general elections between 2014 and 2020, those are the years that compromise the data panel. Figure A.1 maps counties by data availability used in the observational analysis.

In total, these restrictions mean I conduct statistical analysis on a set of 4,453 unique local election officials, encompassing 2,861 jurisdictions across 4 election cycles. I have complete manual race coding between 2014 and 2020 for 1,161 local election officials across 949 jurisdictions.

Figure A.1: **Map of Local Election Official Racial Data Availability For Observational Analysis, 2014-2020**. This graph displays the best panel data of local election official/race ethnicity available in each county used in statistical analysis. Counties in light blue have complete subjective researcher-collected data between 2014 and 2020. Counties in dark blue do not have complete subjective race data, but do have BISG-derived estimates of election official race/ethnicity between 2014 and 2020. Counties in grey are not in scope, either because their elections are administered at the municipal level or there is not a single individual election official with primary responsibility to run elections. Counties in black are not in the data.



A.3 Additional Descriptive Data Visualizations of Changes in Racial Composition of Local Election Officials

Figure A.2: **Local Election Administrator Race By State, 2000-2024 - Preferred Data**. This figure displays over time change in the race/ethnicity of local election officials over the past 25 years, broken down by state. The race data comes from subjective researcher-coded race imputation where available and BISG race imputation otherwise.

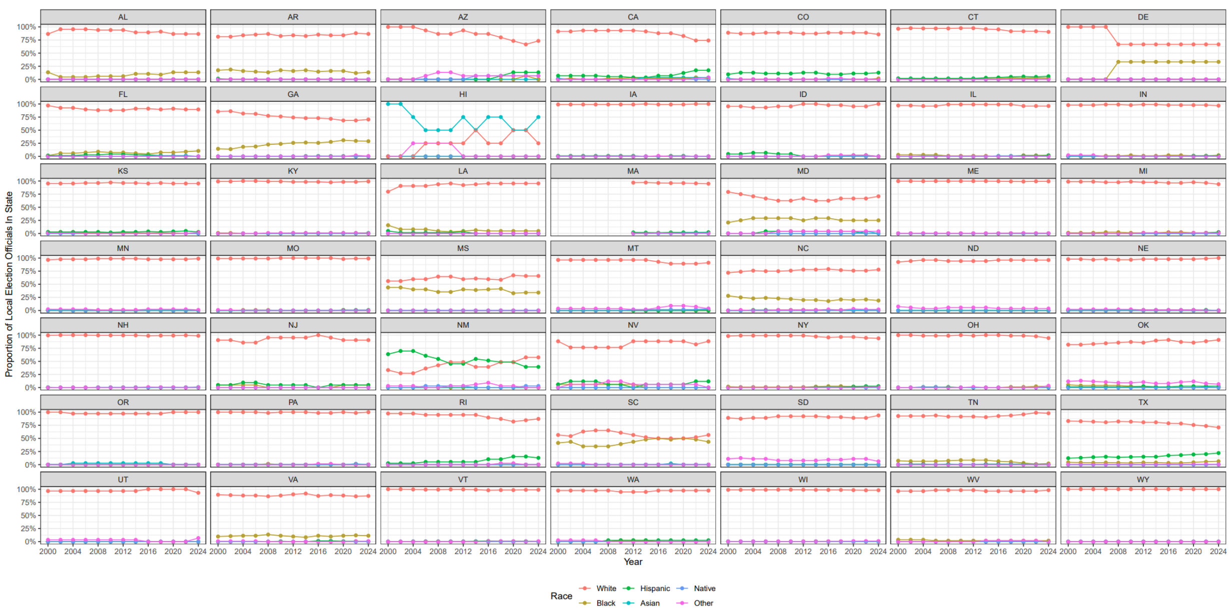


Figure A.3: **Local Election Administrator Race By State, 2000-2024 - BISG Data**. This figure displays over time change in the race/ethnicity of local election officials over the past 25 years, broken down by state. The race data comes from BISG race imputation.

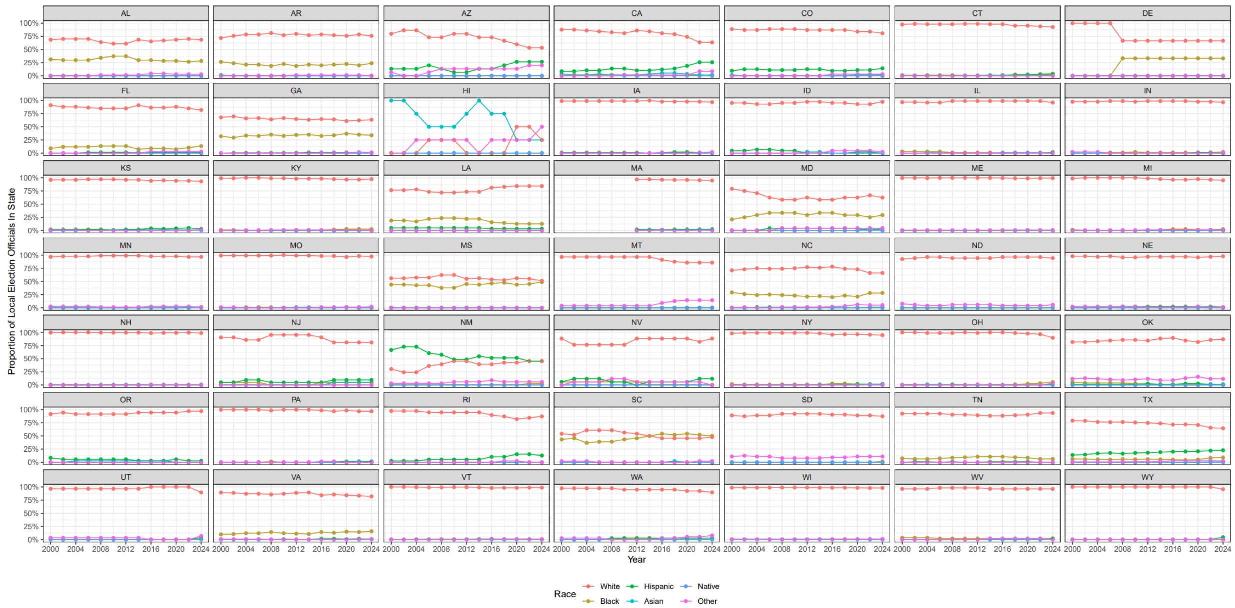


Figure A.4: **Local Election Administrator Race By State, 2000-2024 - Manual Data**. This figure displays over time change in the race/ethnicity of local election officials over the past 25 years, broken down by state, for states with at least 10 jurisdictions with panel data of subjective researcher-coded races. Only jurisdictions with complete panel data are included in this graph.

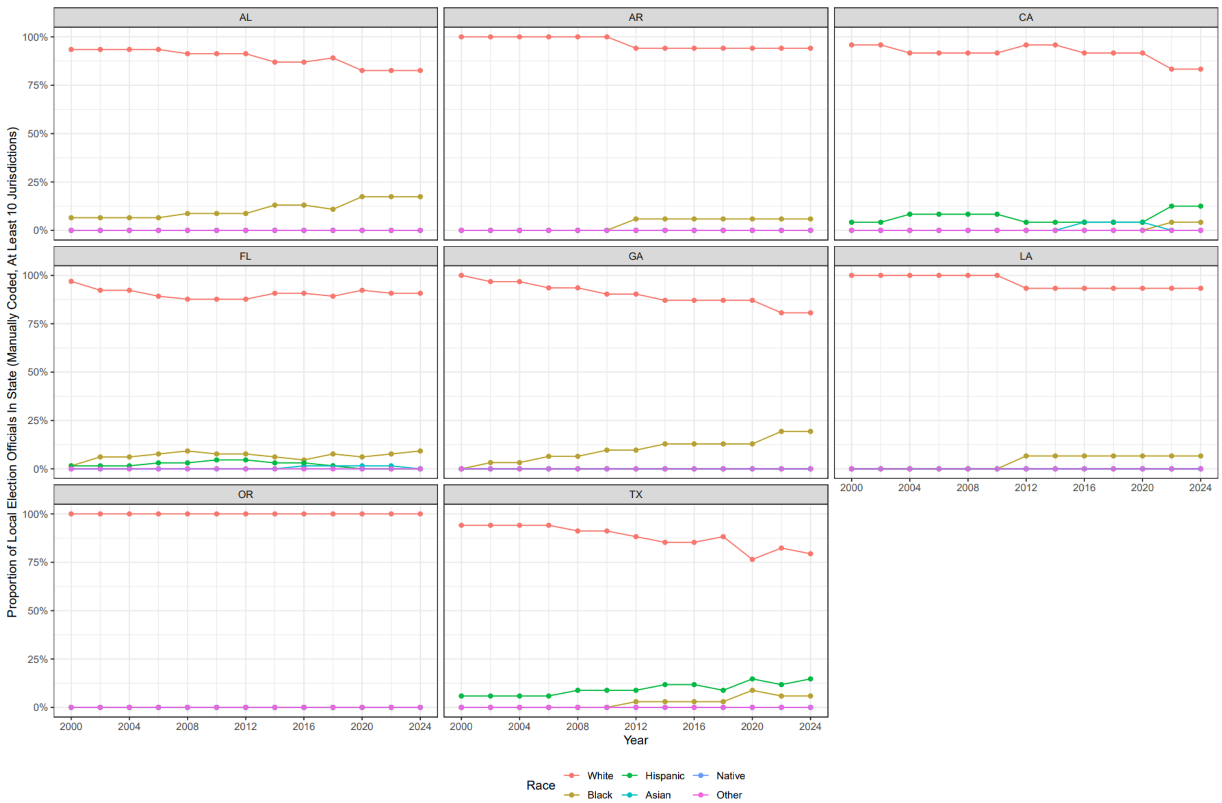


Figure A.5: **Local Election Administrator Race Map, 2000-2024 - Preferred Data**. This figure displays over time change in the race/ethnicity of local election officials over the past 25 years using subjective researcher-coded race imputation where available and BISG race imputation otherwise.

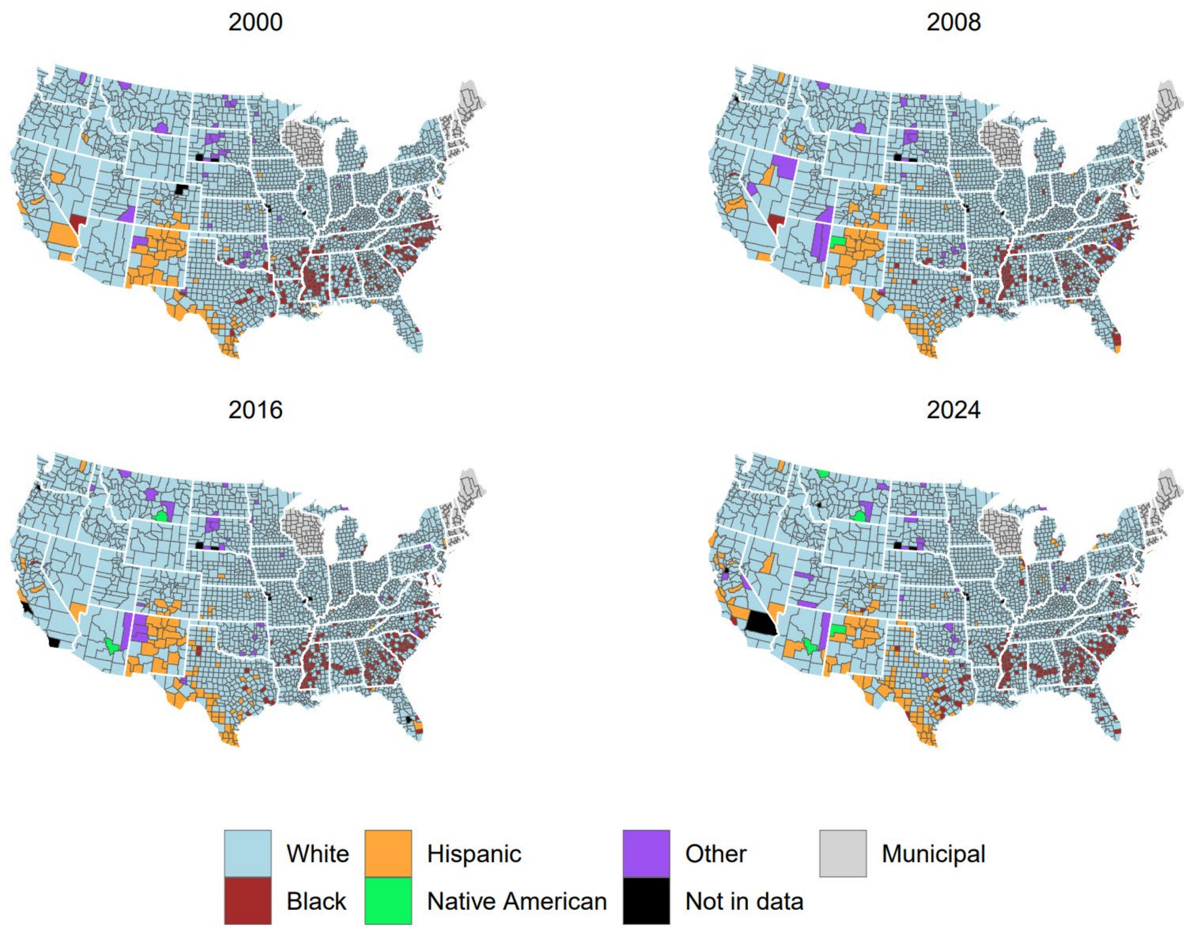
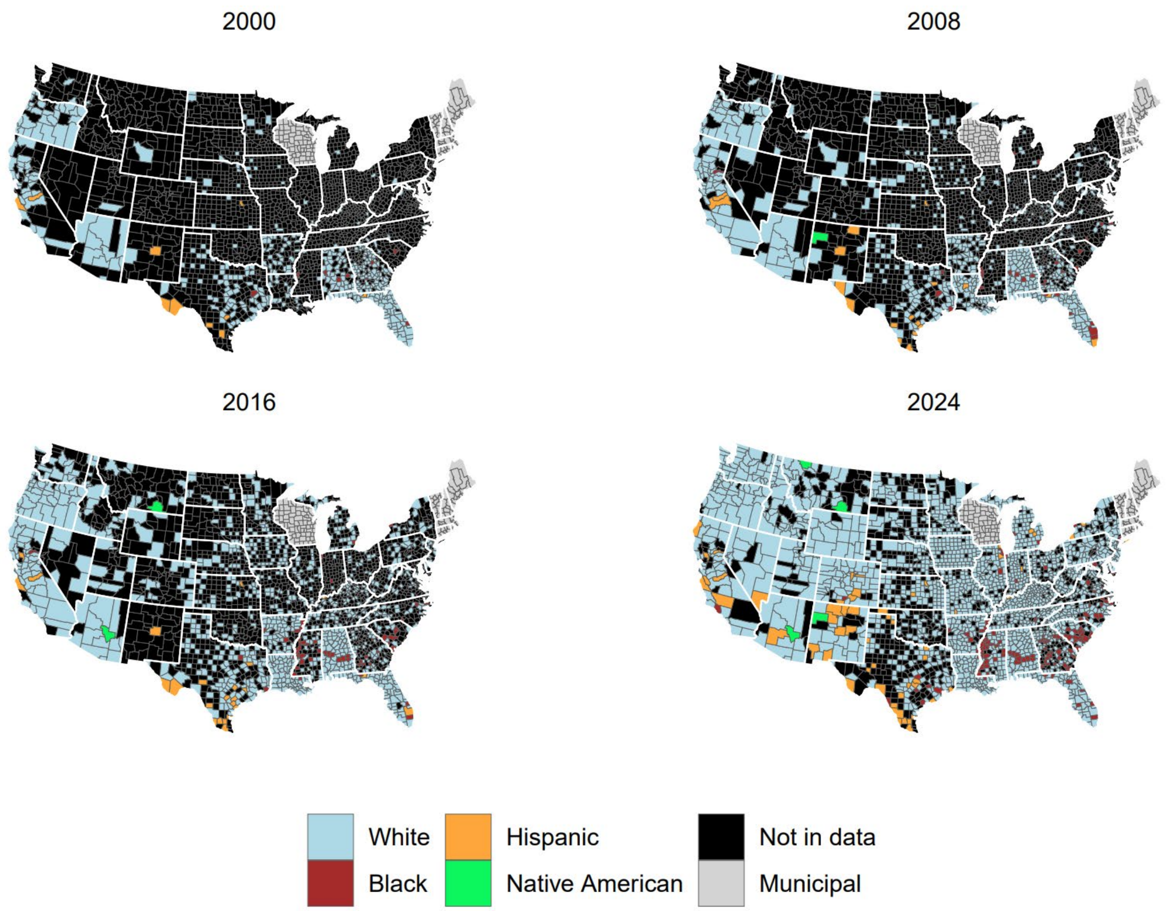


Figure A.6: **Local Election Administrator Race Map, 2000-2024 - Manual Data**. This figure displays over time change in the race/ethnicity of local election officials over the past 25 years using subjective researcher-coded race imputation.



A.4 Replicating Main Results Using Alternative Race Data Specifications

In this section, I replicate the main results with four alternate data specifications: (1) using only manually race-coded data, (2) using only manually race-coded data in jurisdictions with a complete panel of data between 2014 and 2020, (3) using only manual race-coded panel data in jurisdictions where the local election official wields strong authority (they undertake nearly all administrative duties), and (4) jurisdictions where the local election official wields sole authority (they undertake all administrative duties). These data specifications grow increasingly restrictive but are the cases we should be most likely to observe effects. In each case, I replicate five main specifications: Tables 1, 2, 3, 4, and 5.

Across all specifications, the same basic story remains: there is little apparent effect on minority voter turnout or election administration outcomes when a county switches from a white official to non-white official.

A.4.1 Manual Race-Coded Data

Table A.2: Minority Election Officials Do Not Affect Black Participation Rates (Manual Race, 2014-2020)

	Black Voter Turnout	Black Turnout Share	Black Reg	Black Reg Share
	(1)	(2)	(3)	(4)
Minority	−0.0002	0.001	−0.001	−0.0003
	(0.008)	(0.002)	(0.014)	(0.002)
County FEs	Yes	Yes	Yes	Yes
Year x State FEs	Yes	Yes	Yes	Yes
Observations	2,758	3,787	2,758	3,787

Table A.3: Minority Election Officials Do Not Affect Latino Participation Rates (Manual Race, 2014-2020)

	Latino Voter Turnout	Latino Turnout Share	Latino Reg	Latino Reg Share
	(1)	(2)	(3)	(4)
Minority	−0.003	0.001	0.004	0.002
	(0.009)	(0.003)	(0.011)	(0.002)
County FEs	Yes	Yes	Yes	Yes
Year x State FEs	Yes	Yes	Yes	Yes
Observations	3,154	3,787	3,154	3,787

Table A.4: Minority Election Officials Do Not Affect Asian Participation Rates (Manual Race, 2014-2020)

	Asian Voter Turnout	Asian Turnout Share	Asian Reg	Asian Reg Share
	(1)	(2)	(3)	(4)
Minority	0.016	0.002	0.012	0.001
	(0.015)	(0.001)	(0.016)	(0.0004)
County FEs	Yes	Yes	Yes	Yes
Year x State FEs	Yes	Yes	Yes	Yes
Observations	1,718	3,787	1,718	3,787

Table A.5: Minority Election Officials Do Not Affect White Participation Rates (Manual Race, 2014-2020)

	White Voter Turnout	White Turnout Share	White Reg	White Reg Share
	(1)	(2)	(3)	(4)
Minority	−0.001	−0.004	0.003	−0.002
	(0.003)	(0.004)	(0.005)	(0.003)
County FEs	Yes	Yes	Yes	Yes
Year x State FEs	Yes	Yes	Yes	Yes
Observations	3,787	3,787	3,787	3,787

Table A.6: Minority Election Officials Pursue Similar Administration Policies (Manual Race, 2014-2020)

	Polling Places	Prov Share	Prov Rejection	Absentee Rejection	Reg Removal	Wait Share
	(1)	(2)	(3)	(4)	(5)	(6)
Minority	−0.052	0.001	−0.006	0.006	−0.004	−0.007
	(0.080)	(0.002)	(0.045)	(0.005)	(0.008)	(0.018)
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year x State FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,464	3,440	2,698	3,420	3,645	2,575

A.4.2 Manual Race-Coded Panel

Table A.7: Minority Election Officials Do Not Affect Black Participation Rates (Manual Race Panel, 2014-2020)

	Black Voter Turnout	Black Turnout Share	Black Reg	Black Reg Share
	(1)	(2)	(3)	(4)
Minority	0.001	0.0005	−0.002	−0.002
	(0.009)	(0.003)	(0.014)	(0.002)
County FEs	Yes	Yes	Yes	Yes
Year x State FEs	Yes	Yes	Yes	Yes
Observations	2,259	2,968	2,259	2,968

Table A.8: Minority Election Officials Do Not Affect Latino Participation Rates (Manual Race Panel, 2014-2020)

	Latino Voter Turnout	Latino Turnout Share	Latino Reg	Latino Reg Share
	(1)	(2)	(3)	(4)
Minority	−0.008	−0.0004	0.003	0.002
	(0.009)	(0.003)	(0.012)	(0.002)
County FEs	Yes	Yes	Yes	Yes
Year x State FEs	Yes	Yes	Yes	Yes
Observations	2,471	2,968	2,471	2,968

Table A.9: Minority Election Officials Do Not Affect Asian Participation Rates (Manual Race Panel, 2014-2020)

	Asian Voter Turnout	Asian Turnout Share	Asian Reg	Asian Reg Share
	(1)	(2)	(3)	(4)
Minority	0.016	0.002	0.012	0.001
	(0.015)	(0.001)	(0.016)	(0.0004)
County FEs	Yes	Yes	Yes	Yes
Year x State FEs	Yes	Yes	Yes	Yes
Observations	1,415	2,968	1,415	2,968

Table A.10: Minority Election Officials Do Not Affect White Participation Rates (Manual Race Panel, 2014-2020)

	White Voter Turnout	White Turnout Share	White Reg	White Reg Share
	(1)	(2)	(3)	(4)
Minority	−0.001	−0.002	0.004	−0.001
	(0.003)	(0.004)	(0.005)	(0.003)
County FEs	Yes	Yes	Yes	Yes
Year x State FEs	Yes	Yes	Yes	Yes
Observations	2,968	2,968	2,968	2,968

Table A.11: Minority Election Officials Pursue Similar Administration Policies (Manual Race Panel, 2014-2020)

	Polling Places	Prov Share	Prov Rejection	Absentee Rejection	Reg Removal	Wait Share
	(1)	(2)	(3)	(4)	(5)	(6)
Minority	−0.058	0.001	0.009	0.003	−0.0003	−0.003
	(0.088)	(0.002)	(0.045)	(0.004)	(0.008)	(0.018)
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year x State FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,299	2,741	2,177	2,642	2,865	2,053

A.4.3 Manual Race-Coded Panel - Strong Authority

Table A.12: Minority Election Officials With Strong Authority Do Not Affect Black Participation Rates (Manual Race Panel, 2014-2020)

	Black Voter Turnout	Black Turnout Share	Black Reg	Black Reg Share
	(1)	(2)	(3)	(4)
Minority	0.004	0.002	0.017	−0.002
	(0.017)	(0.005)	(0.025)	(0.002)
County FEs	Yes	Yes	Yes	Yes
Year x State FEs	Yes	Yes	Yes	Yes
Observations	909	1,396	909	1,396

Table A.13: Minority Election Officials With Strong Authority Do Not Affect Latino Participation Rates (Manual Race Panel, 2014-2020)

	Latino Voter Turnout	Latino Turnout Share	Latino Reg	Latino Reg Share
	(1)	(2)	(3)	(4)
Minority	−0.020	−0.002	−0.004	0.003
	(0.014)	(0.006)	(0.017)	(0.003)
County FEs	Yes	Yes	Yes	Yes
Year x State FEs	Yes	Yes	Yes	Yes
Observations	1,170	1,396	1,170	1,396

Table A.14: Minority Election Officials With Strong Authority Do Not Affect Asian Participation Rates (Manual Race Panel, 2014-2020)

	Asian Voter Turnout	Asian Turnout Share	Asian Reg	Asian Reg Share
	(1)	(2)	(3)	(4)
Minority	0.001	0.001	0.007	0.0002
	(0.015)	(0.001)	(0.025)	(0.001)
County FEs	Yes	Yes	Yes	Yes
Year x State FEs	Yes	Yes	Yes	Yes
Observations	718	1,396	718	1,396

Table A.15: Minority Election Officials With Strong Authority Do Not Affect White Participation Rates (Manual Race Panel, 2014-2020)

	White Voter Turnout	White Turnout Share	White Reg	White Reg Share
	(1)	(2)	(3)	(4)
Minority	−0.003	−0.002	0.0002	−0.0005
	(0.004)	(0.007)	(0.005)	(0.005)
County FEs	Yes	Yes	Yes	Yes
Year x State FEs	Yes	Yes	Yes	Yes
Observations	1,396	1,396	1,396	1,396

Table A.16: Minority Election Officials With Strong Authority Pursue Similar Administration Policies (Manual Race Panel, 2014-2020)

	Polling Places	Prov Share	Prov Rejection	Absentee Rejection	Reg Removal	Wait Share
	(1)	(2)	(3)	(4)	(5)	(6)
Minority	0.006	−0.001	0.011	0.001	0.006	0.004
	(0.020)	(0.001)	(0.048)	(0.002)	(0.011)	(0.020)
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year x State FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	533	1,310	1,043	1,385	1,354	918

A.4.4 Manual Race-Coded Panel - Sole Authority

Table A.17: Minority Election Officials With Sole Authority Do Not Affect Black Participation Rates (Manual Race Panel, 2014-2020)

	Black Voter Turnout	Black Turnout Share	Black Reg	Black Reg Share
	(1)	(2)	(3)	(4)
Minority	0.043	−0.002	0.098	−0.002
	(0.039)	(0.002)	(0.061)	(0.002)
County FEs	Yes	Yes	Yes	Yes
Year x State FEs	Yes	Yes	Yes	Yes
Observations	551	1,024	551	1,024

Table A.18: Minority Election Officials With Sole Authority Do Not Affect Latino Participation Rates (Manual Race Panel, 2014-2020)

	Latino Voter Turnout	Latino Turnout Share	Latino Reg	Latino Reg Share
	(1)	(2)	(3)	(4)
Minority	0.006	0.002	0.010	0.003
	(0.013)	(0.008)	(0.019)	(0.005)
County FEs	Yes	Yes	Yes	Yes
Year x State FEs	Yes	Yes	Yes	Yes
Observations	817	1,024	817	1,024

Table A.19: Minority Election Officials With Sole Authority Do Not Affect Asian Participation Rates (Manual Race Panel, 2014-2020)

	Asian Voter Turnout	Asian Turnout Share	Asian Reg	Asian Reg Share
	(1)	(2)	(3)	(4)
Minority	−0.015	0.003	0.008	0.0002
	(0.012)	(0.003)	(0.010)	(0.002)
County FEs	Yes	Yes	Yes	Yes
Year x State FEs	Yes	Yes	Yes	Yes
Observations	488	1,024	488	1,024

Table A.20: Minority Election Officials With Sole Authority Do Not Affect White Participation Rates (Manual Race Panel, 2014-2020)

	White Voter Turnout	White Turnout Share	White Reg	White Reg Share
	(1)	(2)	(3)	(4)
Minority	−0.005	−0.003	0.011	−0.002
	(0.007)	(0.014)	(0.004)	(0.008)
County FEs	Yes	Yes	Yes	Yes
Year x State FEs	Yes	Yes	Yes	Yes
Observations	1,024	1,024	1,024	1,024

Table A.21: Minority Election Officials With Sole Authority Pursue Similar Administration Policies (Manual Race Panel, 2014-2020)

	Polling Places	Prov Share	Prov Rejection	Absentee Rejection	Reg Removal	Wait Share
	(1)	(2)	(3)	(4)	(5)	(6)
Minority	0.017	−0.002	−0.164	0.003	−0.019	0.011
	(0.034)	(0.002)	(0.072)	(0.003)	(0.029)	(0.012)
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year x State FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	349	950	699	1,015	982	596

A.5 Analysis of Southern States Using Voter-Provided Race Data

In this section, I conduct a robustness test of the main analysis using higher-quality administrative data from the three states where it is usefully available; Alabama, Florida, and Georgia. Rather than BISG-imputed race in the voter file, in these states voters provide their race on the registration form.

Table A.22 displays difference-in-differences specifications testing the effects of minority election administration on Black voter participation, Table A.23 shows the effects on Latino participation, Table A.24 shows the effects on Asian participation, and Table A.25 shows the effects on white participation. As in the main analysis, Black, Asian, and Latino election officials are pooled together as minority officials to maximize statistical power.

In all four tables, column 1, my preferred specification, uses a combination of Georgia administrative and L2 voter file data from 2014 onward to test the effects of minority election administration on minority voter turnout. Column 2 uses both Georgia administrative data and the full L2 data, calculating turnout rates back to 1996. For Tables A.22 and A.25 testing Black and white turnout rates, respectively, there is enough administrative data from Georgia to use it exclusively. This is done in column 3. In all four tables, the final column tests race-specific registration rates using administrative data. The results are in line with the main analysis, revealing near-zero point estimates that are precisely estimated. I also run a set of specifications using race-specific turnout and registration shares. Tables A.26 through A.29 show participation shares for Blacks, Latinos, Asians, and whites, respectively. The results are similarly null and more precisely estimated.

Table A.22: Minority Election Officials Do Not Affect Black Participation Rates (AL, FL, and GA, 1996-2022)

	Black Voter Turnout			Black Reg
	(1)	(2)	(3)	(4)
Minority	0.001	0.007	−0.008	−0.004
	(0.010)	(0.009)	(0.022)	(0.016)
County FEs	Yes	Yes	Yes	Yes
Year x State FEs	Yes	Yes	Yes	Yes
Data	Admin + L2	Admin + L2 Full	Admin	Admin
Observations	1,641	2,184	842	2,447

Table A.23: Minority Election Officials Do Not Affect Latino Participation Rates (AL, FL, and GA, 1996-2022)

	Latino Voter Turnout		Latino Reg
	(1)	(2)	(3)
Minority	−0.009	0.002	−0.011
	(0.013)	(0.012)	(0.013)
County FEs	Yes	Yes	Yes
Year x State FEs	Yes	Yes	Yes
Data	Admin + L2	Admin + L2 Full	Admin
Observations	944	1,587	1,230

Table A.24: Minority Election Officials Do Not Affect Asian Participation Rates (AL, FL, and GA, 1996-2022)

	Asian Voter Turnout		Asian Reg
	(1)	(2)	(3)
Minority	0.020	0.002	0.014
	(0.015)	(0.016)	(0.032)
County FEs	Yes	Yes	Yes
Year x State FEs	Yes	Yes	Yes
Data	Admin + L2	Admin + L2 Full	Admin
Observations	518	887	631

Table A.25: Minority Election Officials Do Not Affect White Participation Rates (AL, FL, and GA, 1996-2022)

	White Voter Turnout			White Reg
	(1)	(2)	(3)	(4)
Minority	−0.008	−0.008	−0.010	−0.021
	(0.012)	(0.009)	(0.030)	(0.013)
County FEs	Yes	Yes	Yes	Yes
Year x State FEs	Yes	Yes	Yes	Yes
Data	Admin + L2	Admin + L2 Full	Admin	Admin
Observations	1,682	2,228	879	2,495

Table A.26: Minority Election Officials Do Not Affect Share of Black Participation (AL, FL, and GA, 1996-2022)

	Black Turnout Share			Black Reg Share
	(1)	(2)	(3)	(4)
Minority	0.004	0.005	0.001	0.014
	(0.004)	(0.003)	(0.009)	(0.006)
County FEs	Yes	Yes	Yes	Yes
Year x State FEs	Yes	Yes	Yes	Yes
Data	Admin + L2	Admin + L2 Full	Admin	Admin
Observations	1,683	2,229	879	2,496

Table A.27: Minority Election Officials Do Not Affect Share of Latino Participation (AL, FL, and GA, 1996-2022)

	Latino Turnout Share		Latino Reg Share
	(1)	(2)	(3)
Minority	−0.002	0.003	−0.001
	(0.005)	(0.002)	(0.004)
County FEs	Yes	Yes	Yes
Year x State FEs	Yes	Yes	Yes
Data	Admin + L2	Admin + L2 Full	Admin
Observations	1,285	2,225	1,620

Table A.28: Minority Election Officials Do Not Affect Share of Asian Participation (AL, FL, and GA, 1996-2022)

	Asian Turnout Share		Asian Reg Share
	(1)	(2)	(3)
Minority	0.0004	0.0004	0.0005
	(0.001)	(0.001)	(0.001)
County FEs	Yes	Yes	Yes
Year x State FEs	Yes	Yes	Yes
Data	Admin + L2	Admin + L2 Full	Admin
Observations	1,117	2,216	1,139

Table A.29: Minority Election Officials Do Not Affect Share of White Participation (AL, FL, and GA, 1996-2022)

	White Turnout Share			White Reg Share
	(1)	(2)	(3)	(4)
Minority	0.0004	−0.006	0.010	−0.014
	(0.005)	(0.005)	(0.010)	(0.007)
County FEs	Yes	Yes	Yes	Yes
Year x State FEs	Yes	Yes	Yes	Yes
Data	Admin + L2	Admin + L2 Full	Admin	Admin
Observations	1,683	2,229	879	2,496

A.6 Coethnic Election Officials Produce Similar Levels of Participation as Non-Coethnic Officials

The main estimates presented in Section 5 pooled Black, Latino, and Asian local election officials together as minorities. In this section, I explore whether switching to a Black local election official improves Black participation rates and, similarly, whether switching to a Latino official improves Latino participation rates. Table A.30 shows the effects of coethnic representation on Blacks and Table A.31 shows the effects of coethnic representation on Latinos.

Table A.30: Black Election Officials Do Not Affect Black Participation Rates (Manual + Geocoded Races, 2014-2020)

	Black Voter Turnout	Black Turnout Share	Black Reg	Black Reg Share
	(1)	(2)	(3)	(4)
Black LEO	−0.007	−0.004	−0.016	−0.003
	(0.012)	(0.006)	(0.019)	(0.005)
County FEs	Yes	Yes	Yes	Yes
Year x State FEs	Yes	Yes	Yes	Yes
Observations	4,889	8,132	4,889	8,132

The results do not substantively differ from those presented in the main analysis. In fact, the point estimates are more negative than in the main analysis, though less precisely estimated giving the reduced power of the tests. It does not appear that coethnic representation in the local election official boosts turnout or registration.

Table A.31: Latino Election Officials Do Not Affect Latino Participation Rates (Manual + Geocoded Races, 2014-2020)

	Latino Turnout Share	Latino Turnout Share	Latino Reg	Latino Reg Share
	(1)	(2)	(3)	(4)
Latino LEO	0.003	0.006	−0.001	0.001
	(0.006)	(0.004)	(0.008)	(0.002)
County FEs	Yes	Yes	Yes	Yes
Year x State FEs	Yes	Yes	Yes	Yes
Observations	6,202	8,132	6,202	8,132

A.6.1 Minority and White Officials Administer Elections Similarly - Full Dataset

In the main analysis, I showed that minority and white local election officials administer elections similarly across a wide range of policies. The panel was limited to 2014-2020 to keep the data in line with the regressions displayed in Section 5.1. Table A.32 displays the results of an analysis using the full 2004-2022 panel of available data. More data significantly improves the precision of the estimates and flips the direction of some of the point estimates. Even so, all point estimates remain statistically indistinguishable from null results. I read this as further evidence that minority and white election officials administer elections in similar ways.

Table A.32: Minority Election Officials Pursue Similar Administration Policies (Manual + Geocoded Races, 2004-2022)

	Polling Places (1)	Prov Share (2)	Prov Rejection (3)	Absentee Rejection (4)	Reg Removal (5)	Wait Share (6)
Minority	0.036	0.001	0.004	−0.003	−0.001	0.010
	(0.026)	(0.001)	(0.018)	(0.005)	(0.002)	(0.009)
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year x State FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	13,924	14,231	10,630	17,165	16,172	8,697

A.7 Minority and White Officials Produce Similar Levels of Overall Voter Participation

Do minority election officials positively impact aggregate turnout and registration rates? Table A.33 displays the results of a two-way fixed effects regression estimating the effects of switching from a white to a minority local election official on overall voter turnout, using the same 2014-2020 timespan as the main analysis and CVAP as the denominator. Column 1 shows that counties switching from white to minority election officials see an average increase in overall voter turnout of 0.1 percentage points. The result is precisely estimated and we can confidently rule out effects larger than 1 percentage point. Column 2 tightens the comparisons to counties within the same state with similar pretreatment demographic makeup, column 3 makes comparisons between counties with similar pretreatment populations, and column 4 compares counties with similar partisan makeups. In all three, the point estimate is close to 0.

Table A.33: Minority Election Officials Do Not Affect Overall Turnout Rates (Manual + Geocoded Races, 2014-2020)

	Voter Turnout			
	(1)	(2)	(3)	(4)
Minority	0.001	0.003	0.001	0.002
	(0.004)	(0.004)	(0.004)	(0.004)
County FEs	Yes	Yes	Yes	Yes
Year x State FEs	Yes	No	No	No
Year x State x Nonwhite FEs	No	Yes	No	No
Year x State x Pop FEs	No	No	Yes	No
Year x State x Dem VS FEs	No	No	No	Yes
Observations	8,132	8,132	8,132	8,132

Table A.34 displays the output of regression specifications testing the effects of minority local election administration on overall voter registration rates. The results are nearly identical to Table A.33, with near-zero point estimates that are precisely estimated.[47]

Table A.34: Minority Election Officials Do Not Affect Overall Registration Rates (Manual + Geocoded Races, 2014-2020)

	Voter Registration			
	(1)	(2)	(3)	(4)
Minority	−0.002	−0.001	−0.002	−0.001
	(0.004)	(0.004)	(0.004)	(0.004)
County FEs	Yes	Yes	Yes	Yes
Year x State FEs	Yes	No	No	No
Year x State x Nonwhite FEs	No	Yes	No	No
Year x State x Pop FEs	No	No	Yes	No
Year x State x Dem VS FEs	No	No	No	Yes
Observations	8,132	8,132	8,132	8,132

Finally, because overall participation rates are not reliant on the L2 data, we are able to increase the length of the panel. I employ county-level turnout and registration data from Dave Leip's Election Atlas[48] and county-level voting-age population (VAP) from the U.S. National Cancer Institute.[49] Tables A.35 and A.36 display overall turnout and registration results, respectively, this time using the full 2000-2022 panel for voter turnout and 2000-2020 for registration rates. This analysis yields similar results, with the overall registration rates trending, if anything, slightly negative.

In sum, these findings suggest that minority election officials do not significantly improve voter

[47] For both turnout and registration rates, regressions including only Presidential contests yield substantively identical findings.

[48] https://uselectionatlas.org/

[49] https://seer.cancer.gov/popdata/

participation rates, but rather oversee elections with similar levels of participation as white election officials.

Table A.35: Minority Election Officials Do Not Affect Overall Turnout Rates (Manual + Geocoded Races, 2000-2022)

	Voter Turnout (VAP)			
	(1)	(2)	(3)	(4)
Minority	−0.004	−0.003	−0.003	−0.004
	(0.002)	(0.002)	(0.002)	(0.002)
County FEs	Yes	Yes	Yes	Yes
Year x State FEs	Yes	No	No	No
Year x State x Nonwhite FEs	No	Yes	No	No
Year x State x Pop FEs	No	No	Yes	No
Year x State x Dem VS FEs	No	No	No	Yes
Observations	24,396	24,396	24,396	24,396

Table A.36: Minority Election Officials Do Not Affect Overall Registration Rates (Manual + Geocoded Races, 2000-2020)

	Voter Registration			
	(1)	(2)	(3)	(4)
Minority	−0.007	−0.006	−0.006	−0.008
	(0.003)	(0.003)	(0.003)	(0.003)
County FEs	Yes	Yes	Yes	Yes
Year x State FEs	Yes	No	No	No
Year x State x Nonwhite FEs	No	Yes	No	No
Year x State x Pop FEs	No	No	Yes	No
Year x State x Dem VS FEs	No	No	No	Yes
Observations	23,760	23,760	23,760	23,760

A.8 Pilot Survey Appendix

I conducted a pilot survey experiment as part of the 2023 UCLA REPS Lab Omnibus Survey. This survey was a multi-investigator study run by Efrén Pérez and fielded between March and June 2023. It included a convenience sample of 548 undergraduate participants from UCLA, U Riverside, UC Irvine, and Howard University recruited by professors in high-enrollment political science courses and received approval by the UCLA IRB prior to fielding. The survey asked a range of demographic questions about each participant before the experimental modules. Respondents were debriefed at the end of the study about the fictitious nature of the vignette description and the purpose of the experiment. Respondents received course credit for participation in the study and were notified about their right to choose not to participate or withdraw from participation at any time.

Respondents read a short vignette of a local election official who was described as determining the eligibility of absentee ballots in Fayette County, GA in the 2020 presidential election, a salient election and county. The race/ethnicity of the official was included in the description, as well as their job tenure, party affiliation, age, geographic background, and views on voter identification and absentee voting. Respondents were randomly assigned into two conditions: one describing the official as white (control) and one describing the official as the same race/ethnicity as the respondent (treatment). All other variables were held constant. Respondents were asked two questions: whether they trust that official to fairly administer elections, and whether they would be more or less likely to vote if that official was their election administrator. Both were measured on a 5-point Likert response scale. The full module instrument can be found in the following appendix subsection.

Table A.37 displays difference-in-means estimations pooled across Black, Latino, and Asian respondents. Column 1 shows that respondents tend to trust coethnic local election officials to fairly administer elections slightly more than white election officials, on average. Minority respondents give an average trust rating of 3.12 out of 5 to coethnic election officials, compared with 2.91 to white officials otherwise identically described, a standardized effect size of 0.24. Column 2 shows that minority respondents report being slightly more likely to vote when their election official shares their ethnic/racial affiliation than when they are white, although the effect size is similarly modest. Regressions run separately for Black, Latino, and Asian respondents, found in A.9 in the Online Appendix, yield similar results.

Table A.37: Experiment: Coethinic Local Election Officials Boost Voter Confidence and Participation Willingness

	Trust Official	Likelier to Vote
	(1)	(2)
Coethnic LEO	0.209*	0.224**
	(0.112)	(0.094)
Constant	2.906***	2.838***
	(0.079)	(0.066)
Observations	230	230

Table A.38 displays difference-in-means regressions run separately for Black, Latino, and Asian respondents. The results are noisier due to the small sample sizes, though they provide a similar overall picture of modest positive effects due to descriptive representation.

The results show some evidence that descriptive representation among election officials matters, but the magnitude of the effect is rather small.

Table A.38: Experiment: Coethinic Local Election Officials Boost Voter Confidence and Participation Willingness

	Trust Official			Likelier to Vote		
	(1)	(2)	(3)	(4)	(5)	(6)
Coethnic LEO	0.151	0.322*	0.118	−0.151	0.554***	0.529
	(0.163)	(0.169)	(0.343)	(0.139)	(0.119)	(0.320)
Constant	2.925***	2.957***	2.706***	3.038***	2.702***	2.588***
	(0.115)	(0.117)	(0.243)	(0.098)	(0.082)	(0.226)
Race	Latino	Asian	Black	Latino	Asian	Black
Observations	106	90	34	106	90	34

A.8.1 Pilot Survey Instrument

This section includes the pilot survey instrument module as part of the 2023 UCLA REPS Lab Omnibus Survey.

For Black respondents: Don Brown has been the Elections Administrator of Fayette County, Georgia for the past eight years. He is [Black/white], identifies with the Democratic Party, is 53 years of age, and has lived in Fayette County his whole life. Don supports stricter voter identification laws and expanding absentee voting opportunities for voters. In the 2020 presidential election, he was in charge of determining the eligibility of 10,000 absentee ballots received by the county, and rejected those that did not meet state requirements.

Do you trust Don Brown to fairly administer elections? 1 = Strongly distrust, 2 = distrust, 3= neither trust nor distrust, 4 = trust, 5 = strongly trust

If Don Brown was your election administrator, would you be more or less likely to vote in the next presidential election? 1 = Much less likely, 2 = less likely, 3 = no difference, 4 = more likely, 5 = much more likely

For Latino respondents: [David Marin/ Davíd Maríın] has been the Elections Administrator of Fayette County, Georgia for the past eight years. He is [Latino/white], identifies with the Democratic Party, is 53 years of age, and has lived in Fayette County his whole life. Don supports stricter voter identification laws and expanding absentee voting opportunities for voters. In the 2020 presidential election, he was in charge of determining the eligibility of 10,000 absentee ballots received by the county, and rejected those that did not meet state requirements.

Do you trust [David Marin/ Davíd Maríın] to fairly administer elections? 1 = Strongly distrust, 2 =

distrust, 3= neither trust nor distrust, 4 = trust, 5 = strongly trust

If [David Marin/ Davíd Maríın] was your election administrator, would you be more or less likely to vote in the next presidential election? 1 = Much less likely, 2 = less likely, 3 = no difference, 4 = more likely, 5 = much more likely

For Asian respondents: Eric Lee has been the Elections Administrator of Fayette County, Georgia for the past eight years. He is [Asian/white], identifies with the Democratic Party, is 53 years of age, and has lived in Fayette County his whole life. Don supports stricter voter identification laws and expanding absentee voting opportunities for voters. In the 2020 presidential election, he was in charge of determining the eligibility of 10,000 absentee ballots received by the county, and rejected those that did not meet state requirements.

Do you trust Eric Lee to fairly administer elections? 1 = Strongly distrust, 2 = distrust, 3= neither trust nor distrust, 4 = trust, 5 = strongly trust

If Eric Lee was your election administrator, would you be more or less likely to vote in the next presidential election? 1 = Much less likely, 2 = less likely, 3 = no difference, 4 = more likely, 5 = much more likely

A.9 Experimental Survey Appendix

The survey received approval from the UCLA IRB Review Board prior to fielding.

A.9.1 Vignette Experiment Additional Analysis

Table A.39 presents formal regression output for Figure 4. Table A.40 includes controls for age, gender, education, income, party, ideology, political awareness, 2020 presidential vote, and evangelical. Tables A.41 and A.42 show separate results for Black, Hispanic, and Asian respondents, with the latter table including controls.

Table A.39: Experiment: Coethinic Local Election Officials Boost Voter Confidence and Participation Willingness

	Trust Official	Likelier to Vote
	(1)	(2)
Coethnic LEO	0.210***	0.153***
	(0.053)	(0.054)
Constant	−0.143***	−0.137***
	(0.038)	(0.038)
Controls	No	No
Observations	1,400	1,400

Table A.40: Experiment: Coethnic Local Election Officials Boost Voter Confidence and Participation Willingness

	Trust Official		Likelier to Vote	
	(1)	(2)	(3)	(4)
Coethnic LEO	0.210***	0.182***	0.153***	0.174***
	(0.053)	(0.053)	(0.054)	(0.055)
age		0.007***		0.012***
		(0.002)		(0.002)
woman		0.049		−0.027
		(0.055)		(0.056)
educ4		−0.027		−0.007
		(0.028)		(0.029)
income		0.030***		0.034***
		(0.007)		(0.007)
partyIndependent		−0.104		−0.041
		(0.068)		(0.070)
partyOther		−0.318***		−0.337***
		(0.096)		(0.100)
partyRepublican		−0.252**		−0.270**
		(0.112)		(0.116)
conservative		−0.101***		−0.058*
		(0.031)		(0.032)
politically_aware		0.020		0.044
		(0.027)		(0.028)
vote_trump		−0.634***		−0.087
		(0.108)		(0.111)
evangelical		0.077		0.179**
		(0.073)		(0.075)
Constant	−0.143***	−0.148	−0.137***	−0.563***
	(0.038)	(0.123)	(0.038)	(0.127)
Observations	1,400	1,338	1,400	1,338

Table A.41: Experiment: Coethnic Local Election Officials Boost Voter Confidence and Participation Willingness

	Trust Official			Likelier to Vote		
	(1)	(2)	(3)	(4)	(5)	(6)
Coethnic LEO	0.071	0.366***	0.094	0.203**	0.174*	−0.034
	(0.083)	(0.095)	(0.098)	(0.084)	(0.096)	(0.100)
Constant	−0.047	−0.201***	−0.071	−0.163***	−0.132*	−0.012
	(0.058)	(0.067)	(0.070)	(0.059)	(0.068)	(0.072)
Race	Black	Asian	Hispanic	Black	Asian	Hispanic
Controls	No	No	No	No	No	No
Observations	590	393	417	590	393	417

Table A.42: Experiment: Coethinic Local Election Officials Boost Voter Confidence and Participation Willingness

	Trust Official			Likelier to Vote		
	(1)	(2)	(3)	(4)	(5)	(6)
Coethnic LEO	0.087	0.292***	0.134	0.254***	0.186*	−0.040
	(0.082)	(0.095)	(0.098)	(0.080)	(0.097)	(0.109)
age	0.005	0.015***	0.006	0.012***	0.018***	0.004
	(0.003)	(0.004)	(0.005)	(0.003)	(0.004)	(0.005)
woman	0.049	0.113	0.113	0.256***	−0.200**	−0.149
	(0.089)	(0.096)	(0.105)	(0.087)	(0.098)	(0.116)
educ4	−0.053	−0.043	−0.023	−0.015	−0.027	0.056
	(0.046)	(0.052)	(0.055)	(0.045)	(0.053)	(0.061)
income	0.044***	0.010	0.008	0.081***	0.007	−0.015
	(0.012)	(0.014)	(0.012)	(0.012)	(0.015)	(0.013)
partyIndependent	−0.029	−0.198	−0.179	−0.029	0.025	−0.251*
	(0.107)	(0.123)	(0.124)	(0.104)	(0.126)	(0.138)
partyOther	−0.107	−0.524***	−0.451**	−0.320**	−0.419**	−0.243
	(0.140)	(0.184)	(0.187)	(0.136)	(0.189)	(0.208)
partyRepublican	−0.078	−0.553***	−0.210	0.049	−0.527***	−0.219
	(0.207)	(0.191)	(0.200)	(0.202)	(0.197)	(0.222)
conservative	−0.140***	−0.009	−0.155**	−0.158***	0.066	−0.106
	(0.049)	(0.054)	(0.064)	(0.048)	(0.056)	(0.071)
politically_aware	0.077*	0.003	0.041	0.015	0.111**	−0.019
	(0.043)	(0.046)	(0.056)	(0.042)	(0.047)	(0.062)
vote_trump	−0.849***	−0.412**	−0.774***	−0.459**	−0.089	0.478**
	(0.208)	(0.175)	(0.190)	(0.202)	(0.180)	(0.210)
evangelical	0.122	0.015	0.315*	0.288***	−0.065	0.448**
	(0.096)	(0.152)	(0.181)	(0.093)	(0.156)	(0.200)
Constant	−0.007	−0.451**	0.228	−0.859***	−0.682***	0.213
	(0.205)	(0.214)	(0.227)	(0.200)	(0.220)	(0.252)
Race	Black	Asian	Hispanic	Black	Asian	Hispanic
Observations	566	381	391	566	381	391

A.9.2 Conjoint Experiment Additional Analysis

The following three tables are tabular output for the conjoint visualizes displayed in the main text.

Table A.43: Local Election Official Conjoint Experiment AMCEs

Feature	Level	Estimate	Std. Error
Party	Independent	0	
	Republican	-0.096	(0.01)
	Democrat	-0.041	(0.01)
Age	30 years old	0	
	50 years old	0.005	(0.009)
	70 years old	-0.072	(0.01)
Sex	Male	0	
	Female	0.022	(0.008)
Race	White	0	
	Asian	0.014	(0.011)
	Hispanic	0.019	(0.012)
	Black	0.015	(0.011)
Experience	2 years	0	
	5 years	0.095	(0.01)
	10 years	0.182	(0.01)
Require ID	Opposes	0	
	Supports	0.126	(0.01)
Expand VBM	Opposes	0	
	Supports	0.108	(0.009)

Additionally, I include visualizations and tabular output comparing white and minority preferences. The findings are congruent with the evidence presented in the main analysis. Non-whites prefer POCs to whites and that they have more intensive preferences for race than do whites.

Table A.44: Local Election Official Conjoint Experiment AMCEs - By Race

Respondent	Feature	Level	Estimate	Std. Error
White	Party	Independent	0	
		Republican	-0.096	(0.013)
		Democrat	-0.048	(0.013)
	Age	30 years old	0	
		50 years old	0.009	(0.012)
		70 years old	-0.048	(0.013)
	Sex	Male	0	
		Female	0.028	(0.01)
	Race	White	0	
		Asian	0.003	(0.014)
		Hispanic	0.013	(0.015)
		Black	0.011	(0.014)
	Experience	2 years	0	
		5 years	0.095	(0.012)
		10 years	0.174	(0.012)
	Require ID	Opposes	0	
		Supports	0.149	(0.012)
	Expand VBM	Opposes	0	
		Supports	0.104	(0.012)
Asian	Party	Independent	0	
		Republican	-0.059	(0.028)
		Democrat	-0.003	(0.024)
	Age	30 years old	0	
		50 years old	-0.059	(0.028)
		70 years old	-0.118	(0.029)
	Sex	Male	0	
		Female	0.003	(0.023)
	Race	White	0	
		Asian	0.061	(0.031)
		Hispanic	0.002	(0.031)
		Black	-0.011	(0.032)
	Experience	2 years	0	
		5 years	0.085	(0.025)
		10 years	0.197	(0.029)
	Require ID	Opposes	0	
		Supports	0.073	(0.027)
	Expand VBM	Opposes	0	
		Supports	0.135	(0.026)
Hispanic	Party	Independent	0	
		Republican	-0.128	(0.027)
		Democrat	-0.042	(0.032)
	Age	30 years old	0	
		50 years old	0.026	(0.027)
		70 years old	-0.121	(0.034)
	Sex	Male	0	
		Female	0.017	(0.023)
	Race	White	0	
		Asian	0.073	(0.034)
		Hispanic	0.047	(0.035)
		Black	0.015	(0.035)
	Experience	2 years	0	
		5 years	0.107	(0.033)
		10 years	0.193	(0.031)
	Require ID	Opposes	0	
		Supports	0.073	(0.028)
	Expand VBM	Opposes	0	
		Supports	0.115	(0.027)
Black	Party	Independent	0	
		Republican	-0.117	(0.023)
		Democrat	-0.045	(0.024)
	Age	30 years old	0	
		50 years old	-0.027	(0.025)
		70 years old	-0.125	(0.024)
	Sex	Male	0	
		Female	0.009	(0.02)
	Race	White	0	
		Asian	0.010	(0.027)
		Hispanic	0.038	(0.027)
		Black	0.070	(0.026)
	Experience	2 years	0	
		5 years	0.090	(0.022)
		10 years	0.206	(0.023)
	Require ID	Opposes	0	
		Supports	0.087	(0.025)
	Expand VBM	Opposes	0	
		Supports	0.135	(0.021)

Table A.45: Local Election Official Conjoint Experiment AMCEs - Race Diff

Comparison	Feature	Level	Estimate	Std. Error
Asian - White	Age	50 years old	-0.068	(0.03)
		70 years old	-0.070	(0.032)
	Experience	10 years	0.023	(0.032)
		5 years	-0.010	(0.028)
	Expand VBM	Supports	0.031	(0.029)
	Party	Democrat	0.045	(0.027)
		Republican	0.037	(0.031)
	Race	Asian	0.057	(0.034)
		Black	-0.022	(0.035)
		Hispanic	-0.010	(0.034)
	Require ID	Supports	-0.076	(0.03)
	Sex	Female	-0.024	(0.025)
Black - White	Age	50 years old	-0.037	(0.027)
		70 years old	-0.077	(0.027)
	Experience	10 years	0.031	(0.027)
		5 years	-0.005	(0.025)
	Expand VBM	Supports	0.031	(0.024)
	Party	Democrat	0.003	(0.027)
		Republican	-0.021	(0.027)
	Race	Asian	0.007	(0.03)
		Black	0.059	(0.029)
		Hispanic	0.025	(0.031)
	Require ID	Supports	-0.061	(0.028)
	Sex	Female	-0.018	(0.022)
Hispanic - White	Age	50 years old	0.017	(0.03)
		70 years old	-0.072	(0.036)
	Experience	10 years	0.019	(0.033)
		5 years	0.012	(0.035)
	Expand VBM	Supports	0.011	(0.03)
	Party	Democrat	0.006	(0.034)
		Republican	-0.032	(0.03)
	Race	Asian	0.069	(0.037)
		Black	0.004	(0.038)
		Hispanic	0.035	(0.038)
	Require ID	Supports	-0.075	(0.03)
	Sex	Female	-0.010	(0.025)

Table A.46: Local Election Official Conjoint Experiment AMCEs - By White

Respondent	Feature	Level	Estiamte	Std. Error
White	Party	Independent	0	
		Republican	-0.096	(0.013)
		Democrat	-0.048	(0.013)
	Age	30 years old	0	
		50 years old	0.009	(0.012)
		70 years old	-0.048	(0.013)
	Sex	Male	0	
		Female	0.028	(0.01)
	Race	White	0	
		Asian	0.003	(0.014)
		Hispanic	0.013	(0.015)
		Black	0.011	(0.014)
	Experience	2 years	0	
		5 years	0.095	(0.012)
		10 years	0.174	(0.012)
	Require ID	Opposes	0	
		Supports	0.149	(0.012)
	Expand VBM	Opposes	0	
		Supports	0.104	(0.012)
Nonwhite	Party	Independent	0	
		Republican	-0.098	(0.016)
		Democrat	-0.030	(0.017)
	Age	30 years old	0	
		50 years old	-0.005	(0.015)
		70 years old	-0.118	(0.017)
	Sex	Male	0	
		Female	0.011	(0.013)
	Race	White	0	
		Asian	0.035	(0.018)
		Hispanic	0.030	(0.018)
		Black	0.022	(0.018)
	Experience	2 years	0	
		5 years	0.096	(0.017)
		10 years	0.198	(0.017)
	Require ID	Opposes	0	
		Supports	0.083	(0.015)
	Expand VBM	Opposes	0	
		Supports	0.117	(0.015)

Table A.47: Local Election Official Conjoint Experiment AMCEs - White Diff

Comparison	Feature	Level	Estiamte	Std. Error
Nonwhite - White	Age	50 years old	-0.015	(0.019)
		70 years old	-0.069	(0.021)
	Experience	10 years	0.023	(0.021)
		5 years	0	(0.021)
	Expand VBM	Supports	0.013	(0.019)
	Party	Democrat	0.018	(0.021)
		Republican	-0.002	(0.021)
	Race	Asian	0.032	(0.023)
		Black	0.011	(0.023)
		Hispanic	0.017	(0.024)
	Require ID	Supports	-0.066	(0.02)
	Sex	Female	-0.017	(0.016)

Figure A.7: **Conjoint Survey Experiment AMCEs - By White/Nonwhite**. This figure visualizes average marginal component effects of the local election official conjoint experiment conducted as part of the 2024 UCLA Representation Survey, with effects separated by white/nonwhite. 95% confidence intervals are illustrated. Point estimates of 0 without confidence intervals are the reference level for each attribute.

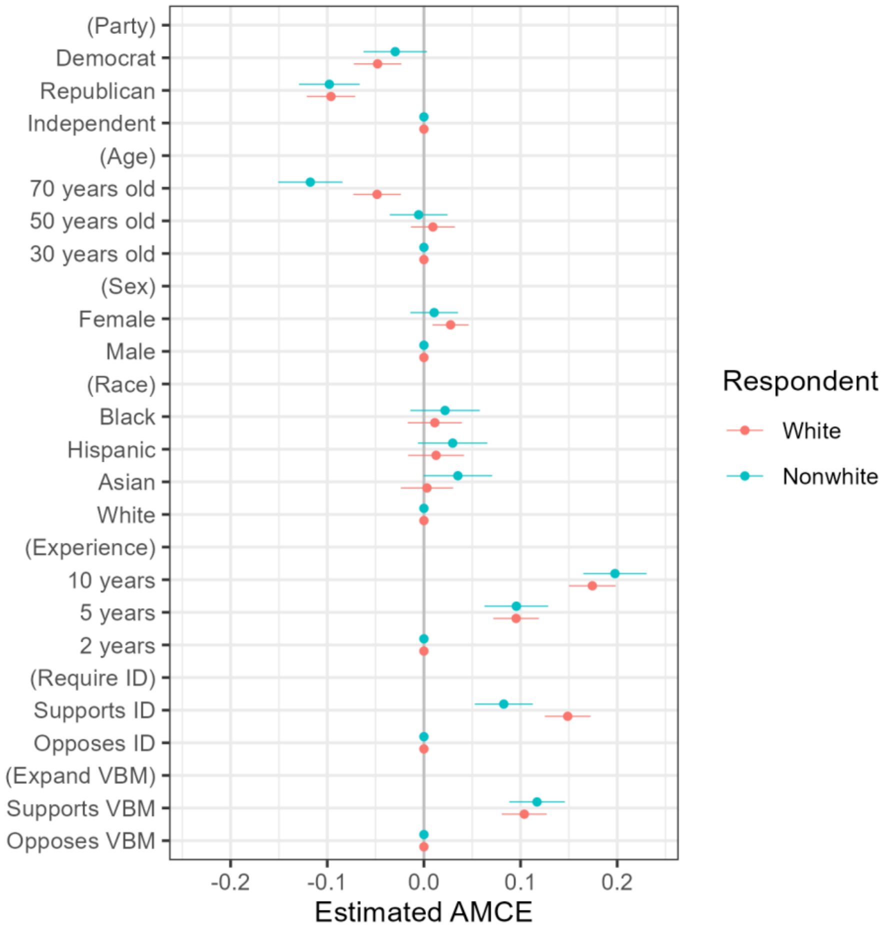
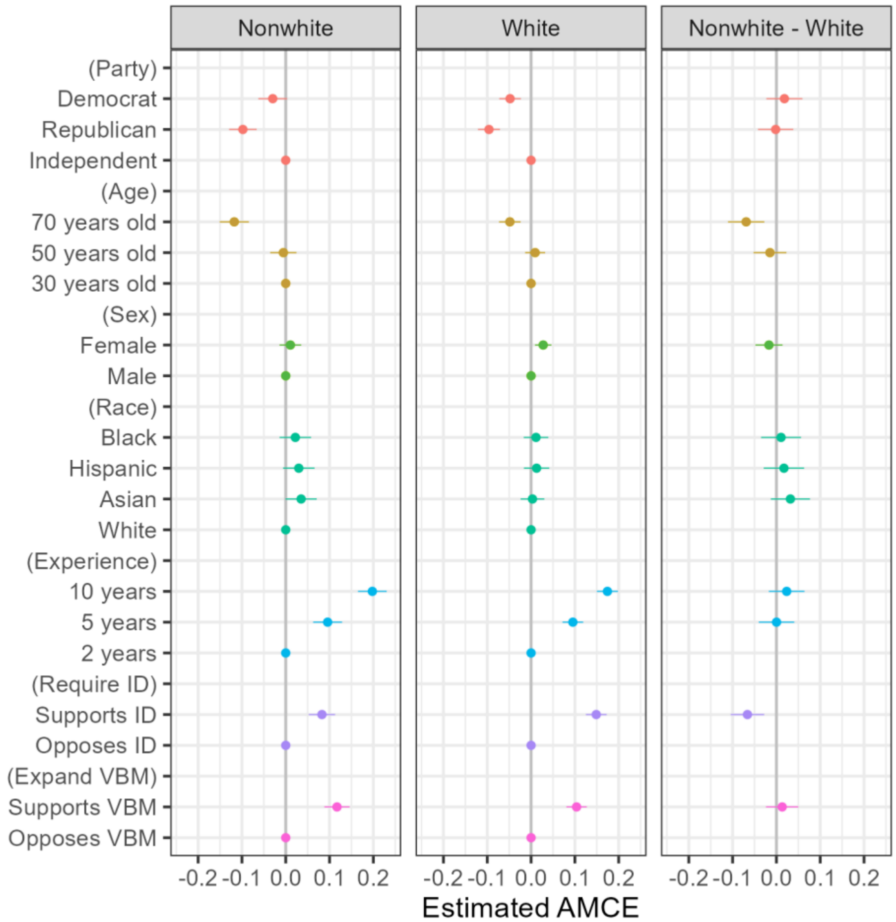


Figure A.8: **Conjoint Survey Experiment AMCEs - By White/Nonwhite**. This figure visualizes differences between racial minorities and whites in average marginal component effects of the local election official conjoint experiment. 95% confidence intervals are illustrated. Point estimates of 0 without confidence intervals are the reference level for each attribute.



A.9.3 Information Provision Experiment Additional Analysis

Tables A.48 and A.49 correspond with Figures 8 and 9 in the main analysis, respectively.

Table A.48: Experiment: Revealing Info About Local Election Official Does Not Boost Voter Confidence (with controls)

	LEO Confidence	Vote Confidence	Juris Confidence	State Confidence	Nation Confidence	Fraud
	(1)	(2)	(3)	(4)	(5)	(6)
LEO Info	−0.025	−0.018	−0.045	−0.022	−0.127***	0.035
	(0.040)	(0.038)	(0.039)	(0.043)	(0.046)	(0.051)
LEO Info + Race	−0.001	−0.012	0.013	−0.009	−0.066*	−0.024
	(0.033)	(0.032)	(0.033)	(0.036)	(0.039)	(0.043)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,699	2,812	2,814	2,834	2,822	2,928

Table A.49: Experiment: Revealing Info About Local Election Official Does Not Boost Voter Confidence (POC Reveal, with controls)

	LEO Confidence	Vote Confidence	Juris Confidence	State Confidence	Nation Confidence	Fraud
	(1)	(2)	(3)	(4)	(5)	(6)
LEO Info	−0.070	0.053	−0.150	−0.094	0.040	0.009
	(0.126)	(0.108)	(0.112)	(0.117)	(0.119)	(0.153)
LEO Info + Race	−0.013	0.173*	0.064	0.049	0.110	−0.216
	(0.107)	(0.093)	(0.098)	(0.102)	(0.104)	(0.133)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	363	384	386	388	382	403

Table A.50 is similar to Table A.48 above, except it subsets respondents to POCs. The results show null effects on voter confidence, in line with the main analysis.

Table A.50: Experiment: Revealing Info About Local Election Official Does Not Boost Voter Confidence (POCs, with controls)

	LEO Confidence	Vote Confidence	Juris Confidence	State Confidence	Nation Confidence	Fraud
	(1)	(2)	(3)	(4)	(5)	(6)
LEO Info	−0.047	−0.035	−0.121**	−0.107	−0.077	0.026
	(0.065)	(0.060)	(0.061)	(0.066)	(0.067)	(0.082)
LEO Info + Race	−0.065	−0.055	0.001	−0.039	−0.063	−0.114
	(0.055)	(0.050)	(0.052)	(0.056)	(0.057)	(0.069)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,178	1,240	1,249	1,262	1,252	1,308

A.9.4 Experimental Survey Instrument

The UCLA Representation Survey instrument, coded in Qualtrics and fielded between April 29 and May 5, 2024 on CloudResearch Connect, can be found at:

https://www.joshuaferrer.com/publication/election_official_representation/election_official_representation.pdf

Chapter 3 References

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Chapter 4: How Partisan Is Local Election Administration?[50]

1 Introduction

In much of the US, elections are administered by partisan elected officials rather than nonpartisan bureaucrats. This sets the US apart from other advanced democracies and leads many experts to worry that election officials give their party an unfair advantage. When asked whether election officials are impartial, election experts rank the US 31 out of 34 OECD countries, ahead of only Hungary, Mexico, and Turkey (Norris and Grömping 2019). Many members of the public are also worried about the American way of conducting elections. According to an ABC News/Ipsos Poll conducted in 2021, 41% of Americans are not so confident or not at all confident in the integrity of the US electoral system.[51] In the fall of 2020, Gallup reported that the share of people who were confident in the accuracy of US elections matched its all-time low.[52] These widespread concerns about election integrity raise an important empirical question: do partisan local election officials give their party an advantage?

Political economy models of elections disagree about whether directly elected local election officials will advantage their party. Candidates improve their chances of winning by moderating their positions and may therefore run elections in a similar manner (Downs 1957). On the other hand, relatively extreme candidates are more likely to run than moderates, and this may result in distinctive Republican and Democratic ways of administering elections that tend to benefit co-

[50] Material from this chapter was co-authored with Igor Geyn and Dan Thompson.

[51] https://abcnews.go.com/Politics/americans-faith-election-integrity-drops-poll/story?id=82069876

[52] https://news.gallup.com/poll/321665/confidence-accuracy-election-matches-record-low.aspx

partisans (Besley and Coate 1997; Osborne and Slivinski 1996). These standard models may do a poor job of describing the considerations of people running to be a local election official. For example, the set of qualified candidates may hold relatively similar views on election administration regardless of their party affiliation (Manion et al. 2021).

Sorting out how much of an advantage clerks give their party is difficult.[53] Democratic clerks are more likely to serve in places where more people vote for Democrats for president, congress, and statewide office. But this does not tell us that clerks advantage their party; voters may simply prefer candidates from the same party in many offices.

We overcome this problem using a close-election regression discontinuity design, comparing Democratic presidential vote share in counties that narrowly elected a Democratic clerk to those that narrowly elected a Republican clerk. To do so, we build an original dataset of 5,880 clerk elections in 1,313 counties from 1998 to 2018. This design ensures that the differences we observe arise from who administers elections rather than pre-existing differences in citizen preferences or local conditions. Using election results as our primary outcome also allows us to evaluate the downstream consequences of partisan clerk elections rather than infer them from changes in policy. Despite widespread concern that partisan election officials advantage their party, we find that Democratic and Republican election officials oversee similar election outcomes when serving comparable counties. We estimate that partisan clerks give their party an advantage of less than 0.4 percentage points. Three of our four estimators can detect an effect of 1.7 percentage points or smaller with 80% power. While our year-by-year estimates are noisier, we find that the effect

[53] We occasionally refer to local election officials as clerks. This is shorthand. In some counties, the local election official is called the election administrator or supervisor of elections. In other counties, the elections officer has additional duties unrelated to elections and their title is auditor, finance officer, probate judge, or tax assessor.

on Democratic vote share is similar in every presidential election from 2004 to 2020. We also present evidence that even clerks who win in a landslide do not noticeably advantage their party and that Democratic and Republican clerks from comparable counties oversee elections with similar turnout and policies.

Why do elected clerks not advantage their party? We provide evidence that clerks do not advantage their party even when they no longer face reelection, suggesting that the reelection incentive is not the primary moderating force on clerks. Clerks who are most able to independently affect statewide outcomes also do not advantage their party, suggesting that collective action problems may not be the main reason clerks fail to advantage their party. Instead, we explain our main findings by pointing to existing research that suggests clerks are more likely than the general public to agree on election administration issues across parties and that election administration may only modestly affect electoral outcomes.

While we find that Democratic and Republican election officials oversee elections with similar outcomes, we cannot rule out small differences between Democratic and Republican officials that could determine very close elections. We also cannot rule out rare but very large effects. If a few election officials dramatically change the outcomes of elections they oversee, the effect in those counties would make up a small share of the average effect and be drowned out by the many officials who do not advantage their party. Still, we find that the average effect of replacing a Republican local election official with a Democrat is small, suggesting that most local election officials are not meaningfully biasing elections in their party's favor. Additionally, our results pertain only to county election officials in past elections. It is possible that partisan election officials at the state level or future county officials are able to bias elections in their party's favor. Finally, our analysis does not imply that electing partisan officials is the best way to select local election officials. Nonpartisan appointed officials may perform better than partisan elected officials (Ferrer 2022).

2 Partisan Advantage in Local Election Administration

Should we expect clerks to advantage their party? Canonical theories of electoral competition reveal that candidates whose policies more closely resemble the median voter's preferred policy are more likely to win reelection, which leads politicians from both parties to implement similar policies (Downs 1957; Fearon 1999). This reelection incentive is especially powerful for executives with meaningful discretion, like governor or mayor, who are especially likely to produce similar outcomes across parties because they make unilateral choices that directly affect their constituents' lives (Mayhew 1974). The role of clerk has many of these qualities: the elected official has considerable discretion over local election administration and citizens directly observe their performance when they vote or communicate with the office (Burden et al. 2013). However, elected partisan clerks must raise money for their campaign and win a partisan primary. These additional steps mean that candidates have to satisfy donors and primary voters who may prefer candidates that administer elections in their preferred way or even promise to tilt the scales in their party's direction (Ansolabehere, Snyder, and Stewart 2001; Burden 2004; Brady, Han, and Pope 2007). This incentive to shift policy away from the median voter's position may be especially strong in places where an overwhelming majority of citizens favor one party.

Citizen-candidate models point out that candidates with moderate policy preferences are unlikely to run if elections are costly because these potential candidates will often be nearly indifferent between the other candidates running (Besley and Coate 1997; Osborne and Slivinski 1996). Candidates with more extreme policy positions will have relatively more reason to run. This is especially true when the office confers few benefits and running is costly.[54] Elected county clerks often receive modest pay (Adona et al. 2019), and running for office requires campaigning which many citizens might view as costly. Given these conditions, we would expect only committed partisans to run for clerk and then implement different policies across parties.

There are three potential countervailing forces within the citizen-candidate model leading clerks to not advantage their party. First, people with experience in election administration may have less polarized election policy views across parties than the public and elections may select for people with experience (Manion et al. 2021; Thompson 2020). Second, Democratic and Republican clerks may want to implement different policies, but if they were to do so they would not be able to noticeably influence turnout or partisan vote share (e.g., Gronke et al. 2008; Thompson et al. 2020). Third, clerks may face costs for changing policies that are only worth bearing if they can influence who wins. This creates a collective action problem: each clerk wants to help their party win but shirks to avoid bearing the costs because they are not pivotal by themselves. This collective action problem is not present when the costs to influencing elections are low and the likelihood of influencing election outcomes is high.

Empirical research directly testing whether US local election officials favor their party, which we review in Table A.1 in the online appendix, is mixed. While some studies find that Democratic and Republican officials implement different policies and other studies find they do not, no study has a research design that can fully account for differences in the places that elect Democratic and Republican clerks that might lead to different policies regardless of which party controls the clerk's office.

[54] See Hall (2019) for further discussion of these models and tests of their implications in legislative elections.

The risk of partisan election administration is not limited to the US. While everyone agrees that election administrators ought to ensure “free and fair elections” (Hall 2018), it is difficult to completely insulate election administration from partisan actors (James 2012). Central election management bodies are most effective when they are independent of the executive (López-Pintor 2000), but in practice partisan actors are involved in virtually every system (Massicotte, Blais, and Yoshinaka 2004). One notable example of partisan election administration comes from Ukraine, where party control of election management committees boosts that party’s vote totals by a few percentage points (Herron 2020). While outright fraud is certainly a factor in many places (Alvarez, Hall, and Hyde 2008), practices that amount to a “soft perversion of the process” are even more common, such as appointing biased poll workers (Alvarez and Hall 2006) and filtering out candidates from the opposing party (Szakonyi 2022). Independent election monitors may curtail election day fraud and violence (Asunka et al. 2019), but they may simply shift fraudulent practices to earlier in the process (Daxecker 2014).

3 The Role of Local Election Officials

Across the United States, thousands of local election officials play a central role in the administration of elections. Clerk responsibilities include registering voters, maintaining an up-to-date list of registered voters, hiring and training poll workers, selecting poll locations, printing ballots, acquiring and maintaining election equipment, running early and absentee voting, educating and communicating with voters, overseeing election day, tabulating the votes cast, handling provisional ballots, and certifying election results (Kimball and Kropf 2006). They also usually have the authority to hire staff and influence department funding levels.

Clerks administer elections within the bounds of complex and frequently changing federal, state, and local laws. They work in concert with a range of other officials to successfully conduct elections. Clerks typically serve at the county level, though in ten mostly Northeastern states important responsibilities are carried out at the municipal level.

Building on the work of Kimball and Kropf (2006), we conduct a review of state and local election laws. Table A.2 in the online appendix shows a simplified division of states into tiers based on how much authority is vested in a single partisan elected official. We identify 32 states that contain at least some jurisdictions with a partisan elected official tasked with election responsibilities. In many of these states, partisan elected officials share responsibilities with other local officials or with boards. In 21 of these 32 states, partisan elected clerks are the sole or primary election administrators. Our main analysis focuses on partisan elected officials in these 21 states.[55]

Even among states that delegate considerable election administration authority to a partisan elected official, there are significant differences in clerks' responsibilities and discretion. We describe this variation in Table A.3 in the online appendix. For example, county clerks in Nevada have complete authority to register voters, maintain the registration list, site polling places, conduct early voting, and purchase voting equipment. They also have some discretion in recruiting poll workers and are not subject to any statewide training requirements. In contrast, probate judges in Alabama do not register voters or maintain registration lists. They are constrained by state law in recruiting poll workers, and both site polling places and select voting equipment in conjunction with the county commission.

Overall, most of the 21 states give registration and voting administration duties to the same partisan

[55] In Table A.9, we run a robustness check using the 14 states where virtually all duties are delegated to a single partisan elected official.

elected official. Most also entrust registration list maintenance and voting equipment decisions to this official. Partisan elected officials choose polling places in 14 states and administer early voting in 13 states, but are usually limited in their ability to hire poll workers, with most states requiring bipartisan appointments.

Clerks could plausibly affect election results with formal or informal practices. Using formal authority, clerks could attempt to increase participation and shift the composition of the electorate by siting many polling places in populated and accessible locations, providing extensive early voting options, ensuring that no eligible voters are removed from the voter roll, purchasing easy-to-use and reliable voting equipment, adequately resourcing polling locations with ballots and poll workers, and showing leniency in their acceptance of provisional and vote-by-mail ballots. Alternatively, officials might minimize participation and alter the composition of the electorate by siting polling places in inconvenient locations, providing limited early voting options, regularly purging voters from the rolls, maintaining old and difficult-to-use voting equipment, inadequately sourcing polling locations, and rejecting borderline provisional and vote-by-mail ballots.

Clerks might also undertake informal practices to reduce voter costs or do only what the law requires. Officials can conduct voter outreach campaigns, advertise how and where to register, maintain an active social media presence, and engage in extensive constituent communication. Alternatively, they could take none of these actions. Local election officials can engage in targeted practices by attempting to increase participation among co-partisans and reduce participation among citizens from the opposing party. Finally, officials could take illegal actions at the risk of litigation. These include siting fewer polling places than the statutory minimum mandates,[56] following procedures that infringe upon the Voting Rights Act, and engaging in vote manipulation.

By estimating the effect of partisan election administrators on Democratic presidential vote share, we measure the sum total effect of all actions election officials take to influence elections.

4 Studying Partisan Control of Local Election Offices

In this section, we first describe our data including original data on the elections of local election officials, county-level election results and turnout for presidential and statewide offices from 2000 to 2020, and county-level administrative data on the number and location of polling places, the number of registered voters, the number of provisional ballots, and survey-reported wait times. Next, we discuss our close-election regression discontinuity design and how we improve the precision of our estimates by first predicting outcomes.

4.1 New Data on the Elections of Partisan Local Election Officials

We gather an original dataset of 5,880 elections of partisan local election officials in 1,313 counties and 21 states held between 1998 and 2018. We collect these results in three steps. First, we scrape state election websites for all county-level results. Next, we visit county election websites for results not available from states. Finally, we contact counties directly to request results not available on their websites.

Figure 1 shows the counties for which we have at least some data in light blue. Counties with partisan elected election officials where we are unable to find any election data are in dark blue. We

[56] https://www.theguardian.com/us-news/2020/mar/02/texas-polling-sites-closures-voting

use dark gray to denote counties where municipalities run elections, boards share responsibilities for elections, or election officials are appointed or nonpartisan. In Table A.4 in the online appendix, we present descriptive statistics for the counties in and out of our sample, as well as out of scope counties. Missing counties tend to be less populous, located in the South, and have larger Black and Hispanic populations.[57]

Notably, the correlation between Democratic presidential vote share and Democratic clerk vote share is very low. In the within-sample counties that elect local election officials on a presidential year cycle, Democratic presidential vote share correlates with lagged Democratic presidential vote share with a coefficient of 0.89. By contrast, Democratic clerk vote share correlates with same-year Democratic presidential vote share with a correlation coefficient of 0.32. Figure A.1 in the appendix captures this pattern.

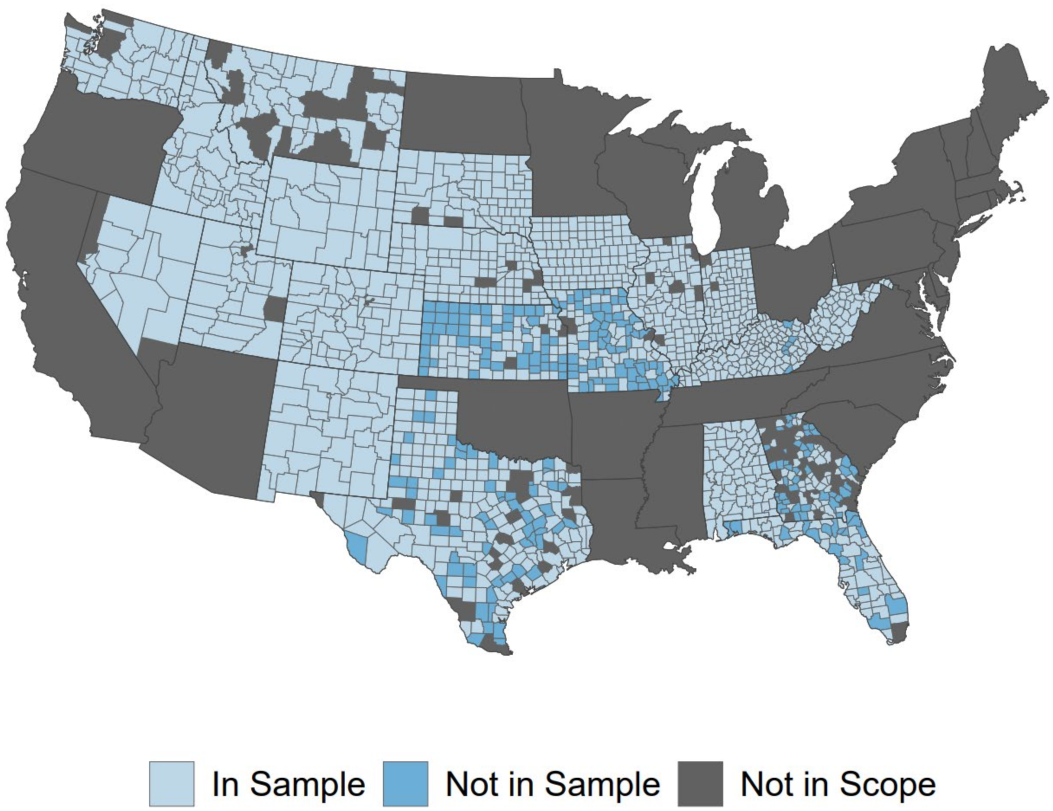
4.2 County-Level Election Results and Voter Participation

We measure turnout as the share of voting age residents who cast valid ballots for the highest office. Voting age population is measured using estimates from the National Cancer Institute's Surveillance, Epidemiology, and End Results Program.[58]

[57] Counties with fewer than 100 residents are excluded from analysis due to data estimation limitations. This excludes Loving County, Texas.

[58] Note that some voting-age residents may be ineligible to vote due to citizenship status or criminal record. This data does not allow us to remove these individuals. While this may make some of our estimates slightly noisier, it should not bias our estimates since it is highly unlikely anyone would decide where to live based solely on the outcome of close elections for the local election official. The data we use is available at https://seer.cancer.gov/popdata/.

Figure 1: **Map of Counties Included in Original Data on the Elections of Partisan Local Election Officials.** Out of 1,582 counties that elect a partisan election official, 1,313 appear in our dataset at least once. Alaska and Hawaii do not have partisan elected election officials. "Not in Scope" indicates jurisdictions that did not elect partisan local election officials between 1998 and 2018.



4.3 County-Level Data on Election Administration

We assemble a set of indicators on how elections have been run over time and across counties using the Election Administration and Voting Survey (EAVS) from the US Election Assistance Commission.[59] We use this survey to measure the following for each federal general election in every county: the number of polling places, provisional ballots cast, provisional ballots rejected, absentee ballots rejected, and the number of registrants removed from the voter roll. We use Dave

[59] https://www.eac.gov/research-and-data/datasets-codebooks-and-surveys

Leip's Election Atlas to measure the number of registered voters in each county and the share of registered voters listed as members of the Democratic party.

Additionally, we follow Pettigrew (2017) in using the Cooperative Congressional Election Study to measure voter wait times.[60] We compute the share of voters who had to wait at the polls for more than 30 minutes for each federal general election between 2006 and 2018, except for 2010 when the CCES did not ask about wait times. We also use data from Chen et al. (2020) who measure wait times by tracking cell phone locations.

4.4 Empirical Strategy: Regression Discontinuity Design

We estimate the advantage election officials give their co-partisans using a regression discontinuity design, fitting regression equations of the form: $Y_{ct+k} = \mu + \tau Dem_{ct} + f(M_{ct}) + \epsilon_{ct+k}$ where Y_{ct+k} is Democratic presidential vote share in elections held k years after the election official was elected in county c, year t. Dem_{ct} is a dummy variable indicating a Democratic local election official winning the election. $f(M_{ct})$ is a flexible function of the margin M_{ct} by which the Democratic local election official won (i.e., the share of the two-party vote they received minus 0.5). M_{ct} ranges from -0.5 to 0.5 and is positive for a Democratic win, negative for a Republican win, and zero in an exact tie. We interpret τ as the average effect of electing a Democratic rather than Republican local election official in counties where the election was an exact tie. In other words, it is the effect of electing the next most likely or marginal Democrat to be a local election official rather than a Republican.

In our turnout and policy analyses, when each clerk election determines control of the office for

[60] https://cces.gov.harvard.edu/data

multiple observations of the outcome, we cluster standard errors by clerk election (Abadie et al. 2017).

Our close-election regression discontinuity design ensures that, when we compare counties that elect a Republican to those that elect a Democrat, both sets of counties have a similar average partisan makeup, state political environment, preferences over election administration, and population, in addition to any other fixed and time-varying county factors. Our regressions identify the average effect of electing a Democratic rather than Republican election official in places with tied elections when the only thing that changes sharply at that point is which candidate was elected (Cattaneo, Idrobo, and Titiunik 2019; Imbens and Lemieux 2008; Lee and Lemieux 2010).[61] We evaluate the plausibility of this assumption by comparing pre-election county-level characteristics in counties that narrowly elected Democratic officials to those that narrowly elected Republicans. We are most interested in the comparison of turnout and Democratic presidential vote share from before the local election official was elected because these are our primary outcomes of interest, and because they tend to correlate highly within a county over time. In Section A.6 in the online appendix, we show that counties where a Democratic election official narrowly won are similar to counties where a Republican narrowly won on a large number of pre-treatment characteristics, including the lagged Democratic presidential vote share and lagged turnout. In Section A.6.2 in the online appendix, we also show that Democrats and Republicans win close races at similar rates in counties controlled by Democrats at the time of the election and those controlled by Republicans.[62] These results serve as evidence to support our claim that the only difference between a district that narrowly elects a

[61] While this assumption has been disputed in a small number of particular cases (Caughey and Sekhon 2011), it holds under the majority of cases studied (Eggers et al. 2015).
[62] This is a version of the standard McCrary (2008) sorting test.

Democrat and a district that narrowly elects a Republican is the partisanship of the elected clerk.

Our intention is to estimate the effect of replacing a marginal Republican with a marginal Democrat, which is identified under the assumptions we mention above. Our design does not identify the effect of a candidate changing the party they associate with or the effect of replacing a typical Republican with a typical Democrat (Hall 2019: Ch. 2; Marshall 2021).

We present results using a variety of regression specifications because of the bias-variance trade-off that must be resolved in every regression discontinuity analysis. If the functional form of the running variable is not flexible enough, it can induce bias, mistaking a smooth curve in the outcome for a discontinuity. On the other hand, less flexible specifications that use more data and fewer degrees of freedom make the estimate more precise. Presenting multiple specifications ensures the robustness of our results across different functional forms of the relationship between Democratic election official vote share and our outcomes. Following Cattaneo, Idrobo, and Titiunik (2019), our primary specification is a local linear regression using triangular kernel weights and the automated bandwidth selection procedure described in Calonico, Cattaneo, and Titiunik (2014).

4.5 Improving Precision by First Predicting Outcomes

One of the main challenges we face when estimating the advantage clerks give their party is statistical precision. Estimating discontinuities is difficult—across many applications, the common estimators produce large standard errors and do not have sufficient power to detect substantively interesting effects (Stommes, Aronow, and Sävje 2021).

We improve the precision of our estimates using a three-step procedure building on the recommendations of Lee and Lemieux (2010):[63]

1. Using leave-one-out cross-validation, we select a regression specification that best predicts Democratic presidential vote share from lagged Democratic presidential vote share.[64] We use the full dataset for this exercise, not just the counties with competitive elections for their local election official. This procedure selects a prediction equation with state-year-specific coefficients on the lag and state-year-specific intercepts.

2. We compute the difference between predicted and observed Democratic vote share using the best-performing specification.

3. We use the residual from step (2) as the outcome in a standard regression discontinuity estimator.[65]

We use this procedure to improve our power for our main findings and for studying voter turnout and election policies.

We conduct power analyses to evaluate whether this more precise estimator is powerful enough to detect substantively meaningful effects. We report the minimum effect detectable 80% of the time with a one-sided t-test at a 5% significance level (i.e., $\alpha = 0.05$ and $\beta = 0.20$). We discuss our approach to calculating power in Section A.5 in the online appendix.

As we report in Table 1, our main estimators have a minimum detectable effect of Democratic election officials on Democratic presidential vote share of between 1.2 percentage points and 2.3

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[63] For a more recent discussion of this estimator, see Noack, Olma, and Rothe (2021). We discuss how this estimator compares with the estimator in Calonico et al. (2019) in Section A.4 in the online appendix.

[64] We discuss the candidate prediction equations and their performance in Section A.4 in the online appendix.

[65] See Lee and Lemieux (2010) for further discussion of why it is not necessary to residualize the running variable.

percentage points. That means our design has sufficient power to detect effects on partisan vote share that are about as large as running 50 television ads (Sides, Vavreck, and Warshaw 2021; Spenkuch and Toniatti 2018) or 15% as large as the effect of nominating a moderate candidate (Hall 2015). Our minimum detectable effect is also approximately half the size of the effect of Democratic local election officials on the Democratic share of turnout reported in previous research (Bassi, Morton, and Trounstine 2009). In Table 2, we report that our estimators have minimum detectable effects of Democratic election officials on turnout of between 1.0 and 1.1 percentage points. Our minimum detectable effect on turnout is less than half the size of a large TV advertising campaign in a presidential election (Green and Vavreck 2008).

5 Clerks Do Not Meaningfully Advantage Their Party

5.1 Descriptive Graphical Evidence Suggests Clerks Do Not Advantage Their Party

First, we show descriptive graphical evidence that presidential candidates from the clerk's party perform no better than expected based on historical election results. Figure 2 captures this result. In the top panel, we plot the regression of Democratic presidential vote share for each county-year on Democratic vote share in the previous presidential election. Counties with a Democratic clerk are colored blue and counties with a Republican clerk are colored red. We fit separate locally weighted regressions for counties with Democratic and Republican clerks.

Counties that vote overwhelmingly for Democratic presidents are also likely to elect Democrats to run their elections. We can see this by noticing that the upper-right quadrant of the plot is made up almost entirely of blue Ds and the bottom-left portion of the plot is primarily composed of red Rs.

Nevertheless, this plot suggests that local election officials are not giving their party a large electoral advantage. We can see this by noticing that the lines are nearly identical. Conditional on being elected in counties with similar historical Democratic vote shares, Democratic and Republican local election officials oversee similar elections. If clerks were advantaging their party, and continuing to seek new advantages every cycle, we would expect the blue line to be higher than the red line, i.e., Democratic presidential candidates would perform better in counties with Democratic clerks than with Republican clerks after accounting for the normal two-party presidential vote in that county. This figure provides us little reason to suspect that clerks are giving their party a substantial advantage in presidential elections.

The bottom panel of Figure 2 plots histograms of the residual of predicted Democratic presidential vote share for counties with Democratic and Republican clerks.[66] The histograms overlap substantially, although the histogram for Democrats is shifted slightly to the left and has a modestly wider dispersion.[67] If clerks were advantaging their party, and continuing to seek new advantages each cycle, we would expect the central tendency of the distribution of blue residuals to be shifted to the right of the central tendency of the red residuals indicating that Democratic presidential candidates perform better in counties with Democratic clerks than with Republican clerks after accounting for the expected presidential vote in that county. This implies that Democratic clerks oversee elections that are getting worse, on average, for Democratic presidential candidates.

One important weakness of these plots is that the party of the clerk is often the same in the previous presidential election. If partisan control of the clerk's office is constant over time and not increasing as the party holds the clerk's office, this plot would tend to understate the effect partisan control of the clerk's office on election results. We address this concern in the next section by using a regression discontinuity design which compares places with Democratic and Republican clerks

[66] See Section 4.4 for a discussion of how we compute the residuals.
[67] The average of the residuals is 0.002 in Republican-controlled counties and -0.004 in Democratic-controlled counties. The standard deviation of the residuals is 0.028 in Republican-controlled counties and 0.034 in Democratic-controlled counties.

that had an equal likelihood of having a Democratic clerk during the previous presidential election.

5.2 Regression Discontinuity Plot Suggests Clerks Do Not Advantage Their Party

Figure 3 captures our main result: local election officials do not improve their party's vote share in presidential elections. On the horizontal axis, we plot the two-party Democratic vote share in the race for local election official. We subset to elections with a Democratic and Republican candidate both on the ballot and finishing in the top two places. This means that a Democratic official runs elections to the right of 0.5, and a Republican official runs elections to the left of 0.5. On the vertical axis is the residual of Democratic presidential vote share in each county in the first presidential election after the election official was elected. Each of the small gray points represents the election of a county election official and the subsequent presidential election result. The large black points are equal-sized binned averages made up of 25 elections each, computed separately for counties that elect a Democratic clerk and those that elect a Republican. The solid lines are simple linear regression lines fit separately for counties that elect Democratic election officials and those that elect Republicans. We plot data within the bandwidth selected by the automated procedure described in Calonico, Cattaneo, and Titiunik (2014).

We can learn about the effect of electing a Democrat rather than a Republican as local election official by focusing on the 50-50 point in the middle of the plot. To the left and right of 0.5, the average residual Democratic presidential vote share is nearly identical. If clerks were advantaging their party, we would expect the average vote share for Democratic presidential candidates to be higher in counties that narrowly elected a Democratic clerk compared to those that narrowly elected a Republican clerk. This would be visible as a vertical jump in the regression line on the plot with the line being noticeably higher on the right side of the 50-50 line than on the left side of the 50-50 line. This suggests that election officials do not noticeably advantage their party.

Figure 2: **Democratic and Republican Election Officials Conduct Elections With Similar Results.** The top panel presents the relationship between Democratic presidential vote share and lagged Democratic presidential vote share separately in counties with Democratic and Republican clerks. The relationship is nearly identical in both sets of counties. The bottom panel presents the distribution of the residuals from predictions of Democratic presidential vote share in counties with Democratic and Republican election officials. On average, Democratic clerks oversee elections that are slightly less favorable for Democratic presidents than expected.

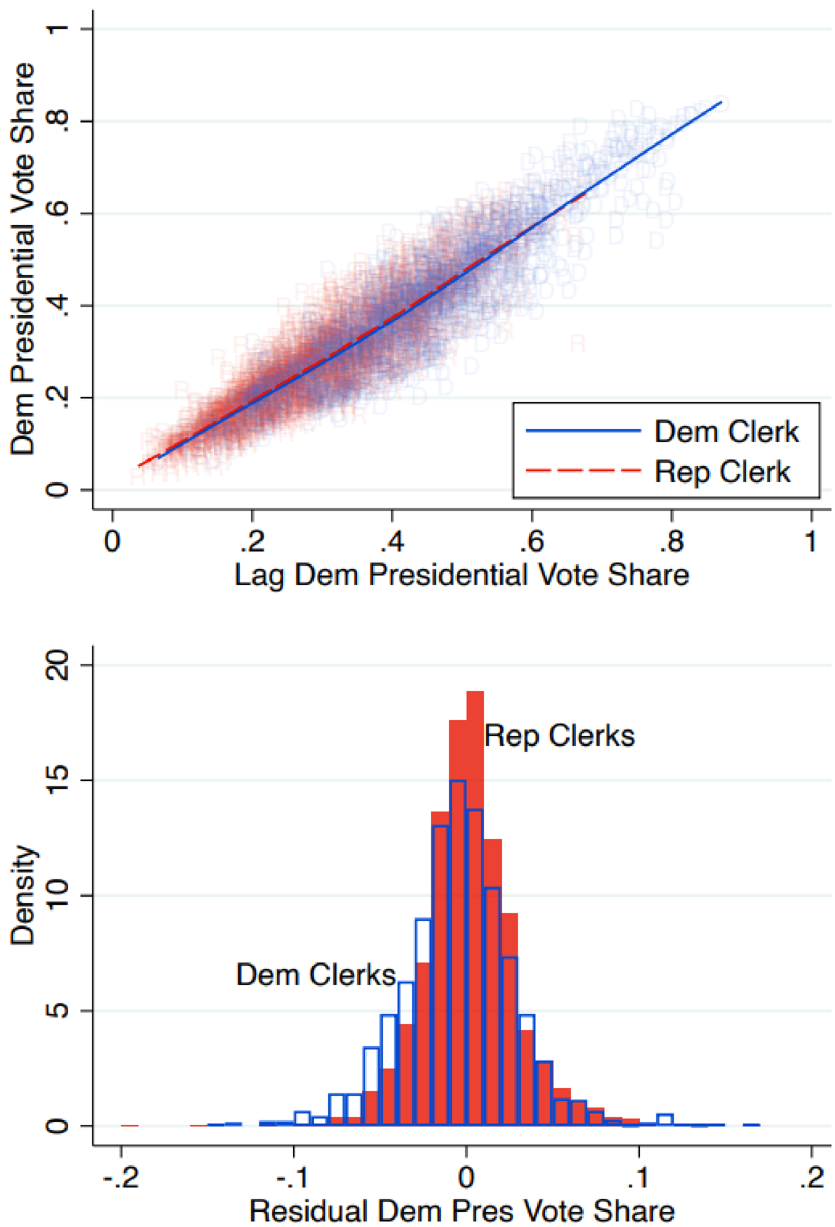


Figure 3: **Electing a Democratic Election Official Rather Than a Republican Does Not Noticeably Increase Democratic Presidential Vote Share.** Two-party Democratic vote share for contested local election official elections is the running variable, making 0.5 the threshold above which a county elects a Democratic election official and below which they elect a Republican. Democratic presidential vote share in the following presidential election is plotted along the vertical axis. The large black points are equal-sized binned averages marking the average of 25 elections each. The binned averages are computed separately for each side of the 50-50 threshold. The black line is a linear regression fit separately on each side of the 50-50 threshold. The full tabular results are found in column 1 of Table 1.

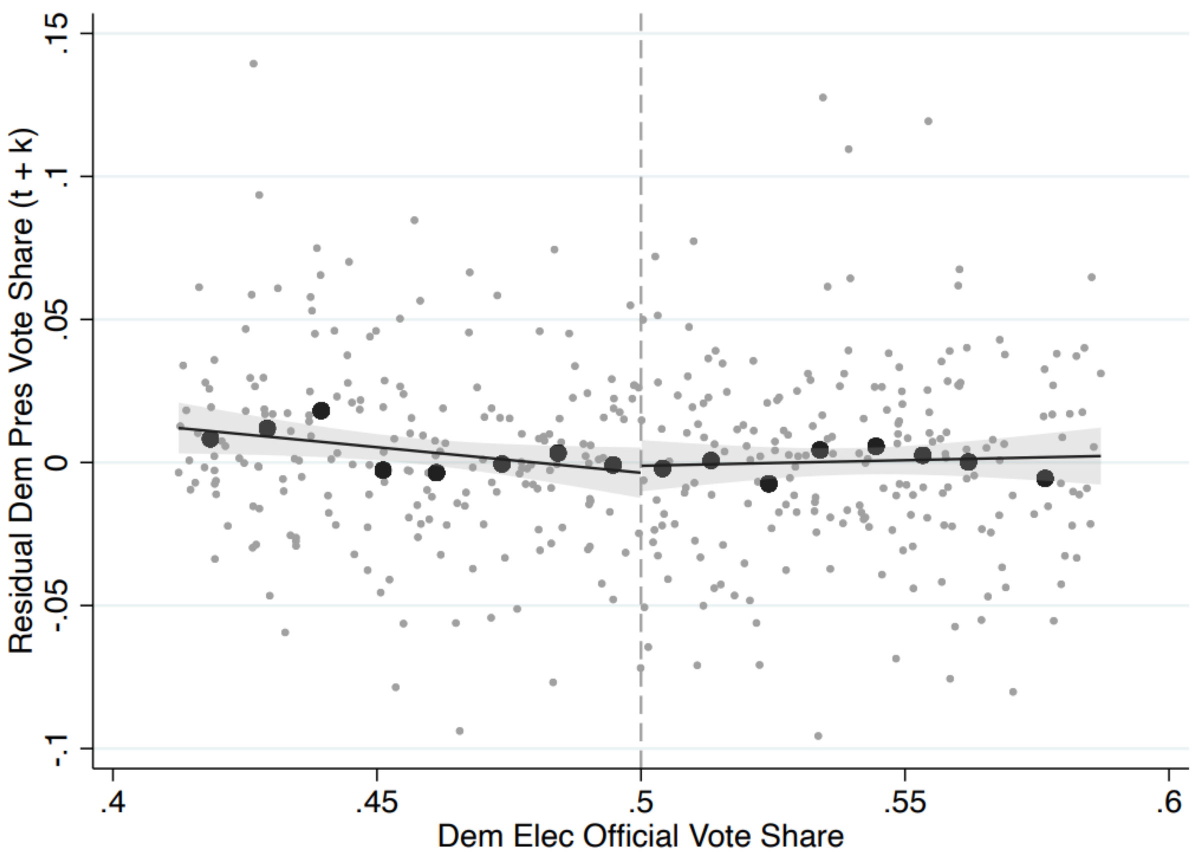


Table 1: **Effect of Democratic Election Officials on Democratic Presidential Vote Share.**

	Dem Pres Vote Share			
	(1)	(2)	(3)	(4)
Dem Elec Official	0.003	0.000	-0.011	0.000
	(0.006)	(0.005)	(0.009)	(0.007)
N	383	696	195	408
Bandwidth	0.08	0.16	0.04	0.09
Bandwidth Selection	CCT	CCT*2	CCT/2	CCT
Kernel	Uniform	Uniform	Uniform	Triangular
Min Detectable Effect	0.016	0.012	0.023	0.017

Robust standard errors in parentheses. The outcome is first regressed on a state- and year-specific lag using all counties including those for which clerk election results are not available. The regression discontinuity is estimated using the residuals from that regression. The bandwidth row reports the number of maximum clerk win margin allowed for inclusion in each specificaiton. CCT refers to Calonico, Cattaneo, and Titiunik (2014) bandwidth selection procedure. Min detectable effect refers to the minimum effect that a one-sided test with a 0.05 alpha would have 80% power to detect.

5.3 Regression Estimates Also Suggest Clerks Do Not Advantage Their Party

In Table 1, we provide formal estimates of the effect of electing a Democrat rather than a Republican as election official on Democratic presidential vote share. Column 1 reports the estimate from a local linear regression with uniform kernel weights and the bandwidth selected by the procedure described in Calonico, Cattaneo, and Titiunik (2014). Column 2 reports estimates from the same procedure used in column 1 but with a bandwidth twice as wide. Column 3 reports estimates from the same procedure used in column 1 but with a bandwidth half as wide. Column 4, our primary specification, reports estimates from a local linear regression with triangular kernel weights and the bandwidth selected by the procedure described in Calonico, Cattaneo, and Titiunik (2014).

We find consistent evidence across all four specifications that local election officials do not meaningfully advantage their party's candidate for president. The point estimates range from -1.1 to 0.3 percentage points, with three out of four point estimates falling below 0.1 percentage points.

Across all four columns, our 95% confidence intervals include zero.

In the final row of Table 1, we present the minimum detectable effect. As we discuss in Section 4.5, three of our four estimators are able to detect partisan advantages as small as 1.7 percentage points with 80% power.

While Table 1 presents results across only four specifications, we estimate very similar effects across a much wider set of potential estimators. Section A.6.4 in the appendix shows that our estimates are similar for every choice of bandwidth from 0.02 to 0.25. In Section A.6.3 in the appendix, we demonstrate that, though our estimates are noisier when using outcomes that are not first residualized, they are substantively similar.

In Table A.14, we extend our data to include all governor, senate, and presidential election results. Despite adding more data, predicting governor and senate election results based on lagged results is more difficult than predicting presidential results, resulting in noisier estimates. Nevertheless, the point estimates are still substantively quite small, and a zero effect falls well within all of the 95-percent confidence intervals in the table.

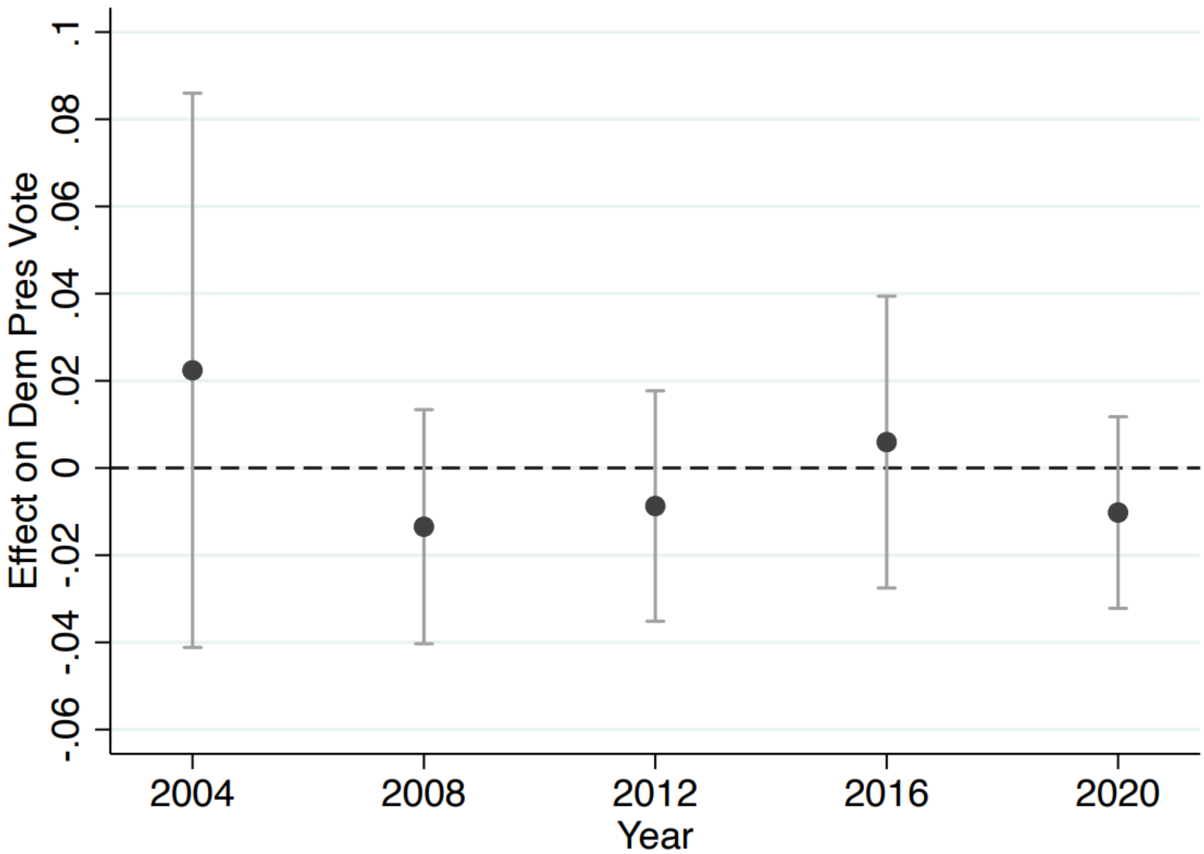
5.4 Similar Findings Across Time and States

This finding—that election officials do not noticeably advantage their party—is not limited to the early part of our study period, to states where officials have slightly less authority, or to regions with distinctive politics. In Figure 4, we present estimates of the effect of electing a Democratic local election official on Democratic presidential vote share in every presidential election since 2004. Despite the concern that election administration has become an increasingly salient and partisan issue, we do not find evidence that the marginal local election official advantaged their party in

2020 or in any previous election since 2004.

In the online appendix, we also study three sets of states where we might expect clerks to give their party a larger advantage. Across all three sets, we find that clerks give their party little to no advantage. First, in Table A.9, we present estimates of the advantage clerks give their party in the 14 states where one partisan elected official handles all local election administration. Three of the four reported point estimates of partisan advantage are negative. Given the long tenure of clerks and the slow pace of the Southern realignment in local offices, we might expect that Democratic clerks in the South may favor the Republican party in statewide and national elections, especially in the first few elections in our data (Kimball et al. 2013). In Table A.11 we report estimates of the partisan advantage clerks provide, removing counties in Southern states from the analysis. We find substantively similar point estimates, implying that our national estimates are not masking positive effects in places where clerks are most likely to favor national co-partisans. Finally, some counties in our data were subject to pre-clearance requirements under the Voting Rights Act prior to the 2013 Supreme Court ruling in *Shelby County v. Holder*. In Table A.12 we find that, even when omitting counties subject to the pre-clearance requirement, clerks do not appear to advantage their party. In Table A.13, we subset to counties previously covered under the pre-clearance provisions but in years after the *Shelby County v. Holder* decision, finding a similar pattern of results. In other words, there is no indication that local election officials have used their new discretion post-*Shelby* to advantage their party. In addition to these more powerful tests, in Figure A.4 in the online appendix, we also present evidence that clerks do not noticeably advantage their party in any of the eight states that we have sufficient data to study. This suggests that state-level laws are not the primary reason clerks do not advantage their party. Put together, these results suggest that clerks do not meaningfully advantage their party.

Figure 4: **Clerks Provide Their Party Minimal Advantages Over Time**. Each dot represents a regression discontinuity-based estimate of the effect of electing a Democratic clerk on residual Democratic presidential vote share in a given presidential election. Vertical lines extending from each point represent 95-percent confidence intervals. Estimates come from regressions that mimic column 4 in Table 1 using local linear regression with triangular kernel weights. Full tabular results are found in Table A.8 in the online appendix.



5.5 Generalizing Beyond Close Clerk Elections

Using a regression discontinuity design, we find that clerks elected in close elections do not give their party a substantial advantage in presidential elections. Might clerks elected by wider margins give their party an advantage?

Our data suggests that, even when clerks win by a relatively large margin, they do not grant their party a sizable advantage. In Figure 2, we document the difference in Democratic presidential vote share between counties controlled by Democratic and Republican clerks. Though the majority of these clerks are elected by large margins or in uncontested races, the average Democratic clerk oversees an election with slightly lower Democratic presidential vote share than the average Republican clerk. This descriptive evidence suggests that our finding is not limited to counties with close clerk elections. In Section A.6.11 in the appendix, we present a more formal analysis of how local our estimates are drawing on the approach described in Angrist and Rokkanen (2015) and Hainmueller, Hall, and Snyder Jr (2015). We find that, even including counties where the Democratic clerk candidate won as little as 25% or as much as 75% of the vote, partisan clerks do not appear to advantage their party on average.

Given this evidence, in Section 6, we consider explanations for clerks not advantaging their party that apply to all clerks rather than just those elected by very small margins.

5.6 Democratic and Republican Clerks Produce Similar Turnout and Policies

While conventional wisdom holds that high-turnout elections favor Democrats (Lijphart 1997; Piven and Cloward 1988), some reforms that increase turnout do not noticeably increase Democratic vote share (see, e.g., Thompson et al. 2020). Might local election officials successfully affect turnout but fail to offer their party an advantage?

Table 2 presents regression discontinuity estimates of the effect of electing a Democrat rather than Republican election official on turnout. The first two columns mirror columns 1 and 4 from Table 1. Across both specifications, we find that, after accounting for differences in where and when Democrats and Republicans run for office, members of both parties oversee similar levels of voter participation on average.

Table 2: **Effect of Democratic Election Officials on Turnout.**

	Votes per Voting-Age Resident					
	All Counties		Rep Counties		Dem Counties	
	(1)	(2)	(3)	(4)	(5)	(6)
Dem Elec Official	0.001	0.001	0.001	0.002	0.002	0.003
	(0.004)	(0.004)	(0.005)	(0.004)	(0.010)	(0.009)
N	541	720	400	584	168	188
Clusters	313	418	237	344	106	120
Bandwidth	0.06	0.09	0.07	0.10	0.07	0.08
BW Selection	CCT	CCT	CCT	CCT	CCT	CCT
Kernel	Unif	Tri	Unif	Tri	Unif	Tri
Min Detectable Effect	0.011	0.010	0.012	0.011	0.025	0.023

Robust standard errors clustered by clerk election in parentheses. Rep counties are those where the last Republican presidential candidate received more votes than the last Democratic presidential candidate. Dem counties are all remaining counties. The outcome is first regressed on a state- and year-specific lag using all counties including those for which clerk election results are not available. The regression discontinuity is estimated using the residuals from that regression. The bandwidth row reports the number of maximum clerk win margin allowed for inclusion in each specificaiton. CCT refers to Calonico, Cattaneo, and Titiunik (2014) bandwidth selection procedure. Min detectable effect refers to the minimum effect that a one-sided test with a 0.05 alpha would have 80% power to detect. Unif refers to a uniform kernel. Tri refers to a triangular kernel.

In the final row, we report the minimum detectable effect using each estimator. Both estimators can detect an effect as small as 1.1 percentage points with 80% power or greater. Even with these high-powered tests, we find no evidence that electing a Democratic rather than a Republican election official increases turnout on average.

While Democrats are often expected to pursue policies that increase turnout, vote-maximizing partisans will only work to increase participation when their party makes up a majority of the people affected by their policies (Burden et al. 2013; Kimball, Kropf, and Battles 2006). Might Democratic clerks oversee lower turnout in Republican-majority counties and higher turnout in Democratic-majority counties?

Table 2 presents evidence that Democratic and Republican officials do not strategically increase turnout when their party makes up a majority and decrease turnout when their party is in the minority. Columns 3 and 4 report the effect of electing a Democratic clerk in Republican-majority counties. There, marginal voters are more likely to be Republicans, so we would expect vote-maximizing Democratic clerks to decrease turnout relative to Republican clerks. Instead, we find that Democratic and Republican clerks oversee similar turnout rates in these counties. Columns 5 and 6 report the effect of electing a Democratic clerk in Democratic-majority districts, where Democrats are most likely to make up a majority of marginal voters. Still, we find that Democratic and Republican clerks oversee similar levels of participation.

These results could arise if partisan clerks implement different policies that have very modest effects on turnout. Committed partisan clerks could pursue these policies anyway if they are unaware of their ineffectiveness or if they have ideological positions about how elections ought to be administered. In Section A.6.12 in the online appendix, we present evidence that Democratic and Republican clerks representing comparable places make similar administrative decisions across many parts of the job, including the number of polling places sited, the share of votes cast provisionally, the provisional ballot rejection rate, the registration rate, the registration removal rate, the partisan balance of registrants, and voter wait times.

Put together, the analyses presented in Table 2 and appendix Section A.6.12 cast doubt on the claim that partisan clerks are strategically changing turnout or policies while failing to convert those changes into noticeable advantages in election results. Instead, partisan clerks oversee similar turnout and policies even when it is in their party's interest for them to increase or decrease turnout.

6 Why Don't Clerks Advantage Their Party?

Why do elected clerks not advantage their party? Drawing on our discussion in Section 2, we explore four explanations. The first explanation we explore is that clerks are elected officials and want to win reelection, so clerks from both parties work to satisfy the median voter in their county and produce similar policies and outcomes. The next three explanations are countervailing forces within the citizen-candidate framework that could lead clerks to not advantage their party: 1) qualified candidates hold similar views across parties, 2) administration has modest effects on turnout and outcomes, and 3) clerks face a collective action problem because elections are decided jointly by many counties. No single piece of evidence we present conclusively answers why clerks do not advantage their party, but we provide suggestive evidence against the reelection incentive and collective action problem as meaningful constraints and discuss existing research that favors preference convergence and the limited ability of clerks to influence electoral outcomes as explanations.

Table 3: **Estimates of Increase in Partisan Advantage Provided by Term-Limited Clerks.**

	Change in Dem Pres Vote Share		
	(1)	(2)	(3)
Dem Elec Official	-0.008	-0.005	-0.006
	(0.017)	(0.012)	(0.014)
Counties	66	66	66
N	75	75	75
Year FE	No	Yes	Yes
Lag Dem Vote Share	No	No	Yes

Robust standard errors clustered by county in parentheses. The data is limited to term-limited, incumbent clerks in Indiana. The outcome is the change in Democratic presidential vote share from the first term to the second term of the term-limited clerk.

6.1 Reelection Incentives Do Not Noticeably Affect Partisan Advantage Clerks Provide

Might Democratic and Republican clerks oversee similar election outcomes because they are competing for the support of the median voter in their next election? This is the prediction of one class of standard political economy models of elections (Downs 1957; Fearon 1999). We study this question using election official term limits. Clerks in Indiana are allowed to serve for no more than two consecutive four-year terms in a 12-year period.[68] If the threat of being thrown out of office is the main constraint on clerks advantaging their party, clerks should advantage their party more in their second term than their first term, since the reelection incentive is removed entirely. To test this prediction, we compare the change in Democratic presidential vote share from the first term to the second term of Democratic clerks to the same change for Republican clerks.

Table 3 presents our estimates. In the first column, we present the simple difference in means between Democratic and Republican clerks in how much more of their county's presidential vote goes to the Democratic candidate in their second term than their first term. The second column presents regression estimates with year fixed effects to account for statewide changes in support for Democratic presidential candidates across years in our data. The third column presents regression estimates with lagged Democratic presidential vote share in addition to year fixed effects to account for any polarization across counties in voting trends over the years.

Across all three regression specifications, we find that clerks do not give their party a bigger advantage when they are ineligible for reelection. While this simple analysis does not fully account for differences in trends in presidential vote across counties unrelated to the party of the clerk, which

[68] The effect of lifetime term limits is larger than consecutive term limits in state legislatures, but consecutive limits still substantially reduce the reelection incentive (Fouirnaies and Hall 2022).

our regression discontinuity estimates do account for, we take this as suggesting that reelection incentives are not a key constraint limiting the advantage clerks give their party.

This result suggests that concerns about reelection are not the main reason clerks do not advantage their party, but it does not imply that elections fail to motivate clerks. Clerks seem to be held accountable for bad behavior in many cases. For example, in 2010, a lawsuit was filed against Boone County, West Virginia clerk Gary Williams alleging sexual harassment right after he was reelected without opposition.[69] He was challenged in the Democratic primary six years later and lost, receiving only 34% of the vote. Bosque County, Texas clerk Brigitte Bronstad was arrested for taking money from the county in 2002, right before the general election. Four write-in challengers quickly jumped into the race, successfully ensuring her defeat.[70] In other cases, election officials caught engaging in malfeasance retired rather than face the voters. This was the case for Montezuma County, Colorado clerk Carol Tullis in 2012, who faced a lawsuit alleging she demoted an employee for running against her,[71] and likely played a role in Whitman County, Washington auditor Eunice Coker's retirement, who faced a lawsuit in 2018 alleging improper denial of employee medical leave, financial mismanagement, ballot irregularities, audit failures, discriminatory behavior, and politically partisan efforts to alter election outcomes.[72]

6.2 Clerk Candidates May Have More Similar Preferences Across Parties

[69] https://www.wvgazettemail.com/news/lawsuit-alleges-sexual-harassment-by-county-clerk/article_dcbac0b3-e6f7-5f8e-bb5c-38c960d76026.html

[70] https://www.mrt.com/news/article/Bosque-County-clerk-pleads-guilty-to-theft-7791967.php

[71] http://api.the-journal.com/articles/8636

[72] https://dnews.com/local/whitman-county-former-auditor-on-the-hook-for-70k/article_9a3cdc46-ac5a-5a43-bc86-ecf6e0ed1bad.html

Might Democratic and Republican clerks agree on how to run elections? Looking at the public, this seems unlikely. The average Democrat and Republican have meaningfully different views on issues like automatic voter registration, all-mail voting, and moving election day to a weekend (Stewart 2021). On the other hand, candidates and winners often have experience in election administration and may have more similar policy views. Manion et al. (2021) surveys members of the public and clerks, and compares their responses across parties. While Democratic and Republican clerks still have meaningfully different responses to some policy questions, their preferences are more similar than Democrats and Republicans in the public and fully converge on some policy issues. For example, Democratic and Republican clerks express equivalent levels of voter confidence in national elections, agree that voting is a duty, and believe that local, state, and federal elections should be consolidated. Like their co-partisans in the public, Democratic and Republican clerks are divided on the issue of voter ID but hold much more similar views across parties on expanded early voting than members of the public—a policy that many clerks have discretion over. This explanation only partially accounts for the similarity in policies, turnout, and vote shares in elections run by Democrats and Republicans serving similar counties, but it is consistent with our main findings and existing survey data of these individuals.

In Section A.6.12 in the appendix, we also document that clerks from both parties serving identical counties implement roughly the same policies. While we cannot rule out that they do this because they expect these policies would have minimal effects (as we discuss below), this is consistent with clerks agreeing more on election administration across parties than the public.

6.3 Clerks May Have Limited Ability to Affect Election Outcomes

Even if clerks are unconstrained by reelection incentives and want to offer their party an advantage, they may not be able to. As we discuss in Section 3, clerks are given wide latitude to make important decisions such as where to locate polling places and when to host in-person early voting. These decisions may make it easier or harder to vote and likely affect some groups more than others. However, these policies do not necessarily affect election outcomes. First, when the cost of voting goes up, citizens may simply find the next cheapest way to vote (Clinton et al. 2020). Second, even if more people vote when the cost goes down, the new voters may be similar in partisan composition to the people already voting (Burden et al. 2014).

This explanation is difficult to directly test. If clerks know that they cannot meaningfully affect outcomes, and they only care about changing policy if it affects outcomes, we may not observe partisan differences in policies or turnout because clerks never even try to advantage their party. Still, based on the existing work on the limited effect of election administration, it is reasonable to expect clerks are at least somewhat constrained by the modest effects these policies have on partisan outcomes.

6.4 Clerks Do Not Advantage Their Party More When It Is Less Costly or When the Stakes Are Higher

Suppose most election officials would like to see their party win and that they all have authority to advantage their party in their county. If they bare costs for tilting elections in their party's favor, they would only want to advantage their party when it would plausibly change the statewide outcome. In this world, the fragmented nature of local election administration creates a collective action problem where partisan clerks would like to work together and swing the election in their party's favor, but they know that every individual clerk would have a reason to shirk and avoid baring the costs. This collective action problem does not arise if an individual clerk could reasonably expect their decisions to be pivotal and worth the cost.

We offer suggestive evidence that even clerks who face the lowest costs to advantaging their party or have the greatest chance of swinging an election in their party's favor do not advantage their party. We do this by identifying six related conditions that either make it less costly for an official to advantage their party or increase the value of the advantage they provide. The first two conditions—residential segregation and racial and ethnic diversity—make use of the fact that race and ethnicity are some of the most useful heuristics for guessing the party a citizen may vote for (Carlson and Hill 2021; Carmines and Stimson 1989; Hersh 2015). Even if clerks are primarily motivated by providing their party an advantage, they may fail to do so if they cannot easily distinguish between members of their party and the opposing party. Accordingly, local election officials may have an easier time giving their party an advantage in counties that are more diverse and segregated. The third factor we consider is county-level partisan balance. As we discuss in Section A.7.3 in the online appendix, we find using a stylized model that clerks serving counties evenly split between Democrats and Republicans will have a larger effect on election outcomes than clerks in places dominated by one party. The fourth factor we consider is the capacity of the office, which we proxy with population. We would expect clerks serving in larger counties to have greater capacity to affect election outcomes (Kimball and Baybeck 2013). The final two factors we consider—how close the last presidential election was in the state and whether the county is large enough to meaningfully alter the outcome—build on the prediction that election officials might be most motivated to advantage their party when it would be most likely to help their party win.

Figure 5 reports estimates of the effect in counties where we would expect clerks to be most likely to advantage their party if collective action problems were the primary barrier. Each point is an effect estimated using local linear regression with triangular kernel weights—the same specification we use in column 4 of Table 1. The lines extending out from the points are 95-percent confidence intervals. From top to bottom, the plot presents estimates using seven subsets of the data: 1) all counties, 2) segregated counties—i.e. those with residential racial dissimilarity scores above the median, 3) counties where non-Hispanic White people make up less than 80% of the population, 4) counties in which the last Democratic presidential candidate won or lost the county by less than 15 percentage points, 5) counties with over 100,000 residents, 6) counties in states in which the last Democratic presidential candidate won or lost by less than five percentage points, and 7) counties with populations that are at least half as large as the margin by which the last Democratic presidential candidate won or lost in the state.

The estimates reported in Figure 5 are more consistent with clerks intending to administer elections in neutral ways than with a collective action problem preventing clerks from advantaging their party. If they want to advantage their party but fail due to a collective action problem, we might observe a partisan advantage in the cases where a county is closer to being pivotal or the cost of advantaging one party is lower. Instead, across the seven subgroups that we study, we cannot reject the null hypothesis that Democratic and Republican clerks fail to advantage their party. Our evidence suggests that clerks do not noticeably advantage their party even when they have the greatest ability to affect the statewide outcome and the lowest costs.

The regression specifications chosen and the rules used for including a county in each subgroup are somewhat arbitrary. In Section A.7 in the online appendix, we present estimates using all four of our regression specifications for every outcome and estimates across many different rules for inclusion in each subgroup analysis. The results reported in Figure 5 are similar to those we estimate across our different specifications and subgroup inclusion rules.

7 Conclusion

The unusual American practice of electing partisan local officials to oversee elections concerns many experts and members of the public. When an official runs as a member of a party, it is natural to expect that they will use their authority to advance their party's goals. Even some local election officials themselves report feeling uncomfortable running as partisans when they have a duty to be neutral.[73]

Using a credible research design with new partisan clerk election data from 21 states, we find that partisan election officials do not typically offer a large advantage to their party. While we cannot be confident that partisan officials do not offer rare and large or very small but consequential advantages to their party, our findings make clear that clerks are not consistently providing their party a meaningful advantage to date.

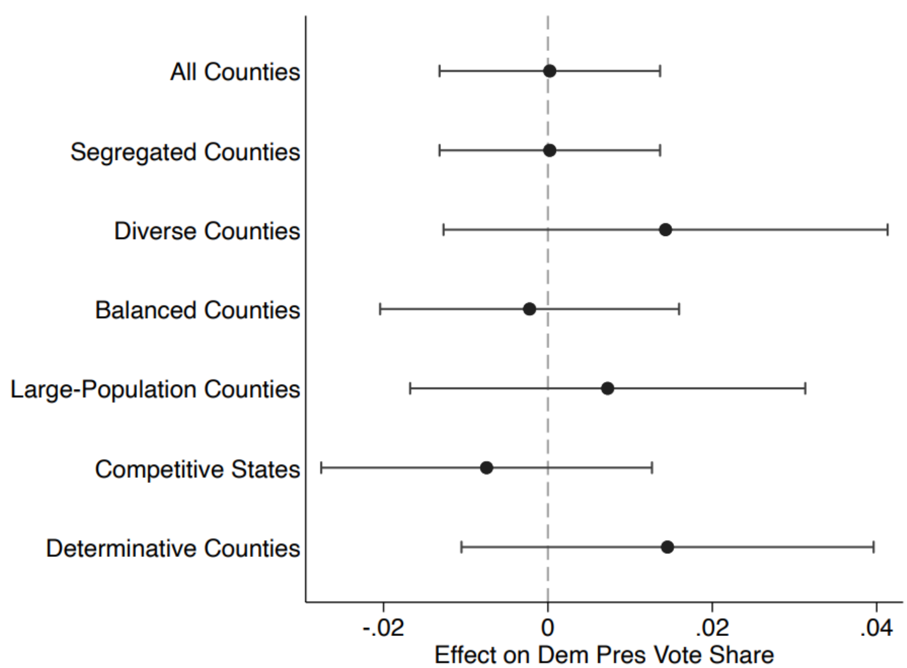
While clerks do not advantage their party, this does not imply that we ought to use partisan elections to select election administrators. In many parts of the country and around the world, elections are run by appointed bureaucrats, and future work should consider how the benefits and costs of such a system weigh against the benefits and costs of the system we study in this article (Ferrer 2022). Also, a recent survey of the public found that about 75% of both Democrats and Republicans support requiring that election officials be selected on a nonpartisan basis (Stewart 2021). Future work should consider if even neutral partisan election administration leaves citizens suspicious that the election was unfair.

How concerned should we be that future changes in who runs and wins clerk races may lead to

[73] https://www.npr.org/2018/11/29/671524134/partisan-election-officials-are-inherently-unfair-but-probably-here-to-stay

highly partisan election administration? Our explanation that election policies only have modest effects on electoral outcomes provides some reason for optimism. However, our explanation that clerks are neutral because they share more similar preferences across parties than the public does leave room for concern. If the next generation of election officials begins to exhibit higher levels of preference polarization, there is no guarantee that partisan election officials will continue to administer elections neutrally.

Figure 5: **Clerks Do Not Advantage Their Party More When It Is Easier or Most Advantageous**. Each dot represents a regression discontinuity-based estimate of the effect of electing a Democratic clerk on residual Democratic presidential vote share for a subset of the data. The lines around each point represent 95-percent confidence intervals. Estimates come from regressions that mimic column 4 in Table 1 using local linear regression with triangular kernel weights. Segregated counties are those with residential racial dissimilarity scores above the median. Diverse counties are those less than 80% non-Hispanic White. Balanced counties are those in which the most recent Democratic presidential candidate won or lost by less than 15 percentage points. Large-population counties are those with over 100,000 residents. Competitive states are those in which the most recent Democratic presidential candidate won or lost by less than 5 percentage points. Determinative counties are those where the population of the county is at least half as large as the most recent Democratic presidential candidate's margin of victory or loss at the state level. Full tabular results are found in Section A.7 of the online appendix.



Chapter 4 Appendix

A.1 Review of Previous Literature on Partisan Differences in Local Election Administration

Table A.1 summarizes the literature to date on partisan differences in local election administration. Each row of A.1 represents a study of partisan differences, and the columns summarize the study's setting, research design, outcome of interest, finding, and any conditional aspects of the finding.

Table A.1: **Review of Partisan Local Election Official Literature.**

Paper	Setting	Design	Outcome	Partisan Difference	Condition
Hamilton and Ladd (1996)	NC	X-Section	Straight party voting option	Yes	
Stuart (2004)	FL	X-Section	Purge rate of potential felons	Yes	
Kimball, Kropf, and Battles (2006)	USA	X-Section	Provisional ballots cast	Mixed	In heavily co-partisan jurisdictions
Kimball, Kropf, and Battles (2006)	USA	X-Section	Provisional ballots counted	Mixed	In heavily co-partisan jurisdictions
Bassi, Morton, and Trounstine (2009)*	USA	County DiD	Change in Turnout	Yes	
Bassi, Morton, and Trounstine (2009)*	USA	County DiD	Dem Margin of Vicotry	Yes	
Dyck and Seabrook (2009)*	OR	X-Section	Vote-by-Mail Acceptance	Yes	
Dyck and Seabrook (2009)*	OR	X-Section	Move Dems to inactive list	Yes	
Kimball and Baybeck (2010)*	USA	Survey	Support for access and security policies	Mixed	In large jurisdictions
Burden et al. (2013)	WI	X-Section	Support for access and security policies	No	
Burden et al. (2013)	WI	X-Section	Turnout	Mixed	For appointed Reps in Dem electorates
Kimball et al. (2013)	USA	Survey	Support for access and security policies	Mixed	In large jurisdictions
Kimball et al. (2013)	USA	Survey	Support for provisional voting programs	Mixed	In heavily co-partisan jurisdictions
Kropf, Vercellotti, and Kimball (2013)	USA	Survey	Support for provisional voting	Mixed	In heavily co-partisan jurisdictions
White, Nathan, and Faller (2015)	USA	Experiment	Bias in email response rate	No	
Merivaki and Smith (2016)	FL	X-Section	Provisional ballots cast	Mixed	In midterm elections
Merivaki and Smith (2016)	FL	X-Section	Provisional ballots rejected	Mixed	In midterm elections
Porter and Rogowski (2018)	WI	Experiment	Co-partisan email response rate	Mixed	In heavily co-partisan jurisdictions
Mohr et al. (2019)	NC	County DiD	Election expenditures	Mixed	In heavily co-partisan jurisdictions
McBrayer, Williams, and Eckelman (2020)	TX	X-Section	Number of early voting sites	Yes	
McBrayer, Williams, and Eckelman (2020)	TX	X-Section	Location of early voting sites	No	
Shepherd et al. (2021)	NC	Individual Panel	Polling location change	No	

X-Section refers to a cross-sectional design, and DiD refers to a difference-in-differences design. *Unpublished manuscript.

A.2 The Responsibilities of Local Election Officials

Table A.2 shows a stylized division of states into tiers based on how much authority is vested in a single partisan elected election official. Table A.3 describes the duties of these officials across states. In cases where officials have limited discretion under state law, we indicate that by describing the discretion they have as high, mid, or low, indicating much, some, or little discretion, respectively.

Table A.2: **States with Partisan Elected Local Election Officals.**

Tier	Description	Examples	States	In Analysis?
1	Partisan elected official does everything or nearly everything	Separate canvassing board (FL)	CO, FL, IA, ID, IL, KS, MO, MT, NE, NV, SD, UT, WA, WY	Yes
2	Partisan elected official has some shared authority	Separate registration board or absentee voting official (AL, GA, NM, TX); Shares authority with elections board but holds the decisive vote (IN, KY); Shares authority with county legislative body (WV)	AL, GA, IN, KY, NM, TX, WV	Yes; excluded in robustness check
3	Partisan elected official has limited authority	Administers registration and early voting but not Election Day voting (AR, AZ, MS); Shares authority with separate board and lacks decisive vote (LA)	AR, AZ, LA, MS	No
4	Partisan elected official has severely limited authority	Municipal official or divided between city and county (CT, MA, MI, RI, VT, WI); Shares authority and has few responsibilities (NJ)	CT, MA, MI, NJ, RI, VT, WI	No
5	No partisan elected official	Election officials nonpartisan and/or appointed	AK, CA, DC, DE, HI, MD, ME, MN, NC, ND, NH, NY, OH, OK, OR, PA, SC, TN, VA	No

This table divides states into tiers based on the amount of responsibility individual partisan elected local officials have in administering elections. In states with local- and county-level variation in responsibilities, only those counties with partisan elected officials are considered. Where there is within-state variation in the presence of other officials (i.e., for IN and TX), the modal case for each state is considered.

Table A.3: **Local Election Offical Responsibilities by State.**

State	Officer	Registration	List Maintenance	Polling Place	Early Voting	Poll Workers	Voting Equipment	Training
Alabama	Probate Judge	Low	Low	Mid	Low	Low	High	High
Colorado	Clerk	High	High	Low	Low	Low	High	Low
Florida	Supervisor of Elections	High	High	Mid	High	Mid	High	High
Georgia	Probate Judge	Low	Low	High	Mid	Mid	High	Low
Idaho	Clerk	High	High	Low	High	Mid	High	High
Illinois	Clerk	High	High	High	High	Low	High	Mid
Indiana	Clerk	High*	High*	Low	High	Low	High	Mid
Iowa	Auditor	High	High	Low	High	Low	High	Low
Kansas	Clerk	High	Mid	High	High	Low	High	Mid
Kentucky	Clerk	High	Mid	Mid	Low	Low	High	Mid
Missouri	Clerk	High	High	High	Low	Low	High	High
Montana	Election Administrator	High	High	Low	Low	Low	High	Low
Nebraska	Clerk	High	Mid	High	High	Mid	High	Mid
Nevada	Clerk	High	High	High	High	Mid	High	High
New Mexico	Clerk	High	High	Low	High	Low	Low	Mid
South Dakota	Auditor / Finance Officer	High	High	Mid	Low	Mid	High	High
Texas	Clerk / District Clerk / Tax Assessor	Varies	Varies	Mid	High	Mid	High	High
Utah	Clerk	High	High	High	High	Low	High	High
Washington	Auditor	High	High	Low	Low	N/A	High	High
West Virginia	Clerk	High	High	Mid	Mid	Mid	High	Mid
Wyoming	Clerk	High	High	High	Low	Mid	High	High

High, mid, and low indicate degrees of discretion with high representing the most discretion and low representing the least. In states with county-level variation in local election official responsibilities, this table applies to officials with primary responsibility over voting administration. *In Indiana, Allen, LaPorte, Madison, Marion, St. Joseph, Vanderburgh, and Vigo counties have separate registration officials.

A.3 Describing the New Data on Election Officials

As we discuss Section 4.1, the top panel of Figure A.1 presents the relationship between Democratic clerk vote share and Democratic presidential vote share in counties that elect clerks on a presidential election cycle. The bottom panel plots the relationship between lagged Democratic presidential vote share and current period Democratic presidential vote share. The correlation between presidential and clerk vote share is quite low, suggesting that voters are considering additional factors and treat Democratic and Republican party labels differently in local election official races. This is even more striking considering the comparison is between clerk and presidential races featured in the same election and presidential contests occurring four years apart. Considering the full dataset of elections and comparing Democratic clerk vote share with lagged presidential vote share weakens the correlation even further, to 0.30.

Table A.4 compares the counties for which we have election data to the counties that elect partisan local election officials but where we do not have election data using 2010 decennial census data.[74] The counties we are missing tend to be less populous, in the South, and have larger Black and Hispanic populations. The counties that do not have elected partisan election officials tend to be much more populous, in the South or Northeast, and have larger Black but smaller Hispanic populations.

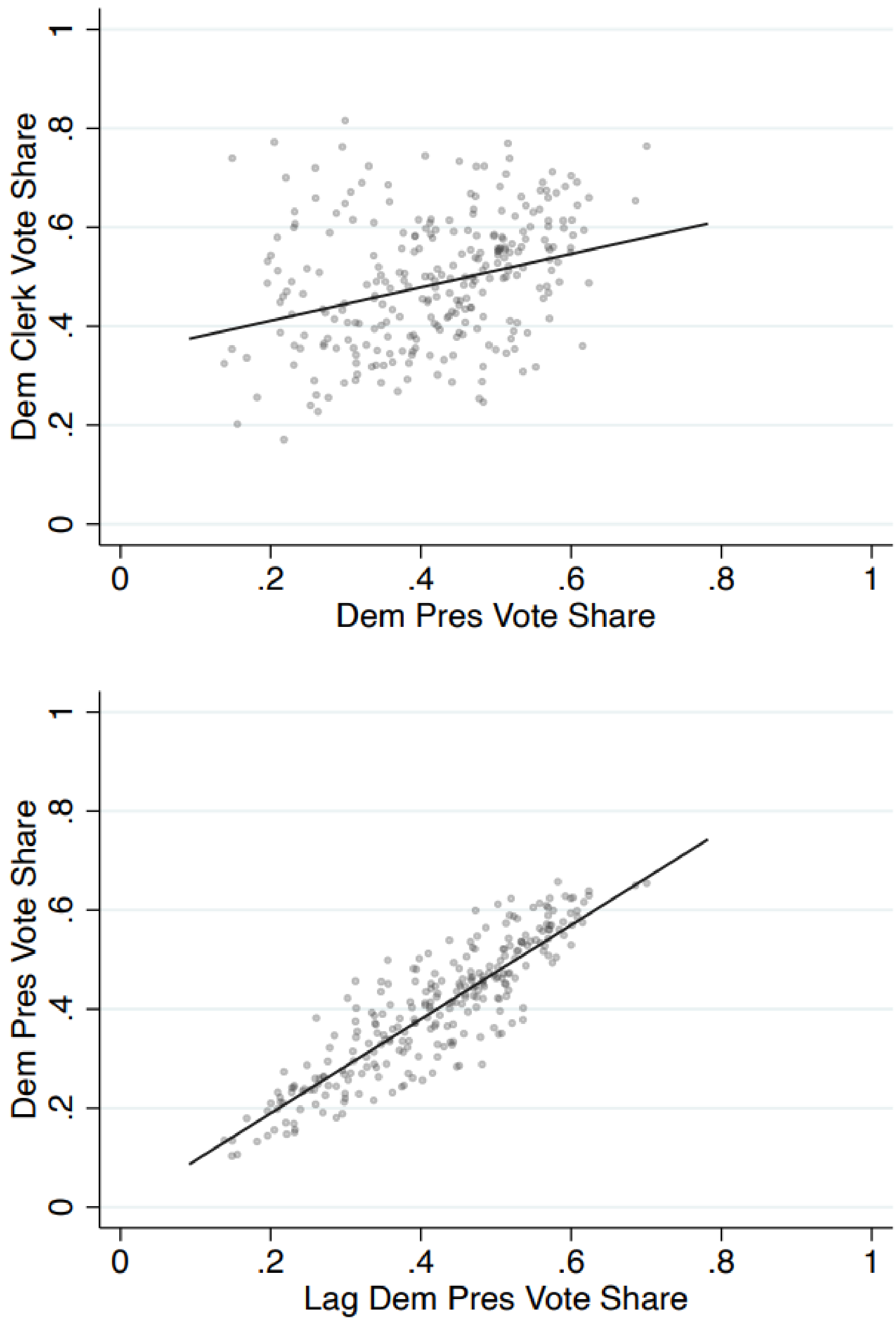
[74] https://www.census.gov/data/datasets/2010/dec/summary-file-1.html

Table A.4: **Description of Counties In and Not In Sample.**

Outcome	In Sample	Not In Sample	Not In Scope
Population (Thousands)	55.51	37.88	143.06
	(171.99)	(111.74)	(404.58)
Share Non-Hispanic White	0.81	0.77	0.76
	(0.19)	(0.22)	(404.58)
Share Black	0.05	0.08	0.12
	(0.11)	(0.13)	(0.16)
Share Hispanic	0.10	0.12	0.06
	(0.15)	(0.20)	(0.10)
Northeast	0.00	0.00	0.14
Midwest	0.41	0.46	0.26
South	0.38	0.54	0.50
West	0.21	0.00	0.10
Num Counties	1,310	237	1,586

Standard deviations reported in parentheses below group means.

Figure A.1: **Low Correlation between Democratic Clerk Vote Share and Democratic Presidential Vote Share**. The top panel presents the relationship between Democratic clerk vote share and Democratic presidential vote share in counties that elect clerks on a presidential election cycle. The bottom panel presents the much stronger relationship between Democratic presidential vote share and lagged Democratic presidential vote share in these counties.



A.4 Predicting Election Results

When a lagged outcome is available, it is standard practice in regression discontinuity designs to improve precision by including the lagged outcome as a covariate in the regression (Calonico et al. 2019). This approach works well when the relationship between the lagged outcome and current-period outcome is constant across units. While the relationship between lagged and current-period Democratic presidential vote share is positive across states and times, there is still considerable variation in this relationship due to differences in candidates over time as well as regional and state-specific political changes. If we had many counties in each state and election year that had close elections for their local election officials, we could include state-year-specific intercepts and coefficients on lagged vote share to account for this variation and improve our precision. However, only a subset of counties have close elections for local election official.

As we discuss in Section 4.5, we improve on standard practice using a three-step process that follows the recommendations of Lee and Lemieux (2010) and Noack, Olma, and Rothe (2021). They study an estimator that first predicts the outcome and then uses the residuals from that prediction exercise as the outcome in a standard regression discontinuity estimator. Under the standard regression discontinuity design assumption of smoothness in predetermined covariates at the treatment assignment threshold, this estimator produces unbiased point estimates and valid inference.

We use this procedure throughout the paper, constructing residualized outcomes by first using a lagged outcome to predict the outcome of interest and then taking the remaining error from this prediction process. We choose the predictor that minimizes out-of-sample prediction error using leave-one-out cross-validation. We fit our regression holding out one observation at a time, use that regression to predict the held out unit's outcome value, and compute the error as the difference between the observed and predicted outcome values.

We test four regression specifications:

- Pooled coefficients and intercepts: $Y_{ct+k} = \beta Y_{ct} + \gamma + \epsilon_{ct+k}$
- State-specific coefficients and intercepts: $Y_{ct+k} = \beta_s Y_{ct} + \gamma_s + \epsilon_{ct+k}$
- Year-specific coefficients and intercept: $Y_{ct+k} = \beta_{t+k} Y_{ct} + \gamma_{t+k} + \epsilon_{ct+k}$
- State-year-specific coefficients and intercept: $Y_{ct+k} = \beta_{st+k} Y_{ct} + \gamma_{st+k} + \epsilon_{ct+k}$

where Y is our outcome variable, c indexes counties, s indexes states, t indexes election years, and $t + k$ is the election k years later (e.g., $k = 4$ for presidential elections and $k = 6$ for senate elections). Predicting Democratic presidential vote share in leave-one-out cross-validation, we find that the mean squared prediction error is 0.030 for the state-year-specific regression, 0.041 for the year-specific regression, 0.053 for the state-specific regression, and 0.056 for the pooled regression. We choose the state-year-specific regression because it minimizes out-of-sample error when predicting presidential election results. We follow this specification for all other outcomes, using state-year-specific regressions to maintain consistency.

A.5 Calculating Minimum Detectable Effects

Throughout the paper, we present estimates of the minimum detectable effect with 80% power. We compute these estimates with the following optimization procedure:

$$\arg\min_{\tau} \; (\phi(\frac{\tau}{\sigma} - z_{\alpha}) - (1 - \beta))^2, \text{ subject to } \tau > 0$$

where τ is the hypothesized effect, σ is the standard error for the effect, z_α is the z score threshold implied by a significance level of α, β is the power level, and ϕ is the standard normal cumulative distribution function. We plug in our estimate of σ from each regression and set $\alpha = 0.05$ and $\beta = 0.80$ per convention. We use numerical optimization to find the positive value of τ that minimizes this function.

A.6 Validating the Main Findings

A.6.1 Counties that Narrowly Elect Democrats vs. Republicans Are Similar on Pre-Treatment Covariates

As we discuss in Section 4.4, our close-election regression discontinuity design should ensure that the local averages of pre-treatment county-level covariates are similar in places that narrowly elect Democrats and those that narrowly elect Republicans. We show that this holds in practice in Tables A.5 and A.6. We find that the design works as expected, giving us balance on all of the pre-treatment covariates we check across our regression specifications.

Table A.5: **Regression Discontinuity Design Balances Pre-Treatment Democratic Presidential Vote Share and Turnout.**

	Lagged Dem Pres Vote Share				Lagged Turnout			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dem Elec Official	0.029	0.040	0.007	0.020	0.008	0.005	0.019	0.013
	(0.022)	(0.017)	(0.029)	(0.022)	(0.019)	(0.014)	(0.026)	(0.019)
N	355	643	178	392	614	1115	307	698
Clusters	355	643	178	392	355	643	179	404
Bandwidth	0.07	0.15	0.04	0.08	0.07	0.15	0.04	0.09
BW Selection	CCT	CCT*2	CCT/2	CCT	CCT	CCT*2	CCT/2	CCT
Kernel	Unif	Unif	Unif	Tri	Unif	Unif	Unif	Tri

Robust standard errors clustered by clerk election in parentheses. The bandwidth row reports the number of maximum clerk win margin allowed for inclusion in each specificaiton. CCT refers to Calonico, Cattaneo, and Titiunik (2014) bandwidth selection procedure. Unif means the specification uses a uniform kernel. Tri means the specification uses a triangular kernel.

Table A.6: **Regression Discontinuity Balances County-Level Covariates.**

Outcome Variable	Balance at RD Cut Point			
	(1)	(2)	(3)	(4)
Log(Population)	0.294	0.131	0.262	0.231
	(0.253)	(0.195)	(0.350)	(0.262)
	[447]	[772]	[772]	[772]
Share Non-Hispanic White	0.007	0.018	0.046	0.022
	(0.035)	(0.027)	(0.052)	(0.042)
	[393]	[650]	[650]	[650]
Share Black	0.029	0.014	0.026	0.017
	(0.024)	(0.016)	(0.034)	(0.020)
	[254]	[479]	[479]	[479]
South	0.016	0.018	0.001	0.040
	(0.097)	(0.070)	(0.131)	(0.094)
	[372]	[675]	[675]	[675]
West	0.017	0.051	-0.066	0.009
	(0.084)	(0.062)	(0.116)	(0.083)
	[406]	[726]	[726]	[726]
Bandwidth Selection	CCT	CCT*2	CCT/2	CCT
Kernel	Uniform	Uniform	Uniform	Triangular

Each unbracketed number is an estimate of balance for a particular variable at the discontinuity using a given RD estimator. Robust standard errors clustered by clerk election in parentheses. Sample size reported in square braces. CCT refers to Calonico, Cattaneo, and Titiunik (2014) bandwidth selection procedure.

A.6.2 Counties Not Sorting into Treatment or Control

As we discuss in Section 4.4, one potential threat to our design is counties sorting into treatment or control. This could happen if local election officials can manipulate the vote total in subtle ways to ensure they win if they would otherwise lose without intervention. We evaluate this concern using a modified version of the density test proposed in McCrary (2008). Since we expect counties with Democratic clerks to be more likely to narrowly elect Democrats, and the same for Republicans, we change the running variable to ask whether the sitting party is more likely to win very close elections.

Figure A.2: **Density of Clerk Election Results.**

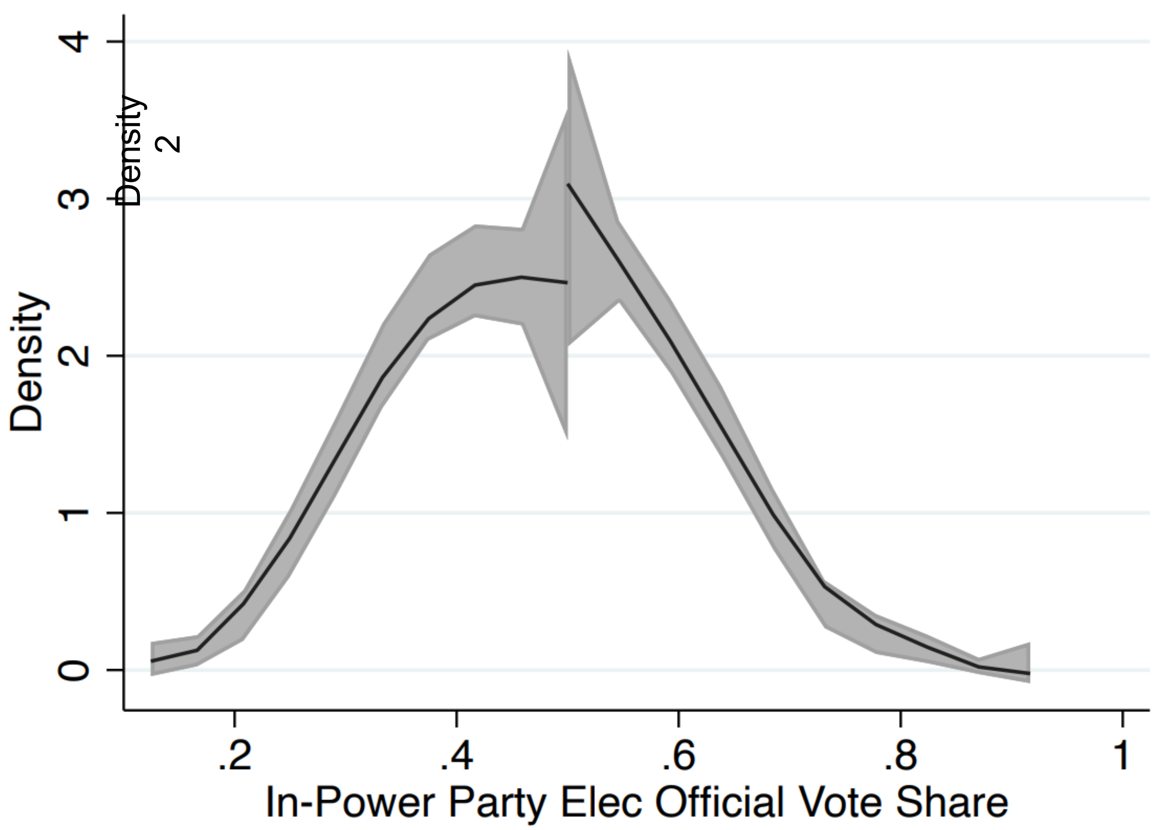


Figure A.2 presents the McCrary plot. While the party in power wins slightly more close elections than they lose, the difference in the densities is small enough that it could easily arise by chance.

A.6.3 Main Findings Not Sensitive to Choice of Estimator

As we discuss in Section 4.5, using the residuals after predicting Democratic presidential vote share can substantially improve precision relative to using vote share as the outcome or adjusting for lagged vote share within the regression. In Table A.7 below, we validate that our main results are not limited to using our residualized outcome. The first four columns of Table A.7 present the simplest regression discontinuity estimates including no covariates and using Democratic presidential vote share as our outcome. While our estimates are noisy, they are consistent with our main finding that clerks do not offer their party a substantial advantage. The point estimates are also quite similar to the point estimates we find in columns 1 through 4 of Table A.5, suggesting that most of the higher Democratic presidential vote share in Democrat-controlled counties arises from a modest imbalance in treatment assignment. In columns 5 through 8 of Table A.7, we include lagged Democratic presidential vote share as a covariate. Our findings are similar to those we report in our main analysis in Section 5. Put together, we find in Table A.7 that our main results are not limited to our chosen estimator.

Table A.7: **Effect of Democratic Election Officials on Democratic Presidential Vote Share.**

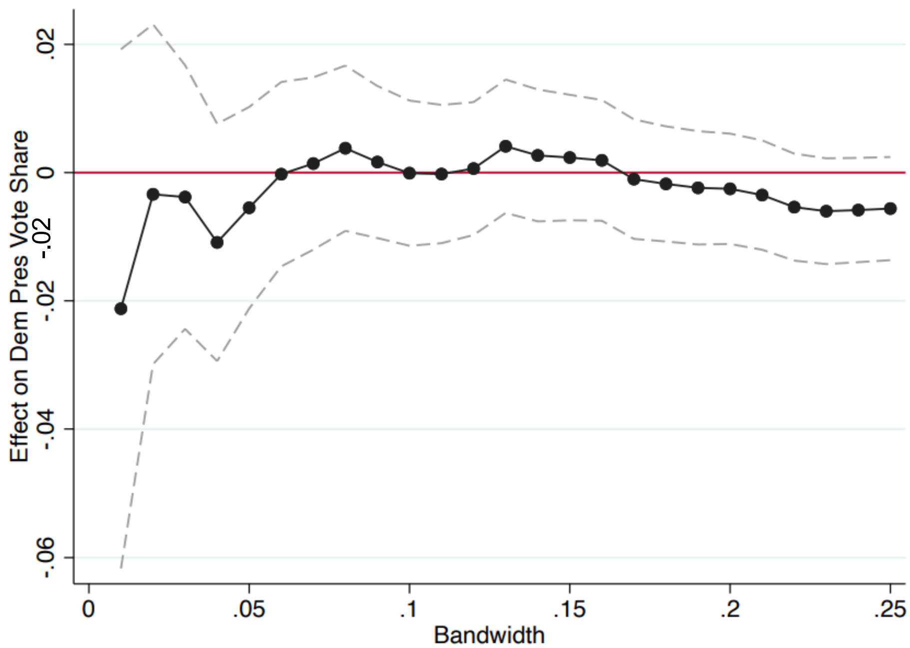
	Dem Pres Vote Share							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dem Elec Official	0.030	0.027	0.002	0.025	-0.005	-0.001	0.003	-0.006
	(0.024)	(0.018)	(0.032)	(0.024)	(0.013)	(0.009)	(0.018)	(0.011)
N	403	723	202	456	327	597	165	462
Clusters	391	702	198	442	327	597	165	462
Bandwidth	0.08	0.16	0.04	0.09	0.07	0.13	0.03	0.10
BW Selection	CCT	CCT*2	CCT/2	CCT	CCT	CCT*2	CCT/2	CCT
Kernel	Unif	Unif	Unif	Tri	Unif	Unif	Unif	Tri
Lagged Vote Share	No	No	No	No	Yes	Yes	Yes	Yes
Min Detectable Effect	0.060	0.046	0.079	0.060	0.032	0.024	0.044	0.028

Robust standard errors in parentheses. The bandwidth row reports the number of maximum clerk win margin allowed for inclusion in each specificaiton. CCT refers to Calonico, Cattaneo, and Titiunik (2014) bandwidth selection procedure. Min detectable effect refers to the minimum effect that a one-sided test with a 0.05 alpha would have 80% power to detect. Lagged vote share captures whether lagged Democratic presidential vote share is included as a covariate in the regression. Unif means the specification uses a uniform kernel. Tri means the specification uses a triangular kernel.

A.6.4 Main Findings Not Sensitive to Choice of Bandwidth

Analyses of regression discontinuities must weigh the bias reduction that comes from only using data close to the cut point against the precision improvement that comes from using data further from the cut point. In Figure A.3 we present our main result across many possible bandwidths. The choice of bandwidth does not meaningfully change the interpretation of our findings. All of these analyses imply that local election officials do not meaningfully advantage their party.

Figure A.3: **Sensitivity of Estimated Effect on Democratic Presidential Vote Share across Bandwidths.**



A.6.5 Main Finding Similar Across Time

In Figure 4 in the main analysis, we presented graphical evidence that our main finding—election officials do not noticeably advantage their party—is not limited to the early part of our study period but rather holds across time. Here, we present the results of our analysis in tabular format, conducting a separate regression discontinuity of electing a Democratic local election official on Democratic presidential vote share in every presidential election since 2004.

Table A.8: **Effect of Democratic Election Officials on Democratic Presidential Vote Share for Each Presidential Election.**

	Dem Pres Vote Share				
	2004	2008	2012	2016	2020
	(1)	(2)	(3)	(4)	(5)
Dem Elec Official	0.022	-0.013	-0.009	0.006	-0.010
	(0.032)	(0.014)	(0.013)	(0.017)	(0.011)
N	46	67	63	93	83
Bandwidth	0.08	0.08	0.07	0.08	0.07
BW Selection	CCT	CCT	CCT	CCT	CCT
Kernel	Tri	Tri	Tri	Tri	Tri

Robust standard errors in parentheses. The outcome is first regressed on a state- and year-specific lag using all counties including those for which clerk election results are not available. The regression discontinuity is estimated using the residuals from that regression. The bandwidth row reports the number of maximum clerk win margin allowed for inclusion in each specificaiton. CCT refers to Calonico, Cattaneo, and Titiunik (2014) bandwidth selection procedure. Tri means the specification uses a triangular kernel.

A.6.6 No Substantial Average Effect in States Granting Full Authority to One Official

In Table A.9, we present the results of our analysis focused only on the 14 states where one official has broad and unilateral authority (i.e., "Tier 1" states as shown in Table A.2, with Tier 2 states excluded). These states are: Colorado, Florida, Idaho, Illinois, Iowa, Kansas, Missouri, Montana, Nebraska, Nevada, South Dakota, Utah, Washington, and Wyoming. Our estimates are substantively similar to the estimates we report in Table 1.

Table A.9: **Effect of Democratic Election Officials on Democratic Presidential Vote Share, States with Full Authority in One Official.**

	Dem Pres Vote Share			
	(1)	(2)	(3)	(4)
Dem Elec Official	0.004	-0.002	-0.011	-0.003
	(0.009)	(0.006)	(0.014)	(0.009)
N	200	370	104	223
Bandwidth	0.07	0.15	0.04	0.09
Bandwidth Selection	CCT	CCT*2	CCT/2	CCT
Kernel	Uniform	Uniform	Uniform	Triangular

Robust standard errors in parentheses. The outcome is first regressed on a state- and year-specific lag using all counties including those for which clerk election results are not available. The regression discontinuity is estimated using the residuals from that regression. The bandwidth row reports the number of maximum clerk win margin allowed for inclusion in each specificaiton. CCT refers to Calonico, Cattaneo, and Titiunik (2014) bandwidth selection procedure.

A.6.7 Main Finding Similar Across States

In Figure A.4 and Table A.10, we present regression discontinuity estimates of the effect of electing a Democratic clerk on Democratic presidential vote share across states. We present all eight states from which we have at least 50 competitive races in our data. While the estimates are noisy, we do not find convincing evidence that clerks are able to advantage their party in any state.

Figure A.4: **Sensitivity of Estimated Effect on Democratic Presidential Vote Share across States.** Each dot represents a regression discontinuity-based estimate of the effect of electing a Democratic clerk on residual Democratic presidential vote share in a given state. Vertical lines extending from each point represent 95-percent confidence intervals. Estimates come from regressions that mimic column 4 in Table 1 using local linear regression with traingular kernel weights. Full tabular results are found below in Table A.10.

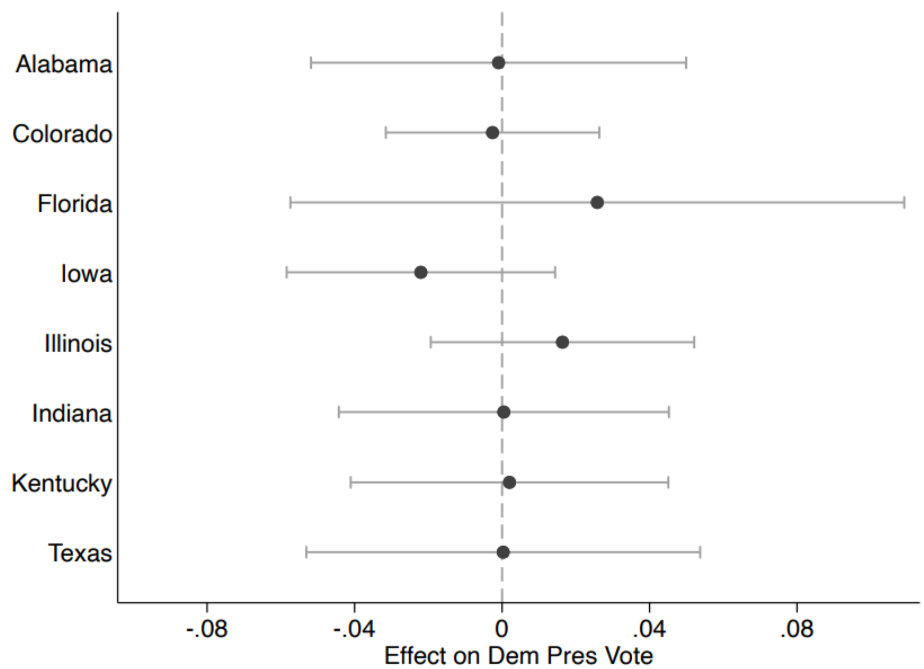


Table A.10: **Effect of Democratic Election Officials on Democratic Presidential Vote Share Across States.**

	Dem Pres Vote Share							
	Alabama	Colorado	Florida	Iowa	Illinois	Indiana	Kentucky	Texas
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dem Elec Official	-0.001	-0.003	0.026	-0.022	0.016	0.000	0.002	0.000
	(0.026)	(0.015)	(0.042)	(0.019)	(0.018)	(0.023)	(0.022)	(0.027)
N	32	24	14	32	44	40	19	24
Bandwidth	0.12	0.06	0.07	0.09	0.06	0.05	0.07	0.08
BW Selection	CCT	CCT	CCT	CCT	CCT	CCT	CCT	CCT
Kernel	Tri	Tri	Tri	Tri	Tri	Tri	Tri	Tri

Robust standard errors in parentheses. The outcome is first regressed on a state- and year-specific lag using all counties including those for which clerk election results are not available. The regression discontinuity is estimated using the residuals from that regression. The bandwidth row reports the number of maximum clerk win margin allowed for inclusion in each specificaiton. CCT refers to Calonico, Cattaneo, and Titiunik (2014) bandwidth selection procedure. Tri means the specification uses a triangular kernel.

A.6.8 Finding Not Sensitive to Excluding the South

In Table A.11, we present the results of our analysis focused only on counties in non-Southern states. We follow the U.S. Census Bureau definition of Southern states. Alabama, Florida, Georgia, Kentucky, Texas, and West Virginia are excluded. Our estimates are substantively similar to those reported in Table 1.

Table A.11: **Effect of Democratic Election Officials on Democratic Presidential Vote Share, Non-Southern Counties.**

	Dem Pres Vote Share			
	(1)	(2)	(3)	(4)
Dem Elec Official	0.001	0.003	0.001	0.000
	(0.008)	(0.006)	(0.012)	(0.008)
N	246	436	122	294
Bandwidth	0.07	0.14	0.03	0.09
Bandwidth Selection	CCT	CCT*2	CCT/2	CCT
Kernel	Uniform	Uniform	Uniform	Triangular

Robust standard errors in parentheses. The outcome is first regressed on a state- and year-specific lag using all counties including those for which clerk election results are not available. The regression discontinuity is estimated using the residuals from that regression. The bandwidth row reports the number of maximum clerk win margin allowed for inclusion in each specificaiton. CCT refers to Calonico, Cattaneo, and Titiunik (2014) bandwidth selection procedure.

A.6.9 Finding Not Sensitive to Excluding VRA Counties

In Table A.12, we present the results of our analysis focused only on counties not covered under the Section 5 pre-clearance provisions of the Voting Rights Act. We use data on Voting Rights Act preclearance coverage from Ang (2019). Our estimates are substantively similar to those reported in Table 1.

Table A.12: **Effect of Democratic Election Officials on Democratic Presidential Vote Share, Counties Not Subject to Pre-Clearance under VRA.**

	Dem Pres Vote Share			
	(1)	(2)	(3)	(4)
Dem Elec Official	0.003	0.004	-0.008	-0.002
	(0.007)	(0.005)	(0.010)	(0.008)
N	336	616	172	335
Bandwidth	0.08	0.15	0.04	0.08
Bandwidth Selection	CCT	CCT*2	CCT/2	CCT
Kernel	Uniform	Uniform	Uniform	Triangular

Robust standard errors in parentheses. The outcome is first regressed on a state- and year-specific lag using all counties including those for which clerk election results are not available. The regression discontinuity is estimated using the residuals from that regression. The bandwidth row reports the number of maximum clerk win margin allowed for inclusion in each specificaiton. CCT refers to Calonico, Cattaneo, and Titiunik (2014) bandwidth selection procedure.

In Table A.13, we present the results of our analysis focused only on counties previously covered under the pre-clearance provision of the Voting Rights Act but after the ruling in *Shelby County v. Holder* that removed them. Our estimates are substantively similar to those reported in Table 1.

Table A.13: **Effect of Democratic Election Officials on Democratic Presidential Vote Share, Counties Formerly Subject to Pre-Clearance.**

	Dem Pres Vote Share			
	(1)	(2)	(3)	(4)
Dem Elec Official	-0.015	0.014	0.001	0.014
	(0.024)	(0.018)	(0.024)	(0.020)
N	25	43	12	18
Bandwidth	0.07	0.14	0.03	0.05
Bandwidth Selection	CCT	CCT*2	CCT/2	CCT
Kernel	Uniform	Uniform	Uniform	Triangular

Robust standard errors in parentheses. The outcome is first regressed on a state- and year-specific lag using all counties including those for which clerk election results are not available. The regression discontinuity is estimated using the residuals from that regression. The bandwidth row reports the number of maximum clerk win margin allowed for inclusion in each specificaiton. CCT refers to Calonico, Cattaneo, and Titiunik (2014) bandwidth selection procedure.

A.6.10 No Substantial Average Effect in Senate, Governor, or Presidential Elections

In Table A.14, we present the results of our analysis including elections for governor, US senate, and president. Our estimates are substantively similar to those reported in Table 1, although are noisier and slightly more positive.

Table A.14: **Effect of Democratic Election Official on Democratic Vote Share, Elections for President, Senate, and Governor.**

	Dem Vote Share			
	(1)	(2)	(3)	(4)
Dem Elec Official	0.006	0.004	-0.006	0.003
	(0.007)	(0.005)	(0.010)	(0.007)
N	1211	2144	610	1460
Clusters	422	750	219	507
Bandwidth	0.09	0.18	0.05	0.11
Bandwidth Selection	CCT	CCT*2	CCT/2	CCT
Kernel	Uniform	Uniform	Uniform	Triangular
Min Detectable Effect	0.018	0.011	0.026	0.018

Robust standard errors clustered by clerk election in parentheses. The outcome is first regressed on a state- and year-specific lag using all counties including those for which clerk election results are not available. The regression discontinuity is estimated using the residuals from that regression. The bandwidth row reports the number of maximum clerk win margin allowed for inclusion in each specificaiton. CCT refers to Calonico, Cattaneo, and Titiunik (2014) bandwidth selection procedure. Min detectable effect refers to the minimum effect that a one-sided test with a 0.05 alpha would have 80% power to detect.

A.6.11 Effect Not Limited To Counties with Close Clerk Elections

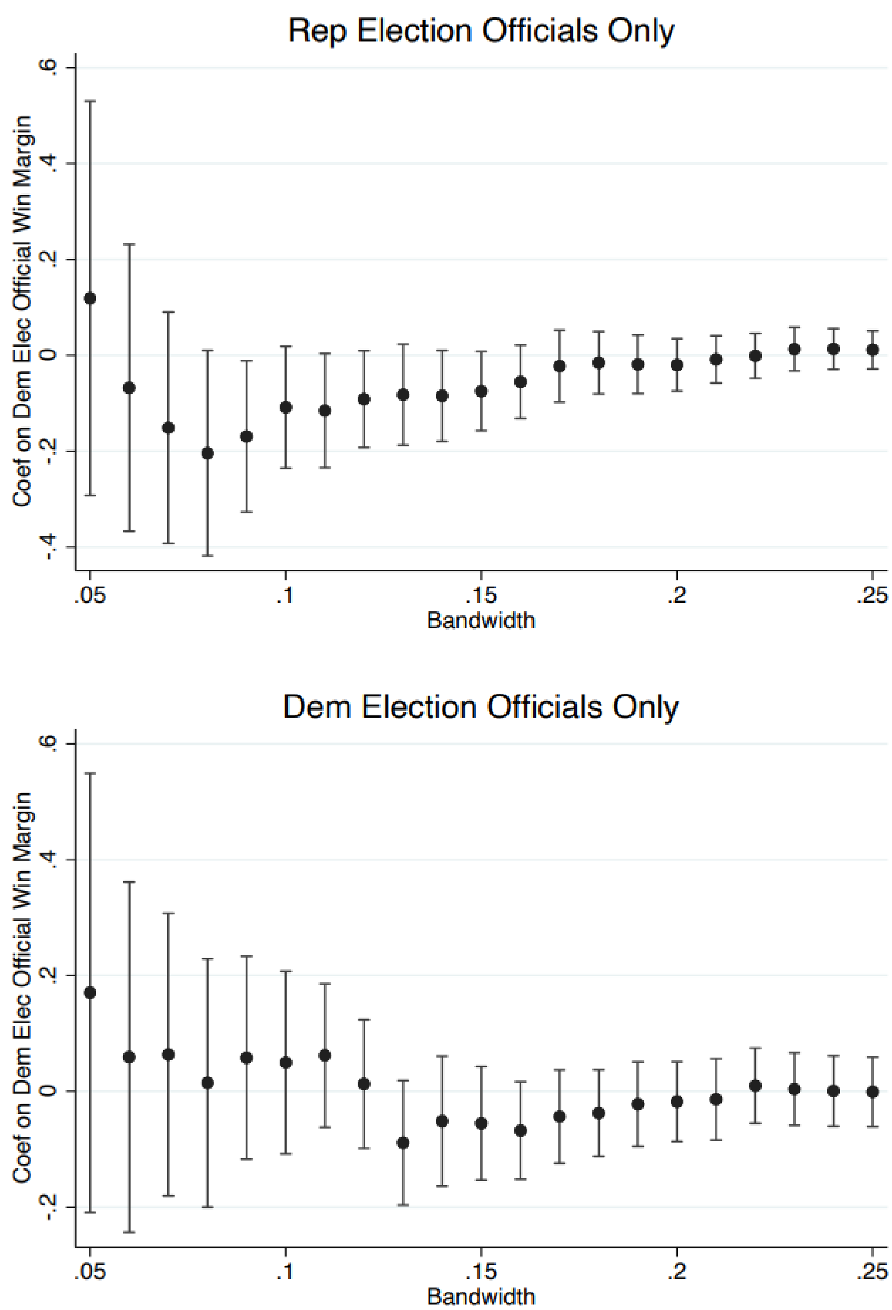
In this section, we draw heavily from Angrist and Rokkanen (2015) and Hainmueller, Hall, and Snyder Jr (2015).

If the treatment (Democratic clerk) were independent of the potential outcomes (Democratic presidential vote share under treatment and control), we could identify the average effect of the treatment without the regression discontinuity design. This would allow us to estimate the average advantage clerks give their co-partisans in elections.

As Angrist and Rokkanen (2015) point out, in regression discontinuity designs, the treatment is a deterministic function of the running variable (Democratic clerk vote share). This means that we can test the independence assumption by looking at the relationship between the potential outcomes and running variable. If the relationship is approximately flat over some region, we can interpret the difference in means in that region as the average effect for that entire region.

We follow Angrist and Rokkanen (2015) and Hainmueller, Hall, and Snyder Jr (2015), regressing residual Democratic presidential vote share on Democratic clerk vote share separately for counties in which Democratic clerks won and lost across multiple bandwidths. Figure A.5 reports the coefficient on Democratic clerk vote share across bandwidths. Across all bandwidths we investigate, even when including clerk elections won with 75% of the vote, we cannot reject a coefficient of zero. This implies that the conditional independence assumption likely holds when we study a much larger set of counties. This also means that the difference in average residual Democratic presidential vote share under Democratic and Republican clerks who win less than 75% of the total vote can be interpreted as the average causal effect of electing a Democratic clerk rather than a Republican. Similar to the results we report in Section 5.1, using all counties where the Democratic clerk won between 25% and 75% of the vote, Democrats decrease Democratic presidential vote share by 0.4 percentage points. The standard error of this estimate is 0.23 percentage points, meaning that we cannot reject the null of no effect.

Figure A.5: **Slope of Residual Dem Pres Vote Share on Dem Clerk Vote Share across Bandwidths.**



A.6.12 Democratic and Republican Clerks Administer Elections Similarly

Our results could arise if partisan clerks implement different policies that have approximately neutral effects on election outcomes. Committed partisan clerks could pursue these policies anyway if they are unaware of their ineffectiveness or if they have ideological positions about how elections ought to be administered.

Table A.15 presents estimates of the effect of electing a Democratic rather than Republican election official on outcomes more proximate to the policy choices these officials make. Across the eight columns, we present the effect of electing a Democratic rather than Republican election official on 1) the number of polling places per 1,000 residents, 2) the share of votes cast provisionally, 3) the share of provisional ballots rejected, 4) the share of absentee ballots rejected, 5) the share of voting-age residents registered, 6) the share of registrants removed from the list, 7) the share of registrants registered with the Democratic party, and 8) the share of voters in the CCES reporting a wait time longer than 30 minutes. Tables A.24 through A.31 show these results are similar across many different specifications.

In all cases except for registration rates, the effect of electing a Democrat rather than a Republican is too close to zero to rule out both groups implementing the same policies on average. We find precise evidence that electing a Democrat does not reduce removals from the voter rolls or increase the share of registrants aligned with Democrats. While not estimated very precisely, the effect on the number of polling places is especially strong evidence against the expectation that Democratic and Republican officials pursue markedly different policies given the central role of local election officials in setting the number and location of polling places. Our estimates of the effect on the number of provisionals, the share of provisionals or absentees rejected, and wait times are noisier due to much more idiosyncratic variation in the raw data. Still, we do not find evidence that electing a Democrat rather than a Republican affects these outcomes either. We do find evidence that registration rates are about 2 percentage points higher under Democratic election officials than Republican officials. However, combined with the other findings it does not seem that increased registration translates into a difference in the partisan balance of registrations, and this positive effect may have arisen by chance given the large number of policies we study.

Table A.15: **Effect of Democratic Election Officials on Policies and More Proximate Outcomes.**

	Polling Places (1)	Prov Share (2)	Prov Rejection (3)	Absentee Rejection (4)	Reg Rate (5)	Reg Removal (6)	Dem Reg Share (7)	Wait Share (8)
Dem Elec Official	-0.068	-0.000	-0.059	0.010	0.019	0.004	0.001	-0.020
	(0.087)	(0.001)	(0.060)	(0.016)	(0.009)	(0.007)	(0.006)	(0.022)
N	222	178	281	496	699	402	428	400
Clusters	165	124	190	324	410	259	247	273
Outcome Mean	0.982	0.005	0.483	0.028	0.857	0.091	0.489	0.045
Bandwidth	0.07	0.04	0.10	0.10	0.09	0.08	0.13	0.10
BW Selection	CCT	CCT	CCT	CCT	CCT	CCT	CCT	CCT
Kernel	Tri	Tri	Tri	Tri	Tri	Tri	Tri	Tri

Robust standard errors clustered by clerk election in parentheses. The outcome is first regressed on a state- and year-specific lag using all counties including those for which clerk election results are not available. The regression discontinuity is estimated using the residuals from that regression. The bandwidth row reports the number of maximum clerk win margin allowed for inclusion in each specificaiton. CCT refers to Calonico, Cattaneo, and Titiunik (2014) bandwidth selection procedure. Tri refers to a triangular kernel.

In Tables A.16 and A.17, we present additional evidence that Democrats and Republicans administer elections similarly across parties regardless of whether they serve in a majority-Democratic or majority-Republican county.

These findings also provide some evidence that countermobilization strategies pursued by party elites in response to clerk actions (Cantoni and Pons 2021) do not explain our finding of minimal partisan differences. Whereas differences in presidential vote share, turnout, and registration rates could potentially be mitigated by strategic elite mobilization strategies, it is less plausible that countermobilization could also affect the number of polling places, registration and absentee rejection rates, or registration removals.

Table A.16: **Effect of Democratic Election Officials on Policies and More Proximate Outcomes (Democrat Majority Counties Only).**

	Polling Places (1)	Prov Share (2)	Prov Rejection (3)	Absentee Rejection (4)	Reg Rate (5)	Reg Removal (6)	Dem Reg Share (7)	Wait Share (8)
Dem Elec Official	-0.190	0.001	-0.252	-0.010	0.031	0.009	-0.018	0.006
	(0.107)	(0.001)	(0.084)	(0.021)	(0.024)	(0.017)	(0.015)	(0.027)
N	77	63	98	122	150	86	112	103
Clusters	203	132	181	252	295	181	168	211
Outcome Mean	0.770	0.006	0.443	0.020	0.858	0.085	0.565	0.036
Bandwidth	0.09	0.04	0.09	0.07	0.06	0.05	0.09	0.08
BW Selection	CCT	CCT	CCT	CCT	CCT	CCT	CCT	CCT
Kernel	Tri	Tri	Tri	Tri	Tri	Tri	Tri	Tri

Robust standard errors clustered by clerk election in parentheses. The outcome is first regressed on a state- and year-specific lag using all counties including those for which clerk election results are not available. The regression discontinuity is estimated using the residuals from that regression. The bandwidth row reports the number of maximum clerk win margin allowed for inclusion in each specificaiton. CCT refers to Calonico, Cattaneo, and Titiunik (2014) bandwidth selection procedure. Tri refers to a triangular kernel. Democrat counties are those in which the Democratic clerk candidate's vote share is greater than 0.50.

Table A.17: **Effect of Democratic Election Officials on Policies and More Proximate Outcomes (Republican Majority Counties Only).**

	Polling Places (1)	Prov Share (2)	Prov Rejection (3)	Absentee Rejection (4)	Reg Rate (5)	Reg Removal (6)	Dem Reg Share (7)	Wait Share (8)
Dem Elec Official	-0.031	0.001	-0.021	0.021	0.021	-0.008	0.007	-0.012
	(0.102)	(0.001)	(0.070)	(0.020)	(0.008)	(0.007)	(0.006)	(0.026)
N	137	221	164	377	690	249	268	280
Clusters	155	233	179	342	539	229	243	273
Outcome Mean	1.044	0.005	0.496	0.031	0.856	0.092	0.455	0.048
Bandwidth	0.06	0.08	0.09	0.11	0.12	0.07	0.13	0.10
BW Selection	CCT	CCT	CCT	CCT	CCT	CCT	CCT	CCT
Kernel	Tri	Tri	Tri	Tri	Tri	Tri	Tri	Tri

Robust standard errors clustered by clerk election in parentheses. The outcome is first regressed on a state- and year-specific lag using all counties including those for which clerk election results are not available. The regression discontinuity is estimated using the residuals from that regression. The bandwidth row reports the number of maximum clerk win margin allowed for inclusion in each specificaiton. CCT refers to Calonico, Cattaneo, and Titiunik (2014) bandwidth selection procedure. Tri refers to a triangular kernel. Republican counties are those in which the Democratic clerk candidate's vote share is less than 0.50.

A.7 Studying Mechanisms

A.7.1 Clerks Do Not Advantage Their Party More in More Segregated Counties

A.7.1.1 Main Estimates of the Effect in Segregated Counties

As we discuss in Section 6, race is one of the most useful heuristics for guessing the party a citizen may vote for (Carmines and Stimson 1989; Carlson and Hill 2021; Hersh 2015). If a county is segregated by race, a local election official may have an easier time identifying areas of the county to send resources in order to increase turnout and where to curtail resources in order to reduce participation. According to this logic, we would expect clerks serving in counties in which different racial groups live in different places to have an easier time affecting election outcomes.

We measure residential racial segregation using the 2010 decennial census to compute a racial dissimilarity score across blocks within a county, following standard practice.[75] We compute the residential dissimilarity score as

$$D = \sum_{b} \left| \frac{W_b}{W} - \frac{N_b}{N} \right|$$

where D is our dissimilarity measure for a county, W_b is the number of non-Hispanic White residents in the Census block, W is the number of non-Hispanic White residents in the county, N_b is the number of Hispanic or non-White residents in the Census block, and N is the number of Hispanic or non-White residents in the county.

In Table A.18, we investigate the prediction that clerks will advantage their party more in more segregated counties. The evidence is consistent with clerks not providing an advantage to their

[75] https://www.census.gov/data/datasets/2010/dec/summary-file-1.html

party even in the most segregated counties. We further validate this finding in Figure A.6, which shows that our finding is not sensitive to the threshold we use to separate more and less diverse counties.

Table A.18: **Effect of Democratic Election Officials on Democratic Presidential Vote Share, More vs. Less Racially Segregated Counties.**

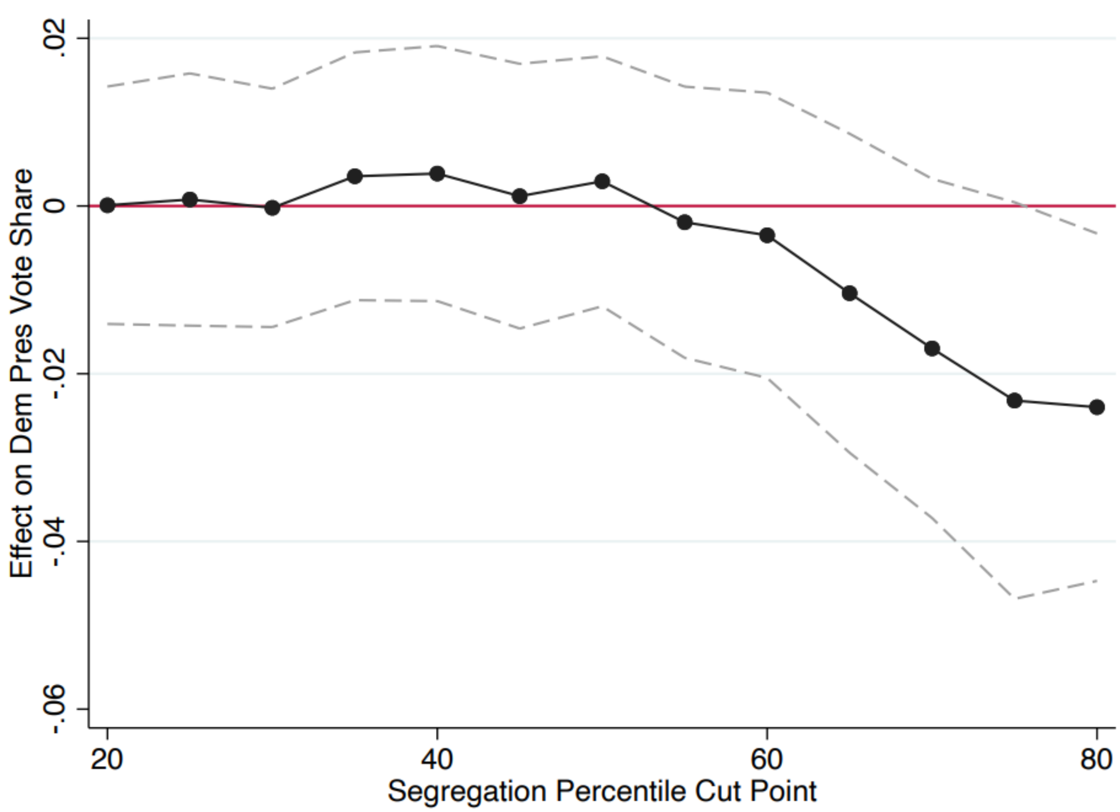
	Dem Pres Vote Share							
	Less Segregated				More Segregated			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dem Elec Official	-0.006	-0.004	-0.009	-0.005	-0.000	-0.003	0.005	0.003
	(0.010)	(0.007)	(0.013)	(0.009)	(0.008)	(0.006)	(0.011)	(0.008)
N	159	288	78	200	229	379	119	286
Bandwidth	0.06	0.12	0.03	0.08	0.11	0.21	0.05	0.14
BW Selection	CCT	CCT*2	CCT/2	CCT	CCT	CCT*2	CCT/2	CCT
Kernel	Unif	Unif	Unif	Tri	Unif	Unif	Unif	Tri

Robust standard errors in parentheses. The outcome is first regressed on a state- and year-specific lag using all counties including those for which clerk election results are not available. The regression discontinuity is estimated using the residuals from that regression. The bandwidth row reports the number of maximum clerk win margin allowed for inclusion in each specificaiton. CCT refers to Calonico, Cattaneo, and Titiunik (2014) bandwidth selection procedure. Unif means the specification uses a uniform kernel. Tri means the specification uses a triangular kernel. More segregated counties are those above the median racial racial dissimilarity index. All other counties are coded as less segregated.

A.7.1.2 Findings Not Sensitive to Definition of Segregated Counties

In Figure A.6, we demonstrate that our finding that clerks do not advantage their party even in more segregated counties holds across many thresholds for defining which counties are more or less segregated. Since segregation should make it easier for clerks to advantage their party, we would expect clerks motivated by advantaging their party to have a large effect in more segregated counties. We find instead that as we tighten our rule to throw less segregated counties out of our analysis, we estimate effects that are increasingly more negative. This is the opposite of what we would expect if election officials are seeking to advantage their party.

Figure A.6: **Effect in Segregated Counties Not Sensitive to Definition of Segregation.** The horizontal axis captures our definition of segregated counties. A value of 50 means that the county must be more segregated than 50% of counties in our sample. Each dot represents a regression discontinuity-based estimate of the effect of electing a Democratic clerk on residual Democratic presidential vote share. The lines above and below each point represent 95-percent confidence intervals. Estimates come from regressions that mimic Column 4 in Table 1 using local linear regression with a triangular kernel.



A.7.2 Clerks Do Not Advantage Their Party More in More Diverse Counties

A.7.2.1 Main Estimates of the Effect in Diverse Counties

As noted above, race is an extremely informative heuristic for party affiliation (Carmines and Stimson 1989; Carlson and Hill 2021). There is also a long history of race-based disenfranchisement in the US (Keyssar 2000), and recent scholarship has identified racial and ethnic disparities in resource and communication decisions made by local election officials (Herron and Smith 2015; Hughes et al. 2020; Merivaki and Smith 2020; Pettigrew 2017; Stuart 2004; White, Nathan, and Faller 2015) Accordingly, we might expect that clerks would have a harder time giving their party an advantage in counties where the population is overwhelmingly composed of non-Hispanic White citizens.

We investigate this prediction in Table A.19. For the purposes of the table, we define racially and ethnically diverse counties as those where non-Hispanic White residents make up less than 80% of the population. We use two census datasets to calculate county-level ethnoracial demographics: the 2000-2010 County Characteristics Intercensal Population Estimates[76] and the 7/1/2019 County Characteristics Resident Population Estimates.[77] These cover all presidential elections between 2000 and 2016. While we do find more positive point estimates in diverse counties, the evidence is consistent with clerks not providing an advantage to their party even in counties with more ethnic and racial minorities. We further validate this finding in Figure A.7, which shows that our finding is not sensitive to the threshold we use to separate more and less diverse counties.

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[76] https://www.census.gov/data/datasets/time-series/demo/popest/intercensal-2000-2010-counties.html

[77] https://www.census.gov/data/tables/time-series/demo/popest/2010s-counties-detail.html

Table A.19: **Effect of Democratic Election Officials on Democratic Presidential Vote Share, More vs. Less Racially and Ethnically Diverse Counties.**

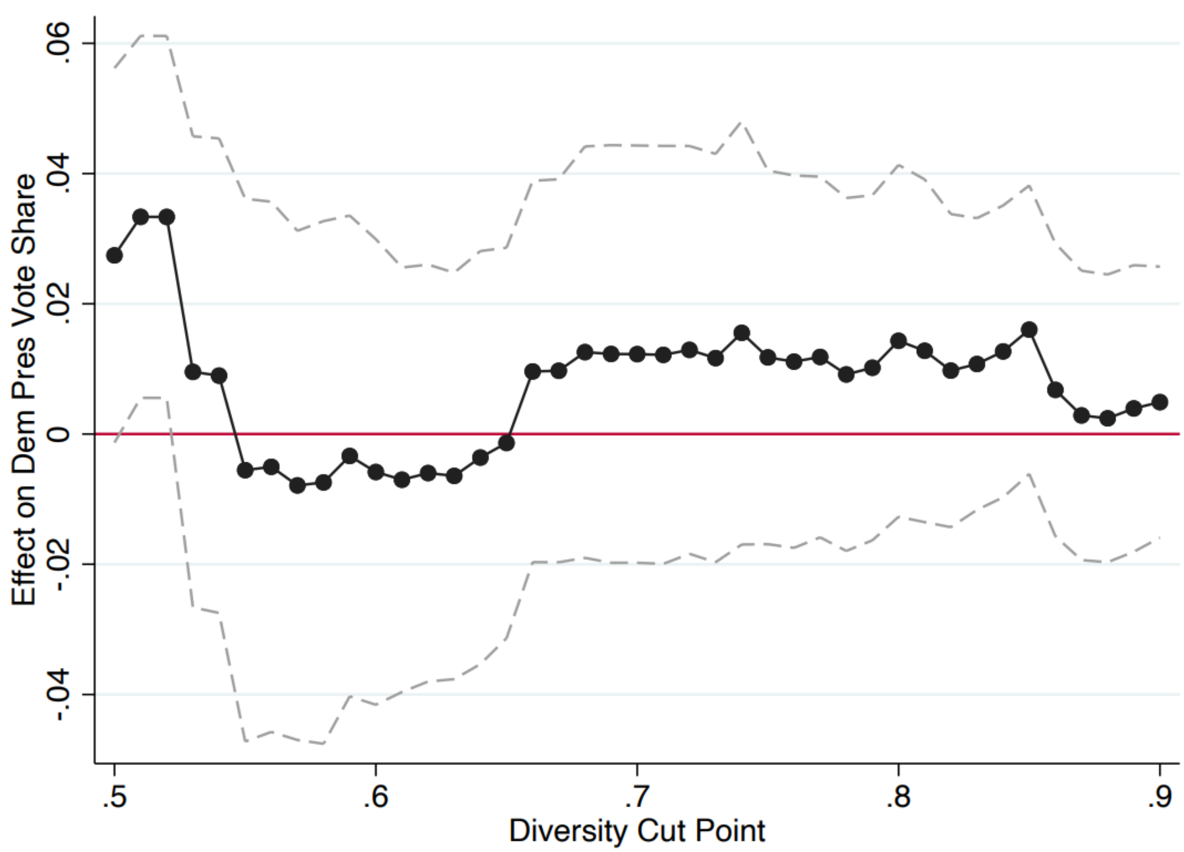
	Dem Pres Vote Share							
	Less Diverse				More Diverse			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dem Elec Official	-0.001	0.001	-0.014	-0.006	0.013	0.006	0.007	0.014
	(0.007)	(0.005)	(0.011)	(0.008)	(0.015)	(0.012)	(0.018)	(0.014)
N	282	505	145	274	83	166	43	103
Bandwidth	0.08	0.16	0.04	0.08	0.07	0.14	0.04	0.09
BW Selection	CCT	CCT*2	CCT/2	CCT	CCT	CCT*2	CCT/2	CCT
Kernel	Unif	Unif	Unif	Tri	Unif	Unif	Unif	Tri

Robust standard errors in parentheses. The outcome is first regressed on a state- and year-specific lag using all counties including those for which clerk election results are not available. The regression discontinuity is estimated using the residuals from that regression. The bandwidth row reports the number of maximum clerk win margin allowed for inclusion in each specificaiton. CCT refers to Calonico, Cattaneo, and Titiunik (2014) bandwidth selection procedure. Unif means the specification uses a uniform kernel. Tri means the specification uses a triangular kernel. More diverse counties are those where the non-Hispanic White residents make up less than 80% of the population. All other counties are coded as less diverse.

A.7.2.2 Findings Not Sensitive to Definition of Diverse Counties

In Figure A.7, we demonstrate that our finding that clerks do not advantage their party even in more diverse counties holds across many thresholds for defining which counties are more or less diverse.

Figure A.7: **Effect in Diverse Counties Not Sensitive to Definition of Diversity.** The horizontal axis captures our definition of diverse counties. Non-Hispanic White citizens must make up a smaller share than the cut point value for a county to be included in the analysis. Estimates on the left side of the figure use fewer counties but restrict the analysis to a stricter definition of diversity. Each dot represents a regression discontinuity-based estimate of the effect of electing a Democratic clerk on residual Democratic presidential vote share. The lines above and below each point represent 95-percent confidence intervals. Estimates come from regressions that mimic column 4 in Table 1 using local linear regression with a triangular kernel.



A.7.3 Estimated Effects No Larger in Balanced Districts

A.7.3.1 Effects Largest in Districts Split Between Parties if Officials Are Committed Partisans

As we discuss in 6, the effect of electing a Democratic rather than a Republican clerk should be larger in counties that are evenly balanced between the parties if the clerks are focused exclusively on advantaging their party. To see why, imagine that the only choice a clerk can make is whether or not to increase the cost of voting for the opposing party such that 20% of opposing party members fail to vote. In a county made up of 90% Democrats and 10% Republicans, a Democratic clerk motivated by partisan advantage would raise the cost of voting for Republicans, resulting in a 91.8% Democratic vote share in the election. In the same county, a Republican clerk motivated by partisan advantage would raise the cost of voting for Democrats, resulting in a 87.8% Democratic vote share in the election. This implies that the effect of electing a Democratic clerk rather than a Republican is a 4-percentage point increase to Democratic vote share in this county.

Now, consider a county made up of 50% Democrats and 50% Republicans. A Democratic clerk motivated by partisan advantage would raise the cost of voting for Republicans, resulting in a 55.6% Democratic vote share in the election. A Republican clerk motivated by partisan advantage would raise the cost of voting for Democrats, resulting in a 44.4% Democratic vote share in the election. This implies that the effect of electing a Democratic clerk rather than a Republican clerk is an 11-percentage point increase to Democratic vote share in this county, 7 percentage points larger than the effect in the Democratic-dominated county.

We generate a more general version of this prediction by studying a very simple model of a clerk's behavior. In the model, clerks can reduce the turnout of either party by a factor $1-p$ or do nothing. Here, p represents the turnout rate of the party affected by the policy and can range from 0 to 1 depending on how effective the policy is at reducing turnout. To maximize their party's vote share, Democratic clerks will always reduce Republican turnout and Republican clerks will always reduce Democratic turnout. Plugging in values of p and the share of citizens who are members of each party, we can compute the Democratic vote share under Democratic clerks as

$$DemVS = \frac{DemPopShare}{DemPopShare + RepPopShare * p}$$

and the Democratic vote share under Republican clerks as

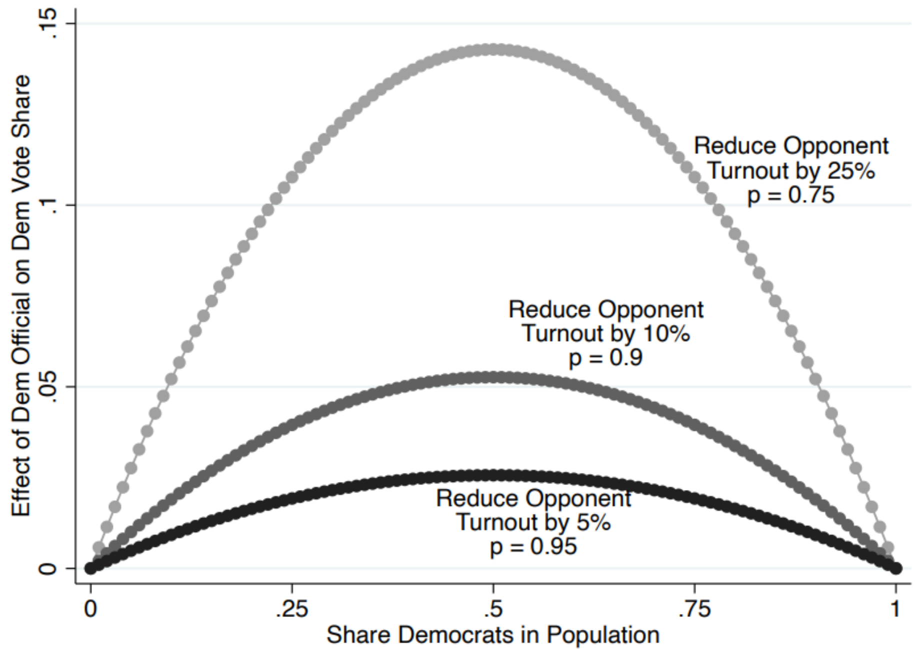
$$DemVS = \frac{DemPopShare * p}{DemPopShare * p + RepPopShare}$$

.

We can then take the difference of these two vote shares to get the effect of electing a Democratic rather than Republican clerk on Democratic vote share.

In Figure A.8 we plot how the effect on Democratic vote share changes when the district has a higher or lower proportion of Democrats in the population. We show how the effect changes for different values of p. Partisan clerks seeking to maximize their party's vote share have the biggest effect when they serve a county where 50% of residents are Democrats and 50% of residents are Republicans.

Figure A.8: **In Model of Partisan Officials Seeking to Advantage Their Party, Effect on Democratic Presidential Vote Share Largest in Balanced Counties.**



A.7.3.2 Main Estimates of the Effect in Balanced Districts

As we discussed in A.7.3.1, election officials who are solely motivated by advantaging their party will have an easier time doing so in places where the public is more evenly split between Democrats and Republicans. This allows us to make a prediction: if clerks are primarily motivated by providing their party an advantage, they will be more effective in counties that are evenly split between Democrats and Republicans.

We evaluate this prediction by estimating the effect of electing a Democratic rather than Republican election official in more and less competitive counties, with imbalanced defined as those where the Democratic presidential candidate won more than 65% or less than 35% in the previous election and all others defined as balanced. Table A.20 presents the results. We find that, despite the prediction that the effects would be larger in more competitive counties, the effects are not noticeably different. Section A.7.3.3 shows that this result is not sensitive to our chosen definition of which counties are most competitive. In summary, the simple model in which local officials are committed partisans seeking to advantage their party is inconsistent with our findings. We also find no evidence that partisan effects are larger in heavily co-partisan (imbalanced) jurisdictions, contrary to previous literature observing an effect only in such counties (Kimball, Kropf, and Battles 2006; Mohr et al. 2019; Porter and Rogowski 2018).

Table A.20: **Effect of Democratic Election Officials on Democratic Presidential Vote Share, Balanced vs. Imbalanced Counties.**

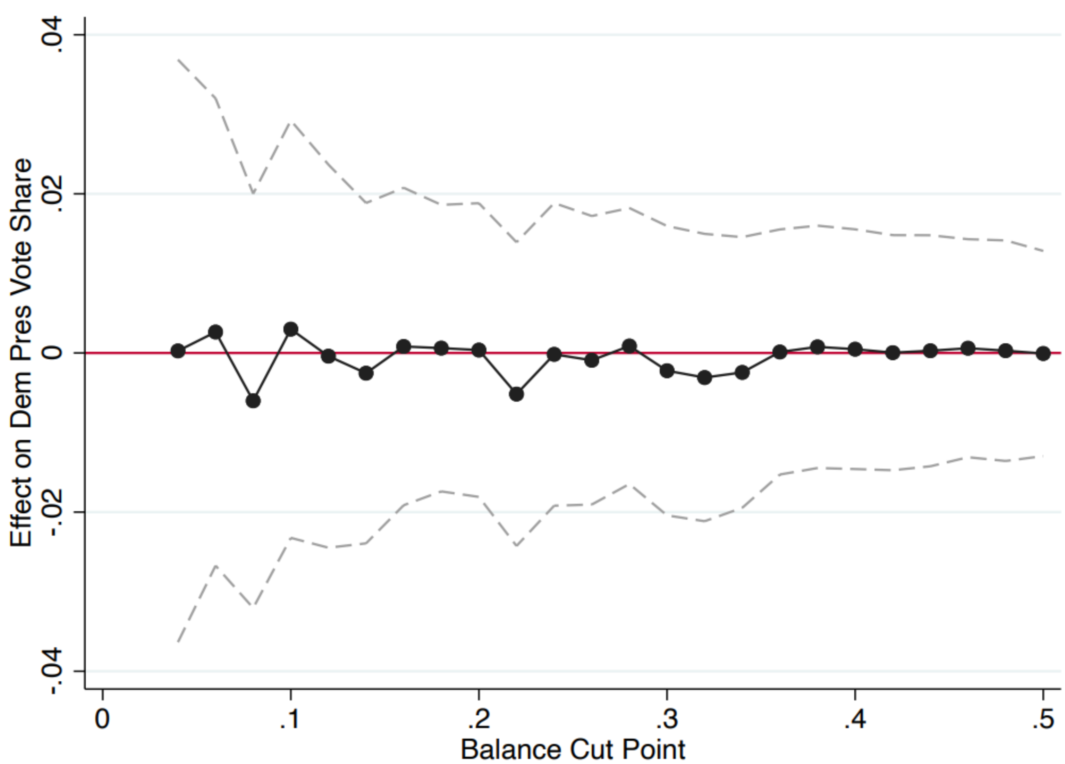
	Dem Pres Vote Share							
	Imbalanced				Balanced			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dem Elec Official	0.005	0.002	-0.012	-0.004	0.000	0.004	0.001	-0.002
	(0.011)	(0.008)	(0.017)	(0.010)	(0.008)	(0.007)	(0.012)	(0.009)
N	94	184	46	135	233	409	118	235
Bandwidth	0.07	0.13	0.03	0.10	0.07	0.13	0.03	0.07
BW Selection	CCT	CCT*2	CCT/2	CCT	CCT	CCT*2	CCT/2	CCT
Kernel	Unif	Unif	Unif	Tri	Unif	Unif	Unif	Tri

Robust standard errors in parentheses. The outcome is first regressed on a state- and year-specific lag using all counties including those for which clerk election results are not available. The regression discontinuity is estimated using the residuals from that regression. The bandwidth row reports the number of maximum clerk win margin allowed for inclusion in each specificaiton. CCT refers to Calonico, Cattaneo, and Titiunik (2014) bandwidth selection procedure. Unif means the specification uses a uniform kernel. Tri means the specification uses a triangular kernel. Imbalanced counties are those where the Democratic presidential candidate won more the 65% or less than 35% in the previous election. All other counties are coded as balanced.

A.7.3.3 Findings Not Sensitive to Definition of Balanced Counties

In Figure A.9, we demonstrate that our finding that clerks do not advantage their party even in more competitive counties holds across many definitions of competitiveness. While we estimate the most positive point estimates in the most competitive states, suggesting that clerks advantage their party more in very competitive states, the estimates are still relatively small (less than one percentage point). The confidence intervals we estimate include zero regardless of the threshold used for defining competitive states.

Figure A.9: **Effect in Balanced Counties Not Sensitive to Definition of Partisan Balance.** The horizontal axis captures our definition of balanced counties. The win margin in the last Democratic presidential election must be smaller than the cut point value for a county to be included in the analysis. Estimates on the left side of the figure use fewer counties but restrict the analysis to a stricter definition of balance. Each dot represents a regression discontinuity-based estimate of the effect of electing a Democratic clerk on residual Democratic presidential vote share. The lines above and below each point represent 95-percent confidence intervals. Estimates come from regressions that mimic column 4 in Table 1 using local linear regression with a triangular kernel.



A.7.4 Clerks Do Not Advantage Their Party More in Larger Counties

A.7.4.1 Main Estimates of the Effect in Large-Population Counties

Election officials who want to advantage their party may have an easier time if they have the resources and staff to carry out their plans. We expect larger counties to have more of these resources (Kimball and Baybeck 2013). Previous literature has also found clerks to diverge along party lines in their support for voter access and security policies only in large jurisdictions (Kimball and Baybeck 2010, 2013). In Table A.21, we investigate the prediction that clerks will advantage their party more in larger counties, defining large counties as those with more than 100,000 residents. We use two census datasets to calculate county-level population: the 2000-2010 County Characteristics Intercensal Population Estimates[78] and the 7/1/2019 County Characteristics Resident Population Estimates.[79] We extrapolate population figures to 2020 using linear regression. Despite the prediction that the effects will be largest in counties with larger populations, we find that the effects are similar in large and small counties.

[78] https://www.census.gov/data/datasets/time-series/demo/popest/intercensal-2000-2010-counties.html

[79] https://www.census.gov/data/tables/time-series/demo/popest/2010s-counties-detail.html

Table A.21: **Effect of Democratic Election Officials on Democratic Presidential Vote Share, Small vs. Large Counties.**

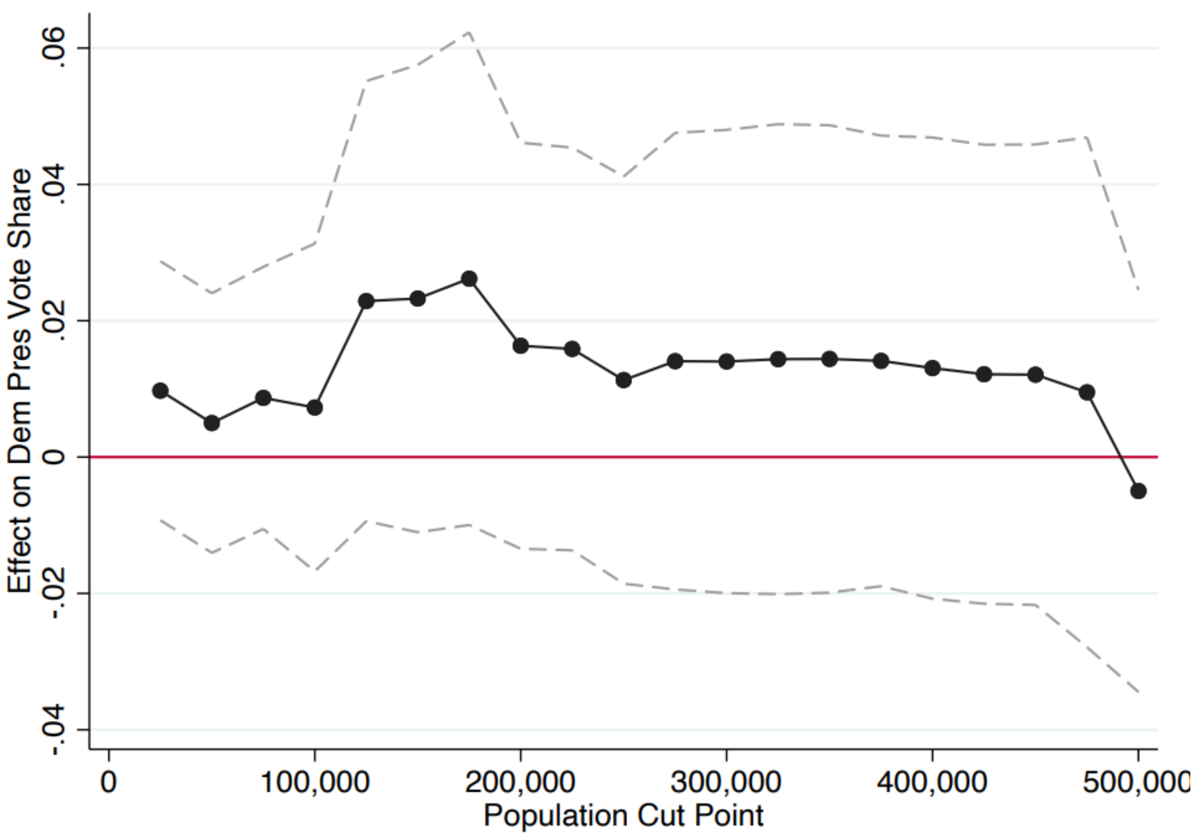
	Dem Pres Vote Share							
	Pop < 100k				Pop ≥ 100k			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dem Elec Official	-0.004	-0.001	-0.009	-0.004	0.011	-0.003	0.003	0.007
	(0.006)	(0.005)	(0.009)	(0.008)	(0.012)	(0.009)	(0.014)	(0.012)
N	341	580	181	292	82	149	40	95
Bandwidth	0.10	0.21	0.05	0.09	0.06	0.12	0.03	0.07
BW Selection	CCT	CCT*2	CCT/2	CCT	CCT	CCT*2	CCT/2	CCT
Kernel	Unif	Unif	Unif	Tri	Unif	Unif	Unif	Tri

Robust standard errors in parentheses. The outcome is first regressed on a state- and year-specific lag using all counties including those for which clerk election results are not available. The regression discontinuity is estimated using the residuals from that regression. The bandwidth row reports the number of maximum clerk win margin allowed for inclusion in each specificaiton. CCT refers to Calonico, Cattaneo, and Titiunik (2014) bandwidth selection procedure. Unif means the specification uses a uniform kernel. Tri means the specification uses a triangular kernel.

A.7.4.2 Findings Not Sensitive to Definition of Large-Population Counties

In Figure A.10, we demonstrate that our finding that clerks do not advantage their party even in large-population counties holds across many thresholds for defining what counts as a large-population county. While we generally estimate the most positive point estimates in more populous counties, suggesting that clerks advantage their party in heavily populated counties, the estimates are still relatively small (less than 1.5 percentage points). The confidence intervals we estimate include zero regardless of the threshold we use for defining large-population.

Figure A.10: **Effect in Large-Population Counties Not Sensitive to Population Threshold for Inclusion.** The horizontal axis captures our population threshold for including a county in the large-population analysis. Each dot represents a regression discontinuity-based estimate of the effect of electing a Democratic clerk on residual Democratic presidential vote share. The lines above and below each point represent 95-percent confidence intervals. Estimates come from regressions that mimic column 4 in Table 1 using local linear regression with a triangular kernel.



A.7.5 Clerks Do Not Advantage Their Party More in More Competitive States

A.7.5.1 Main Estimates of the Effect in Competitive States

Election officials may feel more motivated to advantage their party in more competitive states. In Table A.22, we investigate the prediction that clerks will advantage their party more in more competitive states, defining competitive states as those in which the Democratic or Republican presidential candidate won by less than five percentage points in the previous election. The evidence in consistent with clerks not providing an advantage to their party regardless of whether the clerk serves in a more or less competitive state.

Table A.22: **Effect of Democratic Election Officials on Democratic Presidential Vote Share, More vs. Less Competitive States.**

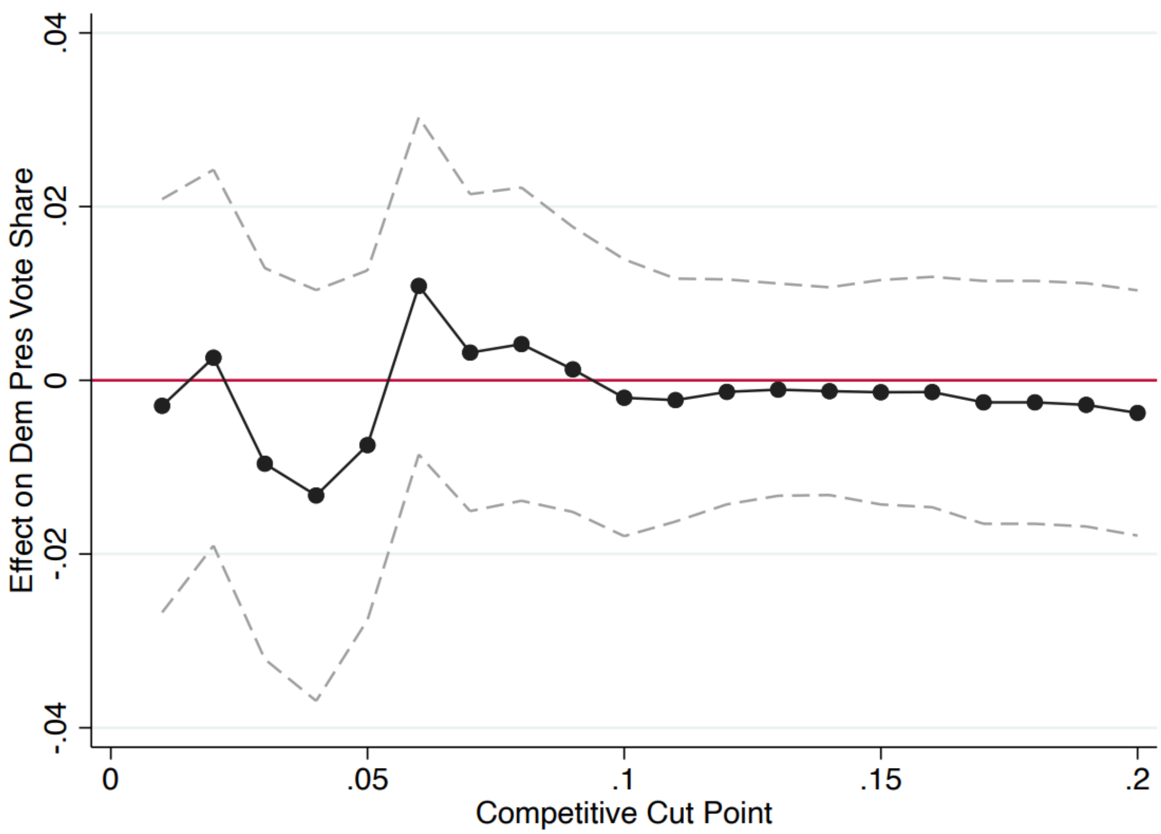
	Dem Pres Vote Share							
	Less Competitive				More Competitive			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dem Elec Official	0.001	-0.001	0.001	0.001	0.006	0.004	-0.006	-0.007
	(0.009)	(0.007)	(0.013)	(0.009)	(0.010)	(0.007)	(0.011)	(0.010)
N	237	432	118	263	143	240	76	101
Bandwidth	0.07	0.14	0.03	0.08	0.11	0.21	0.05	0.07
BW Selection	CCT	CCT*2	CCT/2	CCT	CCT	CCT*2	CCT/2	CCT
Kernel	Unif	Unif	Unif	Tri	Unif	Unif	Unif	Tri

Robust standard errors in parentheses. The outcome is first regressed on a state- and year-specific lag using all counties including those for which clerk election results are not available. The regression discontinuity is estimated using the residuals from that regression. The bandwidth row reports the number of maximum clerk win margin allowed for inclusion in each specificaiton. CCT refers to Calonico, Cattaneo, and Titiunik (2014) bandwidth selection procedure. Unif means the specification uses a uniform kernel. Tri means the specification uses a triangular kernel. More competitve states are those in which the last presidential election was decided by less than five percentage points.

A.7.5.2 Findings Not Sensitive to Definition of More Competitive States

In Figure A.11, we demonstrate that our finding that clerks do not advantage their party even in competitive states holds across many thresholds for defining what counts as a competitive state. The confidence intervals we estimate include zero regardless of the threshold we use for defining competitive states.

Figure A.11: **Effect in Competitive States Not Sensitive to Threshold for Inclusion.** The horizontal axis captures our threshold for counting a state as competitive. Each dot represents a regression discontinuity-based estimate of the effect of electing a Democratic clerk on residual Democratic presidential vote share. The lines above and below each point represent 95-percent confidence intervals. Estimates come from regressions that mimic column 4 in Table 1 using local linear regression with a triangular kernel.



A.7.6 Clerks Do Not Advantage Their Party More in Determinative Counties

A.7.6.1 Main Estimates of the Effect in Determinative Counties

Election officials may feel more motivated to advantage their party when their county makes up a larger share of the win margin in their state. In Table A.23, we investigate the prediction that clerks will advantage their party more in more determinative counties, defining determinative counties as those in which the Democratic or Republican presidential candidate won by less than two times the population of the county in the most recent election. While point estimates are generally more positive in determinative counties, we find that Democratic and Republican clerks oversee similar elections regardless of whether the clerk serves in a determinative county or not.

Table A.23: **Effect of Democratic Election Officials on Democratic Presidential Vote Share, Determinative vs. Not Determinative Counties.**

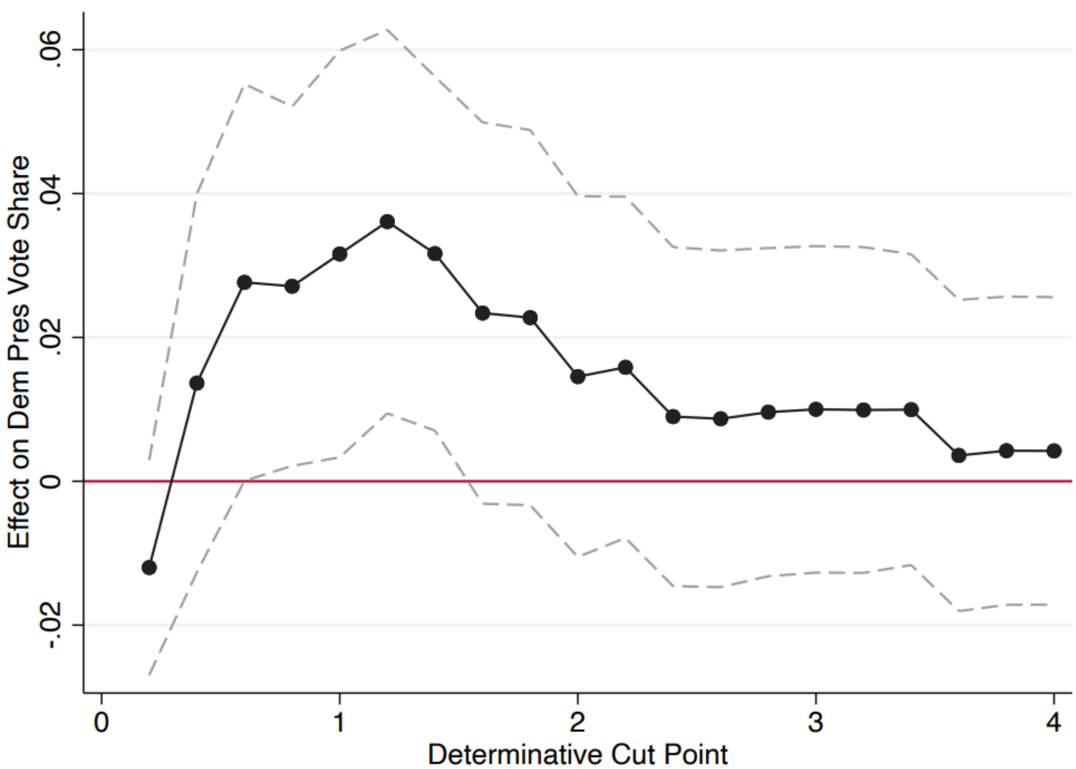
	Dem Pres Vote Share							
	Not Determinative				Determinative			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dem Elec Official	-0.005	-0.001	-0.012	-0.005	0.019	0.004	0.012	0.015
	(0.007)	(0.005)	(0.010)	(0.007)	(0.014)	(0.011)	(0.018)	(0.013)
N	311	531	162	366	72	142	39	95
Bandwidth	0.09	0.19	0.05	0.11	0.05	0.11	0.03	0.07
BW Selection	CCT	CCT*2	CCT/2	CCT	CCT	CCT*2	CCT/2	CCT
Kernel	Unif	Unif	Unif	Tri	Unif	Unif	Unif	Tri

Robust standard errors in parentheses. The outcome is first regressed on a state- and year-specific lag using all counties including those for which clerk election results are not available. The regression discontinuity is estimated using the residuals from that regression. The bandwidth row reports the number of maximum clerk win margin allowed for inclusion in each specificaiton. CCT refers to Calonico, Cattaneo, and Titiunik (2014) bandwidth selection procedure. Unif means the specification uses a uniform kernel. Tri means the specification uses a triangular kernel. Determinative counties are those that have more people than half of the margin in the last presidential election in that state. All other counties are coded as not determinative.

A.7.6.2 Findings Not Sensitive to Definition of Determinative Counties

In Figure A.12, we demonstrate that our finding that clerks do not advantage their party even in determinative counties holds across many thresholds for defining what counts as a determinative county. Our threshold is defined by how many counties of this size would have to swing entirely from one candidate to the other to make up the margin in the state's previous presidential election. On the left side of the plot, only the counties with the largest effects on statewide election outcomes are included. While the point estimates go up and down, we read this as consistent with our other findings that election officials are not dramatically advantaging their party even when it matters most.

Figure A.12: **Effect in Determinative Counties Not Sensitive to Threshold for Inclusion.** The horizontal axis captures our threshold for counting a county as determinative. Each dot represents a regression discontinuity-based estimate of the effect of electing a Democratic clerk on residual Democratic presidential vote share. The lines above and below each point represent 95-percent confidence intervals. Estimates come from regressions that mimic column 4 in Table 1 using local linear regression with a triangular kernel.



A.7.7 Effect of Electing a Democratic Clerk on All Policy Outcomes Collected

In Table A.15 in Section A.6.12, we present evidence that Democratic and Republican election officials implement similar policies when serving in similar counties. Here, we share the full results for each of the policy outcomes. Five indicators use the US Election Assistance Commission's 2014, 2016, 2018, and 2020 Election Administration and Voting Survey (EAVS): the number of polling places, provisional ballots cast, provisional ballots rejected, absentee ballots rejected, and the number of registrants removed from the voter roll.[80] Two indicators use Dave Leip's Election Atlas: the number of registered voters in each county and the share of registered voters listed as members of the Democratic party.[81] One indicator uses the 2006, 2008, 2012, 2014, 2016, and 2018 CCES survey: the share of voters who had to wait at the polls for more than 30 minutes.[82]

We find the same pattern as presented in the main text across all eight policy outcomes. We also run specifications measuring wait times as the share of voters in the CCES reporting a wait time longer than 10 minutes. We find similar results to the 30 minute or longer measure used in the main analysis. Additionally, we report results testing a measure of voter wait times derived from phone location data calculated by Chen et al. (2020). These are only available for the 2016 election, but include county-level measures of both average wait times and racial disparity in wait times. The results are reported below. The results are substantively the same to those reported in Table A.15.

[80] https://www.eac.gov/research-and-data/datasets-codebooks-and-surveys

[81] https://uselectionatlas.org/

[82] https://cces.gov.harvard.edu/data

Table A.24: **Effect of Democratic Election Officials on Polling Places.**

	Polling Places per 1k			
	(1)	(2)	(3)	(4)
Dem Elec Official	-0.041	0.025	-0.100	-0.068
	(0.085)	(0.071)	(0.098)	(0.087)
N	242	422	122	222
Clusters	180	314	94	165
Bandwidth	0.08	0.15	0.04	0.07
Bandwidth Selection	CCT	CCT*2	CCT/2	CCT
Kernel	Uniform	Uniform	Uniform	Triangular

Robust standard errors clustered by clerk election in parentheses. The outcome is first regressed on a state- and year-specific lag using all counties including those for which clerk election results are not available. The regression discontinuity is estimated using the residuals from that regression. The bandwidth row reports the number of maximum clerk win margin allowed for inclusion in each specificaiton. CCT refers to Calonico, Cattaneo, and Titiunik (2014) bandwidth selection procedure.

Table A.25: **Effect of Democratic Election Officials on Provisional Share.**

	Provisional Share of Ballots			
	(1)	(2)	(3)	(4)
Dem Elec Official	0.002	0.000	-0.000	-0.000
	(0.002)	(0.001)	(0.001)	(0.001)
N	194	353	102	178
Clusters	136	243	74	124
Bandwidth	0.04	0.08	0.02	0.04
Bandwidth Selection	CCT	CCT*2	CCT/2	CCT
Kernel	Uniform	Uniform	Uniform	Triangular

Robust standard errors clustered by clerk election in parentheses. The outcome is first regressed on a state- and year-specific lag using all counties including those for which clerk election results are not available. The regression discontinuity is estimated using the residuals from that regression. The bandwidth row reports the number of maximum clerk win margin allowed for inclusion in each specificaiton. CCT refers to Calonico, Cattaneo, and Titiunik (2014) bandwidth selection procedure.

Table A.26: **Effect of Democratic Election Officials on Provisional Rejection Rate.**

	Provisionals Rejection Rate			
	(1)	(2)	(3)	(4)
Dem Elec Official	-0.070	-0.059	0.009	-0.059
	(0.061)	(0.043)	(0.084)	(0.060)
N	236	412	127	281
Clusters	162	277	88	190
Bandwidth	0.08	0.15	0.04	0.10
Bandwidth Selection	CCT	CCT*2	CCT/2	CCT
Kernel	Uniform	Uniform	Uniform	Triangular

Robust standard errors clustered by clerk election in parentheses. The outcome is first regressed on a state- and year-specific lag using all counties including those for which clerk election results are not available. The regression discontinuity is estimated using the residuals from that regression. The bandwidth row reports the number of maximum clerk win margin allowed for inclusion in each specificaiton. CCT refers to Calonico, Cattaneo, and Titiunik (2014) bandwidth selection procedure.

Table A.27: **Effect of Democratic Election Officials on Absentee Rejection Rate.**

	Absentee Rejection Rate			
	(1)	(2)	(3)	(4)
Dem Elec Official	0.003	0.011	0.013	0.010
	(0.017)	(0.013)	(0.020)	(0.016)
N	370	639	188	496
Clusters	242	418	126	324
Bandwidth	0.07	0.14	0.03	0.10
Bandwidth Selection	CCT	CCT*2	CCT/2	CCT
Kernel	Uniform	Uniform	Uniform	Triangular

Robust standard errors clustered by clerk election in parentheses. The outcome is first regressed on a state- and year-specific lag using all counties including those for which clerk election results are not available. The regression discontinuity is estimated using the residuals from that regression. The bandwidth row reports the number of maximum clerk win margin allowed for inclusion in each specificaiton. CCT refers to Calonico, Cattaneo, and Titiunik (2014) bandwidth selection procedure.

Table A.28: **Effect of Democratic Election Officials on Registration.**

	Registered Voters per VAP			
	(1)	(2)	(3)	(4)
Dem Elec Official	0.013	0.018	0.022	0.019
	(0.009)	(0.007)	(0.011)	(0.009)
N	649	1174	330	699
Clusters	380	688	194	410
Bandwidth	0.08	0.16	0.04	0.09
Bandwidth Selection	CCT	CCT*2	CCT/2	CCT
Kernel	Uniform	Uniform	Uniform	Triangular

Robust standard errors clustered by clerk election in parentheses. The outcome is first regressed on a state- and year-specific lag using all counties including those for which clerk election results are not available. The regression discontinuity is estimated using the residuals from that regression. The bandwidth row reports the number of maximum clerk win margin allowed for inclusion in each specificaiton. CCT refers to Calonico, Cattaneo, and Titiunik (2014) bandwidth selection procedure.

Table A.29: **Effect of Democratic Election Officials on Registration Removals.**

	Registrations Removed / Total Registrants			
	(1)	(2)	(3)	(4)
Dem Elec Official	0.005	0.002	0.002	0.004
	(0.008)	(0.006)	(0.011)	(0.007)
N	325	558	161	402
Clusters	207	358	105	259
Bandwidth	0.06	0.12	0.03	0.08
Bandwidth Selection	CCT	CCT*2	CCT/2	CCT
Kernel	Uniform	Uniform	Uniform	Triangular

Robust standard errors clustered by clerk election in parentheses. The outcome is first regressed on a state- and year-specific lag using all counties including those for which clerk election results are not available. The regression discontinuity is estimated using the residuals from that regression. The bandwidth row reports the number of maximum clerk win margin allowed for inclusion in each specificaiton. CCT refers to Calonico, Cattaneo, and Titiunik (2014) bandwidth selection procedure.

Table A.30: **Effect of Democratic Election Officials on Democratic Registration Share.**

	Dem Reg Share			
	(1)	(2)	(3)	(4)
Dem Elec Official	0.001	-0.000	-0.001	0.001
	(0.006)	(0.005)	(0.008)	(0.006)
N	367	663	199	428
Clusters	213	384	116	247
Bandwidth	0.11	0.22	0.06	0.13
Bandwidth Selection	CCT	CCT*2	CCT/2	CCT
Kernel	Uniform	Uniform	Uniform	Triangular

Robust standard errors clustered by clerk election in parentheses. The outcome is first regressed on a state- and year-specific lag using all counties including those for which clerk election results are not available. The regression discontinuity is estimated using the residuals from that regression. The bandwidth row reports the number of maximum clerk win margin allowed for inclusion in each specificaiton. CCT refers to Calonico, Cattaneo, and Titiunik (2014) bandwidth selection procedure.

Table A.31: **Effect of Democratic Election Officials on Wait Times.**

	Share Over 30 min Wait			
	(1)	(2)	(3)	(4)
Dem Elec Official	-0.013	-0.036	-0.043	-0.020
	(0.024)	(0.018)	(0.030)	(0.022)
N	289	515	143	400
Clusters	195	358	93	273
Bandwidth	0.07	0.14	0.03	0.10
Bandwidth Selection	CCT	CCT*2	CCT/2	CCT
Kernel	Uniform	Uniform	Uniform	Triangular

Robust standard errors clustered by clerk election in parentheses. The outcome is first regressed on a state- and year-specific lag of turnout using all counties including those for which clerk election results are not available. The regression discontinuity is estimated using the residuals from that regression. The bandwidth row reports the number of maximum clerk win margin allowed for inclusion in each specificaiton. CCT refers to Calonico, Cattaneo, and Titiunik (2014) bandwidth selection procedure.

Table A.32: **Effect of Democratic Election Officials on Wait Times.**

	Share Over 10 min Wait			
	(1)	(2)	(3)	(4)
Dem Elec Official	-0.029	-0.040	0.007	-0.022
	(0.055)	(0.038)	(0.075)	(0.048)
N	297	537	151	449
Clusters	201	372	98	309
Bandwidth	0.07	0.14	0.04	0.11
Bandwidth Selection	CCT	CCT*2	CCT/2	CCT
Kernel	Uniform	Uniform	Uniform	Triangular

Robust standard errors clustered by clerk election in parentheses. The outcome is first regressed on a state- and year-specific lag of turnout using all counties including those for which clerk election results are not available. The regression discontinuity is estimated using the residuals from that regression. The bandwidth row reports the number of maximum clerk win margin allowed for inclusion in each specificaiton. CCT refers to Calonico, Cattaneo, and Titiunik (2014) bandwidth selection procedure.

Table A.33: **Effect of Democratic Election Officials on Phone Location-Based Wait Times.**

	Average Wait Time			
	(1)	(2)	(3)	(4)
Dem Elec Official	-1.476	0.372	-2.250	-1.779
	(3.656)	(2.664)	(4.949)	(3.983)
N	30	46	19	31
Bandwidth	0.08	0.16	0.04	0.09
Bandwidth Selection	CCT	CCT*2	CCT/2	CCT
Kernel	Uniform	Uniform	Uniform	Triangular

Robust standard errors in parentheses. The outcome is first regressed on a state- and year-specific lag of turnout using all counties including those for which clerk election results are not available. The regression discontinuity is estimated using the residuals from that regression. The bandwidth row reports the number of maximum clerk win margin allowed for inclusion in each specificaiton. CCT refers to Calonico, Cattaneo, and Titiunik (2014) bandwidth selection procedure.

Table A.34: **Effect of Democratic Election Officials on Racial Disparities in Phone Location-Based Wait Times.**

	Average Wait Time Disparity			
	(1)	(2)	(3)	(4)
Dem Elec Official	-0.436	4.164	-8.126	-2.980
	(21.388)	(14.540)	(24.657)	(21.420)
N	31	48	20	34
Clusters	31	48	20	34
Bandwidth	0.08	0.17	0.04	0.10
Bandwidth Selection	CCT	CCT*2	CCT/2	CCT
Kernel	Uniform	Uniform	Uniform	Triangular

Robust standard errors clustered by clerk election in parentheses. The outcome is first regressed on a state- and year-specific lag of turnout using all counties including those for which clerk election results are not available. The regression discontinuity is estimated using the residuals from that regression. The bandwidth row reports the number of maximum clerk win margin allowed for inclusion in each specificaiton. CCT refers to Calonico, Cattaneo, and Titiunik (2014) bandwidth selection procedure.

Chapter 4 Appendix References

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Chapter 5: Does Leader Turnover Degrade Local Government Performance? Evidence from Local Election Officials[83]

1 Introduction

A growing chorus of public officials, scholars, and journalists have sounded the alarm over high turnover of local government officials.[84] The main reason many are concerned about public official turnover is the fear that it takes time to learn how to lead in a new role and offices will perform poorly while new leaders get up to speed. In a recent interview about administrative errors in Pennsylvania's local election offices, Secretary of the Commonwealth Al Schmidt captures these concerns, saying "[Human errors] occur most frequently, overwhelmingly, when you have new election administrators" (Walker 2023). These concerns are also in line with empirical work finding that governors produce better outcomes, presidents gain better control of the bureaucracy, and legislators become more productive over their time in office (Alt, Bueno de Mesquita, and Rose 2011; Krause and O'Connell 2016; Volden and Wiseman 2014; Fouirnaies and Hall 2022).[85] Voters also prefer candidates with experience in office, suggesting there is some value in gaining experience in office (Erikson and Titiunik 2015; Fowler and Hall 2014).

Other public officials and scholars see turnover as an indicator of a healthy institution. As former Fairfax County Registrar Cameron Quinn shared in a recent interview, "You can have officials who are there too long and aren't really up to doing the job" (CBS News 2024). This claim is consistent with findings that

[83] Material from this chapter was co-authored with Dan Thompson.

[84] See, for example, reports of turnover of police chiefs (Bennett 2024), school superintendents (Lambert 2023), and the local officials responsible for running elections (The Boston Globe 2022).

[85] One noteworthy exception to this empirical pattern comes from US mayors and city managers where local government experience is not associated with greater managerial effectiveness (Carreri and Payson 2024).

government performance improves when incumbents lose power at both the national and local level (Marx, Pons, and Rollet 2022; Bazzi et al. 2025) and the concern that low turnover implies that politicians are not being removed for poor performance (Ebanks, Katz, and King 2023).

Still, turnover may not meaningfully affect performance if the process of selecting local government officials lands on replacements with the skills to keep the office running smoothly. Perhaps because of the way the election and appointment processes work in local governments, people who take over local government executive positions like clerk, police chief, sheriff, and school superintendent often have experience as deputies in the office or in the same field prior to taking the role (Bjork and Kowalski 2005; Johnson 2005; Ferrer, Geyn, and Thompson 2023; Thompson 2020). If this selection is strong enough to result in new leaders already equipped to run the office, turnover may have little to no effect on performance. Turnover may also have limited effects on performance if staff other than the leader do not need high-quality leadership to perform well. In most bureaucratic settings, lower-level officials are responsible for many of the important decisions that affect performance, and this may mute the effect leaders have on behavior (Brehm and Gates 1999; Lipsky 1980; Wilson 1968; but see Mummolo 2018).

Do local governments perform worse immediately after a leadership transition on average? In this paper, we present findings from a new dataset on election official turnover. Our new data on chief local election officials is the largest collected to date, spanning more jurisdictions and a longer timespan than any previous effort. In total, our data encompasses 18,644 unique elected and appointed chief election officials across all 50 states, 6,290 election jurisdictions, and 13 election cycles between 2000 and 2024, yielding 81,000 jurisdiction-year observations of turnover. We pair this dataset with data on voter turnout, residual vote, and potential reporting errors at the county and municipal level back to 2004. We focus our analyses on these outcomes because they are the most important indicators of voter experience—direct surveys tell us if voters feel the process of voting is worse after turnover while turnout and residual vote tell us if issues in the voter experience are significant enough to shape participation. Panel data allows us to credibly estimate the effect of turnover on election performance using a variety of difference-in-differences and panel matching analyses.

Despite widespread concern that turnover will degrade government performance, we find consistent evidence that performance is similar following a leadership transition. Among officials with authority to administer nearly all aspects of elections in their jurisdiction, we estimate that turnout does not increase or decrease by more than 0.10 percentage points, or 100 votes in a jurisdiction of 100,000 eligible voters. The 95% confidence interval from our least precise estimator implies that the effect of turnover on turnout is likely between -0.36 percentage points and 0.16 percentage points, and we have 80% power to detect effects as small as 0.38 percentage points. We find similar patterns of results when we estimate the effect of election official turnover on the self-reported rate of problems residents face when voting, confidence that the election was administered properly, and the residual vote, a widely used measure of election administration issues (Kropf et al. 2020; Stewart et al. 2020), though we find suggestive evidence for a small increase in wait times at the polls after turnover. We estimate nearly identical effects of turnover in election offices with more and less authority, when the departing official had more or less experience, for officials departing voluntarily and involuntarily, in large and small jurisdictions, across midterm and presidential years, and in a time of dramatic change and uncertainty like 2020.

As we discussed above, one explanation for these findings is that incoming officials have already developed sufficient experience before entering local leadership positions. We evaluate the plausibility of this explanation by searching for public information on the professional backgrounds of incoming officials. We find that the vast majority of new local election officials have professional experience in the field or a related role prior to taking over the office. While we find that officials without prior professional experience also oversee counties with similar performance, we argue that the substantial experience of incoming officials is consistent with selection based on preparation for the office limiting the harmful effects of transitions.

Another explanation for our findings is that lower-level staff are able to function just as well with good and bad leadership. We investigate this possibility by testing whether voter turnout is meaningfully different under different leaders in the same jurisdiction. We find that leaders in the same jurisdiction oversee similar turnout rates, suggesting either election official leadership has only small effects on performance or the leader selection process constrains the range of performance. Ultimately, we conclude that, while these are both plausible explanations that are consistent with the data and we cannot definitively say that leaders are important, the strong positive selection of local election officials suggests selection may play a role in muting the effects of turnover.

It is important to note that, while we can rule out turnover systematically producing a substantial number of mistakes that degrade election performance on average, turnover may still increase the probability of rare but important administrative errors. We cannot observe minuscule increases in the probability of such an event, but events like those are still important negative outcomes that any full accounting of turnover should consider.

Beyond the main focus of this paper on turnover in local leadership, this paper also contributes to broader research on local election officials. A growing body of research studies how election official institutions (Burden et al. 2013; Ferrer 2024*b*), managerial capacity (Kropf et al. 2020), communication (Suttman-Lea and Merivaki 2022, 2023), race and ethnicity (Ferrer 2024*a*), funding (Lal and Thompson 2024; Mohr et al. 2019), party (Ferrer, Geyn, and Thompson 2023; Kimball, Kropf, and Battles 2006; Porter and Rogowski 2018; White, Nathan, and Faller 2015), and implementation of state law (Atkeson et al. 2010; Bassi, Morton, and Trounstine 2009) contribute to election performance and trust at the local level. Our new findings in this paper suggest that there is not a strong relationship between tenure length and election administration quality.

The paper proceeds as follows. We discuss our reasoning about how turnover may affect performance in Section 2. We then describe our new data on election official turnover in Section 3 and document how turnover has changed over time. In Section 4, we estimate the effect of election official turnover on voter participation and other performance measures and validate our estimates. We evaluate explanations for our findings in Section 5 and discuss their implications in Section 6.

2 Turnover and Local Government Performance

How should we expect turnover to affect local government performance? Across a wide variety of domains, public officials become more effective with experience (see Alt, Bueno de Mesquita, and Rose 2011; Emeriau 2023; Fouirnaies and Hall 2022; Freier and Thomasius 2016; Harris and Sass 2011; but also see Carreri and Payson 2024; Ferraz and Finan 2011). Voters also favor experienced candidates, suggesting that these officials may offer better outcomes for their constituents (Erikson and Titiunik 2015; Ferrer 2024*a*; Fowler and Hall 2014). When experienced officials leave, they take their experience with them, potentially resulting in worse performance. A change in leadership also tends to disrupt the positions of people working for the leader. This disruption can also lead to temporary declines in performance (Akhtari, Moreira, and Trucco 2022).

On the other hand, leader turnover may have a neutral effect on government performance if the local government election and appointment processes tend to select people prepared to lead the office on day one. Elections tend to select candidates with relevant experience or skills for the role and good past performance (see DeLuca 2024; Jacobson 1989; but also see Porter and Treul 2023), and appointment processes likely select leaders based on similar characteristics as well (Rehmert 2022). This may be especially true in local politics where partisan differences in policy between candidates tend to be smaller and the roles involve more implementation (Kirkland and Coppock 2018). If experienced officials are typically replaced with another relatively experienced or skilled official, turnover will not normally result in worse performance.

Beyond these two competing theories, there are other reasons turnover may not degrade performance. First, if institutional knowledge is held by a wide variety of people, lower-level officials and volunteers stay in place following turnover, and street-level officials have independent authority, there is no reason to expect that changing the leader will substantially alter performance (Brehm and Gates 1999; Lipsky 1980; Wilson 1968). Second, turnover may actually improve performance if government officials grow increasingly insulated from accountability the longer they stay in their role (Fiorina 1989). This may be especially true in low-salience offices where voters have less information about the performance of the official and appointing bodies may not feel as much pressure to monitor performance (Ferrer 2024*b*; Hessick and Morse 2019; Olson and Stone 2023; Marx, Pons, and Rollet 2022; Wright 2008; Zoorob 2022). In fact, leader quality has the greatest effect on government performance in autocratic regimes where officials are most insulated from public pressure (Jones and Olken 2005). If public officials are insulated from accountability, bringing in a new leader may improve government performance (Marx, Pons, and Rollet 2022; Bazzi et al. 2025).

Prior to looking at any data, there are plausible arguments for any of these mechanisms to be at work in local government and in election offices in particular. Running elections is a complex, fast-paced job with tight deadlines. It is reasonable to expect that it takes time to learn how to juggle many roles during short, stressful periods. Given the high-stress environment, it is also reasonable to expect that it could take time to adjust to roles that shift after a leadership change. Large incumbency advantages and limited information about performance may also insulate local election officers from accountability. Given the preference voters and appointing bodies have for candidates with experience in local office including in election administration (Ferrer 2024*a*) and the less partisan nature of voting in local races (see Kuriwaki Forthcoming; Thompson 2020; Ferrer, Geyn, and Thompson 2023; but also see de Benedictis-Kessner and Warshaw 2016), it is also likely that election and appointment processes will tend to select experienced officials. Finally, election officials oversee diverse teams of hired and volunteer staff, and these teams are likely to hold institutional knowledge and maintain some independence.

3 New Data on Local Election Official Turnover and Performance

In this section, we describe our new data on election official turnover and the performance measures we study. We then describe the rise in election official turnover, documenting that turnover increased steadily from 2004 to 2022 with a somewhat faster increase in 2022.

3.1 New Data on Local Election Official Turnover

We collect a large-scale panel dataset of chief local election officials across 50 states and 24 years. Our data captures the individual that we understand to have the most responsibility for running each even-year general election between 2000 and 2024 in every local election jurisdiction. For states with multiple individual election authorities at the local level, we capture the individual with primary responsibility for administering elections on Election Day, as defined by Ferrer and Geyn (2024).[86] For states with election boards, we code the statutorily defined individual who handles the day-to-day responsibilities of running elections, which is typically an official appointed by the board.[87] Table A.1 in the online appendix provides a summary of every official included in our data, as well as their selection method and their degree of election administration authority in that state. Table A.2 provides examples of our classification of election officials by their level of authority.

Figure 1 visualizes our data collection, classifying jurisdictions by the amount of authority the individual

[86] There are two exceptions to this due to data constraints. In Michigan, we code the county clerk instead of the municipal clerk. In New Hampshire, we code the municipal clerk instead of the moderator.

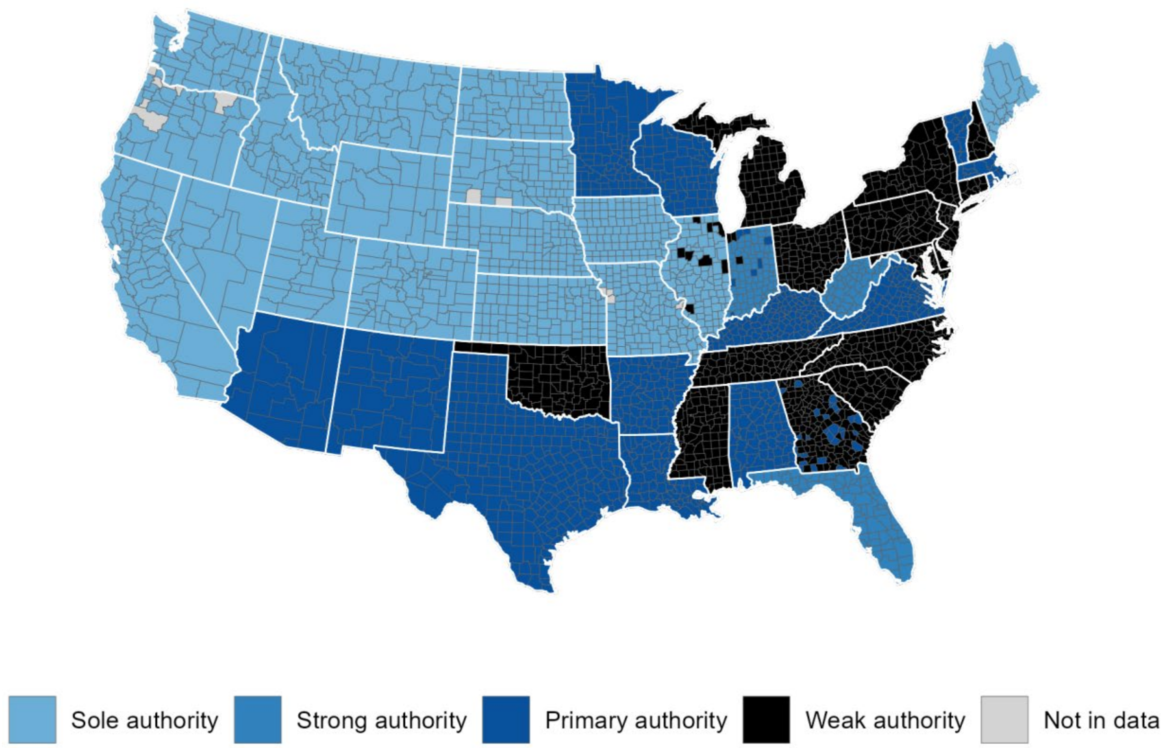
[87] We could not identify a single individual in each election jurisdiction in New York who is in charge of running elections. Instead, we code both the Democratic and Republican co-chairs of each county's election board and weight New York observations by half in our analysis to account for the duplicate entries.

captured in our data wields. In light blue jurisdictions, the election official has complete authority over election administration. In medium blue jurisdictions, the official is in charge of most election duties, but a different local authority carries out some election-related duties such as certification of election results. In dark blue jurisdictions, the official undertakes the majority of Election Day responsibilities but is not responsible for at least some substantial duties, such as registering voters or absentee voting administration. In black jurisdictions, the official is not the primary election authority. This is the case either because the majority of election administration duties are undertaken by an election board rather an individual (i.e., Georgia, North Carolina, and Oklahoma), multiple individuals share the same responsibilities (New Hampshire and New York), or data availability issues prevents us from using the individual with the most election administration duties (Michigan).

We collect the majority of our data from state government websites either through election results for elected officials—building on Ferrer, Geyn, and Thompson (2023)—or from official directories of these officials. Where state-level data is not available, we collect jurisdiction-specific data from past election results, archived website pages, or via direct communication with county offices.[88] We extensively clean the dataset to minimize false positive cases of turnover. When two officials serving in the same jurisdiction share a last name or a first name, we investigate whether this is the same official with multiple names or two different officials. We also examine rare first and last names in our dataset and conduct character string distance matching within jurisdictions to identify spelling errors. We then create a single standardized version of each official's name to use for the purpose of tracing their service tenure.

[88] The only exception is Massachusetts, where the nonprofit Verified Voting provided their list of election officials to complete our dataset.

Figure 1: **Map of Local Election Official Authority by County**. This map captures how much independent authority is given to the election official captured in our data. "Sole authority" means the election official has complete statutory election authority. "Strong authority" means that the election official captured is in charge of virtually all voter and registration administration duties. "Primary authority" means that the official captured is in charge of the majority of election administration duties. "Weak authority" means that the official captured is in charge of some election duties but is not the primary authority in their jurisdiction. Connecticut, New Hampshire, Rhode Island, and Wisconsin administer elections at the municipal level but have the same levels authority across all municipalities within each state. Maine, Massachusetts, and Vermont administer elections at the municipal level but have varying levels of authority across municipalities. The modal category is shown, which accounts for 86%, 97%, and 99% of all jurisdictions within each state, respectively. Alaska and Hawaii jurisdictions are both sole authority.



Throughout most of the paper, we define turnover as a change in a jurisdiction's chief election official since the November election held two years prior. This ensures that we focus on the periods when we expect the most disruption from turnover—the first general federal election that the new official is responsible for running during this period of their service. When reporting changes in turnover over time, we define turnover as a change in a jurisdiction's chief election official since the November election held four years prior. We use this definition to address the fact that election officials are often elected on a four-year cycle in midterm years. This institutional feature adds a cyclical pattern to the trend in two-year turnover that makes it more difficult to interpret. By defining turnover as a change in leadership over the past four years, we remove this cyclical pattern and can interpret any changes in turnover as arising from factors other than the normal election cycle.

In total, our data encompasses more than 18,000 unique elected and appointed chief election officials across 6,290 counties and municipalities. We have complete lists of names in these counties across 13 election cycles, allowing us to compute turnover rates in the 11 elections from 2004 to 2024 and leaving us with over 80,000 jurisdiction-year observations of turnover.[89]

3.2 Measuring Election Performance

We study four performance measures: turnout, the share of ballots without votes at the top of the ticket (residual vote), reported problems voting, and voter confidence.

2.2.1 Measuring Turnout and Residual Vote

We link our dataset of local election officials with federal and statewide election results as well as adult population estimates. We obtain county- and municipal-level ballots cast and total presidential and gubernatorial vote data from David Leip's U.S. Election Atlas.[90] We use county- and jurisdiction-level Census data on population by age to compute voting-age population over time.[91] Putting together the Census and Leip data, we compute turnout as the total number of votes cast in the presidential or gubernatorial election divided by the voting-age population. We also compute residual vote as the number of ballots cast in a jurisdiction

[89] Our snapshot of 2024 election officials was captured in the last week of January 2024. As such, it likely underestimates the amount of turnover that occurred between 2020 and Election Day 2024. Therefore, we are cautious about drawing conclusions from this snapshot of data.

[90] Leip's atlas does not contain municipal-level election results for Wisconsin. We fill this gap using data from the Wisconsin state legislature.

[91] We rely on estimates from the National Cancer Institute's Surveillance, Epidemiology and End Results Program available at https://seer.cancer.gov/popdata/singleages.html.

minus the number of votes cast in the race at the top of the ticket, either the presidential or gubernatorial election.

2.2.2 Measuring Problems Voting and Voter Confidence

We measure problems voting using the Survey of the Performance of American Elections (SPAE). The SPAE conducted surveys following the 2008, 2012, 2014, 2016, 2020, and 2022 November general elections. Each survey typically interviews 200 residents of each U.S. state for a total of 10,000 interviews.

Across all six waves, the SPAE asked respondents if they had any difficulty finding their polling place or a problem with their voter registration, voting equipment, getting their mail ballot, or marking their mail ballot. We code a respondent as having a problem voting if they say they experienced any of these issues. Only people who cast a vote can say they experienced a problem voting. We supplement this with an additional variable that combines voters and non-voters. We count a non-voter as having experienced a problem if they say at least one election administration issue was a major reason they did not vote: they requested but did not receive an absentee ballot, they had a problem with their voter registration, the polling place location or hours were inconvenient, the line at the polling place was too long, they did not know where to vote, or they did not receive a ballot in the mail in time to vote.

We also use the SPAE to measure how long people wait to vote. The options are "not at all", "less than 10 minutes", "10 to 30 minutes", "31 minutes to 1 hour", and "more than 1 hour".[92] Finally, we use the SPAE to measure how confident citizens are in the election. We construct two simple binary measures of confidence, one capturing whether a respondent was very confident that their vote was counted and another capturing whether a respondent was very confident that the votes of others in their city and county were counted.

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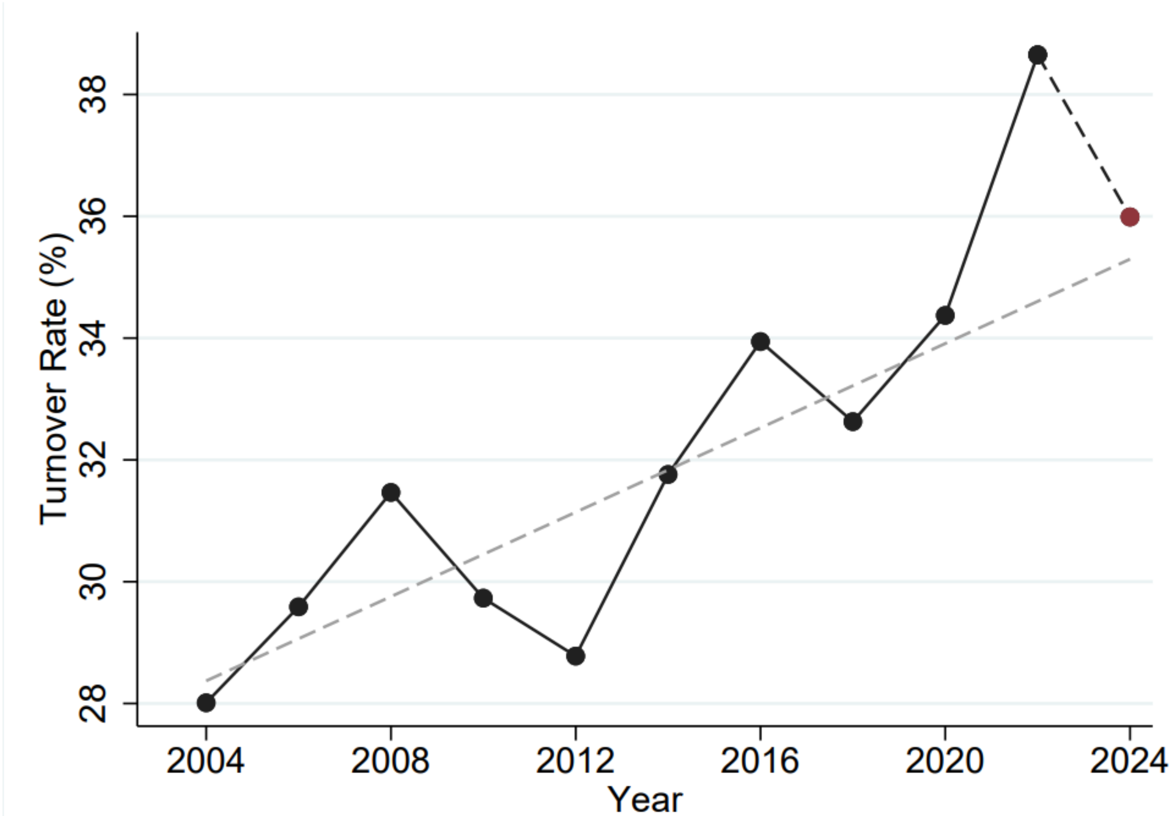
[92] Chen et al. (2020) report that these survey-based measures of polling place wait times correlate remarkably highly with estimates based on cell phone tracking data.

Figure 2: **Increasing Local Election Official Turnover Rates, 2004-2024**. The share of counties with a new chief election official since the election held four years prior has increased steadily from 2004 to 2020 with a modest additional increase in 2022. The dashed line comes from a linear regression of turnover rate on year holding out 2022 and 2024. This plot includes data from 5,928 jurisdictions in 49 states, excluding Massachusetts. The 2024 dot is red to indicate that this data was updated in January 2024 and does not reflect all turnover prior to the November 2024 election.



3.3 Election Official Turnover Increased from 2004 to the Present

The main concern motivating the recent attention to local election official turnover is that officials are leaving the job in large numbers after 2020. Figure 2 provides the data necessary to evaluate the scale of the problem, capturing how turnover has changed over time. Each point represents the average turnover rate across all jurisdictions in the 49 states we study from 2004 to 2024.[93] The dotted gray line plots the fitted line from a

[93] Massachusetts is excluded because we lack a full panel of data for this state.

regression of turnover on time. The regression line is fit only using data from 2004 to 2020 as a tool for predicting turnover in these years if the existing trend had continued into 2022.

Local election official turnover gradually increased from 28% in 2004 to 34% in 2020. Every two years between 2004 and 2020, the turnover rate increased by four-fifths of a percentage point. From 2020 to 2022, turnover increased by over 4 percentage points to 39%. This is the largest single-cycle increase in turnover among the 11 cycles in our data, but only by a modest margin. Turnover increased by almost 3 percentage points between 2012 and 2014.

4 Election Official Turnover Does Not Noticeably Degrade Performance

In this section we study the effect of local election official turnover on election performance. We begin by describing our empirical approach including a brief discussion of our choice to highlight turnout as a measure of election performance. Next, we present graphical evidence that election official turnover does not reduce participation. We then report formal estimates of the effect of election official turnover on participation and we show that the effect is not larger in midterm elections, when a more experienced official leaves, or when we exclude cases where officials are forced out of office. We also present evidence that turnover does not noticeably increase the rate of problems at the polls and does not make election offices more error prone but may modestly increase wait times. Finally, we document that turnover does not have larger effects even in times of policy uncertainty and disruption.

4.1 Studying the Effect of Election Official Turnover on Performance

The main empirical challenge in studying the effect of election official turnover on participation is that the jurisdictions that experience turnover may have different levels of turnout and are possibly on different turnout trajectories. To overcome these challenges, we adopt two approaches for estimating the effect of election official turnover on election performance.

First, we estimate fixed effects regressions of the form

$$Y_{it} = \beta Turnover_{it} + \alpha_{ic} + \gamma_{st} + \varepsilon_{it}$$

where Y_{it} is turnout in jurisdiction i in year t, $Turnover_{it}$ is a binary variable indicating whether the election official has changed since the election held two years earlier, β is our estimate of the effect of turnover on turnout, α_{ic} is a jurisdiction-by-election-type fixed effect,[94] γ_{st} is a state-by-year fixed effect, and ε_{it} is the residual. Under the assumption that turnout is on the same trend in counties that experience turnover and those that do not (Angrist and Pischke 2008) and that turnover does not have effects on turnout beyond the first election cycle (Goodman-Bacon 2021), β is an unbiased estimator of the causal effect of turnover on election performance.

While this approach produces precise estimates of the effect, both assumptions necessary to ensure the effect estimates are unbiased seem unlikely to hold in this case ex ante: local election officials may be more likely to leave after a bad or great performance and the effect of turnover could persist due to election officials learning on the job. We overcome the weaknesses in this approach using a matched difference-in-differences design akin to Imai, Kim, and Wang (2023) and closely related to recent developments in synthetic control (Arkhangelsky et al. 2021; Hazlett and Xu 2018). This approach demands more out of the data and produces less precise estimates, but it is also relies on the weaker assumption that jurisdictions with turnover would have, in the absence of turnover, seen the same change in turnout as other jurisdictions in their state with similar turnout and

[94] We have two election types in our analyses: presidential elections held in November every four years and midterm elections held in November in every even year not divisible by four.

turnover patterns in previous cycles.

In our matching approach, we focus on even-year general elections from 2012 to 2022 one by one. For each of the six elections between 2012 and 2022, the analysis proceeds in three steps. First, for each county where the election official leaves office before the given election, we identify all jurisdictions in the same state that have the exact same turnover history but did not change their election official immediately before the election. We then compute the Euclidean distance between pre-election turnout for each jurisdiction experiencing turnover and their control pool and select as the matched control the control jurisdiction that is closest to the treated unit. Formally, we select match

$$j_i^* = \mathrm{argmin}_{j_i \in J_i} \sum_{t=1}^{T_{pre}} (Y_{it} - Y_{j_i t})^2$$

where j^*_i is the index for the selected matched control, j_i indexes the set of allowable matches J_i for treated unit i, t indexes elections in the pre-treatment period ending at T_{pre}, and Y_{kt} is turnout in jurisdiction k and election t. Finally, we estimate regressions nearly identical to those above but replacing state-by-year fixed effects with matched-pair-by-year fixed effects.[95]

Throughout this section, we focus on turnout as our primary measure of election performance. We do so for four reasons. First, more than 60% of local election officials say in surveys that increasing participation is one of their objectives.[96] Second, misadministration can make it harder for people to vote and is unlikely to increase the number of people who vote. Third, reducing participation through misadministration of an election is among the most important plausible consequences of election official turnover. Fourth, turnout is

[95] While the regression appears similar, one important distinction is that β is now an estimate of the effect of turnover from 2012 and 2022, not in any other period. If the average effect of turnover is changing over time, estimates from these two strategies may differ for reasons other than random noise and bias from unmet identification assumptions.

[96] 2023 EVIC/Reed College Survey of Local Election Officials. Available at https://evic.reed.edu/wp-content/uploads/2023/11/crosstabs.html

widely available and reliably estimated. Put together, studying turnout offers a reliable, important, and convenient way to assess the effects of turnover on election performance.

We also use residual vote as an outcome. Residual vote has been widely used as a measure of election quality (Brady et al. 2001; Kropf et al. 2020; Stewart 2020). While it has important drawbacks—for example, residual vote may reflect dissatisfaction with the candidates running at the top of the ticket rather than administrative error—it should tend to correlate with bad ballot design, faulty equipment, and poor voter assistance among other failures of election administration. We follow Stewart et al. (2020) in adjusting for jurisdiction and year fixed effects in our analysis of residual vote to ensure we are not simply picking up on a widespread increase in abstention or longstanding cross-jurisdiction patterns of abstention.

In some analyses, we subset to the states and jurisdictions where the local election official captured in our dataset is in charge of all ("sole authority"), virtually all ("strong authority"), or the majority ("primary authority") of voter and registration administration duties (see Ferrer, Geyn, and Thompson (2023) and Ferrer and Geyn (2024) for a discussion of a similar categorization). In these analyses, we exclude jurisdictions where election duties are divided between multiple officials and where the chief election official is the chair of an elections board. If higher local election official turnover causes lower voter participation, we would be most likely to observe this effect in these jurisdictions with a strong individual local election official.

To validate our matching approach, we present three complementary analyses in Section A.3 in the online appendix. First, we show that the matching procedure successfully balances the average turnout rate across treatment and control jurisdictions in all pre-treatment periods. We then show that the distribution of pre-treatment turnout is similar in treated and matched control jurisdictions. Finally, we present a placebo analysis where we hold the election immediately preceding treatment out of the matching procedure then evaluate balance in that pre-treatment period. We find that treated and control jurisdictions have similar changes in turnout in the held out election prior to the treatment period. This supports the sequential ignorability assumption that justifies our matching approach.

4.2 Graphical Evidence that Election Official Turnover Does Not Reduce Participation

Figure 3 presents simple averages from our raw data that mimics our analysis of the effect of election official turnover on voter participation. The plot has four lines: the two lines at the top of the plot correspond to our analysis of the effect of turnover between the 2018 and 2020 presidential elections on turnout in the 2020 presidential election. The black line reports the turnout rate for jurisdictions where the election official left office between 2018 and 2020. The grey line reports the turnout rate over time for jurisdictions where the election official serving in 2018 also served in 2020. The bottom two lines report the same analysis but using gubernatorial elections on midterm cycles where the jurisdictions experiencing turnover are those where the election official changed between 2020 and 2022.

The plot suggests that election official turnover did not substantially affect participation. We can see this by focusing our attention on the gap between each black line and its nearest grey line. The differences are relatively stable before and after 2018, implying in both cases that election official turnover did not noticeably alter turnout.

Figure 3 has two main weaknesses: First, it does not account for the expectation that turnover in 2020 or 2022 may be associated with a particular historical pattern of turnover that could have affected voter turnout in previous periods. Second, places with turnover in 2020 and 2022 tend to have lower voter turnout than places without turnover in those years. While this is not a violation of the difference-in-differences identifying assumption per se, it is easier to believe that two groups that are similar on average in the past will continue to be more similar in the future than to believe that two different groups will continue changing in the exact same manner.

We address these concerns by matching each jurisdiction with turnover in 2020 or 2022 to a jurisdiction in the same state without turnover in 2020 or 2022 but with an identical turnover history and the most similar voter turnout history available.[97] Figure 4 graphically captures this analysis. The plot has four lines: the top two lines correspond to our analysis of the effect of turnover between the 2018 and 2020 presidential elections on turnout in the 2020 presidential election. The black line reports the turnout rate for jurisdictions where the election official left office between 2018 and 2020, and the grey line reports the average matched control unit. The two lines at the bottom of the plot report the same analysis but using gubernatorial elections on midterm cycles where the jurisdictions experiencing turnover are those where the election official changed between 2020 and 2022. Here again, the black line reports average turnout over time in jurisdictions with an election official change between 2020 and 2022, and the grey line reports its average matched control.

The fact that the black and grey lines in the top and bottom of the plot are nearly identical before 2020 implies that the average matched control jurisdiction closely resembles the average turnover jurisdiction. Turning to the post-treatment period, we see that in 2020 and 2022 the grey and black lines continue to look similar, meaning that local election official turnover did not lead to substantially lower citizen participation on average. We report formal estimates of this effect in the remaining subsections of Section 4.

[97] We discuss this strategy at length in Section 4.1.

Figure 3: **Election Official Turnover Does Not Noticeably Reduce Turnout**. The black line near the top of the plot represents turnout rates over time in jurisdictions that experience turnover between 2018 and 2020, and the grey line near the top of the plot represents the turnout rate for jurisdictions that did not experience turnover in this period. The black line near the bottom of the plot represents turnout rates in jurisdictions that experienced turnover between 2020 and 2022, and the grey line near the bottom of the plot represents the turnout rate for jurisdictions that did not experience turnover between 2020 and 2022. The dotted vertical line in 2019 splits the pre-treatment and post-treatment periods. The plot only uses jurisdictions where the local election official oversees nearly all or all election administration duties.

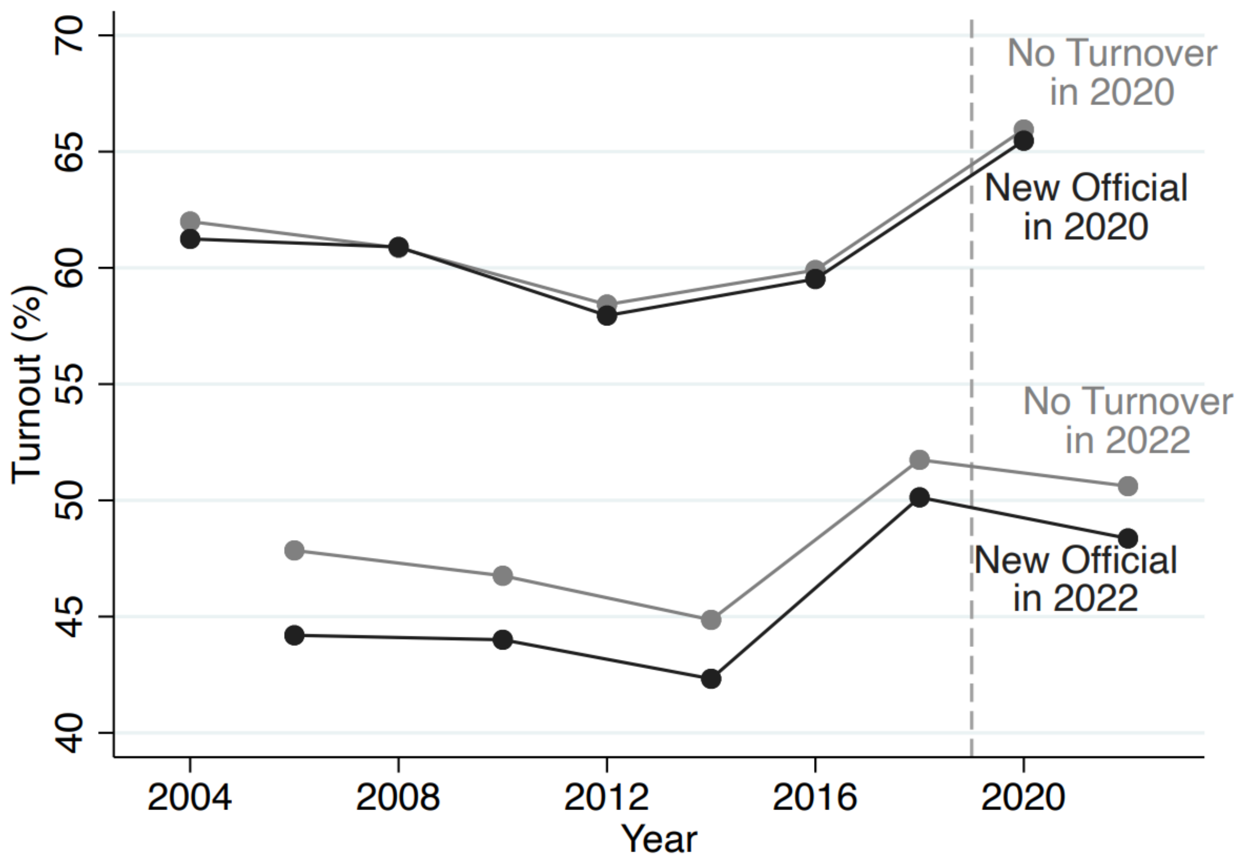


Figure 4: **Election Official Turnover Does Not Noticeably Reduce Turnout, Matched Analysis**. The black line near the top of the plot represents turnout rates over time in jurisdictions that experience turnover between 2018 and 2020, and the grey line near the top of the plot represents the turnout rate for their matched controls. The black line near the bottom of the plot represents turnout rates in jurisdictions that experienced turnover between 2020 and 2022, and the grey line near the bottom of the plot represents the turnout rate for their matched controls. The dotted vertical line in 2019 splits the pre-treatment and post-treatment periods. The plot only uses jurisdictions where the local election official oversees nearly all or all election administration duties.

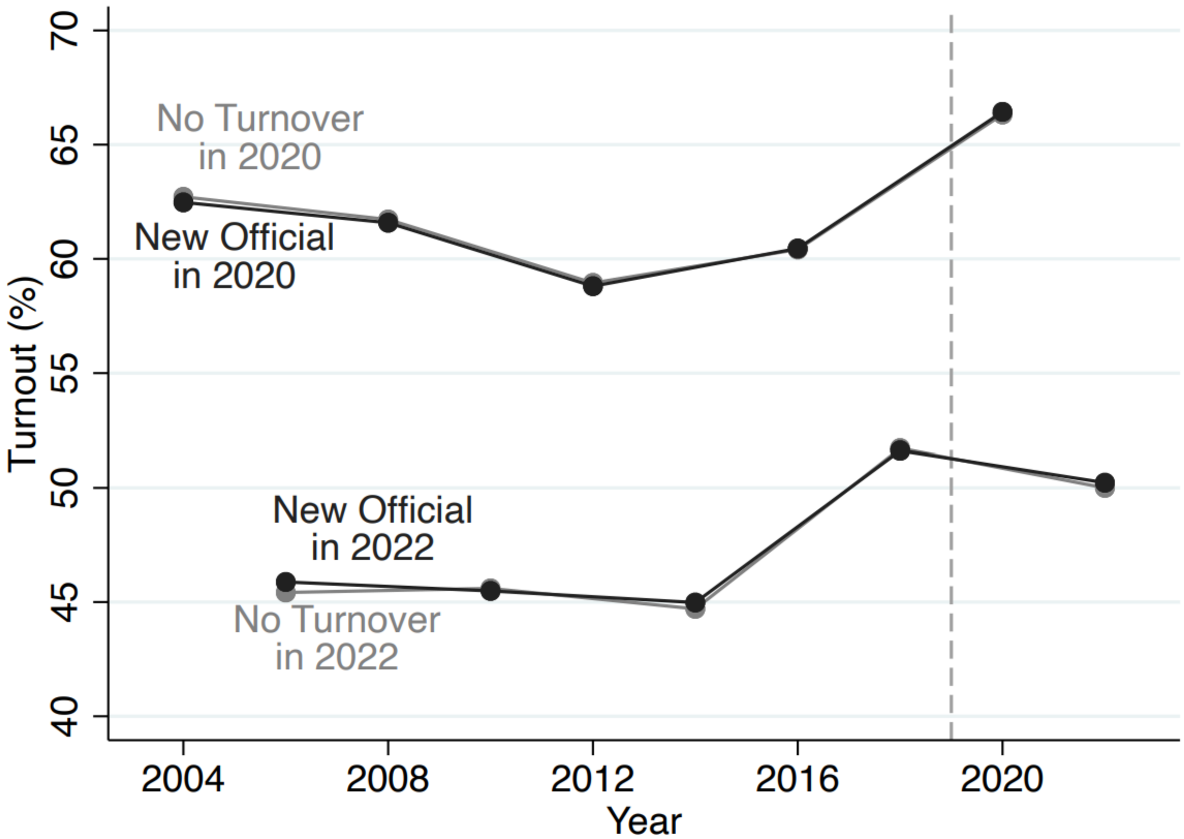


Table 1: **Effect of Election Official Turnover on Turnout and Residual Vote.**

	Turnout (0-100%)				Residual Vote (0-100%)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Turnover	-0.08	-0.12	0.01	-0.10	0.01	0.04	0.05	0.01
	(0.06)	(0.09)	(0.07)	(0.13)	(0.04)	(0.04)	(0.03)	(0.06)
# Jurisdictions	4,060	3,200	1,179	981	1,834	1,596	966	871
# Obs	28,250	22,584	9,675	6,996	15,030	11,230	8,095	5,978
Outcome Mean	59.45	59.85	56.05	56.76	1.38	1.37	1.66	1.63
Min Detectable Effect	0.16	0.27	0.19	0.38	0.10	0.13	0.08	0.16
Strong Official Only	No	No	Yes	Yes	No	No	Yes	Yes
Matched Sample	No	Yes	No	Yes	No	Yes	No	Yes
Juris-by-Elec Type FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-by-Year FE	Yes	No	Yes	No	Yes	No	Yes	No
Pair-by-Year FE	No	Yes	No	Yes	No	Yes	No	Yes

Robust standard errors clustered by jurisdiction reported in parentheses. Data is limited to jurisdictions with one primary official in charge of the majority of election administration responsibilities. Strong official only indicates jurisdictions where one official is responsible for directing nearly all aspects of election administration. Matched sample limits data to jurisdictions that experienced turnover between 2012 and 2022 and a set of matched control jurisdictions from the same state with the same history of turnover and the most similar levels of the outcome in all prior elections using 1-to-1 matching with replacement. Turnover refers to a change in the election official since the election two years prior. Turnout is measured as the share of voting-age residents who cast a vote in the presidential race for presidential years and the gubernatorial race for midterm years. Residual vote is measured as the share of ballots cast without a vote for president in presidential years and for governor in midterm years. Regressions on unmatched data include jurisdiction-by-election cycle (presidential or midterm) fixed effects and state-by-year fixed effects. Regressions on matched data include jurisdiction-by-election cycle fixed effects and matched pair-by-year fixed effects. Min detectable effect refers to the minimum effect that a two-sided test with a 0.05 alpha would have 80% power to detect.

4.3 Formal Evidence that Election Official Turnover Does Not Degrade Performance

Table 1 presents formal estimates of the effect of turnover on turnout and residual vote. The first column presents our two-way fixed effect estimate of the effect of turnover using all instances of turnover from 2004 to 2022 and all jurisdictions with a single election official who oversees at least a majority of election administration tasks. The second column presents our matching-based estimate of the effect on turnout still including all jurisdictions with a single election official overseeing a majority of election administration tasks. The third and fourth columns repeat the first and second columns but limit data to jurisdictions with election officials who are responsible for all or nearly all election administration in the jurisdiction.[98] Columns 5 through 8 repeat columns 1 through 4 but study residual vote as the outcome.

The two-way fixed effect analyses reported in odd-numbered columns are more precise but are more likely to be biased. The matching analyses reported in even-numbered columns overcome the main potential threats to the two-way fixed effects analyses but are less precise. Similarly, our estimates in columns three, four, seven, and eight, using only jurisdictions with a single individual responsible for overseeing all aspects of election administration, are noisier, but these analyses may be more likely to detect effects if they exist given the greater authority of election officials in this subset.

Across all eight estimates, we find consistent evidence that local election official turnover does not meaningfully affect citizen participation or residual vote. Our point estimates imply that turnover did not decrease voter turnout by more than an eighth of a percentage point and did not increase voter turnout by more than one one-hundredth of a percentage point. Our point estimates also imply that turnover did not increase the residual vote rate by more than one tenth of a percentage point and did not decrease the residual vote rate. Across all eight columns, we cannot reject the null hypothesis that turnover has no effect on turnout or residual vote. Focusing on our preferred approach which uses matching and zooms in on jurisdictions where the chief election official has all or nearly all authority over election administration (presented in column 4), the bottom end of our 95% confidence interval is still less than a two-fifths percentage point effect on turnout. Our analysis is powered to detect very small effects on voter turnout. We have 80% power to detect effects as small as the effect of adding a day and a half of early voting (Kaplan and Yuan 2020), one-eighth the effect of a get-out-the-vote ad campaign for young people Green and Vavreck (2008), one-eighth the effect of switching to universal vote-by-mail (Gerber, Huber, and Hill 2013; Thompson et al. 2020), and one-half of the

[98] We include "strong authority" and "sole authority" officials. In Table A.4 in the online appendix, we present estimates for each level of authority independently.

effect of sending a single postcard to everyone encouraging them to vote (Gerber et al. 2017). The effects we estimate on residual vote using our matching approach are similarly small.

Put together, Table 1 suggests that local election official turnover does not substantially decrease turnout or increase residual vote.

4.4 Minimal Average Effects on Turnout Do Not Mask Substantial Heterogeneity

While the analysis above rules out large negative average effects on turnout, these analyses could be misleading if turnover has meaningful negative effects in a substantial share of cases but small or even positive effects in others. We investigate this in five supplementary analyses. Across all of these analyses, we reach the consistent conclusion that election official turnover does not substantially reduce turnout.

First, in Section A.5 in the online appendix, we document that the effect of turnover on turnout is similar in presidential, when turnout and interest in elections is at its highest, and midterm elections, when interest is turnout and interest are lower. This suggests that the effect is not limited to settings where interest is very high. Second, we present evidence in Section A.6 in the online appendix that the effect of turnover is similar when the outgoing official had many or few years in office prior to their departure, suggesting that our average effect is not masking a large negative effect when people with many years of experience leave. Third, in Section A.7, we show that the effects on turnout are minimal even when we subset to cases where the official left voluntarily. Fourth, in Section A.8, we document that the effects are similar in jurisdictions where officials are elected versus appointed. Finally, in Section A.9, we present evidence that the effects are minimal in jurisdictions with large and small populations where we expect the staffing and duties of the leader to be quite different.

Put together, we take this as evidence that most cases of election official turnover do not substantially reduce turnout.

4.5 Turnover Does Not Make Residents Noticeably More Likely to Report Voting Issues but May Modestly Increase Wait Times

Might new officials perform worse than their predecessor without decreasing turnout or increasing residual vote? While unnecessarily preventing an eligible person from voting is among the most important mistakes an election official can make, it may be hard to see this kind of mistake if voters find ways to vote despite the barrier placed in their path.

To evaluate whether turnover makes it harder for people to vote without affecting turnout, we turn to the Survey of the Performance of American Elections (Stewart 2023). The survey interviewed 200 or more residents of every US state following every even-year general election between 2008 and 2022, with the exception of 2010 and 2018. We measure someone as having had a problem voting if they report that they had a problem with their voter registration, a problem with voting equipment, a problem getting a mail ballot, a problem marking their mail ballot, or difficulty finding their polling place. We then match respondents to the counties where they live and run repeated cross-sectional regressions to isolate the effect of election official turnover on reported problems voting.[99]

[99] In all analyses, we weight our regressions by the survey weights provided by the survey team.

Table 2: **Effect of Election Official Turnover on Share of Voters Reporting Problems Voting.**

	Reported Problem Voting {0,1}			
	(1)	(2)	(3)	(4)
Turnover	0.002	0.002	0.002	0.001
	(0.005)	(0.004)	(0.004)	(0.004)
# Counties	1,030	1,030	1,029	905
# Respondents	24,737	24,737	24,650	24,526
Outcome Mean	0.041	0.041	0.041	0.041
Min Detectable Effect	0.013	0.012	0.011	0.012
Strong Official Only	Yes	Yes	Yes	Yes
State-by-Year FE	No	Yes	Yes	Yes
Individual Controls	No	No	Yes	Yes
County Pop Control	No	No	Yes	No
County FE	No	No	No	Yes

Robust standard errors clustered by county reported in parentheses. Data is limited to counties where only one official is responsible for directing all or nearly all aspects of election administration. Each observation is one respondent to the Survey of the Performance of American Elections who reported voting. Observations are weighted according to the weights provided by the survey team. Turnover refers to a change in the election official since the election two years prior. The outcome is a dummy variable with value 1 for respondents reporting a problem with the registration or the voting equipment, an issue obtaining or completing their mail ballot, or difficulty finding the polling place. Individual controls are gender, race, years of education, and party ID fixed effects as well as age included as a single covariate. County pop control is the natural logarithm of voting-age population. Min detectable effect refers to the minimum effect that a two-sided test with a 0.05 alpha would have 80% power to detect.

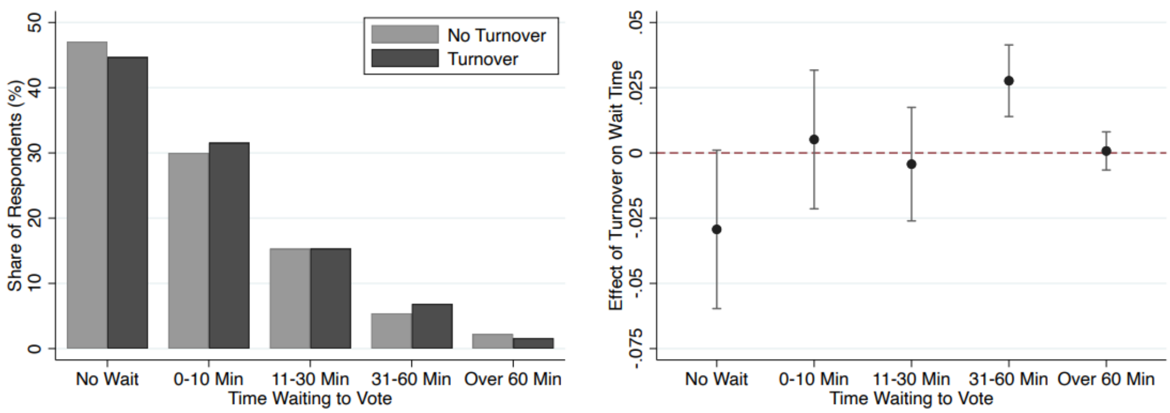
We find that turnover does not substantially increase the share of voters who say they had a problem while trying to vote. Table 2 presents our results. Colum 1 presents the simple difference in the share of people who had an issue voting in counties with turnover vs. those without turnover, finding that people living in counties with turnover were 0.2 percentage points more likely to report a problem. In columns 2, 3, and 4, we adjust for factors that may be different in jurisdictions with turnover from those without and that may affect the tendency of a respondent to experience or report a problem. Across all four columns, we find consistent evidence that turnover does not substantially increase the share of people reporting a problem trying to vote.

In a complementary analysis, we evaluate whether turnover leads to longer wait times at the polls. A significant part of the job for election officials is overseeing a logistically complex event, and having run a prior election may help officials carry it out more effectively and reduce wait times. Figure 5 presents our results. In the left panel, we present the distribution of wait times in jurisdictions and years with election official turnover next to the distribution of wait times in jurisdictions and years without turnover. While the distributions are similar, there is a modest shift towards longer wait times in jurisdictions experiencing turnover. Fewer people experience no wait and more people experience wait times between 30 minutes and an hour in jurisdictions and years that the election office changed hands.

Election official turnover is more common in certain types of jurisdictions and years than others. For example, turnover is much more common before presidential elections than midterms and more common in densely populated places than in suburbs. If wait times are systematically worse in these types of counties and periods, we may incorrectly conclude that wait times are higher because of turnover when it is simply a coincidence about the timing and location of turnover. To assess this possibility, we estimate the effect of turnover on the probability a resident falls in each wait time category adjusting for state-year fixed effects, county-level covariates, and respondent-level covariates.[100] We present our effect estimates in the right panel of Figure 5. We find that turnover increases the share of residents experiencing a wait time between 30 minutes and an hour by about 2.5 percentage points and reduces the share experiencing no wait time by a similar amount. In Section A.11 in the online appendix, we document that this finding is robust to other plausible regression specifications.

[100] We use county-level log population as our county-level covariate. Our individual-level control variables are gender, race, educational attainment, and party identification. We include all individual-level covariates as categorical variables, converting them into dummy variables for each value.

Figure 5: **Election Official Turnover May Modestly Increase Wait Times**. The left panel presents the distribution of wait times in jurisdictions and years with chief election official turnover compared to those without turnover. The right panel presents estimates of the effect of turnover on the share of probability a resident experiences a given wait time. The estimates in the right panel come from separate regressions of a dummy variable for each category of wait time on a dummy for turnover, state-year dummies, county-level log population, and individual-level control variables including gender, race, educational attainment, and party identification. Both plots rely on data from the Survey of the Performance of American Elections and are weighted using the weights constructed by the survey team.



We take this as only suggestive evidence that turnover leads to a modest increase in the time people spend at the polls. We reach that conclusion for two reasons. First, we describe this effect as suggestive because, given the large number of analyses we run, we should expect to occasionally find statistically significant effects even if turnover does not have an effect. Second, we describe this effect as modest based of how it affects citizen behavior. Pettigrew (2021) documents that waiting for 30 minutes to one hour reduces participation by approximately one percentage point. If turnover leads to a 2.5-percentage-point increase in the number of voters who wait 30 to 60 minutes to vote, this would lead to a 0.03 percentage point effect on turnout, roughly one-fifth the effect of a pre-recorded celebrity message GOTV campaign and 33 times smaller than the effect of an average commercial phone bank campaign (Green, McGrath, and Aronow 2013).

Finally, if voters do not have problems voting but still feel the election was administered poorly, this would likely show in their confidence that the vote was counted properly in their community. Table A.12 in the online appendix presents evidence that turnover does not meaningfully affect the share of respondents who are very confident their own vote or the county vote was accurately tallied.

Put together, we read our survey-based results as evidence that election official turnover may modestly increase wait times but it does not increase the number of respondents reporting problems voting or the number of people who have high confidence in the accuracy of the election results and it is not enough to prevent many people from casting a ballot.

4.6 Turnover Does Not Degrade Performance Even in Times of Maximal Change

Even if leadership turnover does not affect performance in normal times, we often expect leaders to be especially important in times of crisis. Does turnover affect performance when election administration is most challenging? To study this question, we focus on the 2020 presidential election, when election officials across the country were asked to navigate major changes in how elections were run and, in many cases, had very little time to prepare for these changes. In Section A.13 in the online appendix, we present our findings that turnover has a similarly small effect on turnout and residual vote in 2020 as in the average year and that respondents living in jurisdictions with turnover were not substantially more likely to have trouble voting by mail in 2020.

Put together, our case study of turnover in 2020 suggests that turnover is not especially harmful when election administration is under strain and change needs to happen rapidly.

5 Why Does Turnover Not Degrade Performance?

We have established that, across a wide variety of outcomes, leadership turnover is not generally associated with substantially lower performance. This runs contrary to the conventional wisdom that, since leaders gain experience over time, replacing them with a new official will result in worse performance (Hays 2004; Perry 2004). This also runs contrary to recent findings in other offices that long-tenured officials perform poorly because they are insulated from accountability implying that turnover will lead to better performance (Marx, Pons, and Rollet 2022). Why might this logic not hold? As discussed in Section 2, we present two novel alternative theories: officials are selected for their relevant qualifications, and the individual leader does not determine performance.

In this section, we offer three pieces of evidence as a partial step toward understanding why turnover does not degrade performance. First, we document that election officials are typically replaced by people with prior paid elections or government experience. Next, we document that new officials without prior experience in elections or government do not oversee large drops in performance. Finally, we present evidence that local election officials serving in the same jurisdiction perform similarly to each other.

While this evidence does not fully explain why election official turnover does not meaningfully degrade performance, it offers some clues: First, there is strong positive selection into the job of local election official with most incoming officials having relevant experience. Second, the fact that new officials without documented relevant experience do not oversee weaker performance and we do not see substantial differences in performance across leaders implies that either selection results in consistent quality leadership or leadership does not meaningfully affect performance.

5.1 New Election Leaders Typically Have Paid Election Administration Experience

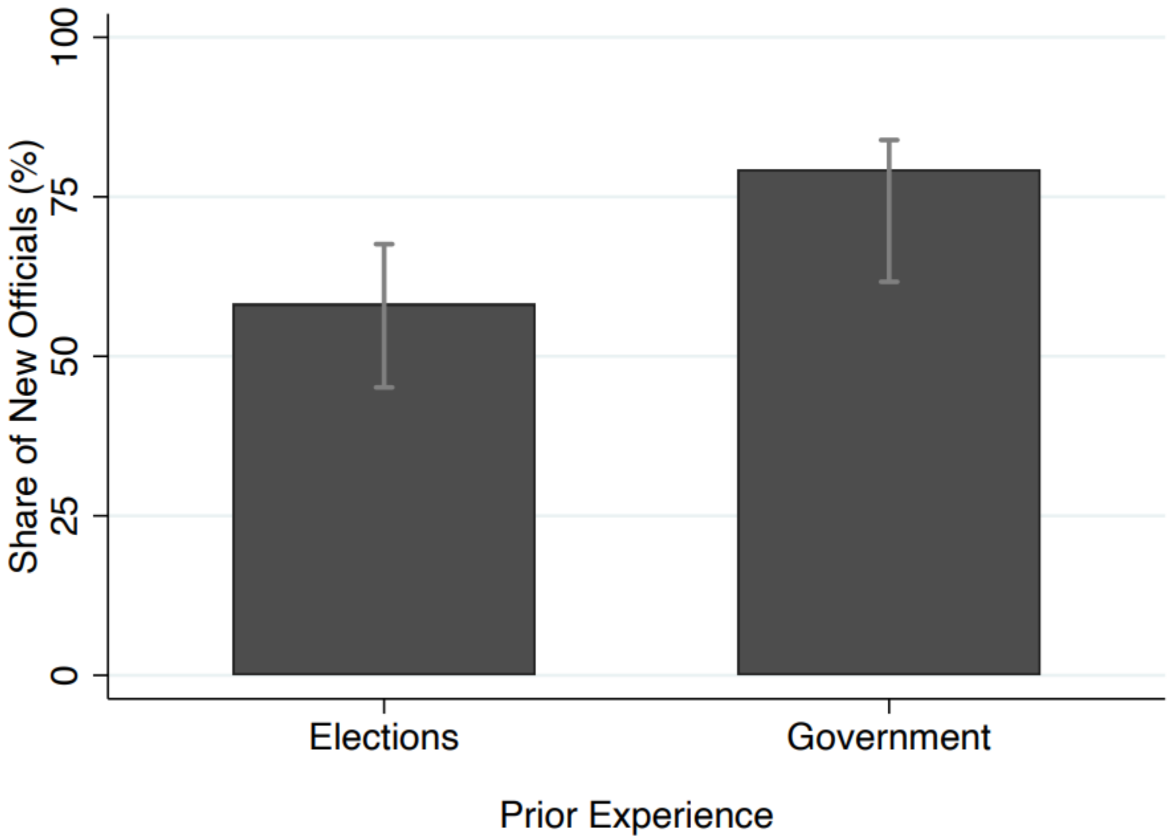
Do elections and appointments select for experienced replacement election officials? To answer this question, we searched for public reports on the professional backgrounds of all election officials who took over their office prior to the 2020 or 2022 general elections and served in offices responsible for all or nearly all election administration duties in their jurisdiction. We review public biographies and news accounts to determine if the official had prior experience in elections or government. Out of the 441 officials included in our search, we found background information on 343.

Figure 6 presents our results. We find that over 60% of new officials have prior professional experience in elections. We also find that nearly 80% have experience in government. Since we are able to code the backgrounds of nearly 80% of all new officials in 2020 and 2022, the Manski bounds are informative—even if all of the officials we cannot code had no prior government experience, more than 60% officials would have government experience.

We take these results as evidence of positive selection in election administration—new local election officials tend to be people with relevant experience in elections or government. The share of new local election officials with government experience is higher than the 63% of newly elected mayors with political experience (Kirkland 2022) and the roughly half of newly elected members of Congress with prior political experience since 2016 (Porter and Treul 2023). Meanwhile, roughly 95% of jurisdictions require that their police chief have experience as a police officer or in police management (Johnson 2005). This suggests that the selection process for local election officials is more like that for other local bureaucratic offices where new leaders are generally expected to have relevant experience.

It is important to note that even the officials who do not have elections or government experience prior to the office may have qualities that make them fit to lead the office. For example, an official may have run a small business or nonprofit and developed similar leadership skills. Our data simply allows us to see that that the selection process is producing a set of new leaders who appear to be well-qualified for the office based on observable traits in their biographies.

Figure 6: **Share of New Officials With Prior Professional Experience in Elections or Government**. Each bar captures the share of new local election officials with prior professional experience in elections or government based on public biographies or news accounts. Error bars capture Manski bounds with the top end of the bar assuming all officials without public biographies had that experience and the lower end of the bar assuming all officials without public biographies did not have that experience. Data collection attempted for all officials who took over a local elections office immediately prior to the 2020 or 2022 election.



5.2 New Officials without Elections or Government Experience Do Not Perform Noticeably Worse

If elections or government experience is necessary to maintain office performance, we would expect performance to degrade most when a new official comes in without that experience. We explore this by subsetting our analysis to cases where the incoming official has either no experience administering elections or no experience working in government and using our panel matching approach to estimate the effect of turnover. Table A.15 in the online appendix captures our results. We find that turnover still has at most a modest effect on turnout and residual vote rates when the incoming election official has limited prior experience. These results tell us that incoming officials without experience perform about as well as incoming officials with this experience. This could mean that either there are compensating differentials where new officials without this experience have other important skills or experience or that leadership does not meaningfully affect performance.

5.3 Similar Performance Across Election Officials in the Same Jurisdiction

Might some unobserved qualities about local election officials shape their performance? To evaluate how much performance varies across election officials, we use the randomization inference approach described in Berry and Fowler (2021). As they discuss, this approach estimates the R^2 of a regression of a performance measure on leader dummy variables then generates a null distribution of R^2 values using a randomization inference procedure that shuffles which leaders were in charge when. By shuffling the leader tenures within each jurisdiction, the null distribution implicitly accounts for jurisdiction fixed effects. In our implementation, we demean turnout by year and state to implicitly account for state-year fixed effects as well.

We find that turnout does not vary much across leaders within the same jurisdiction. As we show in Figure A.7 in the online appendix, the R^2 from the regression using real data falls near the 10th percentile of the null distribution, below the average. This means that knowing when each leader served does not improve our prediction of turnout. Put differently, leaders serving in the same jurisdiction all oversee elections with very similar levels of turnout.

As we note above, this could happen for two reasons. This is consistent with local election officials not affecting performance. It is also consistent with local election officials significantly affecting performance but where the way they are selected results in similar performance across officials.

6 Conclusion

More election officials are leaving office than in the past, and their turnover rate has been rising for at least two decades. This has led a chorus of commentators, academics, and public officials to worry that high turnover means that elections will be poorly run. In this paper, we present a large new dataset on election official turnover over two decades. We find that local election official turnover does not noticeably degrade performance. This finding holds true across the many outcomes we measure, with the possible exception of wait times at the polls, and for the many subsets of the data we study. We also present suggestive evidence that turnover does not affect performance because incoming leaders are typically selected for their experience and skills or because leadership does not meaningfully influence performance. Our evidence suggests that we are unlikely to see major disruptions to local government performance in the short run despite higher turnover in some offices.

One word of caution is warranted when interpreting our findings. While we can rule out turnover systematically producing mistakes that degrade performance on average, turnover may still increase the probability of rare but important negative events. For example, a new official serving a large county in an important swing state who fails to identify a ballot design error could create a crisis of trust or send an election to the courts, as happened in Florida in the 2000 presidential election. We cannot observe minuscule increases in the probability of such an event, but events like those are still important negative outcomes that any full accounting of turnover must consider.

Chapter 5 Appendix

A.1 Local Election Officials Included in Dataset

Table A.1 displays data on the selected local election officials for each state, as well as the number of jurisdictions in the state, the number of jurisdictions with a full panel of data, the level of geography captured, the selection method of the officials, whether the modal official captured in each state is the sole and/or primary election authority, the data sources used, and the start and end year of the data collected.

Table A.1: **Local Election Officials Captured in the Dataset**

State	Jurisdictions	Jurisdictions Used	Geography	Election Official	Selection Method	Sole Authority	Primary Authority	Data Source	Data Start	Data End
Alabama	67	67	County	Probate Judge	Elected	No	Yes	Elections and State	1996	2024
Alaska	5	4	Region	Regional Election Supervisor	Appointed	Yes	Yes	State	2000	2024
Arizona	15	15	County	County Election Administrator / County Recorder	Mixed	No	Yes	State	2000	2024
Arkansas	75	75	County	Clerk	Elected	No	Yes	State	2000	2024
California	58	58	County	Clerk / Registrar of Voters / Auditor / Director of Elections	Mixed	Yes	Yes	State	1996	2024
Colorado	64	63	County	Clerk and Recorder	Mixed	Yes	Yes	Elections and State	1998	2024
Connecticut	178	171	Municipal	Clerk	Mixed	No	No	State	2000	2024
Delaware	3	3	County	Director of Elections	Appointed	No	No	State	1996	2024
Florida	67	67	County	Supervisor of Elections	Mixed	No	Yes	Elections and State	1998	2024
Georgia	159	159	County	Elections Director / Probate Judge	Mixed	No	No	Elections and State	1996	2024
Hawaii	5	4	County	Clerk	Appointed	Yes	Yes	State	2000	2024
Idaho	44	44	County	Clerk	Elected	Yes	Yes	Elections	2000	2024
Illinois	102	102	County	Clerk / Executive Director	Mixed	Yes	Yes	Elections and State	2000	2024
Indiana	92	92	County	Clerk	Elected	No	Yes	Elections and State	1998	2024
Iowa	99	99	County	Auditor	Elected	Yes	Yes	Elections and State	2000	2024
Kansas	105	105	County	Clerk	Mixed	Yes	Yes	State	2000	2024
Kentucky	120	120	County	Clerk	Elected	No	Yes	Elections and State	1998	2024
Louisiana	64	64	Parish	Clerk of Court	Elected	No	Yes	State	1998	2024
Maine	504	502	Municipal	Clerk	Mixed	No	Yes	State	2000	2024
Maryland	24	24	County	Election Director	Appointed	No	No	State	2000	2024
Massachusetts	351	0	Municipal	Clerk / Elections Commissioner	Mixed	No	Yes	Verified Voting	2012	2024
Michigan	83	83	County	Clerk	Elected	No	No	State and NGO	2000	2024
Minnesota	87	87	County	Auditor / Election Director	Mixed	No	Yes	State	2000	2024
Mississippi	82	82	County	Circuit Clerk	Elected	No	No	State	2000	2024
Missouri	115	110	County	Clerk / Director of Elections	Elected	Yes	Yes	State	2000	2024
Montana	56	56	County	Clerk and Recorder / Election Administrator	Mixed	Yes	Yes	Elections and State	1996	2024
Nebraska	93	93	County	Clerk / Election Commissioner	Mixed	Yes	Yes	Elections and State	2000	2024
Nevada	17	17	County	Clerk / Registrar of Voters	Mixed	Yes	Yes	Elections and State	2000	2024
New Hampshire	234	234	Municipal	Clerk	Mixed	No	No	State and NGO	2000	2024
New Jersey	21	21	County	Clerk	Elected	No	No	State	2000	2024
New Mexico	33	33	County	Clerk	Elected	No	Yes	Elections and State	2000	2024
New York	62	58	County	Election Commissioner	Appointed	No	No	State	2000	2024
North Carolina	100	100	County	Election Director	Appointed	No	No	State	2000	2024
North Dakota	53	53	County	Auditor	Elected	Yes	Yes	State	2000	2024
Ohio	88	88	County	County Election Director	Appointed	No	No	State and Local	2000	2024
Oklahoma	77	77	County	Election Board Secretary	Appointed	No	No	State	1996	2024
Oregon	36	36	County	Clerk / Elections Director	Mixed	Yes	Yes	State	2000	2024
Pennsylvania	67	67	County	Director of Elections	Appointed	No	Yes	State	2000	2024
Rhode Island	39	39	Municipal	Clerk / Registrar / Election Director	Mixed	No	Yes	State and Local	2000	2024
South Carolina	46	46	County	Director of Voter Registration and Elections	Appointed	No	No	State	2000	2024
South Dakota	66	64	County	Auditor	Mixed	Yes	Yes	Elections and State	2000	2024
Tennessee	95	95	County	Administrator of Elections	Appointed	No	No	State	2000	2024
Texas	254	254	County	Elections Administrator / Clerk / Tax Assessor	Mixed	No	Yes	State	2000	2024
Utah	29	29	County	Clerk	Elected	Yes	Yes	Elections and State	1998	2024
Vermont	246	246	Municipal	Clerk	Mixed	No	Yes	State	2000	2024
Virginia	133	133	County	General Registrar	Appointed	No	Yes	State and Local	1998	2024
Washington	39	39	County	Auditor / Elections Director	Elected	Yes	Yes	Elections, State, and NGO	2000	2024
West Virginia	55	55	County	Clerk / Elections Coordinator	Mixed	No	Yes	Elections and State	2000	2024
Wisconsin	1851	1779	Municipal	Clerk	Mixed	No	Yes	State	2000	2024
Wyoming	23	23	County	Clerk	Elected	Yes	Yes	Elections and State	1998	2024

Number of jurisdictions are total number of jurisdictions in that state. Jurisdictions Used are the number of jurisdictions with a full panel of data between 2000 and 2024 and used in the main analysis. In states where multiple officials are coded, a '/' separates each distinct official and they are listed in order by frequency. We aim to code the official in each jurisdiction with primary authority to administer elections, especially those who oversee voting administration on Election Day. In jurisdictions with boards, we identify the single official with the most responsibility in running elections. In New York, no single individual could be identified so we code the two election commissioners in each jurisdiction. We exclude jurisdictions in other states where no single individual could be identified. We were unable to collect municipal-level data in Michigan, so we code the most important county-level official. We are unable to collect data on the municipal moderators in New Hampshire, so we code the municipal clerk. Selection method indicates whether all officials coded in each state are elected, appointed, or a mix of both. Sole authority designates whether the official is the only election authority in that jurisdiction, excepting local legislative bodies that determine election administration budgets and appointing bodies whose sole purpose is to select a chief election official. Primary authority indicates whether the official coded is in charge of the majority of election administration responsibilities in the jurisdiction. For both columns, the modal coded official in the state is classified. For election source, 'State' indicates the data derives from the state election authority, 'Elections' indicates the data derives from election results, and 'NGO' indicates the data derives from a state-level independent organization, typically a state association of election officials. The date ranges indicate the maximal amount of data captured for each state, although only data from 2000 onwards is used in the analysis.

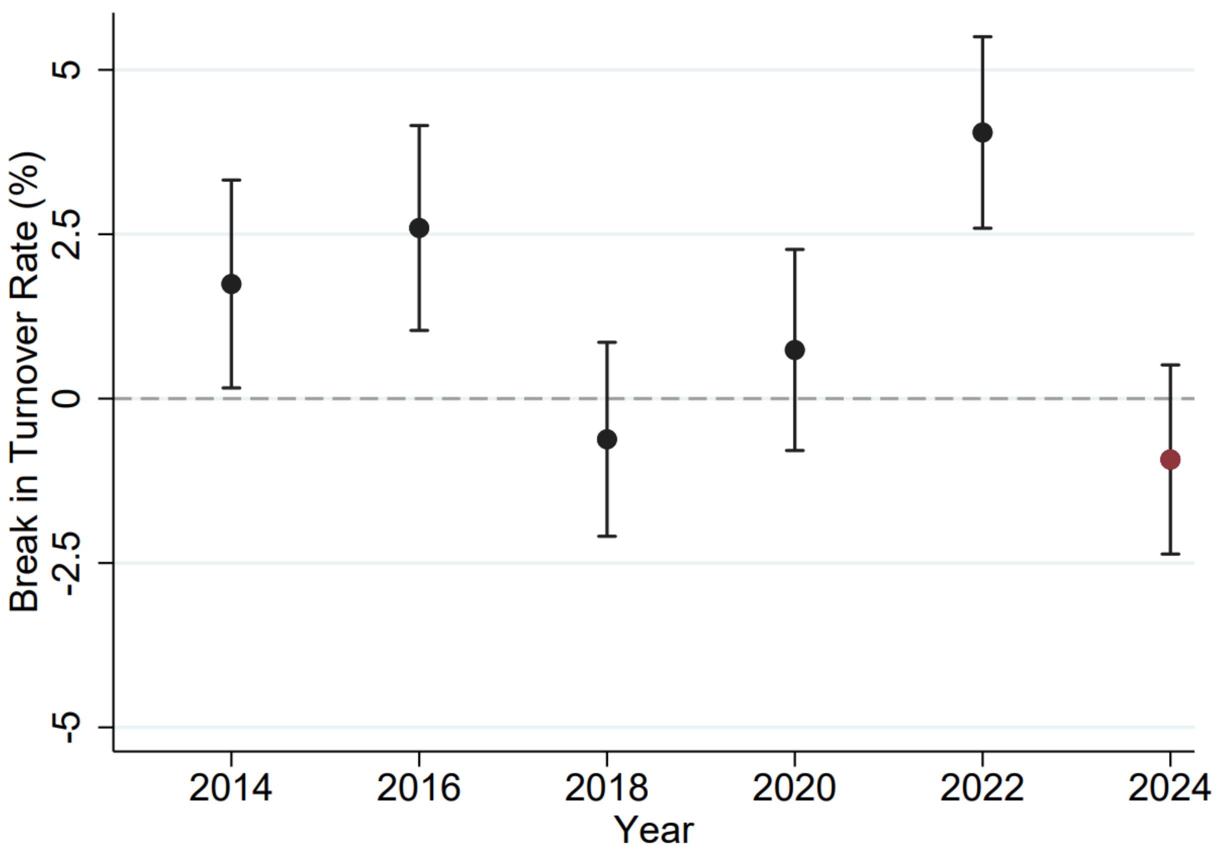
Table A.2 shows our division of states based on how much authority is vested in the selected local election official.

Table A.2: **Local Election Official Responsibilities Division.**

Descriptor	Description	Examples	States	In Analysis?
Sole authority	Election official does everything	All election responsibilities done by county clerk (CO)	AK, CA, CO, HI, ID, IL, IA, KS, ME, MO, MT, NE, NV, ND, OR, SD, UT, WA, WY	Yes
Strong authority	Election official does nearly everything	Separate canvassing board (FL) Shares limited authority with county legislature (WV) Shares limited authority with election board it chairs (IN)	FL, IN, WV	Yes
Primary authority	Election official does majority of admin responsibilities	Separate registration board or absentee voting official (AL, NM, TX, VT); Separate election board that has some responsibilities (AZ, KY, PA, RI, TX)	AL, AZ, AR, KY, LA, MA, MN, NM, PA, RI, TX, VT, VA, WI	Yes
Weak authority	Partisan elected official has	Does not administer Election Day voting (MS) Subservient to Board of Election (GA, MD, NJ, NY, NC, OH, OK, SC, TN) Most responsibilities carried out by municipal official (MI)	CT, DE, GA, MD, MI, MS, NH, NJ, NY, NC, OH, OK, SC, TN	No

This table divides states based on the amount of responsibility the individual chief local official captured in the data has in administering elections. Where there is within-state variation in the presence of other officials, the modal case for each state is shown.

Figure A.1: **Breaks in Election Official Turnover Trends Over Time**. Each point reports a break in the turnover rate in a given election from the pre-existing trend estimated using linear regression. The lines extending from the points are 95% confidence intervals based on standard errors clustered by jurisdiction.



A.2 Characterizing the Magnitude of the Post-2020 Increase in Turnover

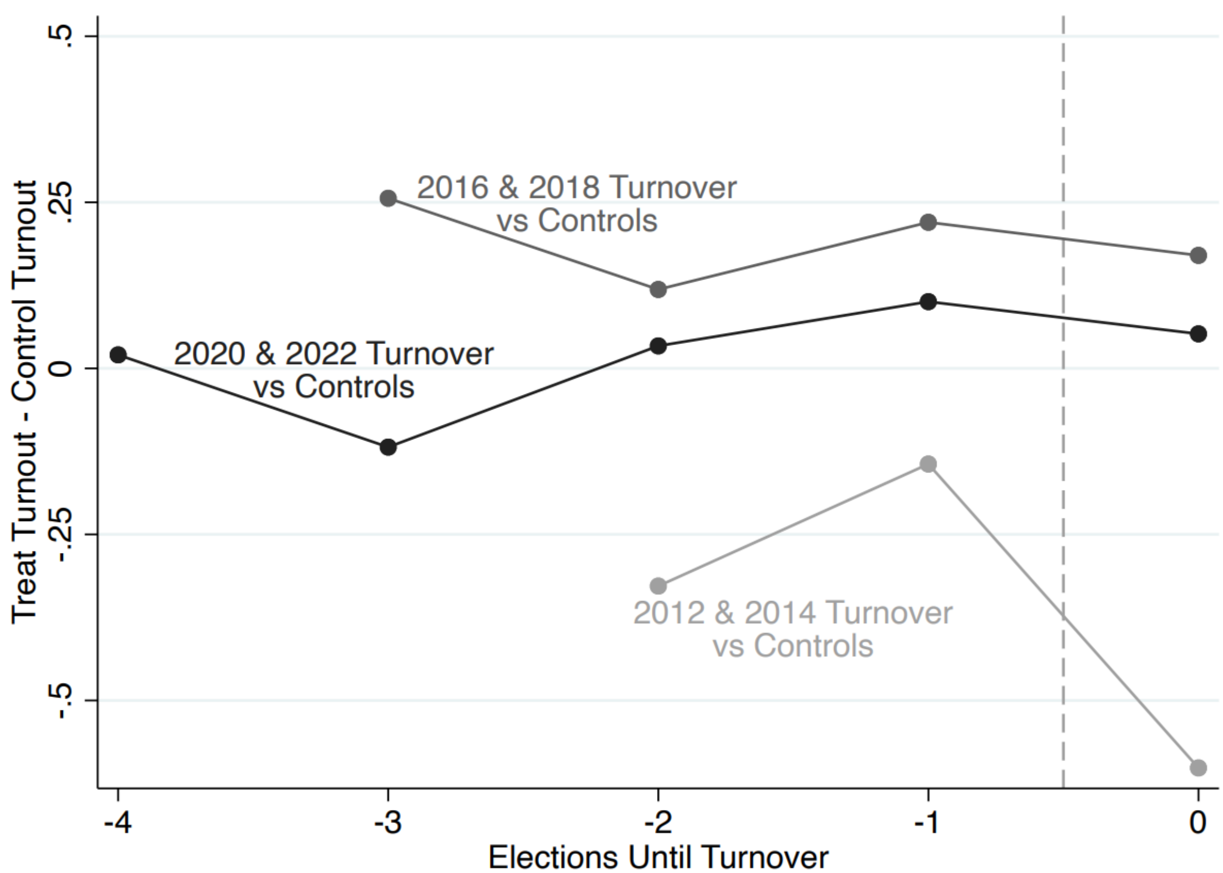
To assess whether the trend break we observe in 2022 is out of the ordinary, we conduct two analyses. First, we use a simple linear regression to predict the turnover rate in 2022 using data from 2004 to 2020 and ask whether observed turnover in 2022 is statistically distinguishable from the turnover rate predicted by the observed trend. Second, we extend this analysis back in time, asking whether observed turnover in 2014, 2016, 2018, and 2020 is noticeably higher or lower than the trend in turnover prior to that year would predict.

Figure A.1 presents the results of our analysis of trend breaks. We find that, among the last six election cycles from 2014 to 2024, 2022 is the largest break in election official turnover, and it is statistically distinguishable from the existing trend. However, it is only modestly larger than other recent breaks in the trend. For example, while turnover was 4 percentage points higher in 2022 than expected, turnover was also 2.6 percentage points higher than expected in 2016 based on existing trends, and the observed turnover in both 2014 and 2016 is also statistically distinguishable from the trend.

A.3 Validating the Matched Turnover Analysis

As we discuss in Section 4.1, we use matching to ensure that jurisdictions that experience turnover and those that do not are on similar turnout and residual vote trajectories prior to the turnover. We conduct a number of complementary analyses to validate that the matching worked as expected.

Figure A.2: **Event Study Plot Comparing Turnout in Jurisdictions with Turnover to their Matched Controls in Pre-Treatment Period**. The plot presents average turnout in every period prior to treatment for jurisdictions with turnover against their matched controls. The three lines capture whether the turnover happened late enough to enable matching on two (2012 and 2014), three (2016 and 2018), or four (2020 and 2022) pre-treatment elections. The plot only includes officials in jurisdictions where the election official has authority over all or nearly all election-related matters.



First, Figure A.2 presents an event study plot that captures the average differences between the jurisdictions with turnover and their matched controls prior to the turnover. Since our data starts in 2004, our matching for turnover prior to the 2012 election relies only on turnout in 2004 and 2008 whereas our matching for turnover prior to the 2020 election relies on turnout in 2004, 2008, 2012, and 2016. To capture these differences, we display one line for each analysis based on the number of pre-treatment periods available. We find that the average differences between treatment and control within each analysis are small ranging from -.31 percentage points and .25 percentage points. These differences also roughly cancel out, resulting in average pre-treatment difference of -0.01 percentage points between the treated and control jurisdictions. Finally, the event study plot also reveals that the differences between the treated and control jurisdictions are approximately flat over the pre-treatment period, implying that the match is balancing the average turnout trajectory of the treatment and control jurisdictions as well.

Figure A.3 presents a histogram of turnout in the jurisdictions with turnover and their matched controls prior to the turnover being studied. The matching produces similar distributions.

Finally, in Table A.3 we present a placebo analysis that evaluates whether the matching approach. In this analysis, we exclude from matching the cycle prior to the turnover we are studying. By holding it out, we can check whether the jurisdiction with turnover and their matched controls have similar turnout and residual vote in the election prior to turnover under study. This need not be the case—the matching could be doing a bad job of adjusting for latent differences in turnout rates between the treated and control jurisdictions, or, if election officials are selected based on performance, turnover may be preceded by an unexpected drop in turnout. Instead, we find across all of our analysis that our estimates are similar in magnitude to the estimates we present in our main analyses, suggesting that the matching is working properly and election officials are not typically leaving immediately following poor performance.

Figure A.3: **Comparing Turnout in Jurisdictions with Turnover to their Matched Controls in Pre-Treatment Period**. The plot presents histograms of turnout in the pre-turnover period for jurisdictions with turnover against their matched controls. Grey bars present the turnout distribution for the jurisdictions with turnover. The clear bars with black outline present the turnout distribution for the matched control jurisdictions. The plot only includes officials in jurisdictions where the election official has authority over all or nearly all election-related matters.

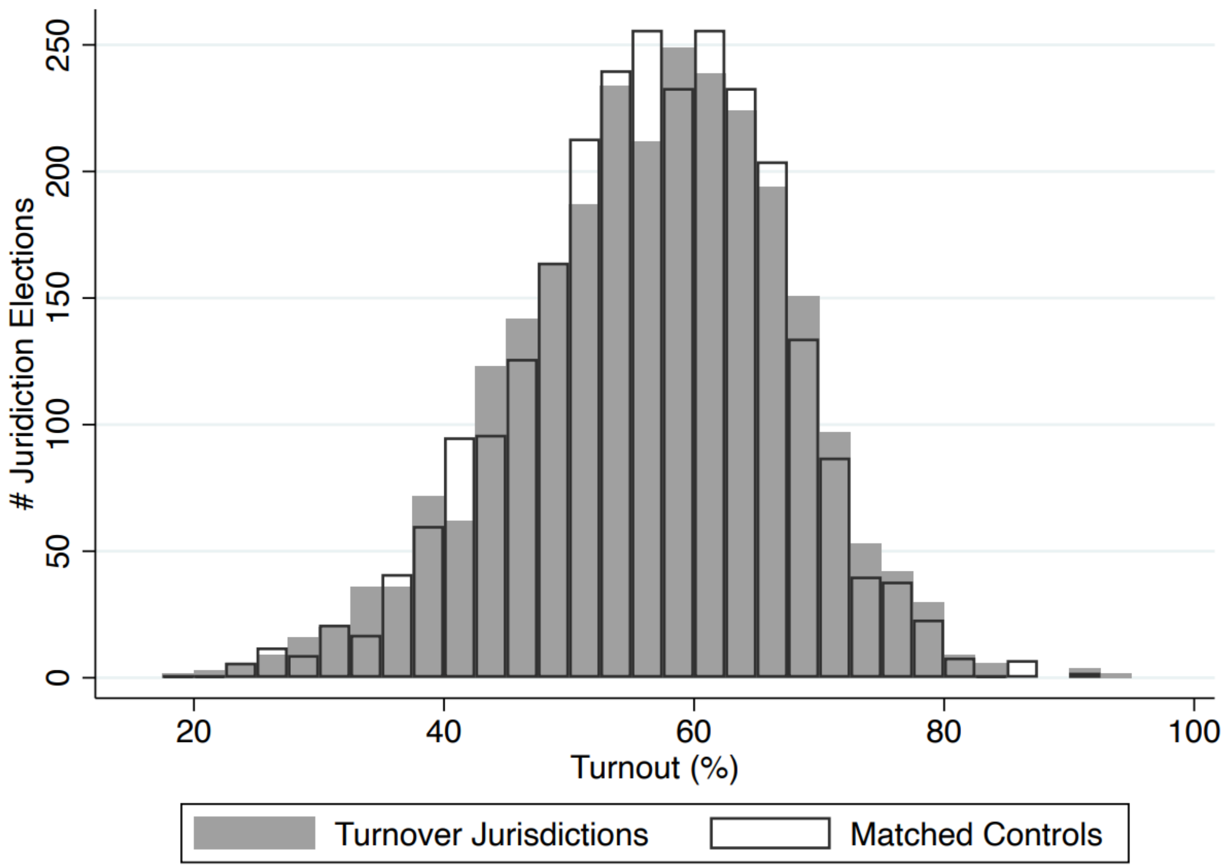


Table A.3: **Effect of Election Official Turnover on Turnout and Residual Vote.**

	Turnout (%)		Residual Vote (%)	
	(1)	(2)	(3)	(4)
Turnover, t+1	-0.09	-0.02	0.01	-0.05
	(0.09)	(0.12)	(0.06)	(0.06)
# Jurisdictions	3,201	978	1,597	863
# Obs	16,980	5,248	8,398	4,466
Strong Official Only	No	Yes	No	Yes
Matched Sample	Yes	Yes	Yes	Yes
Juris-by-Elec Type FE	Yes	Yes	Yes	Yes
Pair-by-Year FE	Yes	Yes	Yes	Yes

Robust standard errors clustered by jurisdiction reported in parentheses. Data is limited to jurisdictions with one primary official in charge of the majority of election administration responsibilities. Strong official only indicates jurisdictions where one official is responsible for directing all or nearly all aspects of election administration. Turnover t+1 refers to a change in the election official prior to the election four years later. Matched sample limits data to jurisdictions that experienced turnover between 2012 and 2022 and a set of matched control jurisdictions from the same state with the same history of turnover and the most similar levels of the outcome in all prior elections. Matching is 1-to-1 with replacement. Matching is conducted using outcomes from the start of the data until two cycles prior to the turnover being studied (prior to the placebo turnover year). Turnout is measured as the share of voting-age residents who cast a vote for president in presidential years and for governor in midterm years. Residual vote is measured as the the share of ballots cast without a vote for president in presidential years and for governor in midterm years. Regressions on unmatched data include jurisdiction-by-election cycle (presidential or midterm) fixed effects and state-by-year fixed effects. Regressions on matched data include jurisdiction-by-election cycle fixed effects and matched pair-by-year fixed effects.

A.4 Similar Effects of Turnover by Authority Level

If election official turnover were leading to substantially lower turnout, we would expect this effect to be largest in places where the election official we study oversees all aspects of elections. In Table A.4, we present estimates of the effect of turnover on turnout by authority level. We find that turnover of officials with weak, primary, and sole authority over local election administration does not substantially reduce turnout, meaning that we are not missing a large effect by including many jurisdictions in our analysis where officials do not have sufficient authority.

The one potential exception to this is for officials we categorize as “strong”. These officials are responsible for nearly all but not all election duties. For example, supervisors of elections in Florida oversee all aspects of elections except for canvassing. In Indiana, clerks are responsible for day-to-day election administration leadership, and clerks chair the election board and appoint its members (one from each party), but appointed board members could work together to block policy changes from the clerk. In one of our two analyses, we find that turnover leads to a noisy but substantial and statistically significant drop in turnout in strong-authority jurisdictions.

Since we find a precise null effect of turnover on turnout for sole-authority officials, and we have very few jurisdictions where the official has nearly all but not all authority, making the analysis imprecisely estimated, we suspect this is a noisy overestimate of the effect.

Table A.4: **Effect of Election Official Turnover on Turnout by Author**

	Turnout (%)							
	Weak		Primary		Strong		Sole	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Turnover	-0.02	0.10	-0.12	-0.15	-0.19	-0.90	0.04	0.02
	(0.06)	(0.10)	(0.08)	(0.12)	(0.13)	(0.36)	(0.08)	(0.14)
# Jurisdictions	1,343	1,159	2,880	2,202	203	138	976	843
# Obs	12,385	9,314	18,570	15,468	1,350	886	8,325	6,110
Outcome Mean	53.73	52.30	61.23	61.32	52.14	49.51	56.68	57.81
Min Detectable Effect	0.15	0.29	0.22	0.35	0.37	1.02	0.21	0.40
Matched Sample	No	Yes	No	Yes	No	Yes	No	Yes
Juris-by-Elec Type FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-by-Year FE	Yes	No	Yes	No	Yes	No	Yes	No
Pair-by-Year FE	No	Yes	No	Yes	No	Yes	No	Yes

Robust standard errors clustered by jurisdiction reported in parentheses. Columns 1 and 2 limit data to elections officials with weak authority. Columns 3 and 4 limit data to election officials who are the primary official responsible for the majority of election administration duties, but share important responsibilties with other officials. Columns 5 and 6 limit data to election officials who are responsible for most but not all election administration in their jurisdiction. Columns 7 and 8 limit data to election officials who are responsible for all election administration in their jurisdiction. Matched sample limits data to jurisdictions that experienced turnover between 2012 and 2022 and a set of matched control jurisdictions from the same state with the same history of turnover and the most similar levels of the outcome in all prior elections using 1-to-1 matching with replacement. Turnover refers to a change in the election official since the election two years prior. Turnout is measured as share of voting-age residents who cast a vote for president in presidential years and for governor in midterm years. Residual vote is measured as the the share of ballots cast without a vote for president in presidential years and for governor in midterm years. Regressions on unmatched data include jurisdiction-by-election cycle (presidential or midterm) fixed effects and state-by-year fixed effects. Regressions on matched data include jurisdiction-by-election cycle fixed effects and matched pair-by-year fixed effects. Min detectable effect refers to the minimum effect that a two-sided test with a 0.05 alpha would have 80% power to detect.

A.5 Similar Effects of Turnover in Presidential and Gubernatorial Elections

One challenge with focusing on presidential elections is that citizens may be especially motivated to participate and find ways to vote even if the election official makes mistakes or erects needless barriers. Might our pooled results mask an effect in midterm elections when citizens often feel less motivated to vote? To investigate whether this explains our small estimates of the effect of turnover on turnout, we conduct separate analyses of presidential cycle and gubernatorial cycle election years. We focus our analysis on jurisdictions where the chief election official has sole authority over election administration.

In Table A.5 we present estimates of the effect of turnover on turnout and residual vote separately for gubernatorial and presidential elections. Columns 1, 2, 5, and 6 present our estimates of the effect on turnout and residual vote in presidential elections. As in our main analysis in Table 1, looking at both two-way fixed effects regression estimates and matching estimates, we find that turnover leads to at most a small drop in turnout and a very modest increase in residual vote. Our estimates of the effects in midterms are less precise because we rely on gubernatorial elections and some states hold these during presidential election years. Nevertheless, the evidence suggests that turnover is not causing turnout to drop by more than three-quarters of a percentage point and is not causing residual vote to increase by more than one-third of a percentage point. Our confidence intervals from our two-way fixed effects regressions of the effect on turnout do not contain effects larger than 0.30 percentage points in either midterm or presidential elections, and our largest point estimate is a noisy decrease in turnout of 0.25 percentage points.

Table A.5: **Effect of Election Official Turnover on Turnout and Residual Vote, Midterm vs General.**

	Turnout (%)				Residual Vote (%)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Turnover	0.01	-0.05	0.01	-0.25	0.07	-0.01	0.00	0.05
	(0.08)	(0.16)	(0.15)	(0.26)	(0.04)	(0.06)	(0.05)	(0.13)
# Jurisdictions	1,181	778	758	327	966	668	653	303
# Obs	5,905	5,104	3,790	1,892	4,830	4,296	3,265	1,682
Outcome Mean	61.33	60.64	48.00	46.29	1.51	1.65	1.87	1.58
Min Detectable Effect	0.21	0.44	0.41	0.74	0.10	0.16	0.13	0.37
Cycle	Pres	Pres	Mid	Mid	Pres	Pres	Mid	Mid
Matched Sample	No	Yes	No	Yes	No	Yes	No	Yes
Juris-by-Elec Type FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-by-Year FE	Yes	No	Yes	No	Yes	No	Yes	No
Pair-by-Year FE	No	Yes	No	Yes	No	Yes	No	Yes

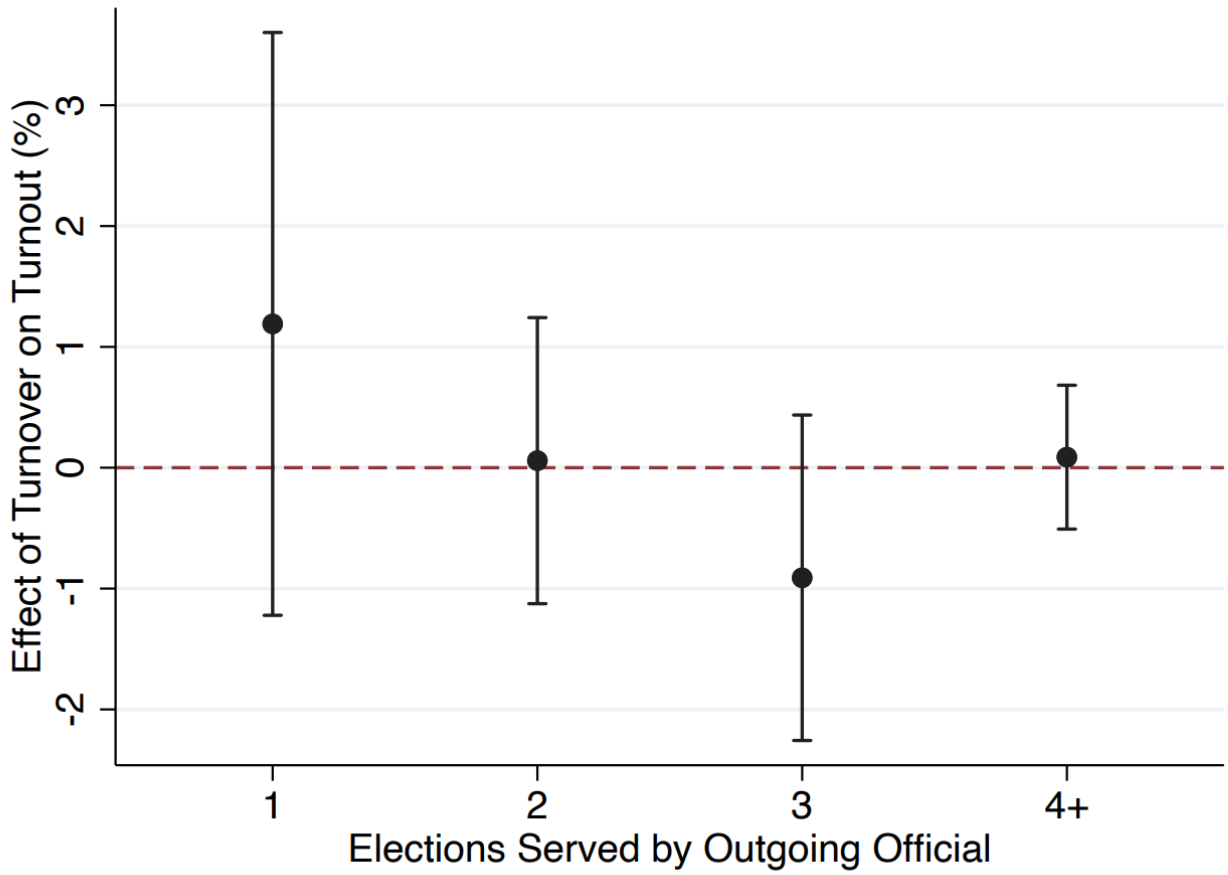
Robust standard errors clustered by jurisdiction reported in parentheses. Data is limited to jurisdictions where one official is responsible for directing all or nearly all aspects of election administration. Cycle is either presidential or midterm with midterms limited to states with midterm gubernatorial elections. Matched sample limits data to jurisdictions that experienced turnover between 2012 and 2022 and a set of matched control jurisdictions from the same state with the same history of turnover and the most similar levels of the outcome in all prior elections using 1-to-1 matching with replacement. Turnover refers to a change in the election official since the election two years prior. Turnout is measured as the share of voting-age residents who cast a vote for president in presidential years and for governor in midterm years. Residual vote is measured as the the share of ballots cast without a vote for president in presidential years and for governor in midterm years. Regressions on unmatched data include jurisdiction-by-election cycle (presidential or midterm) fixed effects and state-by-year fixed effects. Regressions on matched data include jurisdiction-by-election cycle fixed effects and matched pair-by-year fixed effects. Min detectable effect refers to the minimum effect that a two-sided test with a 0.05 alpha would have 80% power to detect.

A.6 Similar Effects of Turnover When Exiting Official Had More vs Less Experience

If turnover is common in some offices and uncommon in others, many new officials will replace individuals who had yet to accrue significant experience. Might this mean our estimates understate the disruption when experienced officials exit? To investigate this, we extend the analysis we presented in column 4 of Table 1. We estimate the effect of turnover on turnout using our matched data with only jurisdictions where the election official has all or nearly all authority. We then limit our data to cases where the previous election official served in a given number of November elections.

We find that, regardless of whether the previous official served only briefly or for a long time, election official turnover does not noticeably decrease turnout. While these estimates are noisy, we take this as suggestive evidence that our main finding is not masking a much larger effect when a veteran election official leaves.

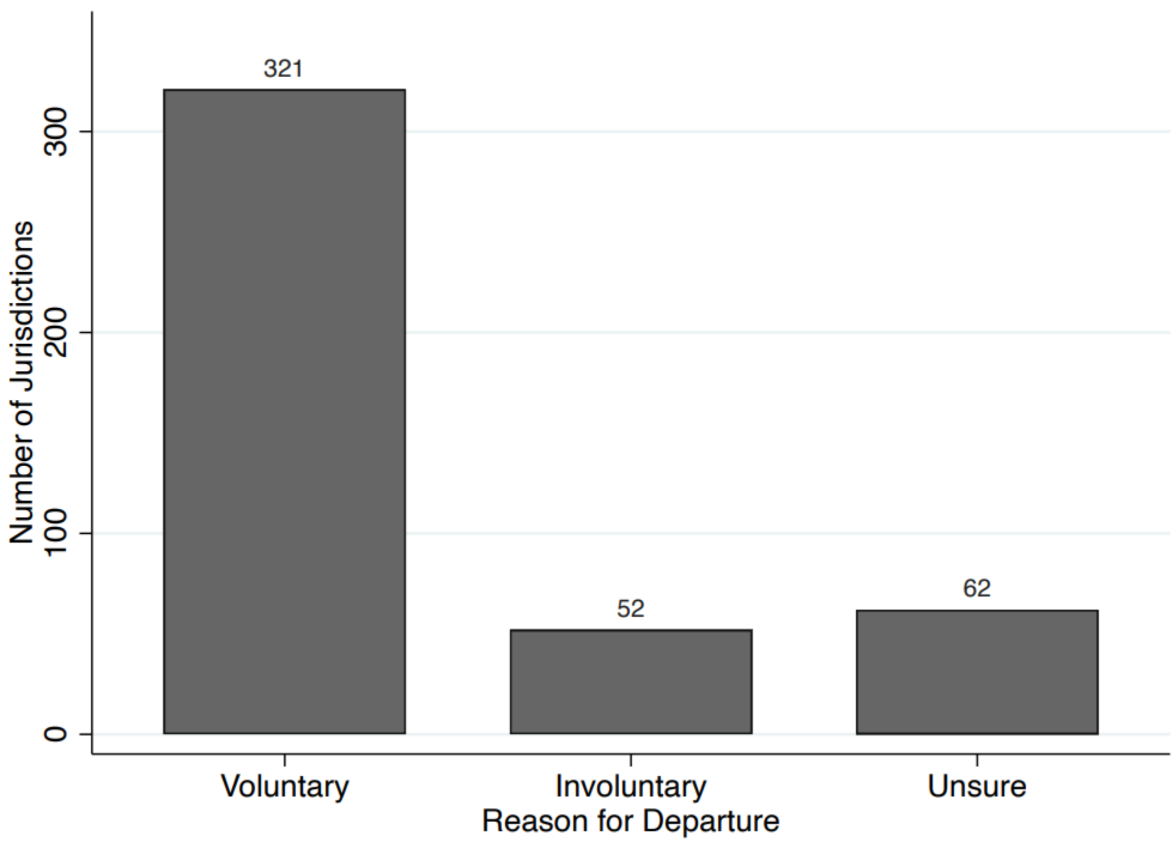
Figure A.4: **Similarly Small Effect of Turnover When Exiting Official Had Longer Tenure**. Each point represents a point estimate based on the matched analysis data, limiting to jurisdictions where the election official is responsible for all or nearly all election administration and those that had turnover after a given number of terms without turnover. The bars represent 95% confidence intervals.



A.7 Turnover Does Not Have a Larger Effect When Election Officials Depart Voluntarily

One concern about our main analysis is that it may average over two effects that run in opposite directions: perhaps turnover has a negative effect when a good official leaves and a positive effect with a bad official leaves. In this case, we might see an average effect close to zero depending on how many good and bad performers end up in office.

Figure A.5: **Reason for Election Official Departure, 2020 and 2022**. Out of the 373 cases of election official turnover prior to the 2020 and 2022 elections where the reason for departure is publicly available, 321 (86%) of the departures were voluntary. Voluntary includes retiring or leaving for a new position. Involuntary includes being fired, being voted out of office, and resigning in scandal. Unsure are cases where there is no public reporting on the departure and the office did not provide a reason when contacted.



Here, we proxy for election official quality using information on why they left office. In Figure A.5, we present evidence that the vast majority of election officials leave office voluntarily, either by retiring or choosing not to run again. This means that our estimates of the average effect of turnover are mostly capturing voluntary turnover. Given this, the effect of the departures of low-quality officials would need to be very positive to be consistent with a small negative effect of people who left voluntarily. We directly estimate these effects in Table A.6. Subsetting to cases where the election official left voluntarily, we find that, if anything, turnout increases. This suggests that our average estimates are not masking large positive effects of the departures of low performers and substantial negative effects from the departures of high performers.

Table A.6: **Effect of Election Official Turnover on Turnout by Reason for Departure.**

	Turnout (%)	
	Any Departure Reason	Left Voluntarily
	(1)	(2)
Turnover	0.01	0.20
	(0.27)	(0.29)
# Jurisdictions	427	374
# Obs	2,520	2,180
Outcome Mean	57.81	57.94
Min Detectable Effect	0.75	0.80
Matched Sample	Yes	Yes
Juris-by-Elec Type FE	Yes	Yes
Pair-by-Year FE	Yes	Yes

Robust standard errors clustered by jurisdiction reported in parentheses. Data is limited to jurisdictions where one official is responsible for directing all or nearly all aspects of election administration. Matched sample limits data to jurisdictions that experienced turnover between 2012 and 2022 and a set of matched control jurisdictions from the same state with the same history of turnover and the most similar levels of the outcome in all prior elections using 1-to-1 matching with replacement. Turnover refers to a change in the election official since the election two years prior. Turnout is measured as share of voting-age residents who cast a vote for president in presidential years and for governor midterm years. Regressions on matched data include jurisdiction-by-election cycle fixed effects and matched pair-by-year fixed effects. Min detectable effect refers to the minimum effect that a two-sided test with a 0.05 alpha would have 80% power to detect. First column restricts data to jurisdictions and years where the reason that the election official left office is known and matched controls. Second column restricts data to jurisdictions and years where the election official left voluntarily and matched controls.

A.8 Similar Effects of Turnover for Elected and Appointed Election Officials

In Table A.7, we present estimates of the effect of turnover on turnout separately for elected and appointed officials. Columns 1 through 4 present estimates of the effect in jurisdictions that directly elect their election official. Columns 5 and 6 present estimates of the effect in jurisdictions that appoint their election official. Across all of our analyses, we find that turnover does not cause a substantial drop in turnout.

Table A.7: **Effect of Election Official Turnover on Turnout, Elected vs Appointed**

	Turnout (%)					
	Elected				Appointed	
	(1)	(2)	(3)	(4)	(5)	(6)
Turnover	-0.06	-0.07	0.00	-0.12	-0.00	-0.26
	(0.06)	(0.11)	(0.07)	(0.14)	(0.21)	(0.52)
# Jurisdictions	1,681	1,417	1,131	940	121	77
# Obs	13,785	10,166	9,245	6,688	740	444
Outcome Mean	54.16	55.18	56.49	57.30	55.88	57.59
Min Detectable Effect	0.16	0.32	0.19	0.39	0.60	1.47
Strong Official Only	No	No	Yes	Yes	No	No
Matched Sample	No	Yes	No	Yes	No	Yes
Juris-by-Elec Type FE	Yes	Yes	Yes	Yes	Yes	Yes
State-by-Year FE	Yes	No	Yes	No	Yes	No
Pair-by-Year FE	No	Yes	No	Yes	No	Yes

Robust standard errors clustered by jurisdiction reported in parentheses. Columns 1 through 4 limit data to irectly elected election officials. Columns 3 and 4 limit data to appointed election officials. Matched sample limits data to jurisdictions that experienced turnover between 2012 and 2022 and a set of matched control jurisdictions from the same state with the same history of turnover and the most similar levels of the outcome in all prior elections using 1-to-1 matching with replacement. Turnover refers to a change in the election official since the election two years prior. Turnout is measured as share of voting-age residents who cast a vote for president in presidential years and for governor in midterm years. Residual vote is measured as the the share of ballots cast without a vote for president in presidential years and for governor in midterm years. Regressions on unmatched data include jurisdiction-by-election cycle (presidential or midterm) fixed effects and state-by-year fixed effects. Regressions on matched data include jurisdiction-by-election cycle fixed effects and matched pair-by-year fixed effects. Min detectable effect refers to the minimum effect that a two-sided test with a 0.05 alpha would have 80% power to detect.

While we can fully reproduce our main analyses subsetting to elected officials only, our analyses of appointed officials is more limited. Appointed officials in our data very rarely have authority to oversee all or nearly all aspects of elections, so do not have sufficient data to subset to appointed officials with strong authority. Instead, we subset to those who are responsible for at least a majority of election administration and registration duties.

A.9 Similar Effect on Turnout in Small and Large Jurisdictions

One explanation for our finding is that staff maintain the operations of the office despite leadership turnover. According to a 2022 survey by the Election and Voting Information Center at Reed College, more than 96% of jurisdictions with more than 25,000 voting age residents have at least two staffers while 26% of jurisdictions with 5,000 to 25,000 voting-age residents have one or no full-time officials and 75% of jurisdictions with fewer than 5,000 voting-age residents have one or no full-time officials.[101] Accordingly, we use a rough population cutoff to evaluate whether turnover has larger effects in jurisdictions with larger offices. Table A.8 presents our results. We find that the effects of election official turnover on turnout are small in small and large jurisdictions alike.

[101] https://evic.reed.edu/2022_workload-and-staffing/

Table A.8: **Effect of Election Official Turnover on Turnout and Residual Vote by Population.**

	Turnout (0-100%)			
	2020 Pop < 25k		2020 Pop >= 25k	
	(1)	(2)	(3)	(4)
Turnover	0.07	-0.18	-0.08	-0.07
	(0.09)	(0.09)	(0.17)	(0.21)
# Jurisdictions	802	377	664	285
# Obs	6,585	3,090	4,560	2,046
Outcome Mean	57.74	52.44	58.67	53.38
Min Detectable Effect	0.25	0.25	0.49	0.59
Strong Official Only	Yes	Yes	Yes	Yes
Matched Sample	No	Yes	No	Yes
Juris-by-Elec Type FE	Yes	Yes	Yes	Yes
State-by-Year FE	Yes	No	Yes	No
Pair-by-Year FE	No	Yes	No	Yes

Robust standard errors clustered by jurisdiction reported in parentheses. Columns 1 and 2 are limited to jurisdictions with fewer than 25,000 voting-age residents as of the 2020 census. Columns 3 and 4 are limited to jurisdictions with 25,000 or more voting-age residents as of the 2020 census. Data is limited to jurisdictions where only one official is responsible for directing all or nearly all aspects of election administration. Matched sample limits data to jurisdictions that experienced turnover between 2012 and 2022 and a set of matched control jurisdictions from the same state with the same history of turnover and the most similar levels of the outcome in all prior elections using 1-to-1 matching with replacement. Turnover refers to a change in the election official since the election two years prior. Turnout is measured as the share of voting-age residents who cast a vote for president in presidential years and for governor in midterm years. Regressions on unmatched data include jurisdiction-by-election cycle (presidential or midterm) fixed effects and state-by-year fixed effects. Regressions on matched data include jurisdiction-by-election cycle fixed effects and matched pair-by-year fixed effects. Min detectable effect refers to the minimum effect that a two-sided test with a 0.05 alpha would have 80% power to detect.

A.10 Alternative Approach to Estimating Effect of Turnover on Problems Voting

In Section 4.5, we present evidence that turnover does not substantially increase the share of voters experiencing problems. One weakness of this analysis is that anyone who fails to vote due to issues with election administration will not be counted in this analysis. We use this as our main measure of problems for two reasons: First, the SPAE does not consistently measure the problems people had trying to vote for those who failed, so we lose 2020 and 2022 in analyses that use a pooled measure of problems voting that includes voters and nonvoters. Second, our main finding is that turnover does not reduce turnout, so we should not see difference in the share of respondents who voted in places with and without turnover. Still, to ensure we are not missing an important change in the experience of nonvoters, we produce a measure of problems voting that includes voters and nonvoters prior to 2020. Table A.9 presents our results. Our estimates are noisier and more positive, suggesting that nonvoters may be slightly more likely to report election administration issues when election leadership turns over, but the effect estimates are still small and statistically indistinguishable from zero.

Table A.9: **Effect of Election Official Turnover on Share of Voters and Non-Voters Reporting Problems Voting.**

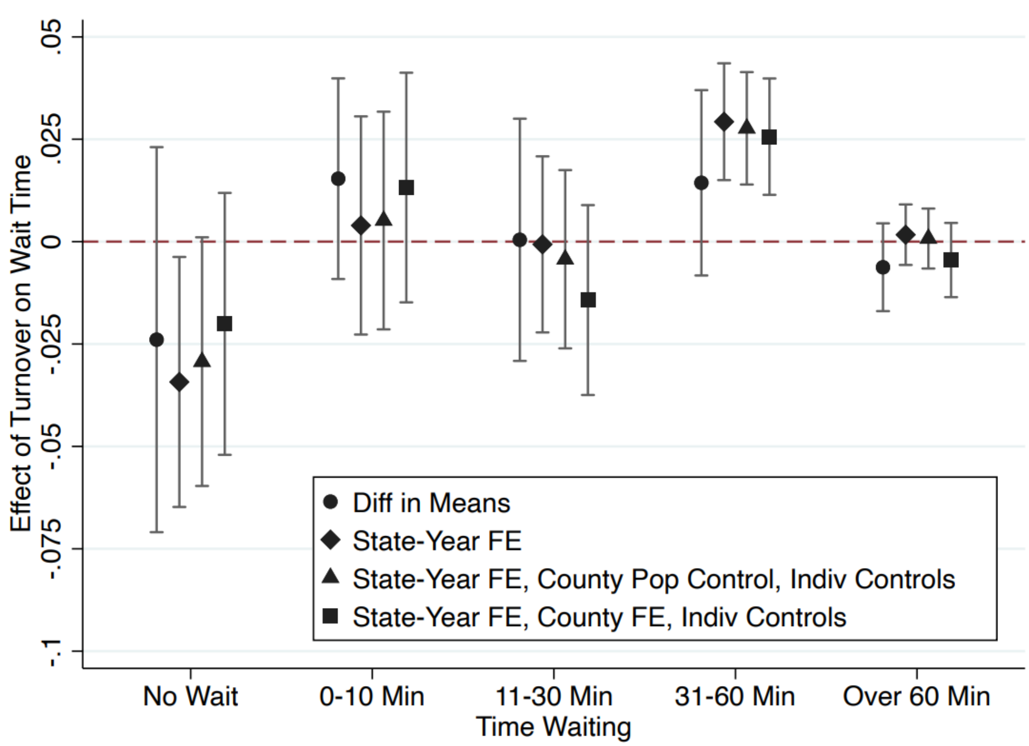
	Reported Problem Trying to Vote {0,1}			
	(1)	(2)	(3)	(4)
Turnover	0.007	0.010	0.011	0.010
	(0.006)	(0.006)	(0.006)	(0.007)
# Counties	965	965	963	800
# Respondents	15,155	15,155	15,068	14,905
Outcome Mean	0.060	0.060	0.060	0.060
Min Detectable Effect	0.017	0.017	0.017	0.019
Strong Official Only	Yes	Yes	Yes	Yes
State-by-Year FE	No	Yes	Yes	Yes
Individual Controls	No	No	Yes	Yes
County Pop Control	No	No	Yes	No
County FE	No	No	No	Yes

Robust standard errors clustered by county reported in parentheses. Data is limited to counties where only one official is responsible for directing all or nearly all aspects of election administration. Each observation is one respondent to the Survey of the Performance of American Elections. Observations are weighted according to the weights provided by the survey team. Turnover refers to a change in the election official since the election two years prior. The outcome is a dummy variable with value 1 for respondents reporting a problem with the registration or the voting equipment, an issue obtaining or completing their mail ballot, or difficulty finding the polling place. Individual controls are gender, race, years of education, and party ID fixed effects as well as age included as a single covariate. County pop control is the natural logarithm of voting-age population. Min detectable effect refers to the minimum effect that a two-sided test with a 0.05 alpha would have 80% power to detect.

A.11 Turnover May Modestly Increase Wait Times

In this section we present additional analyses of the effect of turnover on wait times. Figure A.6 documents the robustness of our finding of a modest effect of turnover on wait times. Across all four of our regression specifications, we see a similar pattern where fewer voters report no wait in jurisdictions with turnover and more voters report wait times between 30 minutes and an hour. All of these effects are relatively small, and the only statistically significant change across most specifications is an increase in wait times over 30 minutes. Still, given the consistent pattern across different specifications, we take this as evidence that wait times may have modestly increased in places with new election officials.

Figure A.6: **Election Official Turnover May Modestly Increase Wait Times**. The figure presents estimates of the effect of turnover on the share of probability a resident experiences a given wait time. The estimates come from four different regression specifications: 1. no covariates (difference in means); 2. state-year fixed effects; 3. state-year fixed effects, county covariates, and respondent covariates; and 4. state-year fixed effects, county fixed effects, and respondent covariates. The plot relies on data from the Survey of the Performance of American Elections and is weighted using the weights constructed by the survey team.



In Table A.10, we present formal estimates of the effect of turnover on wait times. Columns 1 through 4 present the effect of turnover on the share of voters who wait more than 10 minutes.

Table A.10: **Effect of Election Official Turnover on Voter Wait Times.**

	Wait Over 10 Min {0,1}				Wait Over 30 Min {0,1}			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Turnover	0.009	0.030	0.024	0.007	0.008	0.031	0.028	0.021
	(0.026)	(0.014)	(0.014)	(0.014)	(0.015)	(0.009)	(0.008)	(0.009)
# Counties	930	930	929	756	930	930	929	756
# Respondents	13,212	13,212	13,167	12,994	13,212	13,212	13,167	12,994
Outcome Mean	0.230	0.230	0.230	0.232	0.077	0.077	0.077	0.078
Min Detectable Effect	0.073	0.040	0.039	0.040	0.041	0.024	0.023	0.025
Strong Official Only	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-by-Year FE	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Individual Controls	No	No	Yes	Yes	No	No	Yes	Yes
County Pop Control	No	No	Yes	No	No	No	Yes	No
County FE	No	No	No	Yes	No	No	No	Yes

Robust standard errors clustered by county reported in parentheses. Data is limited to counties where only one official is responsible for directing all or nearly all aspects of election administration. Each observation is one respondent to the Survey of the Performance of American Elections who reported voting. Observations are weighted according to the weights provided by the survey team. Turnover refers to a change in the election official since the election two years prior. The outcome is a dummy variable with value 1 for reporting a wait time over 10 or 30 minutes, respectively. Individual controls are gender, race, years of education, and party ID fixed effects as well as age included as a single covariate. County pop control is the natural logarithm of voting-age population. Min detectable effect refers to the minimum effect that a two-sided test with a 0.05 alpha would have 80% power to detect.

When we adjust for county factors that may be associated with longer or shorter wait times in columns 3 and 4, we cannot reject the null hypothesis that counties with turnover and similar counties without turnover have the same shares of voters waiting over 10 minutes at the polls. In columns 5 through 8 we change the outcome to look at wait times over 30 minutes. Here, as we show in Figure A.6, we find that turnover is associated with approximately two percentage points more voters waiting over 30 minutes than we would have expected in similar counties in the same state and year.

In Table A.11, we validate our survey-based estimates using a measure of polling place wait times presented in Chen et al. (2020). The data is only available for 2016, so our estimates are noisy and we cannot conduct the more robust within-county analysis. Still, the descriptive patterns are similar to those we see in the survey data—we see a small increase in wait times in counties with election official turnover. Due to the small size of the effect and the relatively small dataset, the standard errors are large. This is consistent with what we expect given the high correlation between survey and cell-phone-tracking based estimates of wait times (Chen et al. 2020).

Table A.11: **Effect of Election Official Turnover on Wait Times, Cell Phone Tracking Data.**

	Avg Wait Time (Minutes)		
	(1)	(2)	(3)
Turnover	1.404	0.371	0.229
	(1.124)	(0.909)	(0.880)
log(2020 Voting-Age Population)			1.306
			(0.354)
State FE	No	Yes	Yes
Counties	177	177	177

Robust standard errors reported in parentheses. Data is limited to counties where only one official is responsible for directing all or nearly all aspects of election administration. Turnover refers to a change in the election official since the election two years prior. The outcome is average wait time in the county according to cell phone tracking data reported in Chen et al (2020).

A.12 Turnover Does Not Reduce Voter Confidence

If voters do not have problems voting but still feel the election was administered poorly, this would likely show in their confidence that the vote was counted properly in their community. Using the same survey data and regression specifications as Table 2, we study whether turnover leads to fewer respondents saying they are very confident that their vote and the vote of other county residents was counted accurately. Table A.12 presents our results. We see consistent evidence that turnover does not meaningfully affect the share of respondents who are very confident their own vote or the county vote was accurately tallied.

Looking across all eight columns, we see consistent evidence that turnover does not meaningfully affect the share of respondents who are very confident their own vote or the county vote was accurately tallied. In fact, after adjusting state-specific factors, we find that slightly more respondents were very confident that their vote was counted accurately in counties experiencing turnover.

Table A.12: **Effect of Election Official Turnover on Share of Respondents Very Confident Vote is Counted Correctly.**

	Very Confident Vote Counted Correctly {0,1}							
	Own Vote				County Vote			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Turnover	-0.004	0.012	0.011	0.010	-0.015	0.002	0.001	-0.005
	(0.014)	(0.010)	(0.010)	(0.011)	(0.016)	(0.012)	(0.012)	(0.012)
# Counties	1,021	1,021	1,019	876	989	989	989	838
# Respondents	22,010	22,010	21,937	21,794	19,694	19,694	19,680	19,529
Outcome Mean	0.711	0.711	0.711	0.711	0.615	0.615	0.615	0.614
Min Detectable Effect	0.038	0.028	0.028	0.031	0.044	0.033	0.033	0.034
Strong Official Only	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-by-Year FE	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Individual Controls	No	No	Yes	Yes	No	No	Yes	Yes
County Pop Control	No	No	Yes	No	No	No	Yes	No
County FE	No	No	No	Yes	No	No	No	Yes

Robust standard errors clustered by county reported in parentheses. Data is limited to counties where only one official is responsible for directing all or nearly all aspects of election administration. Each observation is one respondent to the Survey of the Performance of American Elections who reported voting. Observations are weighted according to the weights provided by the survey team. Turnover refers to a change in the election official since the election two years prior. The outcome is a dummy variable with value 1 for respondents who say they are very confident the vote was counted as intended, 0 otherwise. Columns 1 through 4 report estimates for the effect of turnover on the respondent's confidence that their own vote was counted as intended. Columns 5 through 8 report estimates for the effect of turnover on the respondent's confidence that their county's vote was counted as intended. Individual controls are gender, race, years of education, and party ID fixed effects as well as age included as a single covariate. County pop control is the natural logarithm of voting-age population. Min detectable effect refers to the minimum effect that a two-sided test with a 0.05 alpha would have 80% power to detect.

A.13 Turnover Did Not Meaningfully Degrade Performance in 2020

Does turnover degrade government performance during more turbulent times? To investigate this possibility, we compare jurisdictions that experienced local election official turnover in 2020 to jurisdictions without these changes. We run two analyses. First, we use our panel matching approach to compare turnout and residual vote in jurisdictions with and without turnover between 2018 and 2020. Second, we compare the rate of mail voting issues and issues voting due to COVID in jurisdictions with and without turnover. Across both analyses, we find that turnover did not substantially affect performance amid the upheaval to election administration brought on by the COVID pandemic and the policy response in 2020.

Table A.13 presents the results of our turnout and residual vote analysis in 2020. Focusing on columns 2 and 4 where we limit our analysis to jurisdictions with officials who are responsible for all or nearly all election administration, we find that turnover led to an very small increase in turnout of around one-tenth of one percentage point and a decrease in residual vote of roughly one-tenth of one percentage point in 2020. Both of these estimates are statistically indistinguishable from zero. We take this as evidence that turnover did not meaningfully affect local election official performance even in 2020 when election officials faced a host of challenges and massive policy change.

Table A.14 presents the results of our analysis of problems voting by mail or failure to vote due to COVID in 2020. In columns 1 through 3 we document a very low rate of issues with mail voting—only approximately 2% of respondents say they had issues voting by mail. People living in jurisdictions with turnover had slightly higher rates of problems voting by mail—people living in jurisdictions with turnover were approximately three-quarters of a percentage point more likely to say that they had a problem voting by mail—but these effects are quite small and they are statistically indistinguishable from zero.

Table A.13: **Effect of Election Official Turnover on Turnout and Residual Vote in 2020.**

	Turnout (0-100%)		Residual Vote (0-100%)	
	(1)	(2)	(3)	(4)
Turnover	-0.09	0.11	-0.03	-0.08
	(0.18)	(0.29)	(0.05)	(0.07)
# Jurisdictions	1,433	359	572	281
# Obs	8,050	2,060	3,370	1,630
Outcome Mean	65.38	62.04	1.19	1.47
Min Detectable Effect	0.51	0.81	0.14	0.19
Strong Official Only	No	Yes	No	Yes
Matched Sample	Yes	Yes	Yes	Yes
Juris-by-Elec Type FE	Yes	Yes	Yes	Yes
Pair-by-Year FE	Yes	Yes	Yes	Yes

Robust standard errors clustered by jurisdiction reported in parentheses. Data is limited to jurisdictions with one primary official responsible for the majority of election administration duties. Strong official only indicates jurisdictions where one official is responsible for directing all or nearly all aspects of election administration. Matched sample limits data to jurisdictions that experienced turnover between 2012 and 2022 and a set of matched control jurisdictions from the same state with the same history of turnover and the most similar levels of the outcome in all prior elections using 1-to-1 matching with replacement. Turnover refers to a change in the election official since the election two years prior. Turnout is measured as share of voting-age residents who cast a vote for president in presidential years and for governor in midterm years. Residual vote is measured as the the share of ballots cast without a vote for president in presidential years and for governor in midterm years. Regressions on matched data include jurisdiction-by-election cycle fixed effects and matched pair-by-year fixed effects. Min detectable effect refers to the minimum effect that a two-sided test with a 0.05 alpha would have 80% power to detect.

It is important to note why we interpret these effect sizes as small: A problem voting by mail in our data case does not need to be serious to be recorded here—respondents were asked whether they had a problem obtaining or returning a mail ballot, so even if the problems were minor inconveniences, we might expect people to share that in they survey. Yet, we still see very few people registering these complaints in their survey.

Columns 4 through 6 of Table A.14 presents our findings on the share of people who reported not voting because they were worried about COVID risk. Respondents who said they did not vote were asked for the top two reasons they did not vote. We count anyone who says COVID was one of their two reasons as having not voted due to COVID. We find that only roughly 2% of respondents said that they failed to vote because of COVID risks. The number is slightly higher in jurisdictions with turnover—an increase of roughly half of a percentage point—but this difference is statistically indistinguishable from zero. We still interpret this as a relatively modest effect but substantively larger and noisier than our estimate of the effect on problems voting by mail since it is in a sense an estimate of the effect on turnout through one mechanism. Still, paired with our estimate of the effect of turnover on turnout in Table A.13, we conclude that the turnout effects and effects on COVID risks are small.

Table A.14: **Effect of Election Official Turnover on Share of Voters Reporting Problems Voting in 2020.**

	Reported Problem Voting by Mail {0,1}			Didn't Vote Due to COVID {0,1}		
	(1)	(2)	(3)	(4)	(5)	(6)
Turnover	0.006	0.008	0.008	0.003	0.006	0.006
	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
# Counties	717	717	717	717	717	717
# Respondents	5,982	5,982	5,982	5,982	5,982	5,982
Outcome Mean	0.019	0.019	0.019	0.018	0.018	0.018
Min Detectable Effect	0.020	0.017	0.017	0.017	0.016	0.016
Strong Official Only	Yes	Yes	Yes	Yes	Yes	Yes
State FE	No	Yes	Yes	No	Yes	Yes
Individual Controls	No	No	Yes	No	No	Yes
County Pop Control	No	No	Yes	No	No	Yes

Robust standard errors clustered by county reported in parentheses. Data is limited to 2020. Data is also limited to counties where only one official is responsible for directing all or nearly all aspects of election administration. Each observation is one respondent to the Survey of the Performance of American Elections who reported voting. Observations are weighted according to the weights provided by the survey team. Turnover refers to a change in the election official since the election two years prior. The outcome is a dummy variable with value 1 for respondents reporting a problem voting by mail or failing to vote due to fear of the COVID risk. Individual controls are gender, race, years of education, and party ID fixed effects as well as age included as a single covariate. County pop control is the natural logarithm of voting-age population. Min detectable effect refers to the minimum effect that a two-sided test with a 0.05 alpha would have 80% power to detect.

A.14 New Officials without Elections or Government Experience Do Not Perform Noticeably Worse

If elections or government experience is necessary to maintain office performance, we would expect performance to degrade most when a new official comes in without that experience. We explore this by subsetting our analysis to cases where the incoming official has either no experience administering elections or no experience working in government and using our panel matching approach to estimate the effect of turnover. Table A.15 captures our results.

We find that turnover still has at most a modest effect on turnout and residual vote rates when the incoming election official has limited prior experience. When we limit our analysis of the effect of turnover on turnout to officials without elections experience in column 2, we find that turnout increases very slightly and we cannot reject the null of no effect of turnover on turnout. When we limit our analysis to cases where the incoming official has no elections or government experience (column 3), we see a very slight negative effect on turnout which is also statistically indistinguishable from no effect. We see similar patterns in columns 5 and 6 where we estimate the effects on residual vote.

These results tell us that incoming officials without experience perform about as well as incoming officials with this experience. While these estimates are somewhat noisy, requiring effects of roughly 1.2 percentage points on turnout and 0.2 percentage points on residual vote to have 80% power to detect, this evidence is most consistent with the claim that officials without direct professional experience in elections or government are not producing much worse outcome upon taking over the office. This could mean that either there are compensating differentials where new officials without this experience have other important skills or experience or that leadership does not meaningfully affect performance. We cannot conclude one way or another based on this evidence.

Table A.15: **Effect of Election Official Turnover on Turnout and Residual Vote by Experience of Incoming Official.**

	Turnout (0-100%)			Residual Vote (0-100%)		
	All Coded	No Elections Experience	No Gov't Experience	All Coded	No Elections Experience	No Gov't Experience
	(1)	(2)	(3)	(4)	(5)	(6)
Turnover	0.05	0.15	-0.17	-0.11	-0.13	-0.12
	(0.27)	(0.42)	(0.48)	(0.06)	(0.08)	(0.07)
# Jurisdictions	386	157	91	309	125	75
# Obs	2,260	840	460	1,820	690	390
Outcome Mean	57.33	57.80	58.56	1.45	1.39	1.27
Min Detectable Effect	0.77	1.16	1.34	0.16	0.21	0.21
Strong Official Only	Yes	Yes	Yes	Yes	Yes	Yes
Matched Sample	Yes	Yes	Yes	Yes	Yes	Yes
Juris-by-Elec Type FE	Yes	Yes	Yes	Yes	Yes	Yes
Pair-by-Year FE	Yes	Yes	Yes	Yes	Yes	Yes

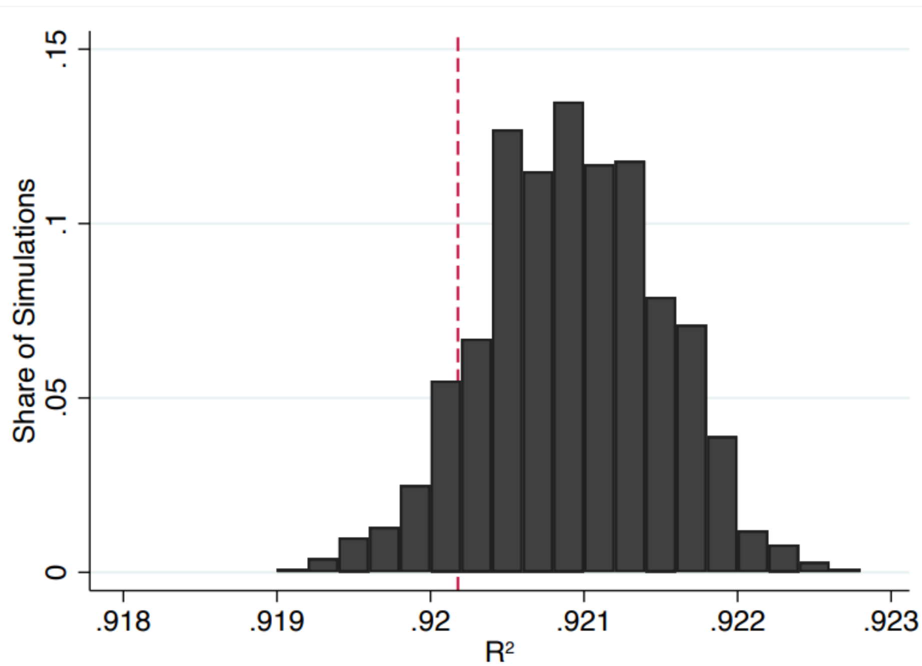
Robust standard errors clustered by jurisdiction reported in parentheses. All Coded columns include all cases of turnover where the incoming official's prior work experience is publicly available. No Election Experience columns include cases of turnover where the incoming official had no professional elections experience. No Gov't Experience columns include cases of turnover where the incoming official had no experience working in government. Data is limited to jurisdictions with one primary official responsible for the majority of election administration responsibilities. Strong official only indicates jurisdictions where one official is responsible for directing all or nearly all aspects of election administration. Matched sample limits data to jurisdictions that experienced turnover between 2012 and 2022 and a set of matched control jurisdictions from the same state with the same history of turnover and the most similar levels of the outcome in all prior elections using 1-to-1 matching with replacement. Turnover refers to a change in the election official since the election two years prior. Turnout is measured as share of voting-age residents who cast a vote for president in presidential years and for governor in midterm years. Residual vote is measured as the the share of ballots cast without a vote for president in presidential years and for governor in midterm years. Regressions on unmatched data include jurisdiction-by-election cycle (presidential or midterm) fixed effects and state-by-year fixed effects. Regressions on matched data include jurisdiction-by-election cycle fixed effects and matched pair-by-year fixed effects. Min detectable effect refers to the minimum effect that a two-sided test with a 0.05 alpha would have 80% power to detect.

A.15 Performance Does Not Noticeably Vary Across Local Election Officials Within Jurisdiction

We evaluate how local election officials affect turnout using the randomization inference procedure described in Berry and Fowler (2021). The procedure computes R^2 values from regressions of a performance measure on leader dummy variables then constructs a null distribution by randomly shuffling when each leader served within each jurisdiction. In our implementation, we use turnout as our performance measure and demean turnout by year and state to implicitly account for state-year fixed effects.

Figure A.7 presents our results. We find that the realized R^2 from the real data falls below the R^2 of nearly 90% of null R^2 values. This implies that there is very little within-jurisdiction variation in leader quality at least insofar as leaders affect turnout.

Figure A.7: **Effect of Local Election Officials on Turnout Against Randomization-Inference-Based Null Distribution**. The figure presents the R^2 of a regression of turnout on local election official dummy variables against a randomization-inference-based null distribution.



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