How Partisan Is Local Election Administration?*

Joshua Ferrer,† *UCLA*

Igor Geyn,‡ *UCLA*

Daniel M. Thompson,§ *UCLA*

December 31, 2021

Abstract

In the US, elections are often administered by directly elected local officials who run as members of a political party. Do these officials use their office to give their party an edge in elections? We answer this question using a newly collected dataset of more than 5,800 clerk elections and a close-election regression discontinuity design, comparing counties that narrowly elect a Democratic election administrator to those that narrowly elect a Republican. Despite the concern that these officials tilt elections in their party's favor, we find that Democrats and Republicans oversee similar election results and turnout. To understand why partisan election officials do not noticeably advantage their party, we analyze data on the policies election officials choose. We present evidence that local officials implement neutral policies rather than attempting and failing to advantage their party. While we cannot rule out some important risks of partisan local election administration, including marginal effects that nevertheless tip close elections or a small number of administrators offering their party large advantages, our results imply that local election officials are not typically and noticeably advantaging their preferred party.

*For helpful discussion and comments, the authors thank Graeme Blair, Ted Enamorado, Paul Gronke, Andy Hall, Apoorva Lal, Hans Lueders, Will Marble, Lachlan McNamee, Toby Nowacki, Avshalom Schwartz, Clemence Tricaud, and Jesse Yoder and participants in the Goldman School of Public Policy research seminar. We would also like to thank David Kimball for providing data from his 2008 survey of local election officials and the hundreds of local election officials who shared election data with us.

†Ph.D. Student, Department of Political Science.

‡Ph.D. Student, Department of Political Science.

§Corresponding author. Assistant Professor, Department of Political Science. danmckinleythompson@gmail.com, http://www.danmthompson.com.

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 and members of the public 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). Describing partisan local and state election officials, election law expert Daniel P. Tokaji captures the concern many have: "Whether or not these officials have acted based on partisan bias is impossible to know for sure. What can be said with confidence is that conflicts of interest are a pervasive problem in U.S. election administration."[1] Do partisan local election officials give their party an advantage?

Experts are concerned for a reason. Local election officials have considerable freedom in how they run elections (Kimball and Kropf 2006), and other partisan local officials such as mayors and county legislatures appear to use their authority to advance their party's goals (de Benedictis-Kessner and Warshaw 2016, 2020; Gerber and Hopkins 2011). The US also has a long history of national, state, and local policies designed to limit the power of racial and ethnic minorities (Keele, Cubbison, and White 2021; Keyssar 2000). On the other hand, the job of local election officials is technical in nature. These more bureaucratic offices may attract—and voters may select—officials who carry out their duties in the same way despite being members of different parties (Kimball et al. 2013; Lim, Silveira, and Snyder 2016; Shepherd et al. 2021; Thompson 2020). Campaigns and political parties also often bring lawsuits against local officials who make changes that disadvantage their party, making it more costly for local officials to advantage their party even if the law permits them to do so.[2]

Research directly testing whether local election officials favor their party, which we review in Table A.1, is mixed for two reason.[3] First, places that elect Democratic rather than Republican

[1] https://www.acslaw.org/?post_type=acsblog&p=7355

[2] https://www.brennancenter.org/our-work/court-cases/voting-rights-litigation-tracker-2020

[3] Democratic and Republican officials may hold divergent beliefs on voter access and security policies, at least in certain jurisdictions (Kimball et al. 2013; Kimball and Baybeck 2010; Kropf, Vercellotti, and Kimball 2013; but see Burden et al. 2013). Democratic officials open more polling places than Republicans, but officials do not strategically locate or change polling places to advantage their party (McBrayer, Williams, and Eckelman 2020; Shepherd et al. 2021). In some cases, partisans reject and accept provisional ballots to improve their party's chances (Kimball, Kropf, and Battles 2006; Merivaki and Smith 2016). Local election officials respond to constituent requests faster when the requester is likely to vote for their preferred party (Porter and Rogowski 2018), but Democrats and Republicans both take more time responding to requests from Latino constituents (White, Nathan, and Faller 2015). Local

election officials are likely different for many reasons, and these reasons may influence how an election official from either party would administer elections. For example, policies like distance limits on polling places that make sense in big cities may not be practical in rural or suburban areas. All of the studies to date compare election administration by Democratic and Republican election officials in counties, and serving populations, that are likely different in ways that cannot easily be observed and that correlate with election administration and support for Democratic election administrators.

Second, how elections are administered often has an ambiguous effect on election outcomes. For example, despite a widespread view that universal vote-by-mail increases turnout and therefore advantages Democrats, it appears to not give either party a substantial advantage (Barber and Holbein 2020; Thompson et al. 2020). Democratic and Republican election administrators may implement different policies, but it is unclear whether these individual policy differences add up in a way that advantages the administrator's party.

In this paper, we credibly estimate how much partisan election officials improve their party's performance in future elections.[4] To do so, we build an original dataset of 5,819 clerk elections in 1,298 counties from 1998 to 2018. Using a close-election regression discontinuity design, comparing places that narrowly elect a Democratic clerk to those that narrowly elect a Republican, we estimate how much of an advantage partisan clerks give their party in presidential elections. This design ensures that the differences we observe arise from who conducts election administration 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 similar counties. Across all four of our regression specifications, we estimate that partisan clerks give their

officials may manage registered voter lists (Stuart 2004), set election administration budgets (Mohr et al. 2019), accept vote-by-mail ballots (Dyck and Seabrook 2009), design ballots (Hamilton and Ladd 1996), and alter turnout to advantage their party (Bassi, Morton, and Trounstine 2009; Burden et al. 2013). On the other hand, Palm Beach County's famous butterfly ballot that lost Al Gore more the 2,000 votes, more than the razor-thin margin in the 2000 Florida presidential election, was designed under the supervision of a Democratic local election official (Wand et al. 2001).

[4] 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.

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 also find that the effect on Democratic vote share is similar in every year from 2004 to 2020.

Do election officials intend to conduct elections in a neutral way? Or do election officials seek to advantage their party but fail to noticeably improve their party's vote share? We study these two alternative explanations using three tests: we examine whether Democratic election officials promote higher turnout, whether Democrats and Republicans administer elections differently on a range of more proximate measures, and whether election officials advantage their party where it would be easiest or most advantageous. While the results are only suggestive, we find little evidence that election officials try and fail to advantage their party. Instead, it appears as though local partisan election officials administer elections in a neutral manner.

Our findings, while not directly testing racial disparities, are connected to a large literature examining race and local election officials (Herron and Smith 2015; Hughes et al. 2020; Merivaki and Smith 2020; Pettigrew 2017; Stuart 2004; White, Nathan, and Faller 2015). Despite our finding that local election officials do not advantage their party, there is substantial evidence that ethnic and racial disparities persist in election administration (Ansolabehere 2009; Atkeson et al. 2010; Baringer, Herron, and Smith 2020; Barreto, Cohen-Marks, and Woods 2009; Chen et al. 2020; Cobb, Greiner, and Quinn 2012; Shino, Suttmann-Lea, and Smith 2021).

Beyond the case of local election administration, our findings also contribute new evidence about the degree of polarization in local policymaking and how much local policymaking resembles state and national policymaking. Election administration is a highly polarized issue at the state and federal levels, with Democrats arguing for expanded convenience voting options and Republicans proposing identification requirements and rollbacks to early and absentee voting options, among many other pitched battles (Hasen 2012, 2020). In a number of offices and across multiple policy domains, local policy is noticeably different when set by a Democrat rather than a Republican (de Benedictis-Kessner and Warshaw 2016, 2020; Gerber and Hopkins 2011; Warshaw 2019). Still, competition among local governments, electoral pressures, interest group influence, and candidate entry and selection may limit this polarization (Anzia 2021; Ferreira and Gyourko 2009; Lim, Silveira, and Snyder 2016; Thompson 2020). Our findings provide further evidence that local

policymaking is not as partisan as federal and state policymaking in many domains and that policymaking by officials serving in more technical roles may be less partisan given their background and prior experience.

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. In elections decided by less than a percentage point of one county's vote, we cannot say whether replacing a Republican county clerk with a Democrat could have swung the election. We also cannot rule out rare but very large effects—if a few election officials dramatically change the outcomes of elections they oversee, this effect will make up a miniscule share of the average effect and be drowned out by the many officials who do not advantage their party. Additionally, our results pertain specifically to local election officials administering state and federal elections. It is possible that partisan election officials at the state level are able to bias elections in their party's favor. 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.

2 The Role of Local Election Officials

Across the United States, thousands of local election officials play a central role in the administration of elections. Local election official responsibilities typically 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 also work in concert with a range of other officials to successfully conduct elections, including federal entities (e.g., the U.S. Election Assistance Commission), state officers, municipal and county staff, and local volunteers who serve as poll workers on Election Day. Clerks typically serve at the county level, though in ten mostly Northeastern states important

responsibilities are carried out at the municipal level. Additionally, the most populous cities in a state frequently have their own forms of election administration.

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 registration or election boards. For instance, in Michigan and Wisconsin, election administration responsibilities are divided between municipal and county officials.

Of the 32 states with partisan elected election officials, 21 entrust partisan elected clerks as the sole or primary election administrators. Fourteen of these states delegate virtually all local administrative duties to one elected official in each county. Of the other seven states, four have separate registration officials, two split authority with an elections board but give the partisan elected official the decisive vote, and in one state some duties are shared with the county legislative body. Our main analysis focuses on partisan elected officials in the 21 states where they are the sole or primary election administrators.[5]

Even among states that delegate considerable election administration authority to a partisan elected official, there are significant differences in these officials' responsibilities and in the degree of discretion they have to carry out these responsibilities. We describe these differences 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 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 severely constrained by state law in recruiting poll workers, and both site polling places and select voting equipment in conjunction with their county commission.

Overall, most of the 21 states give registration and voting administration duties to the same partisan elected official. Most also entrust registration list maintenance and voting equipment

[5] In Table A.8, we run a robustness check using only those 14 states where virtually all duties are delegated to a single partisan elected official.

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.

It is instructive to consider some of the ways that local election officials could plausibly affect election results. Local election officials might affect election results with formal, informal, or even illegal practices. In terms of 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, recruiting an even partisan balance of experienced and motivated poll workers, and showing leniency in their acceptance of both 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, selecting a group of inexperienced and biased poll workers, and rejecting borderline provisional and vote-by-mail ballots.

Clerks might also undertake informal practices to reduce voter costs and increase voter convenience, or they could perform the bare statutory minimum required by the law. Officials can conduct voter outreach and education campaigns, advertise how and where to register, maintain an active media and social media presence, and engage in extensive constituent communication. Alternatively, they could take none of these actions. Local election officials can also engage in targeted practices by attempting to increase participation among co-partisans and reduce participation among non-co-partisans. Finally, illegal actions may sometimes be undertaken at the risk of litigation. These include siting fewer polling places than the statutory minimum mandates[6], 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.

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

3 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 precison of our estimates by first predicting outcomes.

3.1 New Data on the Elections of Partisan Local Election Officials

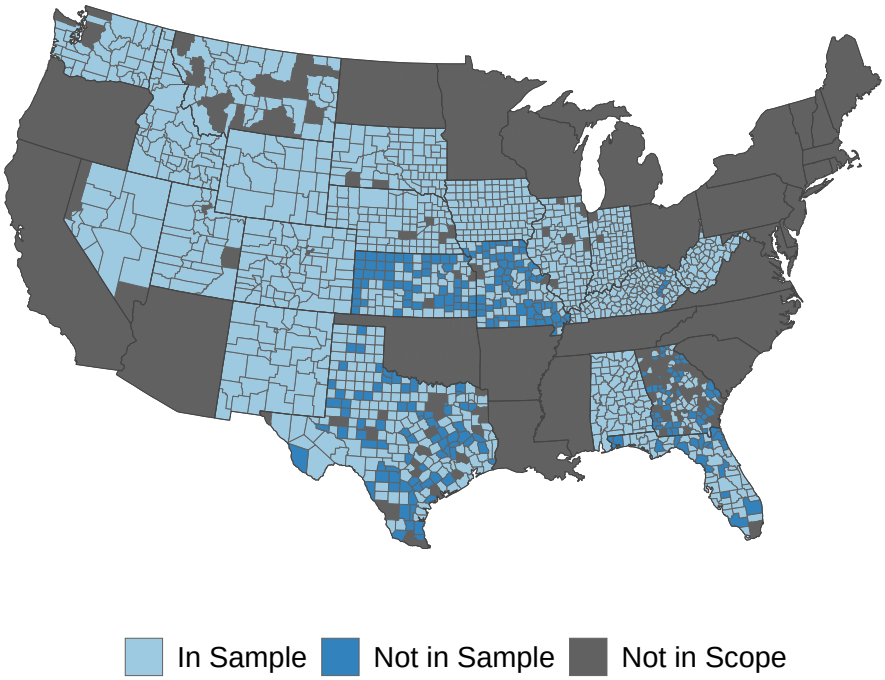
We gather an original dataset of 5,819 elections of partisan local election officials in 1,298 counties and 21 states held between 1998 and 2018. We collect these results in three steps. First, we scrape state election websites for all county-level results. Next, we visit county election websites for 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 use dark gray to denote counties where towns, cities, and villages 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 not in 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.[7]

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.

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

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, 1,295 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.



3.2 County-Level Election Results and Voter Participation

We obtain county-level presidential election results for 1996 to 2020 from Dave Leip's Election Atlas.[8] We also compile data on every regularly scheduled governor election from 1994 to 2017 and every regularly scheduled US Senate election from 1994 to 2020 from Leip's Atlas, as well as the number of votes counted in the race for the highest federal office on the ballot—either representative, senator, or president.[9]

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 Institutes's Surveillance, Epidemiology, and End Results Program.[10]

[8] `https://uselectionatlas.org/`

[9] Due to irregularities in the number of ballots cast in some counties, we use the number of votes in the race for the highest federal office as our measure of turnout.

[10] 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

3.3 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. We use this survey to measure the following for each even-year 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 also use Dave Leip's Election Atlas to measure the number of registered voters in each county as well as 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. 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.

3.4 Empirical Strategy: Regression Discontinuity Design

We estimate the advantage election officials give to their co-partisans using a regression discontinuity design, fitting regressions 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,

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/`.

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 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).[11] We evaluate the plausibility of this assumption by evaluating whether pre-election county-level characteristics are similar in counties that narrowly elected Democratic officials and 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.[12] These results serve as evidence to support our claim that the only difference between a district that narrowly elects a 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, and this is the effect our design identifies under the assumptions we mention above.

[11] 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).

[12] This is a version of the standard McCrary (2008) sorting test.

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).

3.5 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 hard—across many applications, the common estimators produce large standard errors and do not have sufficient power to typically detect substantively interesting effects (Stommes, Aronow, and Sävje 2021). The standard errors are often so large that they are uninformative about the question of interest.

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.

We improve on standard practice using a three-step process that follows the recommendations of 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. They find that, 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. Our estimation procedure works as follows:

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.[13] We use the full dataset for this exercise, not just the counties with competitive elections for their local election official.

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.

In addition to our main findings, we use this procedure to improve our power for studying other outcomes including voter turnout.

We conduct power analyses throughout the paper 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 2.2 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 minimal 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 percentage point and 1.1

[13] We discuss the candidate predictors and their performance in the online appendix.

percentage points. Our minimum detectable effect on turnout is less than half the size of a large TV advertising campaign encouraging young people to vote in 2004 presidential election (Green and Vavreck 2008), less than half the size of the effect of switching to universal vote-by-mail in states with high levels of absentee voting prior to the switch (Gerber, Huber, and Hill 2013; Thompson 2020), and about twice as large as the effect of campaign mailers in high-salience elections (Gerber et al. 2017).

4 Clerks Do Not Meaningfully Advantage Their Party

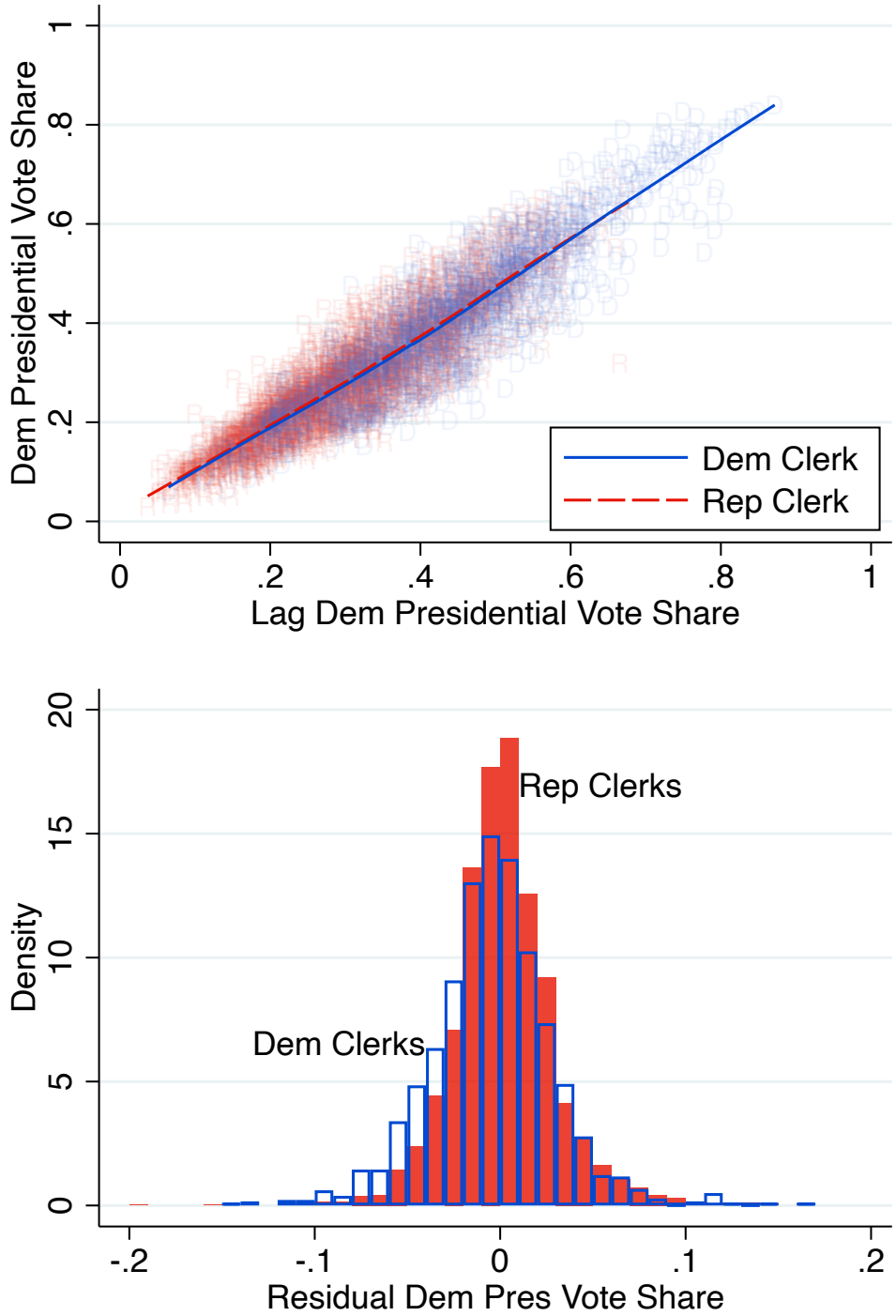
4.1 Descriptive Graphical Evidence Suggests Clerks Do Not Advantage Their Party

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. This provides us little reason to suspect that clerks are giving their party a substantial advantage in presidential elections.

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. Democratic clerks oversee elections that are slightly less favorable for Democratic presidents on average than expected.



The bottom panel of Figure 2 plots histograms of the residual of predicted Democratic presidential vote share for counties with Democratic and Republican clerks.[14] The histograms overlap substantially, although the histogram for Democrats is shifted slightly to the left and has a modestly wider dispersion.[15] This implies that, if anything, Democratic clerks oversee elections that are worse, on average, for Democratic presidential candidates.

4.2 Regression Discontinuity Plot Suggests Clerks Do Not Advantage Their Party

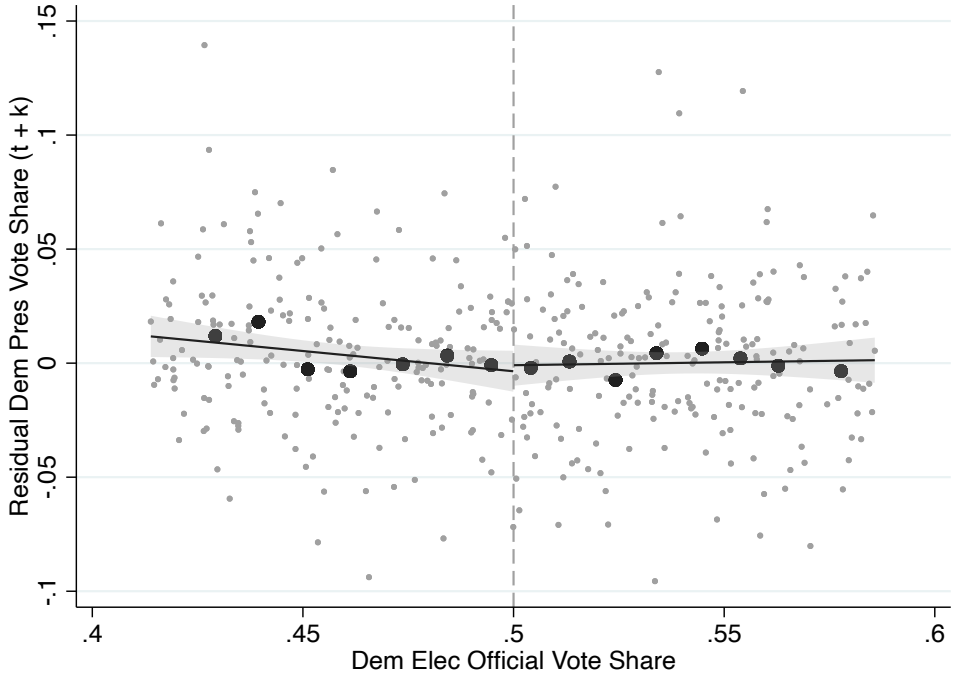
Figure 3 captures our main result: local election officials do not improve their party's vote share in presidential elections. On the horizontal axis, we plot the two-party Democratic vote share in the race for local election official. We subset to elections with a Democrat and Republican candidate both 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. We plot as our vertical axis 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, and we compute them 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. This suggests that election officials do not noticeably advantage their party.

[14] See Section 3.5 for a discussion of how we compute the residuals.

[15] The average of the residuals is 0.002 in Republican-controlled counties and -0.005 in Democrat-controlled counties. The standard deviation of the residuals is 0.028 in Republican-controlled counties and 0.035 in Democrat-controlled counties.

Figure 3: **Electing a Democratic Election Official Rather Than a Republican Does Not Meaningfully 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.



4.3 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). In 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).

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	-0.001	-0.009	0.000
	(0.006)	(0.005)	(0.009)	(0.007)
N	395	706	204	401
Bandwidth	0.08	0.17	0.04	0.09
Bandwidth Selection	CCT	CCT*2	CCT/2	CCT
Kernel	Uniform	Uniform	Uniform	Triangular
Min Detectable Effect	0.016	0.012	0.022	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.

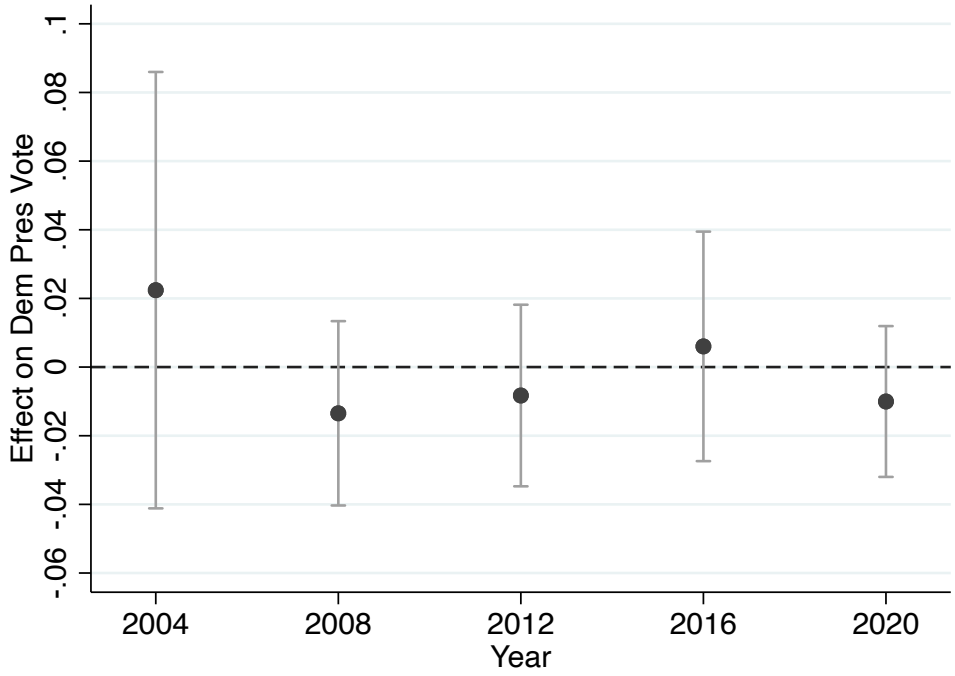
We find consistent evidence across all four specifications that local election officials do not meaningfully advantage their party's candidate for president. The point estimates range from -0.9 to 0.3 percentage points, with three out of four point estimates falling below 0.1 percentage points. Across all four columns, our 95% confidence intervals include zero.

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 online appendix, we also demonstrate that, though our estimates are noisier when using outcomes that are not first residualized, they are substantively similar.

In the final row of Table 1, we present the minimum detectable effect. As we discuss in Section 3.5, we would be able to detect partisan advantages as small as 1.7 percentage points with 80% power using all four estimators.

In Table A.12, 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. Never-

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. 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 traingular kernel weights.



theless, 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.

4.4 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.8, 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, and all four are more negative than the corresponding point estimates we report in Table 1. 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.9 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.10 we remove these counties and find that, even when studying counties not subject to the pre-clearance requirement, clerks do not appear to advantage their party. In Table A.11, 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 co-partisans.[16] Put together, these results suggest that clerks do not meaningfully advantage their party.

4.5 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

[16]This is in line with Komisarchik and White (2021).

with close clerk elections. In Section A.6.9 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.

5 Do Election Officials Choose Different Policies but Fail to Advantage Their Party?

Do election officials intend to conduct elections in a neutral way? Or do election officials seek to advantage their party but still fail to noticeably improve their party's vote share? As we discuss in Section 2, our analysis focuses on officials with considerable authority. Still, if citizens are motivated to overcome barriers to participation such as farther polling places, longer wait times at the poll, and worse communication from their local election official, or if campaigns organize in response to these barriers (Cantoni and Pons 2021), this authority might not meaningfully affect the election outcome or even turnout rates.

We find the explanation that election officials try and fail to help their party largely inconsistent with the data. We evaluate this explanation in three steps. First, we estimate the effect of electing a Democratic rather than Republican election official on voter turnout. This allows us to test whether Democratic election officials promote higher levels of participation (as is conventional wisdom) and yet still do not advantage their party. With a test powered to detect effects as small as one percentage point, we find that Democrats and Republicans oversee elections with very similar turnout rates. Second, we estimate the effect of electing a Democratic rather than Republican election official on eight separate measures of election administration. These measures are more proximate to the election official than are vote shares and should thus be more responsive to policy and practice changes. While many of the measures are noisy, we find that Democratic and Republican clerks oversee similar elections according to these measures, with the possible exception that Democrats oversee higher registration rates. Finally, we estimate the effect of electing a Democratic rather than Republican official in counties and states where an official committed to giving their party an advantage would have an easier time doing so and where they would

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	0.003	0.002	0.002	0.002
	(0.004)	(0.004)	(0.005)	(0.004)	(0.010)	(0.009)
N	541	715	425	603	167	187
Clusters	313	415	251	357	105	119
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.

produce the largest returns for their co-partisans. We find that, even in counties where we would most expect election election officials to advantage their party, Democrats and Republicans oversee similar election results.

Putting these three analyses together, we conclude that Democratic and Republican officials produce similar outcomes because they implement similar policy regimes and not because the different regimes they implement fail to give their party a noticeable advantage.

5.1 Democratic and Republican Clerks Produce Similar Turnout

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 (Thompson et al. 2020). Might local election officials successfully affect turnout but fail to offer their party an advantage?

Table 2 presents 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 regression discontinuity 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.

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. Yet, 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.

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

5.2 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 3: **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	-0.066	-0.000	-0.062	0.010	0.019	0.004	0.001	-0.019
	(0.086)	(0.001)	(0.060)	(0.015)	(0.009)	(0.007)	(0.006)	(0.022)
N	225	178	277	493	696	398	422	401
Clusters	167	124	187	322	408	256	244	274
Outcome Mean	0.980	0.005	0.482	0.029	0.857	0.091	0.490	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.

Table 3 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.19 through A.26 in the online appendix 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.

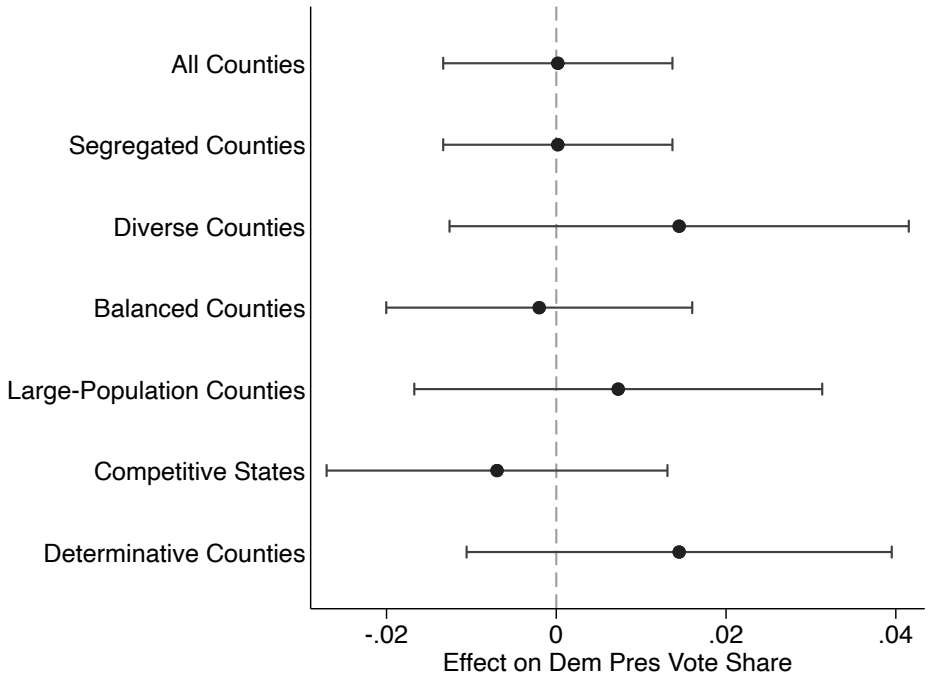
These findings also provide some evidence that countermobilization strategies pursued by party elites in response to clerk actions (Cantoni and Pons 2021) do not explain our finding of minimal partisan differences. Whereas differences in presidential vote share, turnout, and registration rates could 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.

5.3 Clerks Do Not Advantage Their Party More When It Is Easier or When the Stakes Are Higher

If most election officials are trying to advantage their party, we would expect them to have a larger effect when it is easier to do so and when advantaging their party has the largest effect on electoral outcomes. While the average effect is zero across our sample, might a partisan effect be swamped by including counties where election officials have a hard time advantaging their party or where the stakes of that advantage are low?

We identify six related conditions that either make it easier 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 (Carmines and Stimson 1989; Carlson and Hill 2021; 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

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.



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 the effect to be largest if clerks were trying to advantage their party. 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 is consistent with clerks intending to administer elections in neutral ways. If they intended to advantage their party, we would expect that, in at least some cases where advantaging their party is easier or has a greater potential influence on the final outcome, we would observe a partisan difference. Instead, across the seven subgroups that we study, we cannot reject the null hypothesis that Democratic and Republican clerks fail to advantage their party. While these estimates are noisier than our main estimates given that they rely on a subset of our data, they suggest that clerks do not noticeably advantage their party even when the stakes are highest and they have the greatest capacity to affect the outcome.

The regression specifications we chose and the rules we use 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 we report in Figure 5 are similar to those we estimate across our different specifications and subgroup inclusion rules.

6 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. As political science professor Martha Kropf put it in a recent interview,"Having local officials that are elected on a partisan basis running elections seems fishy."[17] Even some local election officials themselves report feeling uncomfortable running as partisans when they have a duty to be neutral.[18]

Since the closely contested 2000 presidential election, election administration has become an increasingly partisan issue at the state and national levels with Democrats typically seeking to expand the number of ways people can vote and Republicans typically seeking voting restrictions ostensibly aimed at combating voter fraud (Hasen 2012, 2020). Throughout, politicians and organized groups have claimed that local election officials have used their authority to advantage their party. In 2018, Republicans Rick Scott and Donald Trump claimed that Democratic election officials in Broward County, Florida forged ballots to give Scott's opponent an advantage.[19] Also in 2018, local Democrats argued that the Republican clerk in Dodge County, Kansas moved the only polling place in Dodge City far away from Latino residents to favor Republican Kris Kobach's candidacy for governor.[20]

Using a credible research design with new partisan clerk election data from across the US, 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.

We also find little evidence that clerks are trying and failing to advantage their party. Instead, clerks appear to be administering elections in a mostly neutral way. This casts further doubt on the claim that local election officials are using their authority to benefit their party.

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, elections are run by appointed, nonpartisan 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.

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

[18] Ibid.

[19] https://www.politifact.com/article/2018/nov/13/trump-rick-scott-allege-fraud-broward-no-evidence/

[20] https://www.theguardian.com/us-news/2018/oct/28/dodge-city-polling-place-voter-suppression-voting-rights

Additionally, many states elect partisan chief election officials who possess wide-ranging authority to influence election administration. It is an open question whether Democratic and Republican state election officials bias election results in their party's favor.

Even though partisan elected clerks are not favoring their party on average, it "seems fishy," as Professor Martha Kropf put it. Future work should consider if even neutral partisan election administration can leave citizens suspicious that the election was unfair.

References

Abadie, Alberto, Susan Athey, Guido W. Imbens, and Jeffrey Wooldridge. 2017. "When Should You Adjust Standard Errors for Clustering?" Working Paper. https://arxiv.org/abs/2109.14526.

Angrist, Joshua D, and Miikka Rokkanen. 2015. "Wanna Get Away? Regression Discontinuity Estimation of Exam School Effects Away from the Cutoff." *Journal of the American Statistical Association* 110(512): 1331–1344.

Ansolabehere, Stephen. 2009. "Is There Racial Discrimination at the Polls? Voters' Experience in the 2008 Election." Caltech/MIT Voting Technology Project Working Paper. https://dspace.mit.edu/handle/1721.1/97649.

Anzia, Sarah F. 2021. "Party and Ideology in American Local Government: An Appraisal." *Annual Review of Political Science* 24: 133–150.

Atkeson, Lonna Rae, Lisa Ann Bryant, Thad E. Hall, Kyle Saunders, and Michael Alvarez. 2010. "A New Barrier to Participation: Heterogeneous Application of Voter Identification Policies." *Electoral Studies* 29(1): 66–73.

Barber, Michael, and John B. Holbein. 2020. "The Participatory and Partisan Impacts of Mandatory Vote-by-Mail." *Science Advances* 6(35).

Baringer, Anna, Michael C. Herron, and Daniel A. Smith. 2020. "Voting by Mail and Ballot Rejection: Lessons from Florida for Elections in the Age of the Coronavirus." *Election Law Journal: Rules, Politics, and Policy* 19(3): 289–320.

Barreto, Matt A., Mara Cohen-Marks, and Nathan D. Woods. 2009. "Are All Precincts Created Equal? The Prevalence of Low-Quality Precincts in Low-Income and Minority Communities." *Political Research Quarterly* 62(3): 445–458.

Bassi, Anna, Rebecca Morton, and Jessica Trounstine. 2009. "Local Implementation of State and Federal Election Law." Working Paper. https://faculty.ucmerced.edu/jtrounstine/BMT_Empirical_May09.pdf.

Burden, Barry C., David T. Canon, Stéphane Lavertu, Kenneth R. Mayer, and Donald P. Moynihan. 2013. "Selection Method, Partisanship, and the Administration of Elections." *American Politics Research* 41(November): 903–936.

Calonico, Sebastian, Matias D. Cattaneo, and Rocio Titiunik. 2014. "Robust Nonparametric Confidence Intervals for Regression-Discontinuity Designs." *Econometrica* 82(6): 2295–2326.

Calonico, Sebastian, Matias D. Cattaneo, Max H. Farrell, and Rocio Titiunik. 2019. "Regression Discontinuity Designs Using Covariates." *Review of Economics and Statistics* 101(3): 442–451.

Cantoni, Enrico, and Vincent Pons. 2021. "Strict ID Laws Don't Stop Voters: Evidence from a US Nationwide Panel, 2008–2018." *The Quarterly Journal of Economics* 136(4): 2615–2660.

Carlson, Taylor N., and Seth J. Hill. 2021. "Experimental Measurement of Misperception in Political Beliefs." *Journal of Experimental Political Science* pp. 1–14.

Carmines, Edward G., and James A. Stimson. 1989. *Issue Evolution: Race and the Transformation of American Politics*. Princeton University Press.

Cattaneo, Matias D., Nicolás Idrobo, and Rocío Titiunik. 2019. *A Practical Introduction to Regression Discontinuity Designs: Foundations.* Cambridge University Press.

Caughey, Devin M., and Jasjeet S. Sekhon. 2011. "Elections and the Regression Discontinuity Design: Lessons from Close US House Races, 1942–2008." *Political Analysis* 19(4): 385–408.

Chen, M. Keith, Kareem Haggag, Devin G. Pope, and Ryne Rohla. 2020. "Racial Disparities in Voting Wait Times: Evidence from Smartphone Data." *The Review of Economics and Statistics* pp. 1–27.

Cobb, Rachael V., D. James Greiner, and Kevin M. Quinn. 2012. "Can Voter ID Laws Be Administered in a Race-Neutral Manner? Evidence from the City of Boston in 2008." *Quarterly Journal of Political Science* 7(1): 1–33.

de Benedictis-Kessner, Justin, and Christopher Warshaw. 2016. "Mayoral Partisanship and Municipal Fiscal Policy." *The Journal of Politics* 78(4): 1124–1138.

de Benedictis-Kessner, Justin, and Christopher Warshaw. 2020. "Politics in Forgotten Governments: The Partisan Composition of County Legislatures and County Fiscal Policies." *The Journal of Politics* 82(2): 460–475.

Dyck, Joshua J., and Nicholas R. Seabrook. 2009. "The Problem with Vote-by-Mail." Working Paper.

Eggers, Andrew C., Anthony Fowler, Jens Hainmueller, Andrew B. Hall, and James M. Snyder. 2015. "On the Validity of the Regression Discontinuity Design for Estimating Electoral Effects: New Evidence from Over 40,000 Close Races." *American Journal of Political Science* 59(1): 259–274.

Ferreira, Fernando, and Joseph Gyourko. 2009. "Do Political Parties Matter? Evidence from US Cities." *The Quarterly Journal of Economics* 124(1): 399–422.

Gerber, Alan S., Gregory A. Huber, Albert H. Fang, and Andrew Gooch. 2017. "The Generalizability of Social Pressure Effects on Turnout Across High-Salience Electoral Contexts: Field Experimental Evidence from 1.96 Million Citizens in 17 States." *American Politics Research* 45(4): 533–559.

Gerber, Alan S., Gregory A. Huber, and Seth J. Hill. 2013. "Identifying the Effect of All-Mail Elections on Turnout: Staggered Reform in the Evergreen State." *Political Science Research and Methods* 1(1): 91–116.

Gerber, Elisabeth R., and Daniel J. Hopkins. 2011. "When Mayors Matter: Estimating the Impact of Mayoral Partisanship on City Policy." *American Journal of Political Science* 55(2): 326–339.

Green, Donald P., and Lynn Vavreck. 2008. "Analysis of Cluster-Randomized Experiments: A Comparison of Alternative Estimation Approaches." *Political Analysis* 16(2): 138–152.

Hainmueller, Jens, Andrew B Hall, and James M Snyder Jr. 2015. "Assessing the External Validity of Election RD Estimates: An Investigation of the Incumbency Advantage." *The Journal of Politics* 77(3): 707–720.

Hall, Andrew B. 2015. "What Happens When Extremists Win Primaries?" *American Political Science Review* 109(1): 18–42.

Hall, Andrew B. 2019. *Who Wants to Run?: How the Devaluing of Political Office Drives Polarization.* University of Chicago Press.

Hamilton, James T., and Helen F. Ladd. 1996. "Biased Ballots? The Impact of Ballot Structure on North Carolina Elections in 1992." *Public Choice* 87(3-4): 259–280.

Hasen, Richard L. 2012. *The Voting Wars: From Florida 2000 to the Next Election Meltdown.* Yale University Press.

Hasen, Richard L. 2020. *Election Meltdown: Dirty Tricks, Distrust, and the Threat to American Democracy.* Yale University Press.

Herron, Michael C., and Daniel A. Smith. 2015. "Precinct Closing Times in Florida During the 2012 General Election." *Election Law Journal: Rules, Politics, and Policy* 14(3): 220–238.

Hersh, Eitan D. 2015. *Hacking the Electorate: How Campaigns Perceive Voters.* Cambridge University Press.

Hughes, D. Alex, Micah Gell-Redman, Charles Crabtree, Natarajan Krishnaswami, Diana Rodenberger, and Guillermo Monge. 2020. "Persistent Bias Among Local Election Officials." *Journal of Experimental Political Science* 7(3): 179–187.

Imbens, Guido W., and Thomas Lemieux. 2008. "Regression Discontinuity Designs: A Guide to Practice." *Journal of Econometrics* 142(2): 615–635.

Keele, Luke, William Cubbison, and Ismail White. 2021. "Suppressing Black Votes: A Historical Case Study of Voting Restrictions in Louisiana." *American Political Science Review* 115(2): 694–700.

Keyssar, Alexander. 2000. *The Right to Vote: The Contested History of Democracy in the United States.* Basic Books.

Kimball, David, and Brady Baybeck. 2010. "Is There a Partisan Way to Administer Elections?" Working Paper. `http://www.umsl.edu/~kimballd/dk_bb_MPSA10.pdf`.

Kimball, David C., and Brady Baybeck. 2013. "Are All Jurisdictions Equal? Size Disparity in Election Administration." *Election Law Journal: Rules, Politics, and Policy* 12(2): 130–145.

Kimball, David C., and Martha Kropf. 2006. "The Street-Level Bureaucrats of Elections: Selection Methods for Local Election Officials." *Review of Policy Research* 23(6): 1257–1268.

Kimball, David C., Martha Kropf, and Lindsay Battles. 2006. "Helping America Vote? Election Administration, Partisanship, and Provisional Voting in the 2004 Election." *Election Law Journal: Rules, Politics, and Policy* 5(4): 447–461.

Kimball, David C., Martha Kropf, Donald Moynihan, and Carol L. Silva. 2013. "The Policy Views of Partisan Election Officials." *UC Irvine L. Rev.* 3(3): 551.

Komisarchik, Mayya, and Ariel White. 2021. "Throwing Away the Umbrella: Minority Voting after the Supreme Court's Shelby Decision." Working Paper. `https://arwhite.mit.edu/sites/default/files/images/vra_post_shelby_current.pdf`.

Kropf, Martha, Timothy Vercellotti, and David C. Kimball. 2013. "Representative Bureaucracy and Partisanship: The Implementation of Election Law." *Public Administration Review* 73(2): 242–252.

Lee, David S., and Thomas Lemieux. 2010. "Regression Discontinuity Designs in Economics." *Journal of Economic Literature* 48(2): 281–355.

Lijphart, Arend. 1997. "Unequal Participation: Democracy's Unresolved Dilemma Presidential Address, American Political Science Association, 1996." *American Political Science Review* 91(March): 1–14.

Lim, Claire S.H., Bernardo S. Silveira, and James M. Snyder. 2016. "Do Judges' Characteristics Matter? Ethnicity, Gender, and Partisanship in Texas State Trial Courts." *American Law and Economics Review* 18(2): 302–357.

Marshall, John. 2021. "Can Close Election Regression Discontinuity Designs Identify Effects of Winning Politician Characteristics?" Working Paper. `https://polmeth2021.com/files/polmeth2021/files/election_rd_paper_v2.pdf`.

McBrayer, Markie, R. Lucas Williams, and Andrea Eckelman. 2020. "Local Officials as Partisan Operatives: The Effect of County Officials on Early Voting Administration." *Social Science Quarterly* 101(4): 1475–1488.

McCrary, Justin. 2008. "Manipulation of the Running Variable in the Regression Discontinuity Design: A Density Test." *Journal of Econometrics* 142(2): 698–714.

Merivaki, Thessalia, and Daniel A. Smith. 2016. "Casting and Verifying Provisional Ballots in Florida." *Social Science Quarterly* 97(3): 729–747.

Merivaki, Thessalia, and Daniel A. Smith. 2020. "A Failsafe for Voters? Cast and Rejected Provisional Ballots in North Carolina." *Political Research Quarterly* 73(1): 65–78.

Mohr, Zachary, JoEllen V. Pope, Martha E. Kropf, and Mary Jo Shepherd. 2019. "Strategic Spending: Does Politics Influence Election Administration Expenditure?" *American Journal of Political Science* 63(2): 427–438.

Noack, Claudia, Tomasz Olma, and Christoph Rothe. 2021. "Flexible Covariate Adjustments in Regression Discontinuity Designs." Working Paper. `https://arxiv.org/abs/2107.07942`.

Norris, Pippa, and Max Grömping. 2019. "Electoral Integrity Worldwide." Electoral Integrity Project. `https://www.electoralintegrityproject.com/the-year-in-elections-2019`.

Pettigrew, Stephen. 2017. "The Racial Gap in Wait Times: Why Minority Precincts Are Underserved by Local Election Officials." *Political Science Quarterly* 132(3): 527–547.

Piven, Frances Fox, and Richard A. Cloward. 1988. *Why Americans Don't Vote*. Pantheon.

Porter, Ethan, and Jon C. Rogowski. 2018. "Partisanship, Bureaucratic Responsiveness, and Election Administration: Evidence from a Field Experiment." *Journal of Public Administration Research and Theory* 28(4): 602–617.

Shepherd, Michael E., Adriane Fresh, Nick Eubank, and Joshua D. Clinton. 2021. "The Politics of Locating Polling Places: Race and Partisanship in North Carolina Election Administration, 2008–2016." *Election Law Journal: Rules, Politics, and Policy* 20(2): 155–177.

Shino, Enrijeta, Mara Suttmann-Lea, and Daniel A. Smith. 2021. "Determinants of Rejected Mail Ballots in Georgia's 2018 General Election." *Political Research Quarterly* pp. 1–13.

Sides, John, Lynn Vavreck, and Christopher Warshaw. 2021. "The Effect of Television Advertising in United States Elections." *American Political Science Review* pp. 1–17.

Spenkuch, Jörg L., and David Toniatti. 2018. "Political Advertising and Election Results." *The Quarterly Journal of Economics* 133(4): 1981–2036.

Stommes, Drew, P.M. Aronow, and Fredrik Sävje. 2021. "On the Reliability of Published Findings Using the Regression Discontinuity Design in Political Science." Working Paper. `https://arxiv.org/abs/2109.14526`.

Stuart, Guy. 2004. "Databases, Felons, and Voting: Bias and Partisanship of the Florida Felons List in the 2000 Elections." *Political Science Quarterly* 119(3): 453–475.

Thompson, Daniel M. 2020. "How Partisan Is Local Law Enforcement? Evidence from Sheriff Cooperation With Immigration Authorities." *American Political Science Review* 114(1): 222–236.

Thompson, Daniel M., Jennifer A. Wu, Jesse Yoder, and Andrew B. Hall. 2020. "Universal Vote-by-Mail Has No Impact on Partisan Turnout or Vote Share." *Proceedings of the National Academy of Sciences* 117(25): 14052–14056.

Wand, Jonathan N., Kenneth W. Shotts, Jasjeet S. Sekhon, Walter R. Mebane, Michael C. Herron, and Henry E. Brady. 2001. "The Butterfly Did It: The Aberrant Vote for Buchanan in Palm Beach County, Florida." *American Political Science Review* 95(4): 793–810.

Warshaw, Christopher. 2019. "Local Elections and Representation in the United States." *Annual Review of Political Science* 22: 461–479.

White, Ariel R., Noah L. Nathan, and Julie K. Faller. 2015. "What Do I Need to Vote? Bureaucratic Discretion and Discrimination by Local Election Officials." *American Political Science Review* 109(1): 129–142.

Online Appendix

Intended for online publication only.

Contents

A.1 Review of Previous Literature on Partisan Differences in Local Election Administration 35

A.2 The Responsibilities of Local Election Officials 36

A.3 Describing the New Data on Election Officials 37

A.4 Predicting Election Results 40

A.5 Calculating Minimimum Detectable Effects 41

A.6 Validating the Main Findings 42

A.6.1 Counties that Narrowly Elect Democrats vs. Republicans Are Similar on Pre-Treatment Covariates 42

A.6.2 Counties Not Sorting into Treatment or Control 44

A.6.3 Main Findings Not Sensitive to Choice of Estimator 45

A.6.4 Main Findings Not Sensitive to Choice of Bandwidth 46

A.6.5 No Substantial Average Effect in States Granting Full Authority to One Official 47

A.6.6 Finding Not Sensitive to Excluding the South 48

A.6.7 Finding Not Sensitive to Excluding VRA Counties 49

A.6.8 No Substantial Average Effect in Senate, Governor, or Presidential Elections 51

A.6.9 Effect Not Limited To Counties with Close Clerk Elections 52

A.7 Studying Mechanisms 54

A.7.1 Clerks Do Not Advantage Their Party More in More Segregated Counties . 54

A.7.2 Clerks Do Not Advantage Their Party More in More Diverse Counties . . . 57

A.7.3 Estimated Effects No Larger in Balanced Districts 59

A.7.4 Clerks Do Not Advantage Their Party More in Larger Counties 63

A.7.5 Clerks Do Not Advantage Their Party More in More Competitive States . . 65

A.7.6 Clerks Do Not Advantage Their Party More in Determinative Counties . . . 67

A.7.7 Effect of Electing a Democratic Clerk on All Policy Outcomes Collected . . 69

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 3.1, 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 occuring 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.

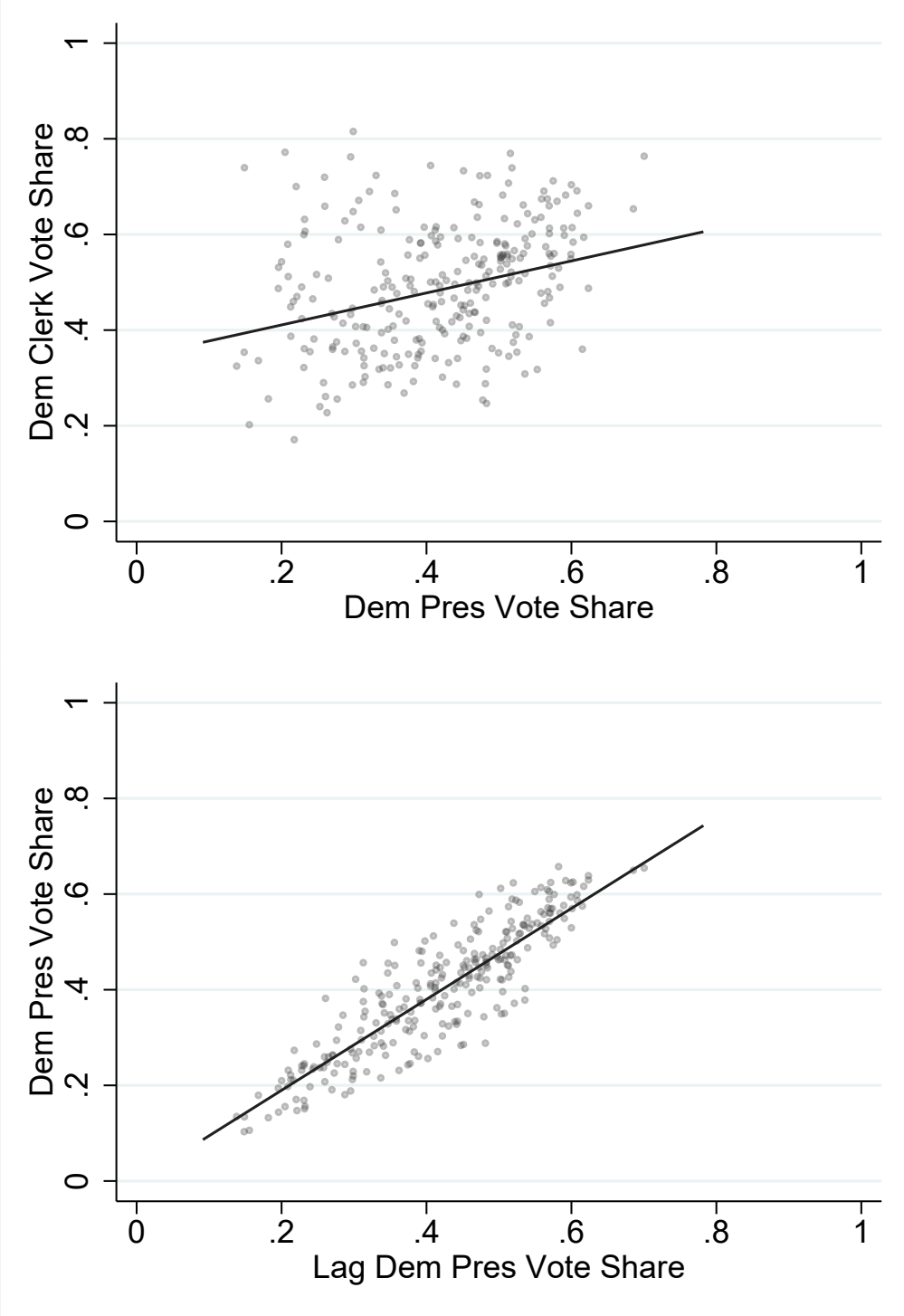
Table A.4 compares the counties for which we have election data to the counties that elect partisan local election officials but where we do not have election data. The counties we are missing tend to be less populous, in the South, and have larger Black and Hispanic populations. The counties that do 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.

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

Outcome	In Sample	Not In Sample	Not In Scope
Population (Thousands)	55.88	37.01	143.06
	(172.94)	(108.52)	(404.58)
Share Non-Hispanic White	0.82	0.76	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	0.13	0.06
	(0.15)	(0.20)	(0.10)
Northeast	0.00	0.00	0.14
Midwest	0.41	0.44	0.26
South	0.37	0.56	0.50
West	0.21	0.00	0.10
Num Counties	1,295	252	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

Throughout the paper, we use residualized outcomes that are constructed by first using a lagged outcome to predict the outcome of interest and then taking the remaining error from this prediction process. As we discuss in Section 3.5, we chose the predictor that minimizes out-of-sample prediction error. We measure the out-of-sample prediction using leave-one-out cross validation, fitting our regression holding out one observation at a time, using that regression to predict the held out unit's outcome value, and computing the error as the difference between the observed and predicted outcome values.

We test four regression specifications:

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

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 speicifcation for all other outcomes, using state-year-specific regressions to maintain consistency.

A.5 Calculating Minimimum 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 3.4, 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	0.026	0.037	0.005	0.020	0.008	0.004	0.019	0.013
	(0.022)	(0.017)	(0.029)	(0.022)	(0.019)	(0.014)	(0.026)	(0.019)
N	349	634	177	393	611	1108	307	687
Clusters	349	634	177	393	353	639	179	398
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	Balance at RD Cut Point			
	(1)	(2)	(3)	(4)
Log(Population)	0.281	0.142	0.278	0.211
	(0.254)	(0.195)	(0.351)	(0.265)
	[443]	[768]	[768]	[768]
Share Non-Hispanic White	0.007	0.018	0.046	0.022
	(0.035)	(0.027)	(0.052)	(0.041)
	[391]	[647]	[647]	[647]
Share Black	0.029	0.014	0.026	0.017
	(0.024)	(0.016)	(0.034)	(0.020)
	[253]	[476]	[476]	[476]
South	0.030	0.018	-0.004	0.039
	(0.097)	(0.070)	(0.131)	(0.094)
	[372]	[673]	[673]	[673]
West	0.013	0.042	-0.008	0.010
	(0.088)	(0.064)	(0.122)	(0.083)
	[374]	[678]	[678]	[678]
Bandwidth Selection	CCT	CCT*2	CCT/2	CCT
Kernel	Uniform	Uniform	Uniform	Triangular

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 3.4, 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.**

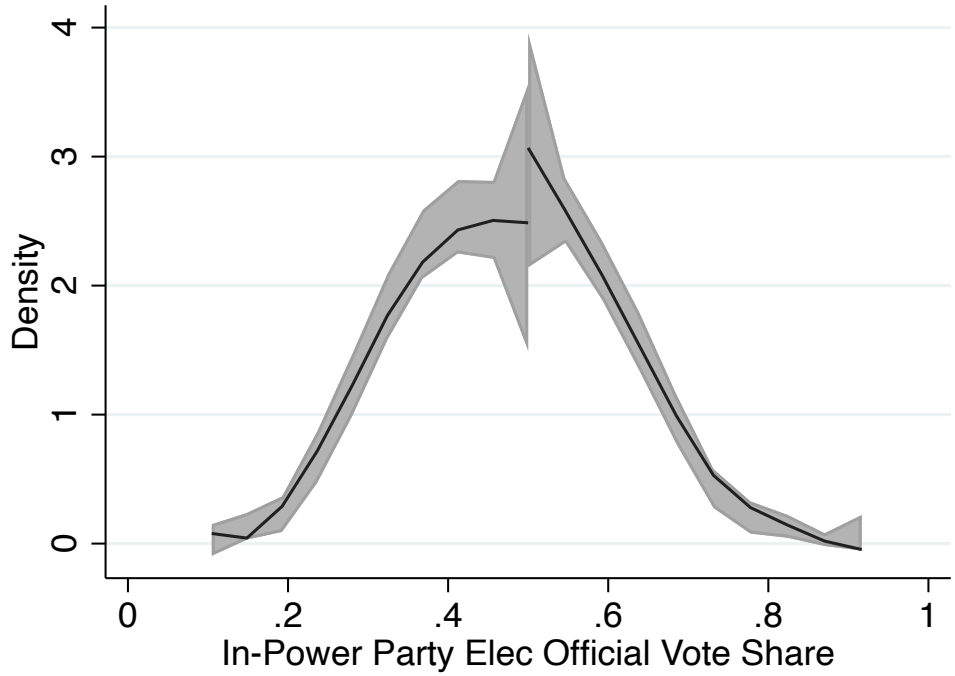


Figure A.2 presents the McCrary plot. While the party in power wins slightly more close elections than they lose, the difference in the densities is small enough that it could easily arise by chance.

A.6.3 Main Findings Not Sensitive to Choice of Estimator

As we discuss in Section 3.5, 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 thorugh 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 4. 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.**

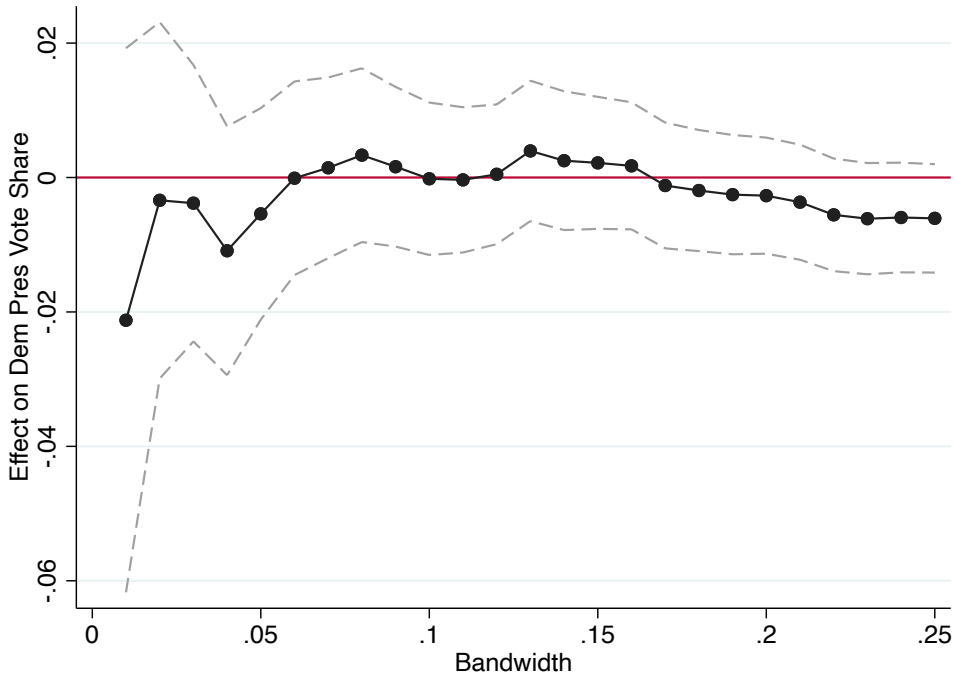
	Dem Pres Vote Share							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dem Elec Official	0.028	0.028	0.002	0.025	-0.005	-0.004	-0.006	-0.006
	(0.024)	(0.018)	(0.032)	(0.024)	(0.012)	(0.009)	(0.017)	(0.012)
N	401	721	202	464	343	618	175	444
Clusters	389	700	198	450	343	618	175	444
Bandwidth	0.08	0.17	0.04	0.10	0.07	0.14	0.04	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	0.045	0.079	0.059	0.031	0.023	0.043	0.029

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

In Table A.8, 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.8: **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	0.003	-0.002	-0.013	-0.003
	(0.009)	(0.006)	(0.013)	(0.009)
N	201	374	107	221
Bandwidth	0.08	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.6.6 Finding Not Sensitive to Excluding the South

In Table A.9, we present the results of our analysis focused only on counties in non-Southern states. We follow the U.S. Census Bureau defintion 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.9: **Effect of Democratic Election Officials on Democratic Presidential Vote Share, Non-Southern Counties.**

	Dem Pres Vote Share			
	(1)	(2)	(3)	(4)
Dem Elec Official	0.002	0.002	-0.007	0.000
	(0.008)	(0.006)	(0.012)	(0.008)
N	246	440	126	293
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.6.7 Finding Not Sensitive to Excluding VRA Counties

In Table A.10, 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. Our estimates are substantively similar to those reported in Table 1.

Table A.10: **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	0.002	0.002	-0.011	-0.002
	(0.007)	(0.005)	(0.009)	(0.008)
N	353	651	184	336
Bandwidth	0.08	0.16	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.11, 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.11: **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.6.8 No Substantial Average Effect in Senate, Governor, or Presidential Elections

In Table A.12, 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.12: **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	0.004	0.006	-0.007	0.003
	(0.007)	(0.006)	(0.011)	(0.007)
N	1146	2052	578	1574
Clusters	401	717	208	546
Bandwidth	0.09	0.17	0.04	0.12
Bandwidth Selection	CCT	CCT*2	CCT/2	CCT
Kernel	Uniform	Uniform	Uniform	Triangular
Min Detectable Effect	0.018	0.014	0.026	0.017

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.6.9 Effect Not Limited To Counties with Close Clerk Elections

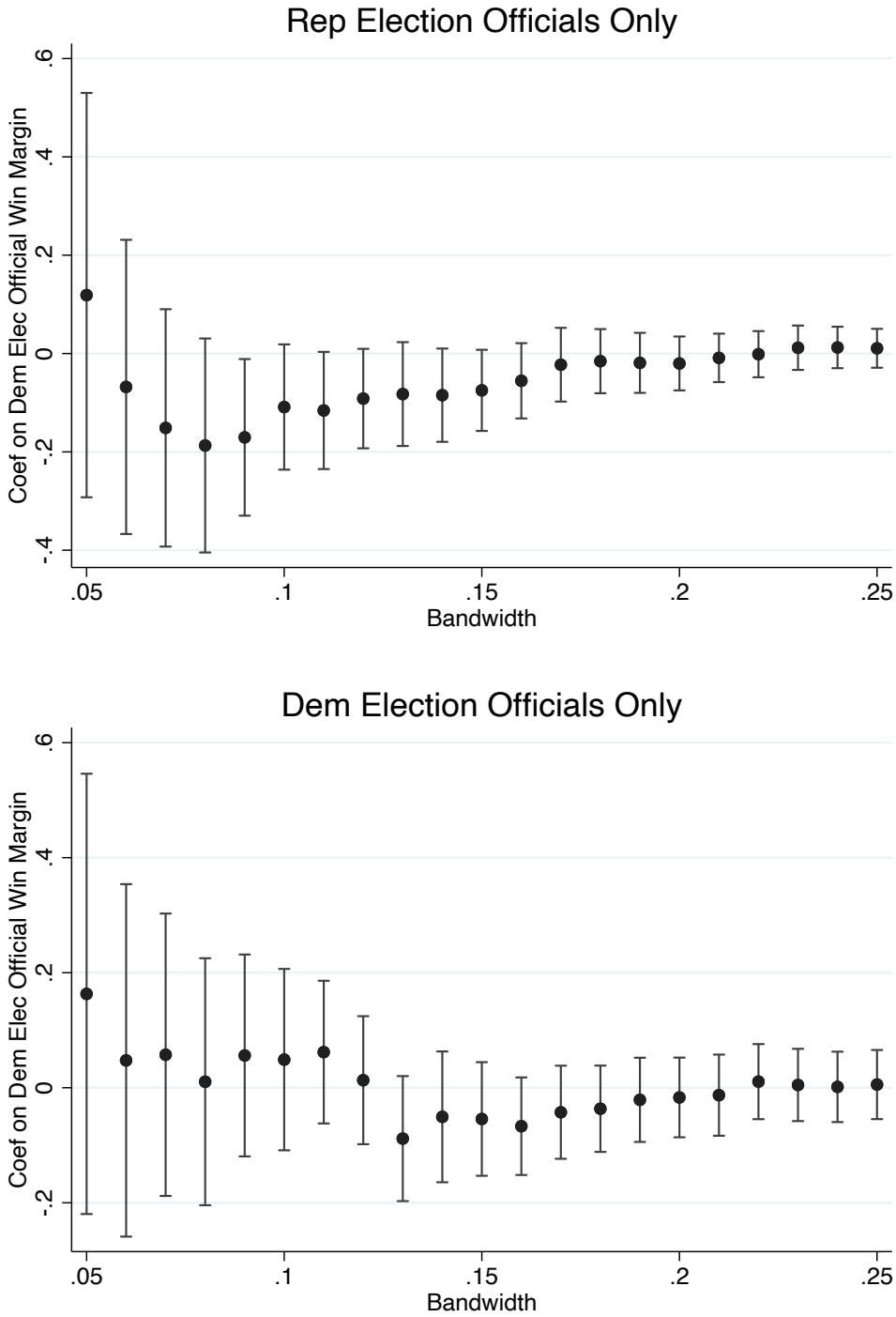
In this section, we draw heavily from Angrist and Rokkanen (2015) and Hainmueller, Hall, and Snyder Jr (2015).

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.4 report 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 4.1, 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.4: **Slope of Residual Dem Pres Vote Share on Dem Clerk Vote Share across Bandwidths.**



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 5.3, 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 census to compute a racial dissimilarity score across blocks within a county, following standard practice. We compute the residential dissimilarity score as

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

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.13, 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 party even in the most segregated counties. We further validate this finding in Figure A.5, which shows that our finding is not sensitive to the threshold we use to separate more and less diverse counties.

Table A.13: **Effect of Democratic Election Officials on Democratic Presidential Vote Share, More vs. Less Racially Segregated Counties.**

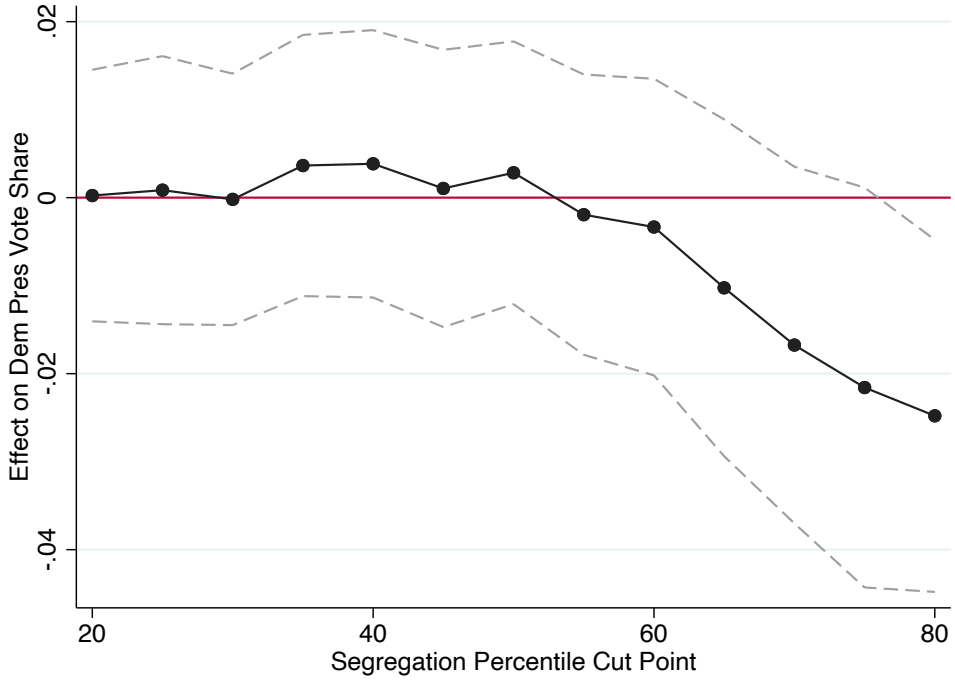
	Dem Pres Vote Share							
	Less Segregated				More Segregated			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dem Elec Official	-0.009	-0.002	-0.010	-0.005	-0.001	-0.002	0.005	0.003
	(0.010)	(0.008)	(0.013)	(0.009)	(0.008)	(0.006)	(0.011)	(0.008)
N	137	246	68	198	228	376	119	285
Bandwidth	0.05	0.10	0.03	0.08	0.11	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.5, 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.5: **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 traingular 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 aboved, 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.

Table A.14: **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	0.014	0.000	0.010	0.014
	(0.007)	(0.005)	(0.011)	(0.008)	(0.014)	(0.010)	(0.018)	(0.014)
N	290	512	147	275	103	192	53	103
Bandwidth	0.08	0.16	0.04	0.08	0.09	0.18	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

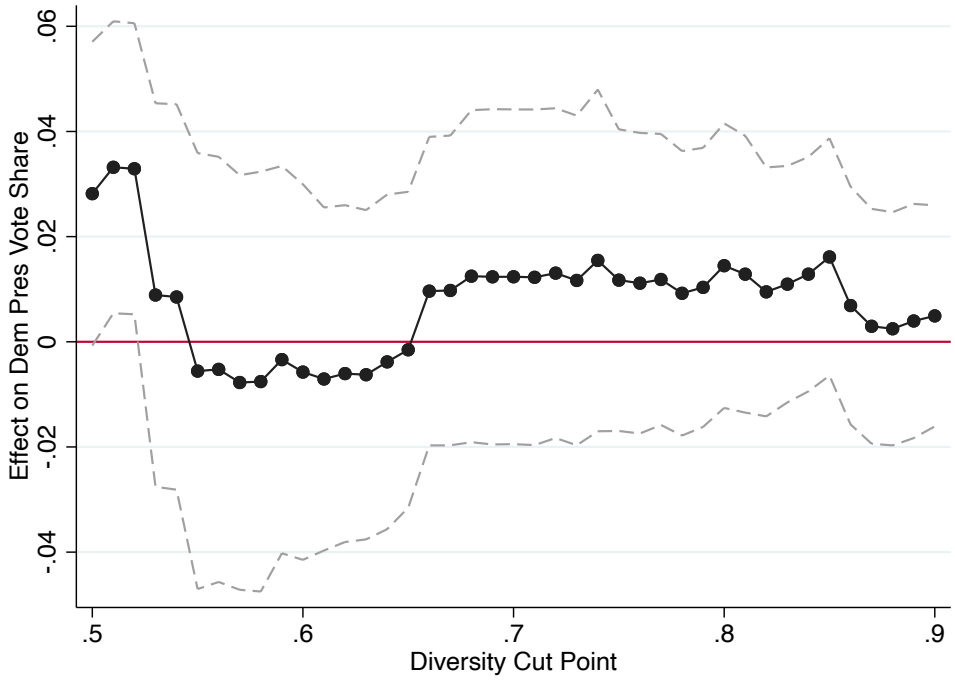
Robust standard errors in parentheses. The outcome is first regressed on a state- and year-specific lag using all counties including those for which clerk election results are not available. The regression discontinuity is estimated using the residuals from that regression. The bandwidth row reports 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.

We investigate this prediction in Table A.14. 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. 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.6, which shows that our finding is not sensitive to the threshold we use to separate more and less diverse counties.

A.7.2.2 Findings Not Sensitive to Definition of Diverse Counties

In Figure A.6, 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.6: **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 traingular 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 5.3, 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 Democrat-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.7 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.7: **In Model of Partisan Officials Seeking to Advantage Their Party, Effect on Democratic Presidential Vote Share Largest in Balanced Counties.**

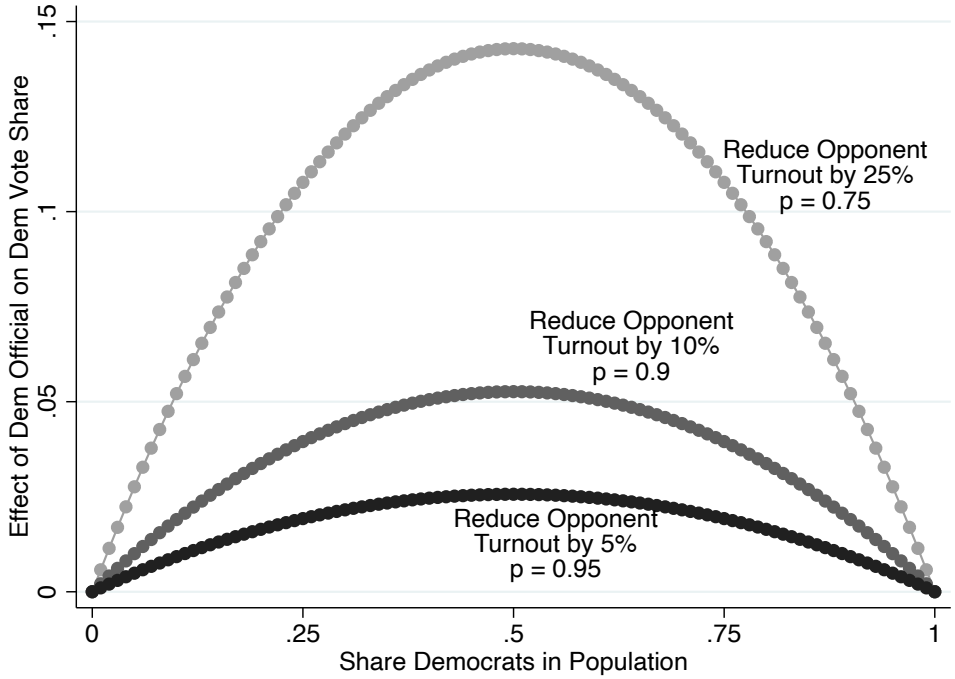


Table A.15: **Effect of Democratic Election Officials on Democratic Presidential Vote Share, Balanced vs. Imbalanced Counties.**

	Dem Pres Vote Share							
	Imbalanced				Balanced			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dem Elec Official	-0.000	0.000	-0.024	-0.004	0.000	0.003	-0.005	-0.002
	(0.012)	(0.008)	(0.022)	(0.010)	(0.008)	(0.007)	(0.012)	(0.009)
N	98	188	49	141	239	415	121	237
Bandwidth	0.07	0.14	0.03	0.10	0.07	0.14	0.04	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.7.3.2 Main Estimates of the Effect in Balanced Districts

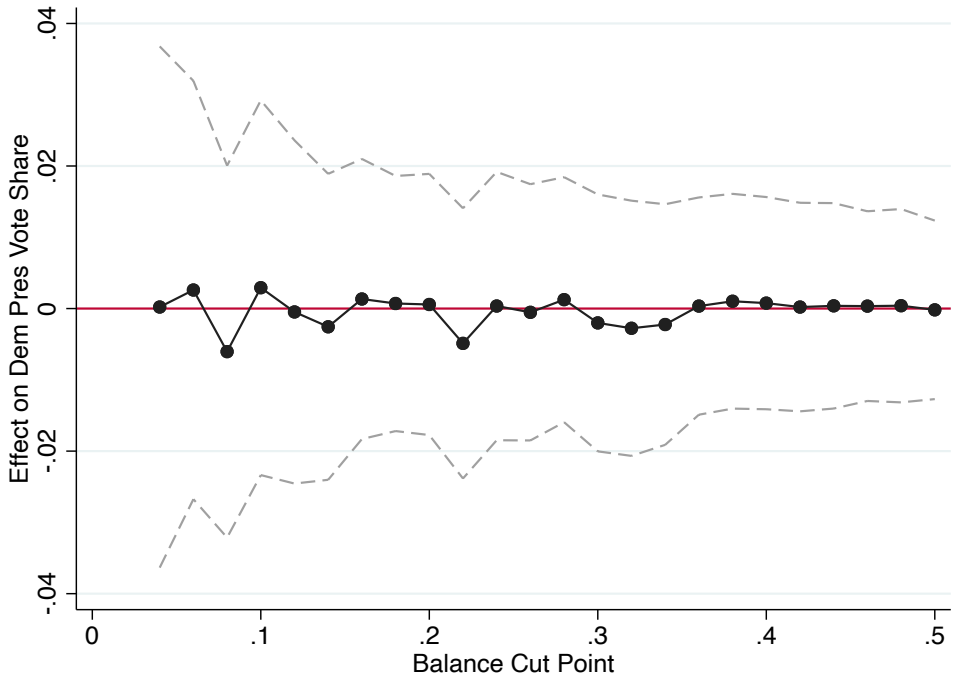
As we discussed in A.7.3.1, election officials who are solely motivated by advantaging their party will have an easier time doing so in places where the public is more evenly split between Democrats and Republicans. This allows us to make a prediction: if clerks are primarily motivated by providing their 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. Table A.15 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.7.3.1 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).

A.7.3.3 Findings Not Sensitive to Definition of Balanced Counties

In Figure A.8, 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.8: **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 traingular 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.16, 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. 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.

Table A.16: **Effect of Democratic Election Officials on Democratic Presidential Vote Share, Small vs. Large Counties.**

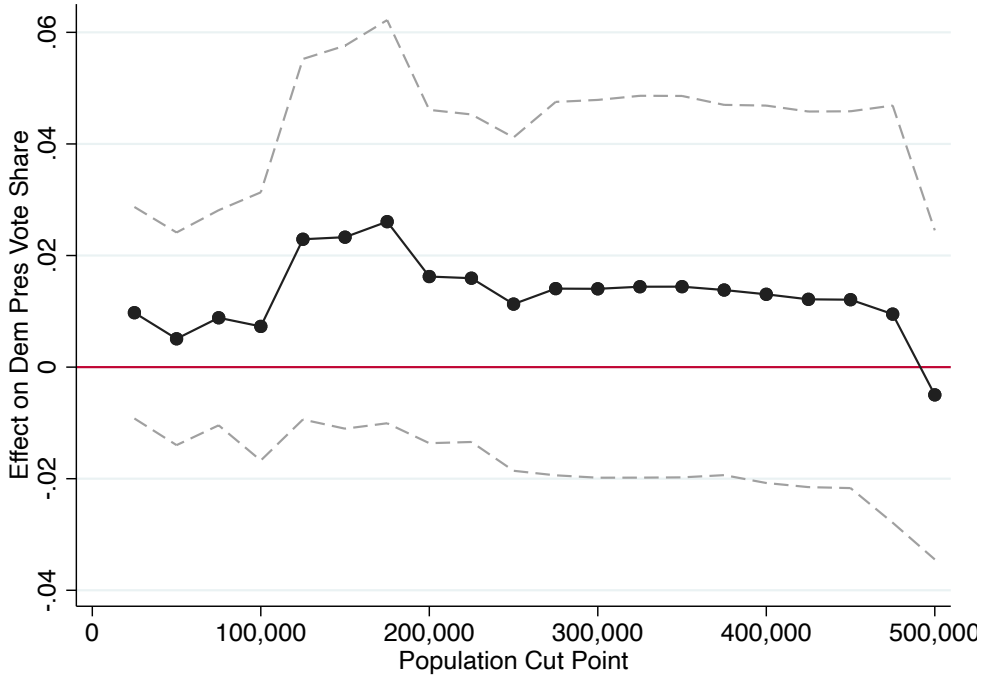
	Dem Pres Vote Share							
	Pop < 100k				Pop ≥ 100k			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dem Elec Official	-0.004	-0.001	-0.009	-0.004	0.011	-0.003	0.001	0.007
	(0.006)	(0.005)	(0.009)	(0.008)	(0.012)	(0.009)	(0.014)	(0.012)
N	340	577	181	287	81	147	41	94
Bandwidth	0.10	0.21	0.05	0.09	0.06	0.12	0.03	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.9, 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.9: **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 traingular 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.17, 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.17: **Effect of Democratic Election Officials on Democratic Presidential Vote Share, More vs. Less Competitive States.**

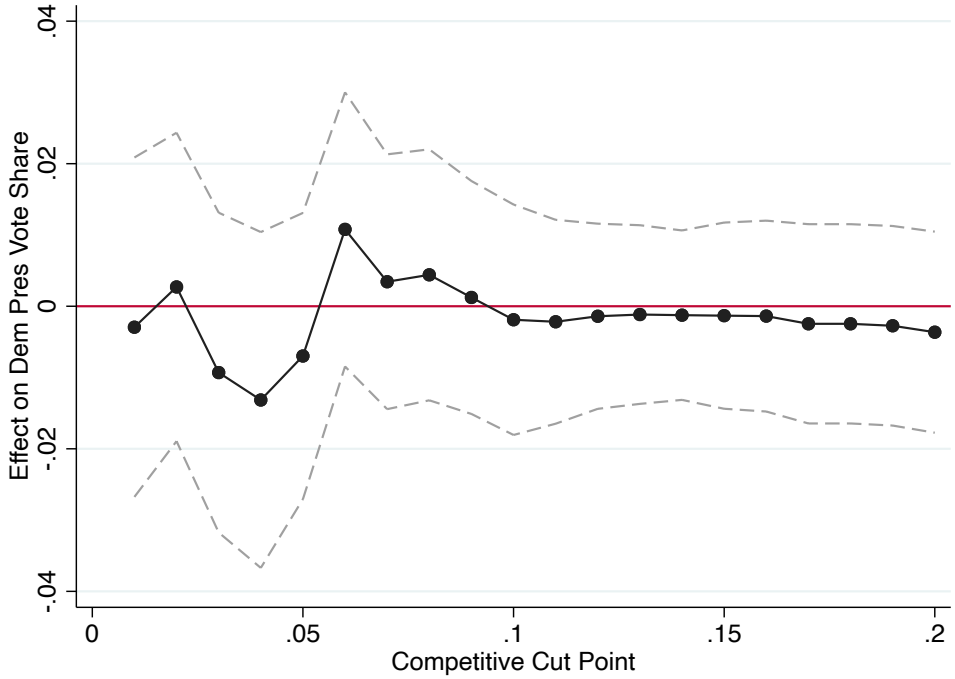
	Dem Pres Vote Share							
	Less Competitive				More Competitive			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dem Elec Official	0.001	-0.001	0.002	0.001	0.008	0.003	-0.003	-0.007
	(0.009)	(0.007)	(0.013)	(0.009)	(0.009)	(0.007)	(0.011)	(0.010)
N	234	427	117	264	149	244	81	101
Bandwidth	0.07	0.14	0.03	0.08	0.11	0.22	0.06	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.10, 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.10: **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 traingular 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.18, 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 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.18: **Effect of Democratic Election Officials on Democratic Presidential Vote Share, Determinative vs. Not Determinative Counties.**

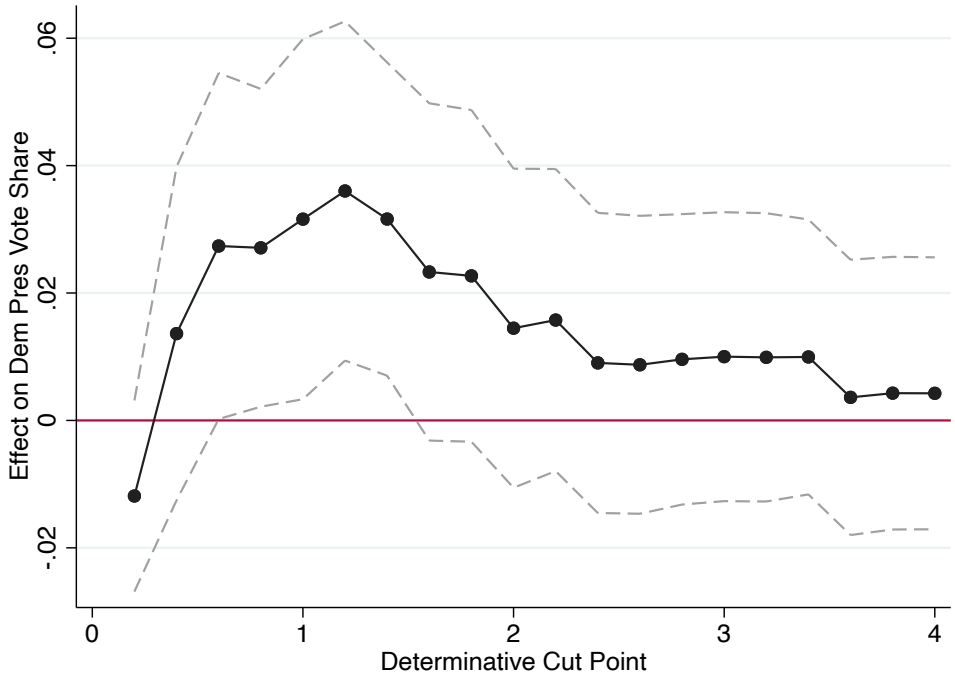
	Dem Pres Vote Share							
	Not Determinative				Determinative			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dem Elec Official	-0.004	-0.001	-0.013	-0.005	0.019	0.004	0.012	0.014
	(0.007)	(0.005)	(0.010)	(0.007)	(0.014)	(0.011)	(0.018)	(0.013)
N	310	529	164	354	72	141	39	96
Bandwidth	0.09	0.19	0.05	0.11	0.05	0.11	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.11, 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.11: **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 traingular kernel.



A.7.7 Effect of Electing a Democratic Clerk on All Policy Outcomes Collected

In Table 3 in Section 5.2, 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. 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 3.

Table A.19: **Effect of Democratic Election Officials on Polling Places.**

	Polling Places per 1k			
	(1)	(2)	(3)	(4)
Dem Elec Official	-0.049	0.001	-0.119	-0.066
	(0.085)	(0.071)	(0.097)	(0.086)
N	241	431	124	225
Clusters	180	319	96	167
Bandwidth	0.08	0.16	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.20: **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	195	348	102	178
Clusters	136	240	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.21: **Effect of Democratic Election Officials on Provisional Rejection Rate.**

	Provisionals Rejection Rate			
	(1)	(2)	(3)	(4)
Dem Elec Official	-0.067	-0.050	0.041	-0.062
	(0.063)	(0.045)	(0.085)	(0.060)
N	223	378	123	277
Clusters	154	254	86	187
Bandwidth	0.07	0.14	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.22: **Effect of Democratic Election Officials on Absentee Rejection Rate.**

	Absentee Rejection Rate			
	(1)	(2)	(3)	(4)
Dem Elec Official	0.003	0.011	0.010	0.010
	(0.017)	(0.013)	(0.020)	(0.015)
N	370	641	192	493
Clusters	242	419	128	322
Bandwidth	0.07	0.14	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.23: **Effect of Democratic Election Officials on Registration.**

	Registered Voters per VAP			
	(1)	(2)	(3)	(4)
Dem Elec Official	0.014	0.017	0.021	0.019
	(0.009)	(0.007)	(0.011)	(0.009)
N	659	1175	338	696
Clusters	385	688	200	408
Bandwidth	0.08	0.17	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.24: **Effect of Democratic Election Officials on Registration Removals.**

	Registrations Removed / Total Registrants			
	(1)	(2)	(3)	(4)
Dem Elec Official	0.004	0.003	0.002	0.004
	(0.008)	(0.006)	(0.011)	(0.007)
N	321	554	161	398
Clusters	204	355	105	256
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.25: **Effect of Democratic Election Officials on Democratic Registration Share.**

	Dem Reg Share			
	(1)	(2)	(3)	(4)
Dem Elec Official	0.000	0.001	0.004	0.001
	(0.007)	(0.005)	(0.008)	(0.006)
N	328	620	177	422
Clusters	192	358	104	244
Bandwidth	0.10	0.20	0.05	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.26: **Effect of Democratic Election Officials on Wait Times.**

	Share Over 30 min Wait			
	(1)	(2)	(3)	(4)
Dem Elec Official	-0.012	-0.034	-0.043	-0.019
	(0.025)	(0.018)	(0.030)	(0.022)
N	283	507	143	401
Clusters	191	352	93	274
Bandwidth	0.07	0.13	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.27: **Effect of Democratic Election Officials on Phone Location-Based Wait Times.**

	Average Wait Time			
	(1)	(2)	(3)	(4)
Dem Elec Official	-0.564	1.020	-2.454	-1.812
	(3.650)	(2.526)	(4.748)	(4.052)
N	31	50	21	30
Bandwidth	0.09	0.18	0.05	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.28: **Effect of Democratic Election Officials on Racial Disperities in Phone Location-Based Wait Times.**

	Average Wait Time Disparity			
	(1)	(2)	(3)	(4)
Dem Elec Official	-0.574	1.333	-13.312	-2.584
	(22.017)	(15.678)	(24.364)	(21.921)
N	29	45	19	33
Clusters	29	45	19	33
Bandwidth	0.08	0.16	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.