Online Appendix

To Elect or Appoint? Evidence from Local Election Administration

Intended for online publication only.

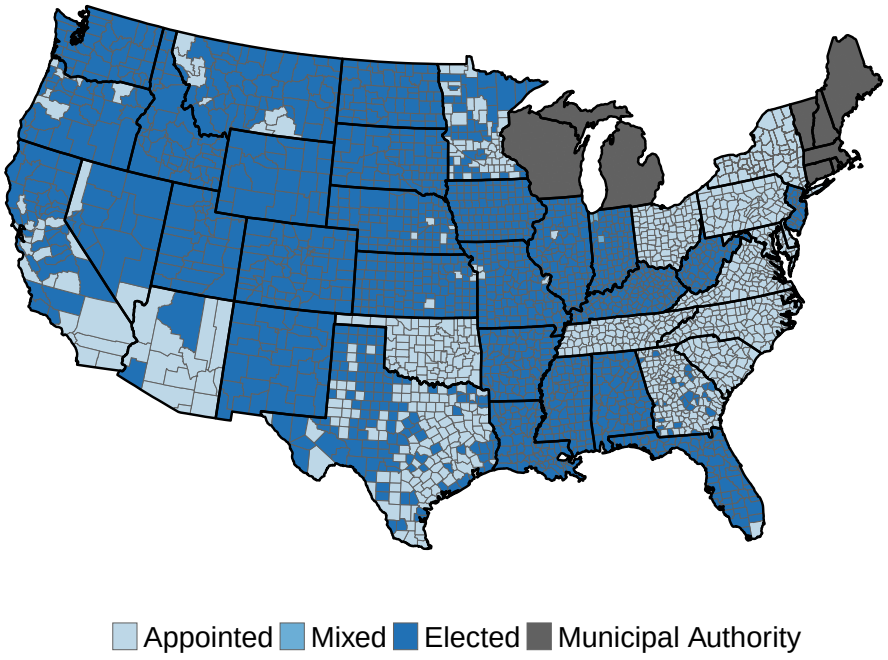
Contents

A.1 Descriptive Appendices

A.1.1 Local Election Official Selection Method Map

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

Figure A.1: **Local Election Official Selection Method by County**. This map displays the selection method of the central election authority for each county in the United States where elections are administered at the county-level, as of 2022. In counties where municipal jurisdictions have separate administrators, the selection method for the county official is reflected. Data is from Ferrer and Geyn (2024). All election jurisdictions in Alaska use appointed officials and all counties in Hawaii use elected officials.



A.1.2 Descriptive Comparison of the Data Sample

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

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

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

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

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

A.1.3 Local Election Official Selection Method Changes by State

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

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

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

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

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

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

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

	Appointed (1)	Elected (2)
Population (Thousdands)	164.89	63.44
	(465.11)	(221.15)
Dem Pres Vote Share	0.37	0.30
	(0.16)	(0.15)
Voter Turnout	0.62	0.63
	(0.10)	(0.10)
Voter Registration	0.86	0.89
	(0.09)	(0.11)
Share Non-Hispanic White	0.70	0.78
	(0.21)	(0.19)
Share Black	0.13	0.08
	(0.15)	(0.14)
Share Hispanic	0.10	0.10
	(0.14)	(0.14)
Northeast	0.13	0.00
Midwest	0.13	0.41
South	0.67	0.39
West	0.07	0.20
Num Counties	1092	1816

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

A.1.5 Descriptive Comparison of Counties that Switched from Elected to Appointed vs. Always Elected Their Local Election Official

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

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

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

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

A.2 Robustness Tests

A.2.1 Participation Effects Excluding Midterm Races

Table A.5 displays the results of a two-way fixed effects regression estimating the effects of directly electing a local election official on voter participation. These regressions only include data from presidential elections. The results are similar to those displayed in Table 1 in the main analysis, albeit slightly less precise.

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

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

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

A.2.2 Participation Effects with Alternative Administrative Data

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

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

	Voter Turnout			Registration Rate		
	(1)	(2)	(3)	(4)	(5)	(6)
Appointed	0.018	0.016	0.014	0.009	0.008	0.008
	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)
Counties	1116	1116	1116	942	942	942
Elections	28	28	28	13	13	13
Observations	31123	31123	31123	12213	12213	12213
Outcome Mean	0.50	0.50	0.50	0.84	0.84	0.84
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year x State FEs	Yes	No	No	Yes	No	No
Year x State x Dem vote share FEs	No	Yes	No	No	Yes	No
Year x State x Population FEs	No	No	Yes	No	No	Yes

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

A.2.3 Inclusion of County Time Trends

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

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

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

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

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

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

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

	Voter Turnout			Registration Rate		
	(1)	(2)	(3)	(4)	(5)	(6)
Appointed	0.006	0.005	0.005	0.000	-0.000	0.001
	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)
Counties	1116	1116	1116	942	942	942
Elections	28	28	28	13	13	13
Observations	31146	31146	31146	12216	12216	12216
Outcome Mean	0.50	0.50	0.50	0.84	0.84	0.84
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year x State FEs	Yes	No	No	Yes	No	No
Year x State x Dem vs FEs	No	Yes	No	No	Yes	No
Year x State x Pop FEs	No	No	Yes	No	No	Yes
County Time Trend	Yes	Yes	Yes	Yes	Yes	Yes

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

Robust standard errors clustered by county in parentheses.

A.2.4 Exploring State and Office Heterogeneity

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

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

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

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

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

which I group with clerks in this analysis due to their similar roles.[34] A few Texas counties use elected tax assessors as their election official. Auditor is also a fairly common position. All elected election officials in Minnesota and Washington are auditors, as well as a small number of counties in California. Finally, probate judges are the elected election officials in Georgia. Table A.14 shows that participation increases when appointed officials (the omitted category) replace elected auditors, clerks, and probate judges. The increase is larger when probate judges and auditors are replaced, and somewhat smaller when clerks are replaced. The point estimate for tax assessors is negative but imprecisely estimated, as it relies on a relatively small set of observations.

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

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

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

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

A.2.5 Results by Clerk Selection Method Reform Mechanism

One threat to causal inference is that reforms caused by some specific mechanism—state legislature, county legislature, and/or county referendum—are not exogenous to an increase in citizen participation. This seems most likely for referenda. Perhaps initial voter participation in a referendum that caused a change in clerk selection method spurs more turnout in future elections due to increased political efficacy. Or, perhaps the places with stronger cultures of direct democracy are more likely to have a referendum on the matter. Another scenario is that counties with local backing in the change are more likely to equip their newly appointed clerk with the tools to succeed or choose reform at the moment when it is most needed, compared with places where the state legislature initiates the reform. In Table A.15, I run regressions separating counties that have experienced a reform into three categories according to the reform initiator: county legislature, county referendum, and state legislature. Each regression also includes all counties that did not experience a move into or out of treatment throughout the dataset ("always elected" and "always appointed").

Table A.15: **Appointing Local Election Officials Increases Citizen Participation Across Reform Mechanisms**

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

Robust standard errors clustered by county in parentheses.

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

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

One related concern is that the reforms to clerk selection method that were initiated as part of a county charter suffer from similar endogeneity issues. The bundled treatment nature of these cases could also mean that the turnout effects are due to other changes in county governance that happened to coincide with the change to selection method. Table A.16 removes counties that changed their clerk selection method along with other amendments to their county charter. The results are similar to the main results shown in Table 1. Virtually all other reforms concerned only the clerk selection method itself or, in rare cases, a reorganization of a few county departments, and thus the turnout effects cannot be attributed to other state or local policy changes.

Table A.16: **Finding that Appointing Local Election Officials Increases Citizen Participation is Robust to Removing County Charter Changes**

	Voter Turnout			Registration Rate		
	(1)	(2)	(3)	(4)	(5)	(6)
Appointed	0.018	0.016	0.014	0.009	0.008	0.008
	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)
Counties	1108	1108	1108	934	934	934
Elections	28	28	28	12	12	12
Observations	30922	30922	30922	12112	12112	12112
Outcome Mean	0.50	0.50	0.50	0.84	0.84	0.84
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year x State FEs	Yes	No	No	Yes	No	No
Year x State x Dem vote share FEs	No	Yes	No	No	Yes	No
Year x State x Population FEs	No	No	Yes	No	No	Yes

Robust standard errors clustered by county in parentheses. Voter turnout and registration rate are measured as proportions out of 1. Counties that switched the selection method of clerk as part of a package of reforms to their county charter are removed.

A.2.6 Examining Dynamic, Group, and Time Period Effects of Appointing Election Officials

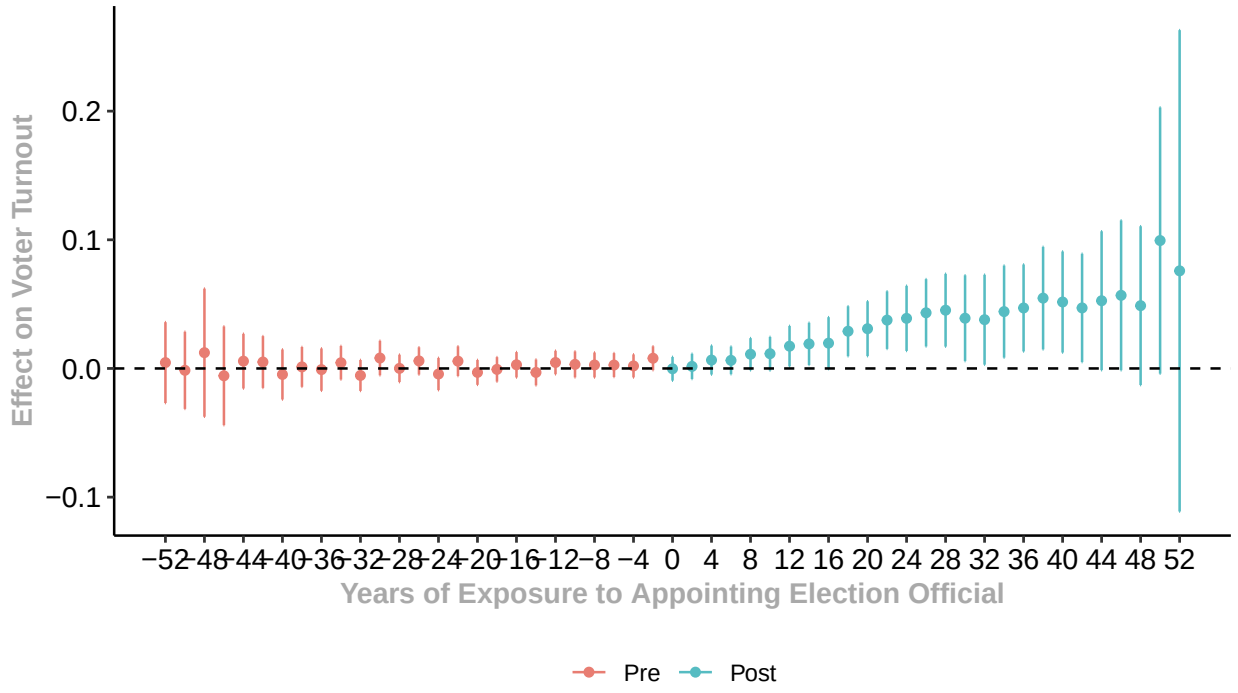
I use specifications from the Callaway and Sant'Anna (2021) estimator to examine dynamic, cohort, and time period effects of switching from elected to appointed clerks on presidential voter turnout. State dummies are used as covariates in these estimates to correct for state-specific trending in voter turnout. Dynamic effects are visualized in Figure A.2, cohort effects are visualized in Figure A.3, and time period effects are visualized in Figure A.4.

As seen in Figure 4 in the main analysis, the effect of appointments on voter turnout appears to increase over time for counties that switch to appointed administrators, relative to counties with elected officials. Two potential explanations exist for this: appointed officials increasingly outperform elected officials as their tenure lengthens, or the value of appointed officials over elected ones has grown over time. In the former scenario, institutional learning effects and start-up costs of switching selection methods mean appointed officials need the practice of administering a few elections to realize their full potential compared to elected officials. Recent work has found that voter wait times may increase after the turnover of a local election official, although turnout rates do not dip when a change of leadership takes place (Ferrer and Thompson 2024). In the latter scenario, the declining ability of voters to adequately select and sanction elected officials combined with the increasing technical demands of the job and growing recruitment problems create a bigger gap between elected and appointed officials over time.[35] The evidence I present regarding the differential effect of selection method by the presence of a local newspaper in Table 6 supports this theory.

Figure A.2 shows a fairly large increase in the effect on turnout several elections far after the initial switch, to about 5 percentage points. This estimator accounts for heterogeneous treatment effects but does not correct for pre-trending so should be interpreted cautiously. What is more plausible is the increase in effect magnitude shown in the generalized synthetic

[35] https://www.inquirer.com/politics/election/spl/pennsylvania-election-2020-officials-retiring-nightmare-20201221.html

Figure A.2: **Average Effect of Appointed Election Officials on Voter Turnout by Length of Exposure to Appointing**. Year 0 is the even-year general election after a county's first switch from electing to appointing an election official. Each point is the estimated effect of appointing an election official on voter turnout, at x years of exposure since first selecting the official via appointment and with state dummy covariates. The lines above and below each point represent 95-percent confidence intervals. Red points indicate pre-treatment effects, blue points indicate treatment effects. Estimates are from the Callaway and Sant'Anna (2021) estimator for dynamic two-way fixed effects designs, which corrects for bias due to heterogeneous treatment effects.



control (Figure 4 in the main analysis), which is an approximately one additional percentage point boost in turnout three elections after the switch to appointments.

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

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

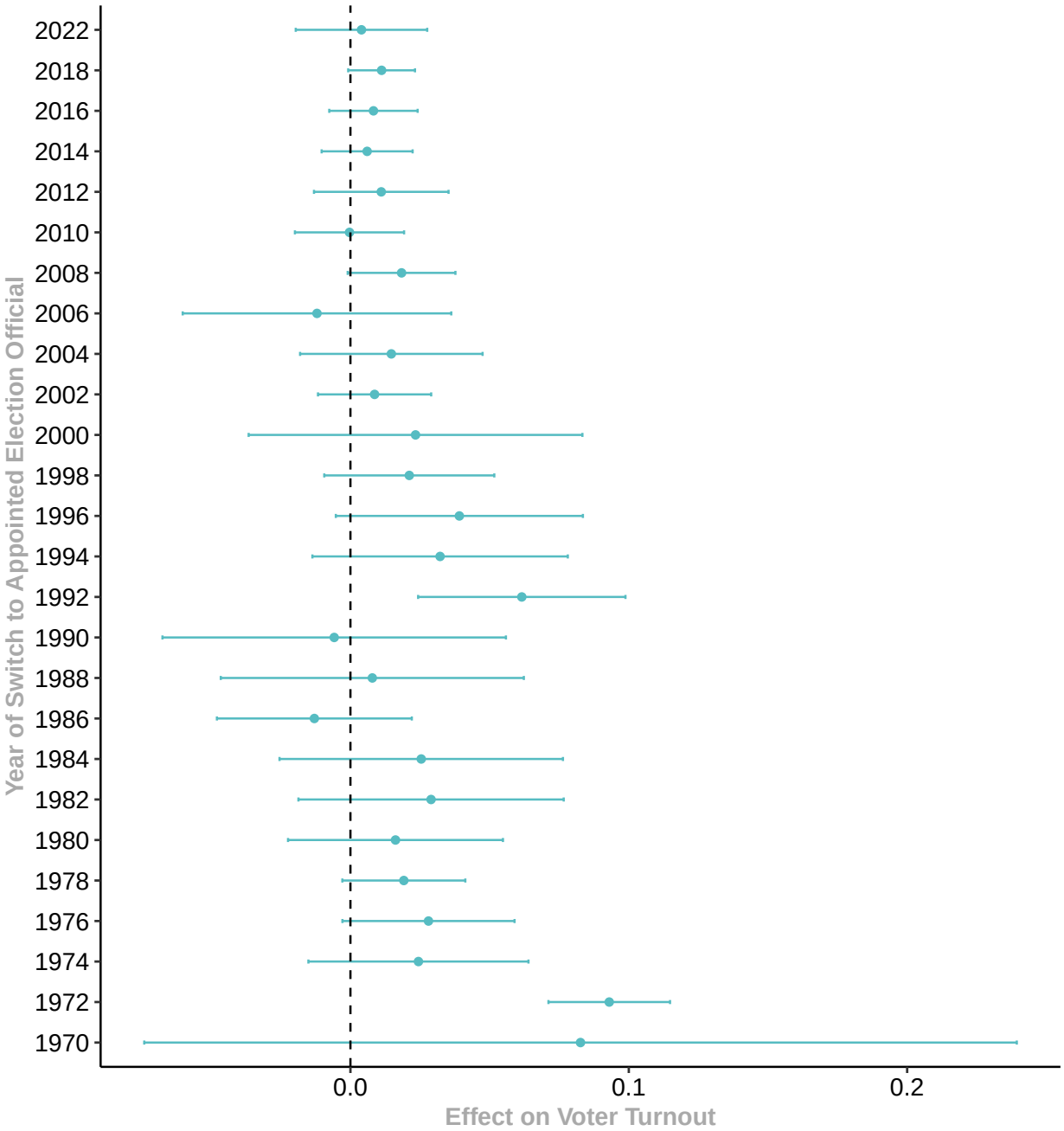
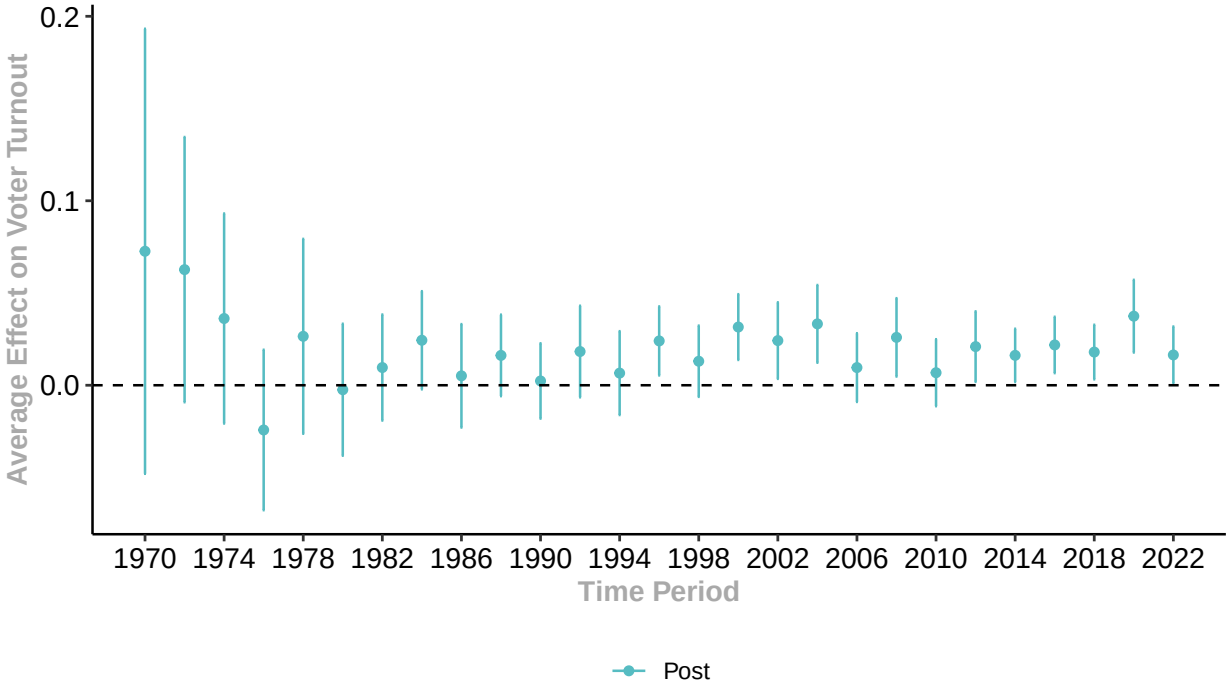


Figure A.4: **Average Effect of Appointed Election Officials on Voter Turnout by Time Period**. Each point is an estimate of the average time period effect of appointing election officials on voter turnout, with state dummy covariates. The lines above and below each point represent 95-percent confidence intervals. Estimates are from the Callaway and Sant'Anna (2021) estimator for dynamic two-way fixed effects designs, which corrects for bias due to heterogeneous treatment effects.



A.2.7 Are the Results an Artifact of the Jim Crow South?

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

Table A.17 displays three truncated cuts of the data: starting with the 1980 presidential election, the 1992 presidential election, and the 2000 presidential election. The main analysis displayed in Table 1 relies on turnout data beginning with the 1968 presidential election. Because registration data is only available from 1996, I focus on voter turnout here. The point estimates do grow similar when older data is discarded. However, in all specifications the point estimates are substantively large and statistically distinguishable from zero. In the most restrictive analysis, which only uses data from 2000 onwards, counties that switch to appointed clerks are estimated to boost turnout by half a percentage point.

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

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

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

A.3 Validation Exercises

A.3.1 Validating the Staggered Rollout Design with Alternative Estimators

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

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

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

I also use the (Imai, Kim, and Wang 2023) matching strategy for two other tests: voter registration rate in Table A.20 and election administration budget expenditures in Table A.21. The matching results for registration rates are also in line with those found in the

Table A.18: **Finding that Appointing Local Election Officials Increases Citizen Participation Is Robust to Alternative Estimators (Even-Year General Elections, 1968-2020)**

	Voter Turnout				
	(1)	(2)	(3)	(4)	(5)
Appointed	0.018	0.020	0.020	0.028	0.017
	(0.003)	(0.003)	(0.003)	(0.005)	(0.004)
Counties	1116	1085	1062	1062	873
Elections (avg)	28	28	28	28	28
Observations	31146	30366	29735	560421	153503
Outcome Mean	0.50	0.51	0.51	0.53	0.53
County FEs	Yes	Yes	Yes	Yes	Yes
Year x State FEs	Yes	Yes	Yes	Yes	Yes
App to Elect Excluded	No	Yes	Yes	Yes	Yes
Always Treated Excluded	No	No	Yes	Yes	Yes
Stacked DiD	No	No	No	Yes	Yes
Shortened Event Window	No	No	No	No	Yes

Robust standard errors clustered by county in parentheses. Column 1 is identical to the specification shown in column 1 of Table 1. Column 2 excludes 28 counties that switch from appointing to electing their clerks. Column 3 additionally excludes counties that have not elected their clerk since 1966. Column 4 implements a stacked difference-in-difference regression following the procedure described by Cengiz et al. 2019. Column 5 additionally shortens the event window for each county to within 8 years before its switch and within 16 years after its switch.

main analysis. The matching results for the expenditure analysis is uninformative due to the small number of matches made.

Table A.19: **Finding that Appointing Local Election Officials Increases Voter Turnout Is Robust to Imai et al. 2024 Matching Estimator (Even-Year General Elections, 1968-2020)**

	Voter Turnout		
	(1)	(2)	(3)
Appointed	0.015	0.013	0.015
	(0.017)	(0.017)	(0.017)
Matches	147	147	147
County FEs	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes
Pre-treatment voter turnout matching	Yes	Yes	Yes
State exact matching	Yes	No	No
State x Dem vote share exact matching	No	Yes	No
State x Population exact matching	No	No	Yes

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

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

	Registration Rate		
	(1)	(2)	(3)
Appointed	0.011	0.010	0.009
	(0.009)	(0.009)	(0.009)
Matches	124	123	124
County FEs	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes
Pre-treatment voter turnout matching	Yes	Yes	Yes
State exact matching	Yes	No	No
State x Dem vote share exact matching	No	Yes	No
State x Population exact matching	No	No	Yes

Bootstrapped standard errors with a degree-of-freedom adjustment in parentheses. All regressions use a pooled estimator that averages over the first 4 elections after treatment and matches over 4 elections prior to treatment. Matching is done using mahalanobis distance

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

	Ln expend per reg		
	(1)	(2)	(3)
Appointed	-0.027	0.096	-0.063
	(0.443)	(0.437)	(0.462)
Matches	10	10	9
County FEs	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes
Pre-treatment election expenditure matching	Yes	Yes	Yes
State exact matching	Yes	No	No
State x Dem vote share exact matching	No	Yes	No
State x Population exact matching	No	No	Yes

Bootstrapped standard errors with a degree-of-freedom adjustment in parentheses. All regressions use a pooled estimator that averages over the first 2 elections after treatment and matches over 2 elections prior to treatment. Matching is done using mahalanobis distance

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

I run the de Chaisemartin and D'Haultfœuille (2020) and the Callaway and Sant'Anna (2021) estimators separately for each state with at least 10 counties that have switched their election official selection method since 1960. The results are displayed in Tables A.22, A.23, A.24, and A.25. The de Chaisemartin and D'Haultfœuille (2020) estimator employs dynamic effects with placebos. The Callaway and Sant'Anna (2021) estimator employs dynamic effects after aggregating counties into cohorts that begin treatment at the same time. This estimator is very similar to the stacked difference-in-differences estimator displayed in column 4 of Table A.18. First, always treated units are removed from the dataset (i.e., counties that have appointed their election officials since at least 1960). This eliminates a handful of counties that were extremely early adopters of appointed election administrators. Next, each county's time period of first treatment is identified. The counties that switch from appointment to election are assigned to treatment even after their switch. Finally, those counties that are never treated (i.e., have always had elected election officials since 1960) are separated out as the "true control" by which each cohort can be compared with. Doing so avoids negative weights, thereby addressing the problems introduced by heterogeneous treatment and timing effects.

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

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

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

Robust standard errors clustered by county in parentheses.

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

	Voter Turnout		
	Two-Way FEs	de Chaisemartin and D'Haultfoeuille	Callaway and Sant'Anna
	(1)	(2)	(3)
Appointed	0.022	0.006	0.050
	(0.005)	(0.004)	(0.012)
Counties	159	159	155
Elections	28	28	28
Observations	4452	3305	5088
Outcome Mean	0.40	0.40	0.40
County FEs	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes

Robust standard errors clustered by county in parentheses.

Table A.24: **Main Finding that Appointing Local Election Officials Increases Voter Turnout is Robust to Alternate Specifications - Minnesota**

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

Robust standard errors clustered by county in parentheses.

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

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

Robust standard errors clustered by county in parentheses.

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

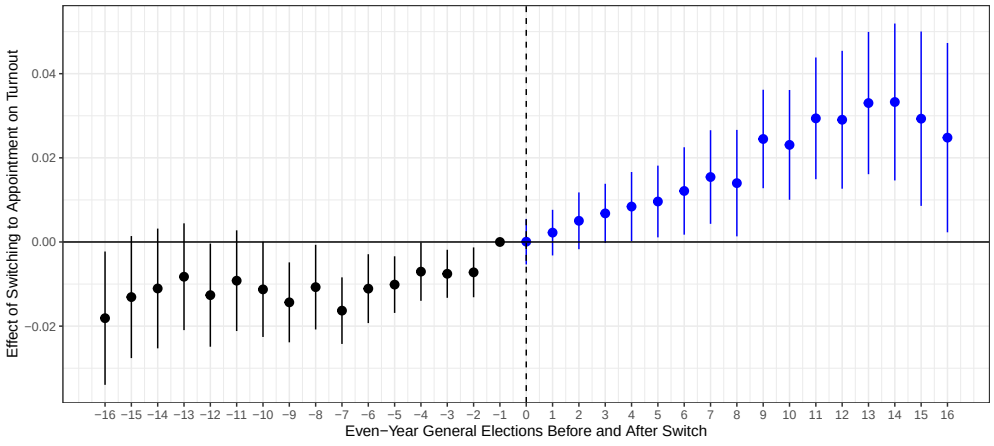
I investigate the validity of the parallel trends assumption for difference-in-difference specifications using the Dube et al. (2022) local projections event studies estimator. I make a series of pooled two-period two-group comparisons and estimate period-by-period effects, eliminating biases due to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

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

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

Finally, Figure A.8 examines the validity of the parallel trends assumption for the effect of appointments on expenditures. I find no evidence of pre-trending and imprecisely estimated

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



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

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

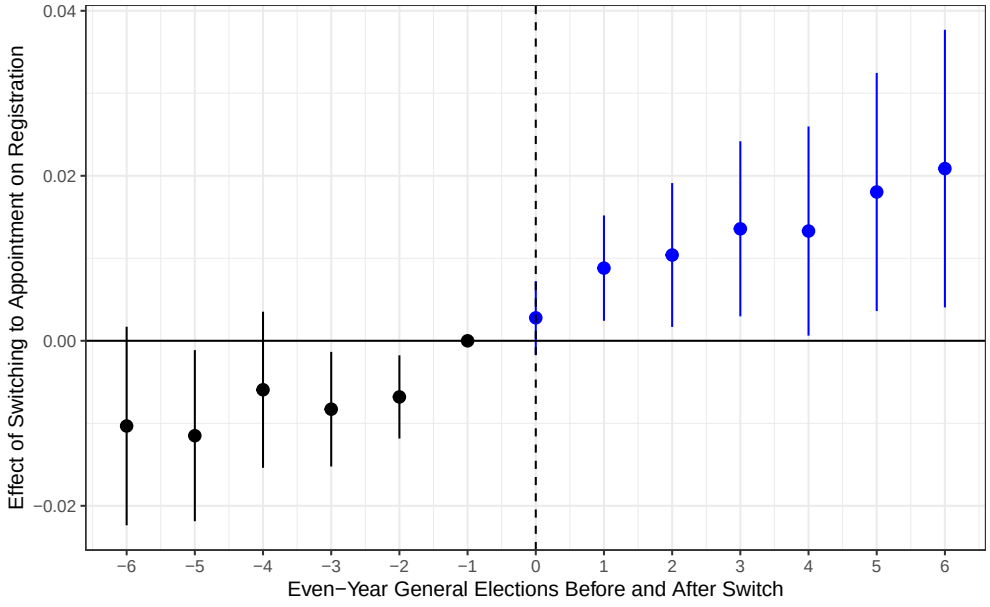


Figure A.7: **Dube et al. (2022) Local Projections Difference-in-Differences Estimate of Effect of Appointing an Election Official on Turnover**. Year 0 is the even-year general election after a county's first switch from electing to appointing an election official. Each point is the estimated effect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

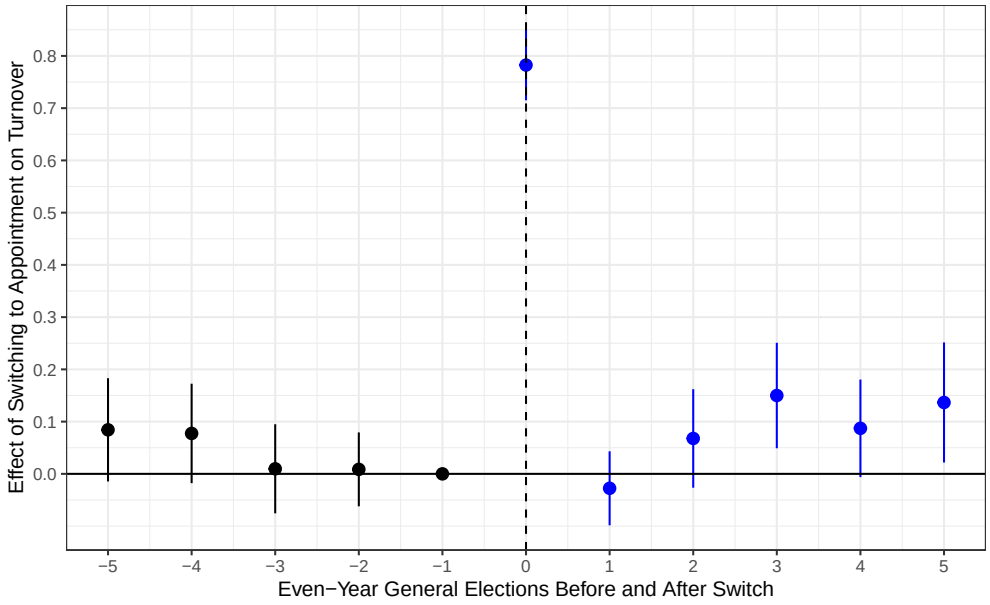
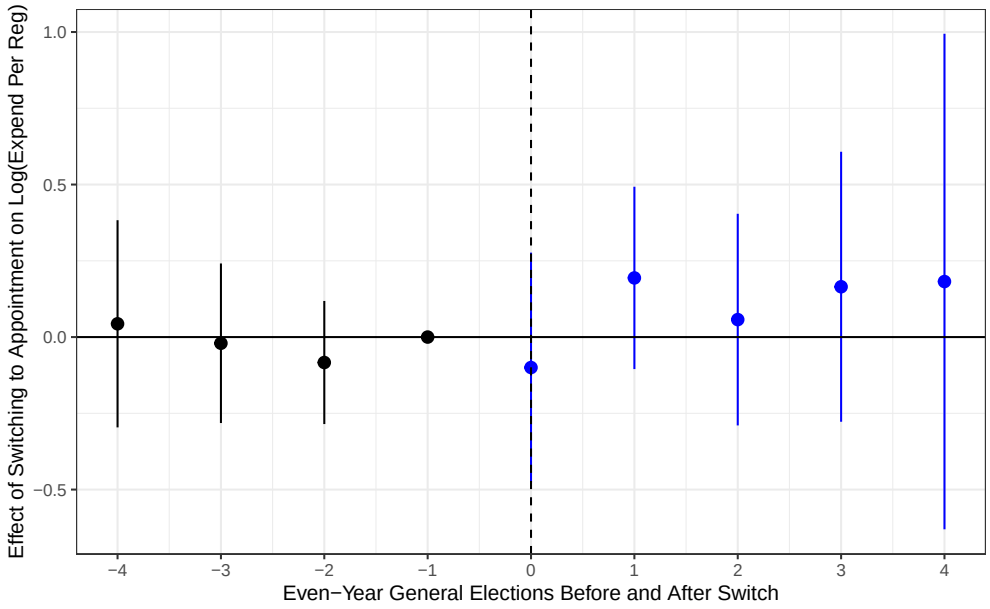


Figure A.8: **Dube et al. (2022) Local Projections Difference-in-Differences Estimate of Effect of Appointing an Election Official on Logged Expenditures Per Registrant**. Year 0 is the even-year general election after a county's first switch from electing to appointing an election official. Each point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



A.3.4 Generalized Synthetic Control Regression Output

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

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

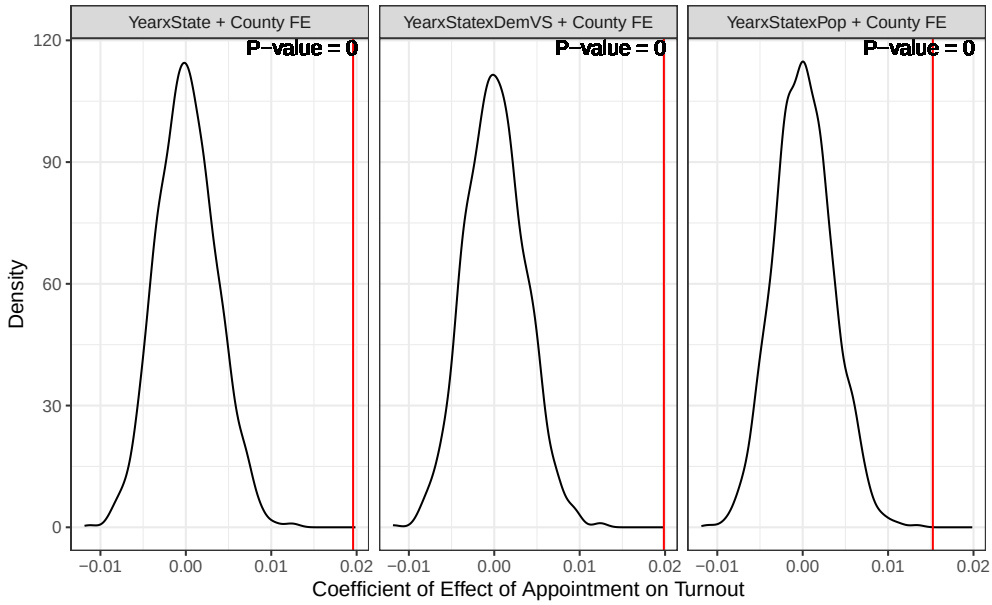
	Voter Turnout
	(1)
Appointed	0.008
	(0.006)
Counties	1042
Elections	28
Observations	29176
Outcome Mean	0.50

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

A.3.5 Randomization Inference Additional Output

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

Figure A.9: **Randomization Inference for Table 1, Columns 1-3 - Treatment and Timing**. This graph displays the output of randomization inference for the main effects of appointed local election officials on voter turnout. Both which counties are treated and when counties are treated are randomly permuted. The black distribution shows the resulting coefficients of 1,000 iterations. The red solid vertical line is the actual coefficient observed, and the p-value is the share of coefficients that are equal to or larger than the one estimated in the respective specification in Table 1.



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

In most states, the switch from elected to appointed election officials involves both registration administration and voting administration duties. In Arizona and Georgia, the shift only impacts voting administration; registration duties are primarily carried out by separate appointed officials. It is possible that election administrators in these states impact registration rates by referring individuals to registration officials or providing a better overall voting experience. However, if appointed officials outperform their elected counterparts, we should expect to see a larger effect on registration rates when the official directly in charge of registration duties switches from elected to appointed. Table A.27 displays the results of this placebo test. The first four columns individually test registration rates for the four states with at least 10 counties that have switched between electing and appointing their local election official: California, Georgia, Minnesota, and Texas. We should observe greater effects of appointed administration on registration rates in California, Minnesota, and Texas than in Georgia. The point estimate is smallest for Georgia, although the magnitude of the effect is similar across Georgia, Minnesota, and Texas.

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

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

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

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

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

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

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

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

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

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

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

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

Table A.29 shows difference-in-differences regressions testing the effect of a switch to a Democratic party controlled appointing body on three outcomes: Democratic presidential 2-party vote share (columns 1-2), Democratic gubernatorial 2-party vote share (columns 3-4), and share of registrants that are Democrats (columns 5-6). All specifications include

state-by-year fixed effects to account for differential partisan trending in each state. The even columns also include county linear time trends, because counties that switch from Republican to Democratic local leadership are likely trending in a Democratic direction. In other words, parallel trend concerns are particularly acute in this analysis. The inclusion of unit-specific time trends means that the outcomes are tested in excess of the underlying partisan trend specific to each county. Concerns exist that including unit linear time trends in a two-way fixed effects analysis might absorb potential treatment effects, biasing the analysis downward (Borusyak and Jaravel 2018; Wolfers 2006)—concerns I more fully explore in Section A.2.3. However, in this dataset many counties switch back and forth between Democratic and Republican control, making this a less pressing worry.

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

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

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

Two pictures emerge from these results depending on if the county time trend is included. Columns 1, 3, and 5 indicate that switching from a Republican-controlled appointing body to a Democratic-controlled body increases Democratic presidential vote share by 2.3 percentage points, increases Democratic gubernatorial vote share by 1.7 percentage points, and increases Democratic share of registrants by 2.7 percentage points. However, the odd columns show

this to be an artifact of pre-trending: counties that start electing Democrats majorities to their county legislature or a Democrat to their county chief executive also become more favorable to Democratic state and national candidates. The largest point estimate including unit-specific linear time trends (columns 2, 4, and 6) is three-tenths of a boost in Democratic gubernatorial vote share and Democratic share of registrants, but both fall well within a 95% confidence interval. The coefficient for Democratic presidential vote share is slightly negative. In sum, the evidence is consistent with no effect of appointed election officials benefiting their principals' majority party but not dispositive.

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

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

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

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

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

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

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

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

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

Table A.31 tests whether appointed election officials were more likely to apply for the Center for Tech and Civic Life's (CTCL) COVID-19 Response Grant program in September 2020.[36] I obtain data on CTCL applications as well as population, income, metro, non-Hispanic white share, COVID death rate, social distancing share, and National Association of Counties membership covariate data from Lal and Thompson (2024) and follow Lal and Thompson's specification strategy. I combine this data with 2020 election official selection methods across all counties ((Ferrer and Geyn 2024) and employ state fixed effects. In total, the data covers 37 states and over 2,600 counties.

A bivariate specification is shown in column 1, comparing the likelihood that appointed and elected counties within the same state applied for the CTCL grant. Counties with appointed election officials were 21 percentage points more likely to apply for the grant than counties that elect their election official. Column 2 controls for lagged Democratic presidential vote share to account for skepticism toward the grant among some Republicans.[37] This attenuates the estimated effect to 16 percentage points. Column 3 adds controls for logged county population and logged county median income. Column 4 adds an indicator for urban and suburban counties and a control for the share of the county that is non-Hispanic white. Column 5 adds controls for COVID death rate and the share of respondents to the Nationscape survey who reported always complying with recommended social distancing in Fall of 2020. Column 6 adds an indicator for county membership in the National Association of Counties. The effect magnitude is consistent at 7 percentage points across columns 3

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

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

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

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

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

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

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

Table A.32 presents the results of difference-in-difference regressions testing the effects of increased election expenditures on voter turnout. The first three specifications test the overall effects of an increase in expenditures on turnout and columns 4 through 6 test the additional effect of expenditures in small jurisdictions. The point estimates can be interpreted as the percentage change to voter turnout due to a doubling of election expenditures per registered voter. Column 1 shows that a doubling of election expenditures increases voter turnout by 0.27 percentage points on average. Column 4 shows that the effect appears concentrated in small counties, where a doubling of election expenditures increases voter turnout by 0.39 percentage points on average. There does not appear to be any relationship between election expenditures and turnout in populous jurisdictions.

Table A.32: **Additional Election Expenditures Increases Voter Turnout (Even-Year General Elections, 2004-2016)**

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

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

A.4.3 Using EAVS Data to Examine Administrative Outcomes

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

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

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

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

A.4.4 Appointed Election Officials May Pursue More Constituent Communication

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

Table A.34: **Appointed Local Election Officials May Be More Likely To Maintain Official Social Media Accounts**

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

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

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

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

In the main text, I use the 2020 EVIC Survey of Local Election Officials to show that appointed clerks possess more formal education than elected clerks serving in similarly sized jurisdictions within the same state. Table A.35 shows the results of additional indicators of official quality. Column 1 tests whether appointed clerks possess greater previous experience in election administration than elected officials. I find that appointed officials actually possess 1.6 fewer years of election administration experience upon assuming their current position in the field. In column 2, I show that appointed officials are a member of marginally more professional election administration organizations than elected officials, but the difference is small and cannot be confidently distinguished from 0. Column 3 shows that appointed officials are slightly more likely than elected officials to have served as an election official in other jurisdictions. Among those who have served in elsewhere, appointed officials are much more likely to have served in multiple other jurisdictions (column 4). Appointed officials are 15 percentage points less likely to be 65 years of age or older (column 5) and make $5,000 more a year on average than elected officials in the same state serving jurisdictions of a similar size. While this effect is statistically indistinguishable from zero, it represents an 8% salary premium. In column 6, I find that appointed officials hire an additional 0.6 FTEs, approximately 10% more than elected officials, although we cannot rule out that the finding arose by chance.

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

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

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

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

potentially an artifact of higher turnover rates among appointed officials, which is examined in Section 5.3.3.

A.5.2 Voter Knowledge Survey Technical Appendix

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

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

A.5.3 Local Newspaper Analysis

Table 6 in the main analysis examined the differences in the effect of appointments on citizen participation based on whether jurisdictions continuously had a local newspaper between 1968 and 2020. Table A.36 allows counties to switch in and out of having a local newspaper. The results are in line with those found in Table 6.

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

	Voter Turnout			Registration Rate		
	(1)	(2)	(3)	(4)	(5)	(6)
Appointed	0.026	0.024	0.020	0.011	0.011	0.006
	(0.005)	(0.005)	(0.005)	(0.006)	(0.007)	(0.006)
Appointed X Newspaper	-0.021	-0.021	-0.014	0.000	0.001	0.007
	(0.006)	(0.007)	(0.007)	(0.008)	(0.009)	(0.009)
Counties	1243	1243	1243	1011	1011	1011
Elections	14	14	14	7	7	7
Observations	15571	15571	15571	6577	6577	6577
Outcome Mean	0.57	0.57	0.57	0.85	0.85	0.85
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year x State x Newspaper FEs	Yes	No	No	Yes	No	No
Year x State x Dem vs x Newspaper FEs	No	Yes	No	No	Yes	No
Year x State x Pop x Newspaper FEs	No	No	Yes	No	No	Yes

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