Should I Mail or Should I Go: Voting Behavior After a One-Time All-Postal Election*

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Abstract

We investigate how reducing information costs due to forced experimentation with postal voting affects voting behavior. Leveraging a natural experiment during the Bavarian 2020 Mayoral Elections, we employ an event study design. We find a transitory increase in total turnout and a persistent substitution from in-person to postal voting. Notably, municipalities with a higher turnout in the past show a larger effect. Investigating the distribution of the information costs shows an age gradient with the highest information costs for the oldest age group. The conservative governing party (CSU) gains persistently from higher postal turnout and other conservative parties' in-person voters.

Keywords: Postal voting, Voter turnout, Local elections, Information costs, COVID-19 pandemic, Bavaria, Political participation, Election administration, Public policy experiment

JEL-Codes: D72, H11, H70, D83

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1 Introduction

Policies that alter decision-making costs can lead to lasting behavioral changes, even after the policies are removed. This idea is central to the Porter Hypothesis, which states that updating information by forced experimentation can lead to more efficient behavior (Porter, 1996). While Porter (1996) investigates this in the context of environmental regulation, this idea is applicable more generally. For instance, Larcom et al. (2017) extend it to optimized commuting paths in the context of the London Underground when strikes forced commuters to alter their route. A notable instance of a temporary policy change affecting decision costs in the context of voting occurred during the 2020 Bavarian Local Elections, when the state government, in response to the surge in COVID-19 cases in March 2020, mandated that the runoff be conducted entirely by absentee ballots.[1] After this one-time intervention, voting rules reverted to their original form.

This paper investigates the impact of this one-time all-postal runoff during the mayoral elections in Bavaria, Germany, in March 2020 on subsequent voting behavior in the 2021 Federal and 2023 State Elections. Using administrative data at the municipality level, we employ an event study design to analyze the dynamics of postal, in-person, and total turnout between municipalities with the all-postal runoff (treatment group) and municipalities without it (control group). We find that the all-postal runoff leads to a short-term increase in total turnout (in $t+0$, the 2021 Federal Election) and a persistent substitution from in-person to postal voting, even 3.5 years after the treatment (in $t+1$, the 2023 State Election). A key feature of the data is that we can also differentiate party results by the mode of voting. We find an increase for the CSU, the conservative and by far the largest party in Bavaria, stemming from an increase in its postal vote share while not decreasing its in-person vote share. This result likely originates from changed party preferences of other right-wing voters as there is a decrease in their in-person vote share without an increase in their postal vote share, and potentially as well from compositional effects of the treatment on the electorate as we find stronger effects, for instance, for municipalities with a high turnout in the past. However, changes in party preferences might not be separable from the pandemic.

Importantly, the administrative voting rules and procedures did not change before or after the treatment in the six federal and state elections we analyze between 2013 and 2023.[2] This is

[1] The most common regulatory response concerning organizing elections during the pandemic was to postpone elections to a later date, as was the case, for example, in Poland, Austria, France, or Spain. The citizens of Vorarlberg, Austria, additionally were encouraged to vote by mail (Radjenovic et al., 2020). Remotely related to our research is the effect of COVID-19 and other non-political crises on voter turnout. For example, Campante et al. (2024), Fernandez-Navia et al. (2021), Leromain and Vannoorenberghe (2022), Picchio and Santolini (2022), Gasper and Reeves (2011), or Stancea and Muntean (2024) find a negative effect of crises on voter turnout. However, Giommoni and Loumeau (2022) find that stringent restrictions as a policy response to COVID-19 increase voter participation. Baskaran et al. (2023) find a negative effect on the incumbency advantage. Instead, more competent candidates are elected.

[2] Postal voting has long been readily available and widely used, possibly reducing the treatment effect compared to a setting with no postal voting option. Germany, in general, and Bavaria, in particular, already had a long tradition of postal voting. It was introduced in West Germany in 1957 and had a high postal turnout before 2020. In the 2017 Federal Election, 28.6 (37.3) percent of cast votes in Germany (Bavaria) were postal ballots (https://www.bundeswahlleiterin.de/dam/jcr/b4aeabb8-7fac-473e-8581-cd718cb7a007/BTW_ab94_briefwahl.pdf, accessed 03.03.2024). Most of the world, for instance, Italy, France, or Brazil, has no convenient alternative to polling place voting. All over Germany, postal voting is a common practice and easily

a specific feature differing from the literature on the introduction of postal voting (e.g., Funk, 2010; Hodler et al., 2015). Thus, a change in postal voting costs stems from a reduction in individual information costs of voting only. Moreover, there is a large share of eligible voters with already no information costs for postal voting.

Our paper contributes to the literature investigating how altered voting costs affect voting behavior. There is extensive literature on how changing electoral costs through administrative policies affect voter participation.[3] These studies primarily focus on altered *in-person* voting costs. Findings on how administrative changes in *absentee* voting costs affect voter turnout diverge significantly. Gerber et al. (2013), Frank et al. (2023), and Hodler et al. (2015) find a positive effect of increased availability of postal and early voting on overall voter turnout, identifying convenience and flexibility offered by postal voting, and therefore reduced voting costs, as key factors. This effect might be lower where postal voting participation diminishes the social pressure to vote, as shown by Funk (2010) for small Swiss municipalities. Moreover, negative unanticipated consequences of early voting might be the reduced civic significance of elections (Burden et al., 2014). We contribute to this literature by analyzing the effect of removed information costs in a setting with constant administrative voting costs.

The all-postal runoff in March 2020 can be seen as an implicit first stage that we do not include in our main framework as we investigate federal and state elections only. We assume that in the all-postal election, some voters who have never voted by mail before are making their first contact with postal voting. Several studies have shown that all-postal elections can increase total turnout at least slightly (e.g., Thompson et al., 2020; Barber and Holbein, 2020; Karp and Banducci, 2000; Gerber et al., 2013; Frank et al., 2023). We differ from this strand of the literature by investigating turnout dynamics after the all-postal elections were repealed, in our case, after a one-time all-postal election. Frank et al. (2023) use the same policy change as we do and show that for Bavarian municipalities with more than 10,000 inhabitants, turnout was about 10 percentage points higher in this runoff than it would have been in a regular runoff. Our data, which also includes smaller municipalities in a comparable design, confirms a similar increase in total turnout for the treated municipalities of about 8 percentage points. Unfortunately, turnout is not separately provided by voting mode for local elections. However, an increase in total turnout during an all-postal election inherently implies an even larger, potentially much larger, increase in postal turnout. Thus, we are confident that the all-postal election made some

accessible (by simple request) for all eligible voters (similar to the UK and parts of the US). However, this is only the case for about 15 percent of the countries in the European Union and for only about 6 percent of all countries.

[3] Cantoni and Pons (2021) find no overall effect of stricter ID laws on voter turnout, though notable heterogeneities for socio-economic groups. Garmann (2017) and Potrafke and Roesel (2020) provide evidence that a change in polling place opening hours affects turnout. Scholars have investigated the turnout effects of the introduction of compulsory voting with weakly enforced fines (Hoffman et al., 2017) and also its abolition (Bechtel et al., 2018). Fujiwara (2015) studies voting technology and Braconnier et al. (2017) registration procedures. There are a few studies investigating changes in in-person voting costs due to polling place relocations and the substitution effect to postal voting (Alipour and Lindlacher, 2025; Clinton et al., 2021; Tomkins et al., 2023). For an extensive overview, see Cantoni et al. (2024).

voters experiment with postal voting who had never voted by mail before and would not have experimented with postal voting in the absence of the all-postal election.[4]

During the all-postal runoff, some voters experience postal voting for the first time, reducing information costs for postal voting for them in future elections. Having a simple rational choice model of voting in mind (Riker and Ordeshook, 1968), this should increase postal and decrease in-person turnout as postal voting costs decrease relative to in-person voting costs. Further, total turnout should increase as, for some abstainers, voting costs fall below the benefits of voting after the experimentation with postal voting. However, during the all-postal runoff, postal voting was not only the only voting mode, its procedure was also simplified in this one election as all eligible voters received the postal ballot papers directly with the runoff information instead of requesting the postal ballot after receiving the election information. Hence, eligible voters who came in contact with postal voting for the first time during that election might *perceive* different postal voting costs as for regular elections. Thus, *after* voting (by mail) in the 2021 Federal Election, they might again adjust their postal voting costs, this time to a *higher* value for all *subsequent* elections. This should result in a lower postal turnout in the 2023 State Election in comparison to the 2021 Federal Election but not to pre-treatment elections.

Our findings show meaningful and lasting impacts of the experimentation with postal voting. On average, the all-postal runoff causes a temporary increase in total turnout of 0.4 percentage points, 0.5 percent at the mean, in the 2021 Federal Election ($t + 0$), 1.5 years after the treatment.[5] This increase results from a stronger increase in postal turnout in comparison to a reduction in in-person turnout (1.1 versus -0.7 percentage points). In the 2023 State Election ($t + 1$), 3.5 years after the treatment, a small but statistically significant decline in the postal turnout point estimate to 0.7 leads to a vanished effect on total turnout, while the point estimate for in-person turnout remains at -0.7. Consequently, while the switch from in-person to postal voting persists, the overall increase in turnout is only temporary. This is similar to studies on the abolition of compulsory voting (Bechtel et al., 2018; Gaebler et al., 2020). Compulsory voting changes voting costs but does not reduce any information costs as there might be no unfamiliarity with voting. Consequently, abolishing it does not alter voting costs in comparison to before its introduction. However, our setup differs as it alters the information costs of a voting mode that was unfamiliar to some voters before. Thus, while there are no permanent effects on overall turnout, we find lasting effects on postal and in-person turnout. Similar to Alipour and Lindlacher (2025), there is an alteration in the second subsequent election after the treatment. While in Alipour and Lindlacher (2025) some eligible voters are surprised by the polling place relocation and cannot request the postal ballot and thus return by postal voting

[4] Of course, it is also likely that some voters experiment with postal voting for the first time due to the pandemic. We see a large increase in postal voting in 2021 (larger than in 2023). However, an increase on average is absorbed by the fixed effects. Additionally, a larger share of postal voters would only lead to an underestimation of the true effect. We show, for robustness, estimates where the control group only consists of off-cycle elections, i.e., municipalities that did not vote at all in 2020.

[5] This is comparable to the turnout increase of two additional early voting day in the US (Kaplan and Yuan, 2020) or of a 5 percent increase in opening hours of polling places in Austria (Potrafke and Roesel, 2020). The effect sizes are very similar to those in Alipour and Lindlacher (2025), but naturally in the opposite direction, as their study examines increases in in-person voting costs due to polling place relocations, whereas our paper focuses on reductions in postal voting costs.

only in the next election, we rationalize our finding by a second updating of the postal voting costs. As postal ballots were sent out automatically only in the all-postal runoff, postal voters in the 2021 Federal Election had to request postal voting. Thus, they realize only then their actual postal voting costs and have to update them for the 2023 State Election.

One might be worried that other circumstances around the all-postal runoff may cause the estimated effects. We, therefore, run placebo tests by going back in time, using the 2014 Local Elections in Bavaria and going to another German state, Hesse, using the postponed local elections from 2020 and 2021, to ensure that it is not the runoff itself and that the runoff took place during a pandemic. Then, we use municipalities that had an all-postal election due to a runoff in the 2020 County Commissioner Election in Bavaria. Here, municipalities with and without a mayoral runoff do not have different turnout effects as everyone experiments with postal voting in the all-postal county commissioner runoff.

In our setting, postal voting becomes universal for only one election. We are thus able to investigate lasting changes in electoral participation and the mode of voting in a setting with constant administrative voting costs. Our study may add to the existing body of research on habit formation in voting, which posits that voting behavior itself enhances its perceived value, thereby increasing the likelihood of future voting.[6] However, information costs might not only be adjusted once after the forced experimentation but updated after experiencing postal voting in a regular election. We can reject habit formation because we find statistically significant deviations from the previous election, respectively, aligning with our theoretical considerations on adjusted voting costs.

We investigate which parties profit from the higher turnout.[7] For left-wing parties, results look very similar to the turnout effects but show smaller coefficients: There is only a temporary increase in the parties' vote share of 0.2 percentage points, which lacks statistical significance, and persistently only substitution in the mode of voting by less than half a percentage point. In contrast, the largest conservative party (CSU) has very similar estimates in both post-treatment elections ($t{+}0$ and $t{+}1$) and gains in the total vote share by about 0.3 percentage points. Instead of a stronger substitution, there is no loss in the in-person vote share but an increase in the postal vote share. This is about the size of other right-wing parties' losses in the in-person vote share. We rationalize these *persistently* changed party preferences with voters giving credit to the CSU for how they managed the pandemic and specifically how they were able to retain the local elections as an important good for democracies. However, this is not strong enough to convince left-wing voters and might be strongly associated with the pandemic and thus not be generalized as an implication of lifted information costs. Additionally, heterogeneous effects may change the composition of the electorate, potentially favoring the CSU as well.

[6] Fujiwara et al. (2016) finds a lasting turnout reduction after a shock affecting only voting costs. While this is less clear in other studies that find lasting positive effects (Gerber et al., 2003; Meredith, 2009), studies on compulsory voting in Switzerland and Austria found no enduring effects on turnout after its abolition (Bechtel et al., 2018; Gaebler et al., 2020).

[7] Partisan consequences of higher turnout have also been the subject of some research. For example, Thompson et al. (2020), Barber and Holbein (2020), or Hodler et al. (2015) find no effect on the election winner; Frank et al. (2023) shows a positive effect on the incumbent vote in the same context as this paper.

One explanation for the intervention causing permanent changes in the electorate might be social pressure (Dellavigna et al., 2017). In municipalities with higher turnout in the past, "lazy" non-voters might face greater pressure to vote when voting costs fall as there is a higher awareness of voting as a civic duty (Ali and Lin, 2013). In contrast, in low-turnout municipalities, some voters might become aware that just claiming to have voted can be an option. Using a triple-difference-in-differences strategy, we find that high-turnout municipalities experience a persistently greater increase in total turnout compared to low-turnout municipalities. This might be partly surprising as with fewer abstainers, treatment intensity might be lower. Moreover, we find differences between larger and smaller municipalities. For treated municipalities in the bottom tercile, we find the largest increase in total turnout. This aligns with our hypothesis of social pressure (Dellavigna et al., 2017) and awareness of voting as a civic duty (Ali and Lin, 2013) but contrasts with Funk (2010) who finds smaller turnout effects from the *introduction* of postal voting and a decline in total turnout in small municipalities. The substitution effect of in-person voting to postal voting is particularly large for treated municipalities in the middle tercile. Estimates are close to the baseline for treated municipalities in the top tercile.

Finally, we learn about the distribution of the information costs by investigating heterogeneity across socio-economic characteristics of municipalities. We find an age gradient in the effect size: Municipalities with a higher share of elderly eligible voters (aged 60 and above) show larger substitution effects from in-person voting to postal voting. In municipalities with a higher share of younger voters (aged 18–29), the effect is weaker as they made only recently their decision on their voting mode. In contrast, a higher share in the youngest age group (aged 15–17), who did not vote before, show no different behavior than the average. The same holds for the middle-aged group (aged 30–59). Positive deviation in total turnout from the baseline is highest, where the substitution is lowest. Moreover, we find a stronger substitution but a lower increase in total turnout for municipalities with a higher share of females and a marginally statistically lower increase in total turnout for municipalities with a higher share of unemployed. We find no differential effects for municipalities with a higher share of households with children.

The rest of the paper proceeds as follows. The setting and the data are described in Section 2, followed by theoretical considerations and the empirical strategy in Section 3. The main results are presented in Section 4. Section 5 concludes.

2 Institutional Background and Data

2.1 Institutional Background

We analyze six elections for the German Federal Parliament *(Bundestag)* and the Bavarian State Parliament (*Landtag*), which were held between 2013 and 2023. All elections in our analysis follow the principles of proportional representation but differ in electoral rules.[8] Eligible voters (i.e., German citizens of at least 18 years of age) are automatically enrolled in the electoral roll, and there is no need for a separate voter registration, as resident registration is compulsory.

[8] For more information on the electoral systems, see Appendix A.

Eligible citizens receive a polling notification at least 21 days before election day, informing them about the election, its date, and the assigned polling place.[9] Furthermore, they are informed about the possibility of postal voting.

A distinctive feature of the German electoral legislation is the widely accepted option for postal voting. Postal voting was introduced in West Germany in 1957, and since 2008, it has been possible to request it without giving an excuse. Since 2013, it has been possible to request the postal ballot online. Alternatively, it can be requested either in person, by mail, or by telephone. The request has to be submitted no later than two days before the election.[10] Postal ballots can be submitted to any mailbox without postage.[11]

The treatment originates from the mayoral elections held in March 2020, where mayors must secure an absolute majority. If no candidate achieves the absolute majority in the first round, the two best-ranked candidates go to a runoff two weeks later. Under the pandemic's unique circumstances, local elections were held in Bavaria on March 15, 2020.[12] Since the election had already been prepared and the Bavarian State Government did not yet consider the COVID-19 situation urgent, the election was held in the ordinary mode despite the health risks of the pandemic (STMGP, 2020).

On March 18, three days after the first round, an official notice was issued stating that runoffs would be conducted solely through postal ballots. Unlike earlier, including the first round or in any subsequent election, postal ballots were automatically sent to all eligible citizens for the runoffs. The runoffs took place two weeks after the first round on March 29, as it is the usual procedure and, hence, after the electoral law adjustment due to the pandemic. This led to the unusual situation that all eligible voters who wanted to vote in the runoff—explicitly also those who had never voted by postal ballot before and were possibly even skeptical about it—had to vote by postal ballot.

2.2 Data

We use official statistics of the elections in the empirical analysis stemming from the Federal Chief Electoral Officer (*Bundeswahlleiterin*) for federal elections and the Bavarian Statistical Office for state elections, respectively.[13] The data contains the number of eligible voters and

[9] To cast a vote at a different polling place than the one assigned in the election notification, a polling card can also be requested. This process is identical to requesting postal voting ballots.

[10] https://www.statistik.bayern.de/wahlen/index.html, accessed on 23.05.2024.

[11] Often, it is also possible to submit them at official administration buildings, such as the municipal administration or the citizen's office. There is usually at least one open option on election Sundays.

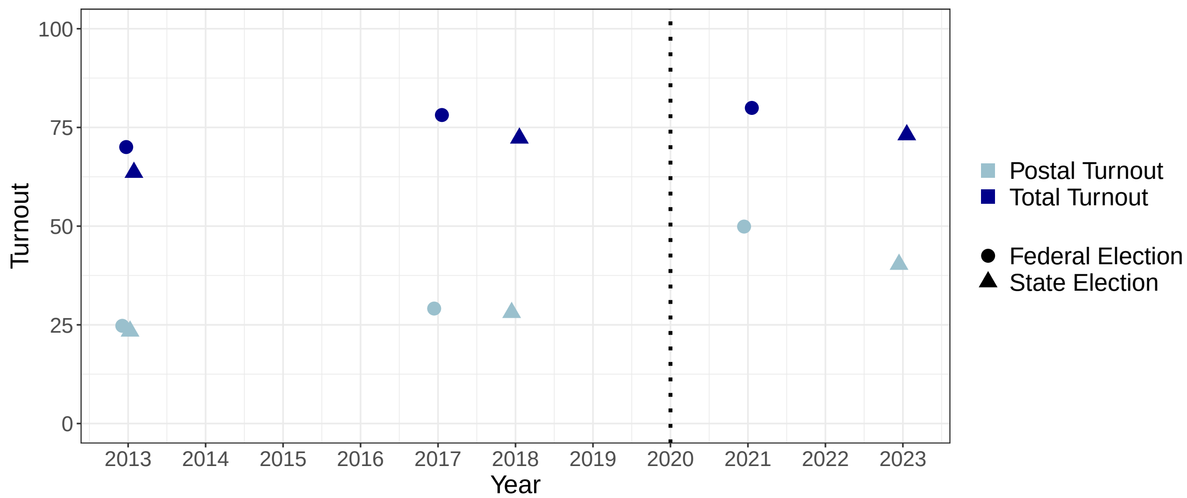
[12] In addition to the county councils (*Kreistag*) and municipal councils (*Gemeinderat*), the mayor (*Bürgermeister*) and the county commissioner (*Landrat*) were also elected. Bavaria consists of a total of 2056 municipalities, of which 25 are independent cities (*Kreisfreie Städte)*, meaning that the area of the city and the county are identical. There, instead of mayors and a county commissioner, a lord mayor (*Oberbürgermeister*) is elected. No mayoral elections were held in 126 of the 2056 municipalities, with one of them being an independent city., and no county commissioner elections were held in 7 of 71 counties, as some municipalities (counties) have different beginning of the term of office after the death or resignation of a mayor (county commissioners). Municipalities with an off-cycle mayoral election and no county commissioner election are exploited in robustness checks in Appendix C, using them for an alternative definition of the control group.

[13] https://www.bundeswahlleiterin.de/ and https://www.statistik.bayern.de/wahlen/landeswahlleitung/, both accessed on 15.05.2024.

(party) votes and provides this information separately by polling places and postal precincts.[14] We aggregate the data to the municipality level for two reasons: First, polling places are not necessarily nested within postal precincts. Thus, municipalities are the smallest level at which we can analyze total turnout and party vote shares (i.e., election results). Second, precinct boundaries are regularly adjusted between elections, whereas municipality boundaries change less frequently, providing a time-constant unit of observation.[15] Appendix Table E1 presents summary statistics for the turnout and other municipal characteristics provided by the Bavarian Statistical Office.

Figure 1 shows the development of total turnout and postal turnout in federal and state elections in Bavaria since 2013. The dotted vertical line depicts the 2020 Mayoral Elections, which are decisive for the treatment.[16] In 2013, both a federal election and a state election were conducted, occurring one week apart on September 15 and September 22, respectively. Federal elections are held every four years, while state elections are held every five years. In federal elections, total turnout is slightly higher than in state elections. Bavaria follows the overall German trend of a strong increase in the share of postal voting in recent years.[17]

Figure 1: Turnout by Election Type in Bavaria since 2013



Notes: The figure presents postal and total turnout (0-100) for the three federal and state elections included in our sample. The treatment is defined by the 2020 Mayoral Elections. The turnout for the 2020 Local Elections is not considered in the analyses.

[14] For Germany, voting data is not recorded at a finer resolution. Polling places are legally limited to 2,500 eligible voters but are usually smaller, whereas postal precincts can be much larger.

[15] In fact, there are no municipality boundary changes in our time frame.

[16] In local elections, turnout is lower than in major elections. In the first round of the 2020 Mayoral Elections, turnout was 58.9 percent and in the first round of the 2014 Mayoral Elections, turnout was 56.2 percent. Postal turnout is not separately reported for local elections, which take place every six years.

[17] While both total turnout and postal turnout increased between all elections of the same type, postal turnout increased more strongly relative to the baseline. The postal voting share increased from 35.3 percent in 2013 to 55.1 percent in 2023, with a spike of 62.4 percent in 2021 during COVID. The postal voting share in Germany increased from 24.3 percent in 2013 to 47.3 percent in 2021.

For the treatment definition, we use data from the 2020 Local Elections. This data is also provided by the Bavarian Statistical Office at the municipality level and includes the election dates for all municipalities.[18] We use this information to define the treated municipalities as these with a mayoral runoff on March 29, 2020. We use this information also to define the estimation sample. The estimation sample only contains municipalities in counties where no county commissioner had to go through a runoff on the same day, as county commissioner runoffs were also all-postal in 2020. Thus, control group municipalities without a mayoral runoff in counties with a county commissioner runoff also experienced an all-postal runoff. As these would contaminate the treatment, we disregard them from the analysis.[19] Moreover, independent cities, being identical to the county, are removed from the estimation sample as they contain no within-county variation.[20]

Figure 2 shows a map of all Bavarian municipalities. Government districts and county borders are delineated by thick and thin red lines, respectively, and municipality borders by grey lines. The estimation sample contains 1,457 municipalities. A runoff in the mayoral race was held in 205 municipalities. They are shown in blue. Control group municipalities, where no runoffs had to be held, are shown in a lighter blue (1,252 municipalities). 25 independent cities and 574 municipalities with a council commissioner runoff in 2020 are disregarded and shown in grey.

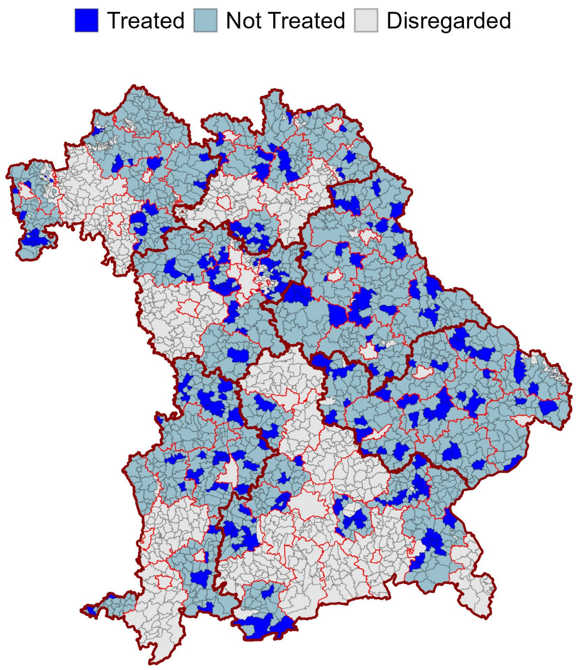
Runoffs are not very common in Bavarian mayoral elections. In Appendix Figure D2, we depict the frequency of mayoral runoffs during the last four mayoral elections for our estimation sample. The modal case is no runoff, and there are more municipalities with only one runoff than with more than one runoff. For the treatment group, one runoff is the modal case. This implies that for many municipalities in the treatment group, this was the first runoff at least during the last four mayoral elections. Only seven municipalities (0.5 percent of the estimation sample and 3.4 percent of the treatment group) had a runoff in all four mayoral elections. Even though the majority of municipalities never faced a runoff, there are ten municipalities in the control group, 0.8 percent of the control group, that had three runoffs during the last four mayoral elections (always but in 2020). We provide robustness checks in Appendix C restricting the estimation sample with respect to the number of runoffs in the last mayoral elections to rule out that the effect is driven by control group municipalities with no runoffs in the recent past or by municipalities from the treatment group for which runoffs are very common.

[18] `https://www.statistik.bayern.de/wahlen/kommunalwahlen/bgm/`, accessed on 15.05.2023.

[19] We use them later in a placebo exercise.

[20] We estimate using election×county fixed effects. Thus, only variation within counties is used.

Figure 2: Map of Runoffs in Mayoral Elections in Bavaria 2020



Notes: The figure presents a map of Bavaria. Thick red lines show government district boundaries and thin red lines show county boundaries. Municipality boundaries are depicted with grey lines. Dark blue municipalities are treated, while light blue ones are in the control group. Municipalities in which there was a council commissioner runoff in 2020 as well as independent cities, are disregarded (grey).

3 Theoretical Considerations and Empirical Framework

3.1 Theoretical Considerations from the Downsian Framework

To fix ideas, we present a simple theoretical framework to analyze how a one-time all-postal election affects voting behavior in subsequent elections by altering individual information costs of postal voting. We extend the standard Downsian voting model (Downs, 1957) and consider three periods: the pre-treatment period $(t-1)$, the first post-treatment election $(t+0)$, and the second post-treatment election $(t+1)$. Voters decide whether to participate by comparing the net benefits of voting, expressed as

$$P_{it} = I_i - C_{it} = I_i - \min\{C_i^b, C_i^p\} = I_i - \min\{C_i^b, \bar{C}_i + \mathbb{1}_{it}^{\tilde{p}} C_i^{request} + (1 - \mathbb{1}_{it}^{p}) C_i^{information}\}, \quad (1)$$

where P_{it} represents the individual's i net payoff from voting in period t. The benefits of voting, I_i, are assumed to be independent of the voting mode and constant over time. The costs of in-person and postal voting are denoted by C_i^b and C_i^p, respectively. If $P_{it} < 0$ individual i abstains. Contrarily, if $P_{it} \geq 0$ and $C_i^b \leq C_i^p$, she turns out at the polling place, otherwise she votes by mail. For C_i^b, these costs might be transportation costs to the polling place or congestion costs at the polling place. The costs of postal voting (C_i^p) can be separated into transportation costs to the postal box $(\bar{C}_i)$, costs for requesting the postal ballot $(C_i^{request})$, and information costs due to unfamiliarity with postal voting ($C_i^{information}$). While $\mathbb{1}_{it}^{\tilde{p}}$ is equal to one if an individual voted by mail in a regular election, $\mathbb{1}_{it}^{p}$ is equal to one if an individual ever voted by mail (including the all-postal runoff).

The forced experimentation with postal voting between $t-1$ and $t+0$ sets $\mathbb{1}_{i,t+0}^{p}$ to one for all voters in the treatment group, eliminating the information costs for postal voting. This shift induces two effects: Some in-person voters switch to postal voting, and some abstainers, who previously considered postal voting too costly, participate. Consequently, postal and total turnout increase, with a larger increase for postal turnout compared to total turnout.

However, as postal ballots were sent without request in the all-postal election, $\mathbb{1}_{i,t+0}^{\tilde{p}}$ remains unchanged, and in comparison to eligible voters who voted by mail before the all-postal runoff, treated voters *assume* lower postal voting costs. The need to request a ballot in $t+0$, which is different from the all-postal election but not from the pretreatment election ($t-1$), leads to a second updating of the costs of postal voting as $\mathbb{1}_{i,t+1}^{\tilde{p}}$ is set to one in $t+1$. This raises the cost of postal voting in $t+1$ for the treatment group, prompting some voters to return to their previous voting mode (in-person voting or abstaining). Hence, postal and total turnout decrease in comparison to $t+0$ but not to $t-1$. Postal and total turnout would only decrease to their original values ($t-1$) if $C_i^{request} > C_i^{information}$.

The largest effects should be observed among individuals with high initial information costs, such as elderly voters (Alvarez et al., 2008). These predictions guide our empirical analysis using administrative data and an event study design to assess changes in turnout and voting mode choices at the municipality level.

3.2 Estimation Equation

In our analysis, we employ an event study to investigate changes in turnout after the all-postal runoffs in 2020 in Bavaria. The regression equation of the main specification for the event study is as follows:

$$Y_{ict} = \sum_{\tau \neq -1} \beta^{\tau}(\mathbb{1}_{\tau=t} \times treatment_i) + \log(pop_i^{2018})\delta_t + \alpha_i + \alpha_{ct} + \varepsilon_{ict}, \tag{2}$$

where Y_{ict} is postal, in-person, or total turnout and party vote shares in municipality i in election t, relative to the all-postal runoff from 2020 (where $t = e-5$, with election $e \in \{1, 2, ..., 6\}$, as the fifth election is the first one after the treatment). The indicator $treatment_i$ is one if municipality i had the mayoral runoff in March 2020 and zero otherwise, and represents interacted with the (relative) election indicators the variable of interest. In the main specification, we only control for the time-invariant log population, $\log(pop_i^{2018})$, measured as the number of eligible voters in 2018, interacted with election-specific coefficients δ_t.[21] To account for time-invariant differences influencing the outcome, we include municipality fixed effects (α_i). To account for common shocks to voting behavior in counties, we include county-specific election fixed effects

[21] We choose 2018 as it is the last year with an election before 2020. All elections in the estimation sample follow the same rules for eligible voters.

(α_{ct}). Hence, we compare municipalities of similar sizes within the same county.[22] We estimate Equation 2 using OLS. We cluster standard errors at the level of municipalities to account for the correlation of model errors over time. Robustness checks on the sensitivity to alternative assumptions about the variance-covariance matrix are provided in Appendix C.

The identifying assumption for a causal interpretation is parallel trends, which, of course, cannot be tested directly.[23] However, we show that parallel pre-trends hold for the three types of turnout and for party vote shares. Robustness checks are provided in Appendix C. These include shrinking the sample around the margin of how the first round of the mayoral election was decided, restricting the sample by the number of runoffs in the past to estimate on municipalities with a similar propensity to have a runoff, and using off-cycle elections as the control group.

We run several placebo exercises to rule out that the estimated effects generally stem from runoffs or from local elections during the pandemic. If the reason for a change in turnout is the all-postal aspect of the election and not the runoff itself, one might expect to find no effect from other mayoral runoffs. We check this by employing placebo tests in Bavaria in 2014, when the last mayoral elections took place. If the circumstances of an election during the pandemic cause our results, as some voters might switch to postal voting to mitigate contact with other people at the polling place, we would expect to find similar results in other states.[24] Hesse postponed its local elections to the fall of 2020 and spring of 2021. This was about a year later but under similar circumstances (during the second lockdown in Germany, which was between December 2020 and May 2021). Furthermore, we use municipalities that had an all-postal election due to a runoff in the 2020 County Commissioner Elections as a placebo sample. Thus, we can show that it is not Bavaria or March 2020 but the all-postal election.

4 Results

4.1 Event Study Results

We start by showing event study results based on Equation 2 for the effect of experiencing the all-postal runoff on total turnout, as well as postal and in-person turnout, on the municipality level in subsequent elections. Figure 3 plots the estimates for the three outcomes.[25]

For all pre-treatment periods, estimates are very close to zero and lack statistical significance for all three outcomes. The absence of pre-trends lends credibility to the assumption of parallel trends.[26] In the first election after the treatment, the 2021 Federal Election, we observe a

[22] Eliminating variation across counties accounts for different electoral conditions. Counties might be directly contested for parliamentary seats. Moreover, (rainy) weather, for instance, on election day might vary less within counties than throughout Bavaria. In Appendix C, we provide further robustness checks using different approaches to match on observables.

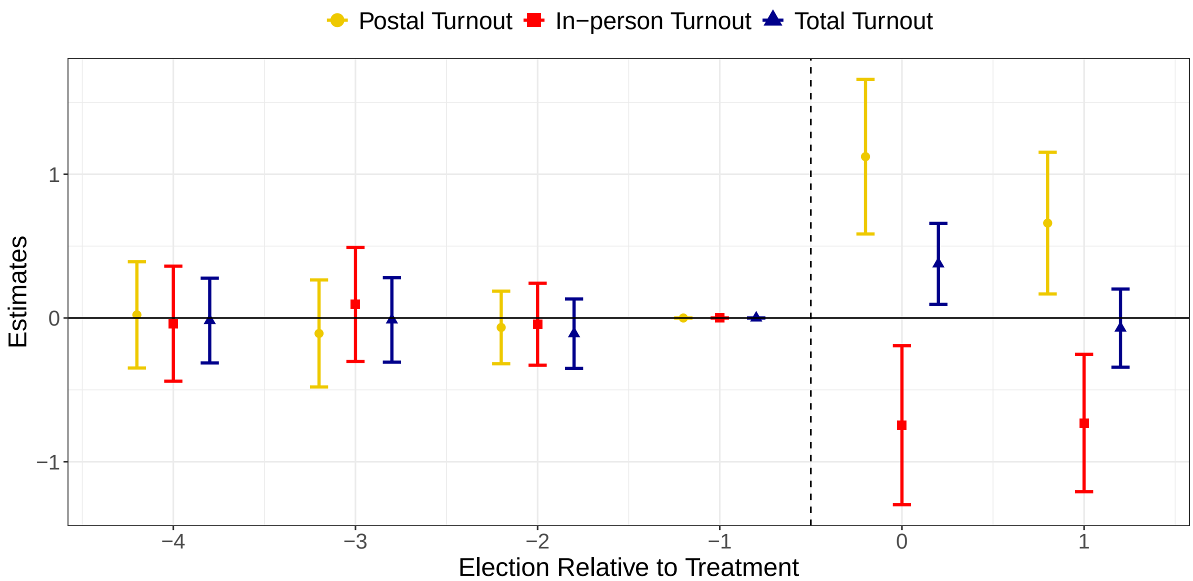
[23] Anticipation of the treatment is unlikely as the change in the voting procedure was only announced shortly prior to the election date (see Subsection 2.1).

[24] We additionally run robustness checks using off-cycle elections for the control group in Appendix C.

[25] Total turnout is defined as the number of cast votes divided by the number of eligible voters. For postal and in-person turnout, we take the number of cast postal votes and in-person votes, respectively. Point estimates and standard errors for the main effect and the population control are shown in Appendix Table E3.

[26] We provide sensitivity analysis on the parallel trends assumption following Rambachan and Roth (2023) in Appendix C.

Figure 3: Main Specification



Notes: The figure presents event study results based on Equation 2 for postal, in-person, and total turnout (0-100). The reference election ($t-1$) is the 2018 State Election. All specifications include the log population, measured as the number of eligible voters for the 2018 State Election, as a time-invariant covariate, interacted with election-specific coefficients. Confidence intervals are drawn at the 95 percent level using standard errors clustered at the municipality level. The point estimates and standard errors underlying the results are shown in Table E3.

statistically significant increase in total turnout of 0.38 percentage points. This increase results from a stronger increase in postal turnout of 1.1 percentage points, compared to a decrease in polling place turnout of only 0.75 percentage points. Hence, in the short run, we find both a substitution from in-person to postal voting as well as a reduction of abstaining. In the subsequent election, the 2023 State Election, taking place three and a half years after the treatment, the effect on total turnout returns to zero. While the in-person turnout estimate remains at -0.73, the postal turnout estimate shrinks by about half a percentage point to an estimate of 0.66.[27]

Appendix Table E3 presents additionally to the point estimates and standard errors of the treatment the same for the population control. In recent years, smaller municipalities increase their total turnout in comparison to larger municipalities (Column 3). This originates from a larger increase in terms of postal voting (Column 1). The difference between the first and last period is about 3 percentage points. Changes in in-person voting are present but not large enough to offset this shift (Column 2). These estimates show the necessity to control for municipality size as the treatment is not balanced in this dimension (Appendix Figure D1).

We show robustness in several dimensions in Appendix C. First, we limit the sample to municipalities with close first-round elections in 2020, ensuring that our results are not driven by extreme electoral outcomes. We present additional robustness checks, including variations in assumptions about the variance-covariance matrix, addressing potential concerns about cor-

[27] Appendix Table E4 compares the difference between the post-treatment coefficients in $t+0$ and $t+1$. Only the difference of the in-person turnout estimates lacks statistical significance.

related model errors. We also explore alternative specifications of the population control to assess the sensitivity of our results to different population measures. To further account for unobserved heterogeneity, we include further controls in both time-variant and time-invariant manners, respectively. We implement matching procedures to mitigate selection bias, ensuring that treated and control group municipalities are comparable. Additionally, we select the sample by the number of mayoral runoffs in the past to account for the propensity of having a runoff. We also investigate potential violations of the parallel trends assumption, a critical assumption for event study analyses following Rambachan and Roth (2023). Finally, we consider the role of off-cycle elections in the control group. Their turnouts are not affected by voters' health precautions during the early phase of the COVID-19 pandemic. All specifications confirm the robustness of the results.

The findings for the 2021 Federal Election, the first election after the treatment $(t+0)$, are in line with changes in postal voting costs (Downs, 1957): For some eligible voters, postal voting costs are reduced due to their experience of postal voting as their information costs on postal voting is now reduced. If they had voted before at the polling place and their postal voting costs are now lower than their in-person voting costs, they switch from in-person to postal voting. If they had abstained from voting before and their postal voting costs are now lower than their benefits of voting, they switch from non-voting to postal voting. While the latter leads to a positive estimate for postal and total turnout, the first leads to a positive estimate for postal turnout and a negative estimate for in-person turnout.

The change in postal turnout in the 2023 State Election, the second election after the treatment $(t+1)$, is explained by further adjustments in postal voting costs: In the 2020 Runoffs, postal ballots were sent directly to all eligible voters. However, in subsequent elections, eligible voters had to request postal ballots again if they wished to vote by mail. Consequently, the cost changes associated with directly mailing postal ballots in the runoffs might lead to a distorted perception of the true postal voting costs and the behavior in period $t+1$ can be seen as ongoing experimentation. Thus, we can reject habit formation as a mechanism of the altered total turnout. Instead, consistent with rational choice models of electoral turnout, eligible voters update their voting costs twice, again after experiencing postal voting in its usual administrative procedure in the 2021 Federal Election $(t+0)$. Moreover, we would observe a persistent increase in total turnout, also in the 2023 State Election $(t+1)$, if voting were habit forming. Instead, this effect is only transitory.

It should be noted, however, that we only observe the municipalities' aggregate effects. Thus, we cannot rule out that the altered turnout in the previous election $(t+0)$ indirectly affects turnouts in $t+1$. It is possible that some voters who vote at the polling place in $t+0$ realize that there are fewer voters turning out there and thus abstain from voting in the subsequent election.[28] At the same time, some voters who vote by mail in $t+0$ might return to in-person voting due to the above-described adjustments in postal voting costs. We will investigate this further when analyzing heterogeneities by past turnout.

[28] This potential mechanism is similar to what Funk (2010) finds.

Our estimates can be interpreted as a reduced form estimation. We imply a first stage in which the all-postal procedure in the 2020 Runoff introduces some eligible voters to postal voting for the first time. In addition to reducing information costs for postal voting in subsequent elections, we expect a positive effect on total turnout for the all-postal election itself, where the actual postal voting process was facilitated, i.e., the postal voting documents were sent without request. Hence, we assume that in the 2020 Runoffs, some eligible voters vote by mail who either would not have voted in the absence of an all-postal election or would not have voted by mail. Unfortunately, there is no individual-level data to show directly that some eligible voters voted for the first time by mail in the 2020 Runoff. However, with the data available, we can show increases in total turnout, which imply increases in postal turnout, as in the runoff, only postal voting was available. Actually, the estimated effect is positive and with about about 8 percentage points a lot larger than in the main setting (Appendix Table B1). Further details are provided in Appendix B.

4.2 Party Outcomes

Next, we examine whether the all-postal election and the associated changes in voter participation lead to changes in party preferences. We do so by re-estimating Equation 2 using party vote shares as dependent variables. Similar to turnout, we are able to differentiate between postal and in-person party vote shares. This is possible due to the aggregation at the municipality level, as municipalities usually collect all postal votes centrally.[29] We are thus also able to differentiate between shifts from in-person to postal vote shares across parties. This allows for a better understanding in comparison to investigating aggregate shifts across parties.

We start by showing party votes among eligible voters for a better understanding of the consequences of the turnout effects.[30] We first show estimation results with election results of the CSU (*Christlich Soziale Union*) in Figure 4 (Panel A). We focus on the CSU as it is the largest and most dominant party in Bavaria.[31] In the political spectrum, the CSU is classified as center-right (Jolly et al., 2022). While the point estimate for the in-person vote share is close to zero and lacks statistical significance, the point estimate for the postal vote share is positive and marginally significant in $t+0$. But in comparison to the main effects from Figure 3, these gains are smaller with a coefficient of 0.27. This results in a positive and marginally significant effect on the total vote share with a coefficient of 0.30. In $t+1$, we do not find altered point estimates for the CSU. The point estimate of the total vote share remains, as there is no reduction in the point estimate of the postal vote share, unlike in the turnout effects in Figure 3. However, its

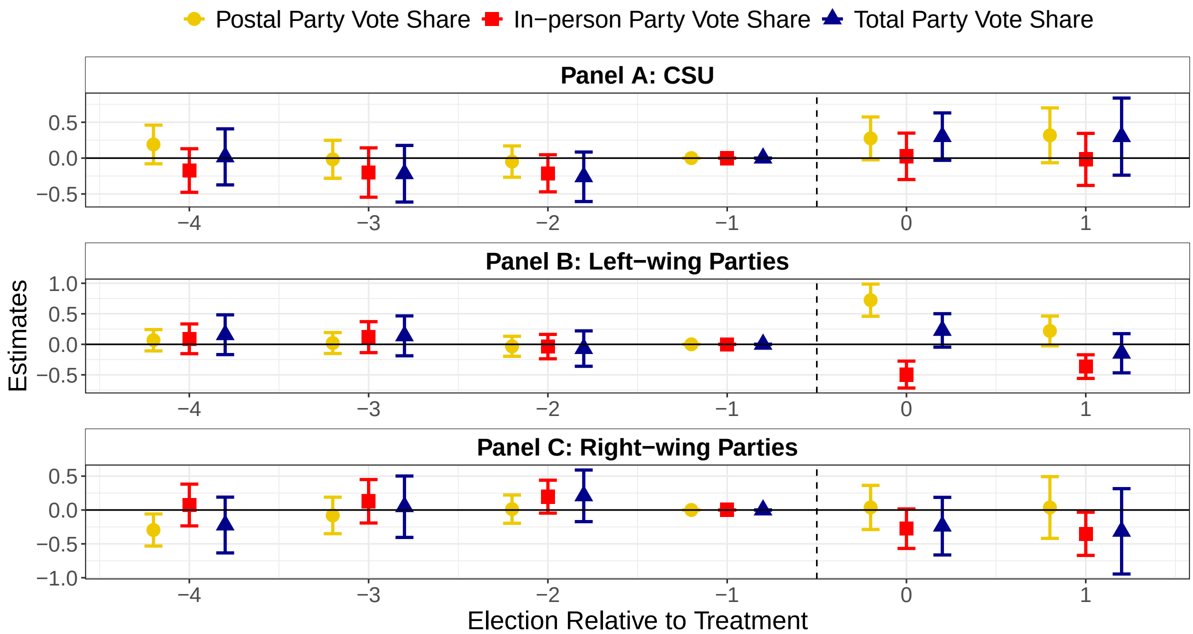
[29] An allocation is not feasible for 12 municipalities, as these were not broken down in at least one year. This applies in particular to small municipalities (< 1000 eligible voters) and does not occur for any treated municipality.

[30] Party votes among cast votes as a measure being more relevant for parliament composition is shown in Appendix Figure D3.

[31] Since 1957, the CSU has continuously held the office of the Bavarian Prime Minister. In the last two state elections, the CSU won 37 percent of the votes, and in 2013, the CSU even won the absolute majority of seats in the Bavarian Parliament. The next largest party was the FW (*Freie Wähler*) with 15.8 percent in 2023, the Greens (*Bündnis 90/Die Grünen*) with 17.6 percent in 2018, and the SPD (*Sozialdemokratische Partei Deutschland*) with 20.6 percent in 2013, respectively.

confidence intervals increase. Additionally, the lack of pre-trends lends further credibility to our empirical design. There is no evidence that treated municipalities are on a different trend in their party vote share prior to the treatment.

Figure 4: Election Results (Eligible Voters)



Notes: The figure presents event study results based on Equation 2 for postal vote share, the in-person vote share, and the total vote share (0-100) – each as a share of eligible voters – of the party *Christlich Soziale Union* (CSU, Panel A), and of left- and right-wing parties (Panels B and C). Left-wing parties include *Die Linke* (Linke), the *Sozialdemokratische Partei Deutschland* (SPD), and *Bündnis 90/Die Grünen* (Greens); right-wing parties include *Freie Wähler* (FW), the *Alternative für Deutschland* (AfD), and the *Freie Demokratische Partei* (FDP). The reference election ($t-1$) is the 2018 State Election and it is controlled for the population as in Figure 3. Confidence intervals are drawn at the 95 percent level using standard errors clustered at the municipality level. The point estimates and standard errors underlying the results are shown in Appendix Table E5.

For left-wing parties (Panel B), estimates are much closer to, but smaller than, the main effects from Figure 3. First, an increase in the in-person vote share of 0.72 percentage points is mirrored by a decrease in the postal vote share of 0.50 percentage points, resulting in a total increase of 0.23 percentage points. However, this slightly lower total effect size lacks statistical significance, suggesting that for left-wing parties, the all-postal election leads to a substitution in the voting mode of existing voters and less to a gain from former abstainers. After adjusting postal voting costs for another time, the coefficient for the postal vote shares drops to 0.22 in $t+1$, and some left-wing voters return to abstaining.

Contrarily, we find in $t+0$ that for right-wing parties (Panel C), there is no effect on postal voting, and specifically no increase, as we found before for other parties. However, there is still a marginally statistically significant reduction in the in-person vote share of 0.28 percentage points, which also translates to a (statistically insignificant) drop in the total vote share of 0.24 percentage points. Estimates are very similar in $t+1$.

When moving to the measure of party votes as a share of cast votes, results are very similar in all three panels. As the basis shrinks, estimates tend to increase in size (Appendix Table E6).

Importantly, we find increases in total turnout for the CSU of about half a percentage point. Hence, there are electoral consequences.

There is no loss in the *in-person* vote share for the CSU. This could mean that the CSU gains stem primarily from former abstainers. These would be "lazy" voters who were less informed about voting modes and needed the reduction in their postal voting costs to cast their ballot but would have always voted for the CSU if they voted at all. Another explanation is that some in-person CSU voters switched to postal voting, while some other right-wing in-person voters kept voting at the polling place but switched to voting for the CSU. This is particularly likely as estimates for the CSU and other right-wing parties barely change between $t+0$ and $t+1$, unlike for left-wing parties. The CSU was part of the German as well as the Bavarian Government in 2020 during the pandemic. Additionally, it had most mayors and county commissioners. Thus, some right-wing voters might have switched to the CSU to reward them for continuing the electoral process during the pandemic. This is not unlikely as other right-wing parties proposed very different policies to deal with the pandemic.[32] As we see a switch from in-person to postal, it is very likely that these voters agree more with the stricter COVID policy of the CSU, especially as they experienced the consequences of this policy in the form of the all-postal election. Hence, the results suggest that the CSU benefits not only from the information treatment about postal voting by gaining "lazy voters", as our theoretical considerations suggest, but also through other effects, such as shifts in party preferences. However, these might be specifically due to the pandemic and not a general effect of the information treatment.

4.3 Heterogeneity by Municipality Characteristics

Our analysis provides average turnout effects across treated municipalities, potentially masking heterogeneity among different groups. First, we analyze the role of past turnout and differences across municipality sizes to uncover the potential influence of social pressure (Funk, 2010; Dellavigna et al., 2017) and awareness of voting as a civic duty (Ali and Lin, 2013). Second, we assess heterogeneity across socio-economic municipality characteristics, such as age, gender, unemployment status, and the share of households with children, to understand better the distribution of the information costs (Alvarez et al., 2008). To examine these heterogeneities, we extend the standard difference-in-differences (DiD) model in Equation 2 to a triple-difference-in-differences model by incorporating interaction terms between the event-time indicators and a variable Z_i along which we allow for heterogeneity. This variable Z_i is measured at the municipality level and is time-invariant by construction. Although we lack exogenous variation to explore this heterogeneity with a causal interpretation, we present suggestive evidence on who drives the results and where information costs are particularly high. Nevertheless, Appendix Table E2 shows some correlation across covariates, which we cannot rule out to be the actual cause of the

[32] While the CSU acted more consciously, the *Freie Demokratische Partei* (FDP), *Freie Wähler* (FW), and the *Alternative für Deutschland* (AfD) suggested a more liberal policy.

heterogeneity instead of the investigated covariate Z_i. The triple-difference-in-differences model is specified as follows:

$$Y_{ict} = \sum_{\tau \neq -1} \gamma^{\tau}(\mathbb{1}_{\tau=t} \times treatment_i) + \sum_{\tau \neq -1} \mu^{\tau}(\mathbb{1}_{\tau=t} \times treatment_i \times Z_i) + \log(pop_i^{2018})\theta_t + \eta_i + \eta_{ct} + \epsilon_{ict}. \tag{3}$$

The same controls and fixed effects as in Equation 2 are included. Importantly, all first- and second-order interaction terms necessary for identifying the triple-difference estimator are either included in the specification or absorbed by the fixed effects. The estimates μ^{τ} trace the differential turnout trend in treated municipalities before and after the treatment across Z_i. It is always time-invariant and takes values from 2018, the year of the last election before the treatment. For the heterogeneity by municipality size, Z_i is an indicator. For the other municipality characteristics, Z_i is the variable's z-score (i.e., mean zero and unitary standard deviation). Thus, in these cases, the base effects γ^{τ} mainly reflect the average treatment effects on the treated (ATT) shown in Figure 3. Therefore, we show in the figures only the triple-difference estimates μ^{τ}, which correspond to the differential turnout trend among treated municipalities when Z_i is increased by one standard deviation. The base effects are reported in tables in the Appendix.

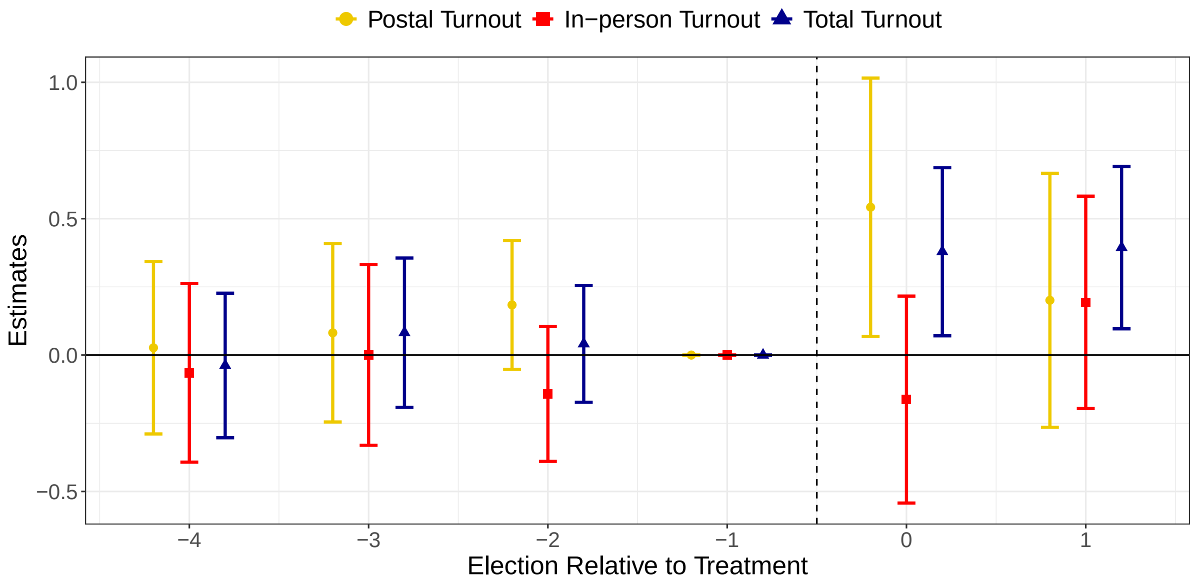
4.3.1 Social Pressure: Heterogeneity by Past Turnout and Municipality Size

Past Turnout Social pressure significantly influences voting behavior (Funk, 2010; Dellavigna et al., 2017). In municipalities with high turnout, non-voters may feel greater social pressure to justify their lack of participation, increasing their likelihood to vote. We hypothesize that the treatment will have a larger effect on non-voters in these high-turnout municipalities, encouraging them to vote. We test this by interacting the treatment indicator with the scaled turnout (i.e., mean zero and unitary standard deviation) from the 2018 State Election for Z_i.

In the immediate post-treatment period ($t + 0$), high-turnout municipalities experience a larger increase in postal and total turnout compared to low-turnout municipalities (Figure 5). The deviation for in-person turnout is negative but lacks statistical significance. The positive and statistically significant deviation for total turnout persists into the subsequent election cycle ($t+1$). This positive deviation stems from positive estimates for postal and in-person turnout. The estimate for postal turnout decreases and turns statistically insignificant, whereas the estimate for in-person turnout turns positive but remains statistically insignificant. This indicates a substitution from postal to in-person voting in $t + 1$ when voters adjust their postal voting costs for another time. It further suggests that social pressure in high-turnout municipalities motivates voters to continue participating, even if they do not stick to postal voting.

We want to add a few remarks. First, social pressure is only one interpretation. We consider it the most likely one when thinking about past turnout. Similarly, in high-turnout municipalities, there is a higher awareness of voting as a civic duty (Ali and Lin, 2013). We will discuss this further in the next part, which is on heterogeneity by municipality size. Second, this finding cannot be explained fully by altered postal voting costs, i.e., the change in voting costs should not be different between municipalities with high or low past turnout. Thus, the treatment may also shift the benefits of voting. Last, we want to highlight that in municipalities with a high past turnout, a smaller fraction of non-voters exists. Hence, treatment intensity might be lower, and, in relative terms, the effect is even larger.

Figure 5: Heterogeneity by Past Turnout



Notes: The figure presents triple-difference estimates based on Equation 3 for postal, in-person, and total turnout (0-100), interacted with the total turnout in the 2018 State Election. The reference election ($t-1$) is the 2018 State Election and it is controlled for the population as in Figure 3. Confidence intervals are drawn at the 95 percent level using standard errors clustered at the municipality level. The point estimates and standard errors underlying the results are shown in Appendix Table E7. All first and second-order interaction terms required for the identification of the triple-difference estimator are included in the specification or are absorbed by the fixed effects.

Municipality Size To examine differences by municipality size, we use indicators Z_i for interaction terms, denoting the quantiles of a municipality based on its population, measured as the number of eligible voters in 2018. Starting with a median split, at 2040 eligible voters, heterogeneity by municipality size does not seem to be very large (Appendix Figure D4). Nevertheless, we find slightly stronger effects in the bottom half of the sample's municipalities.[33] However, when investigating smaller group sizes with terciles, with cutoffs at 1427 and 3094 eligible voters, we find stronger heterogeneities, specifically for the bottom group (Figure 6). While total turnout increases for the bottom tercile by 1.24 percentage points, there is no increase for the middle tercile. For the top tercile, we find a moderate increase of 0.39 percentage points,

[33] Throughout this paragraph, we depict marginal effects for each category instead of coefficients of interaction terms. Appendix Figure D4 shows an increase in total turnout in smaller municipalities of 0.52 percentage point in $t+0$, while this increase is in larger municipalities only 0.33 percentage point. Also, the increase in postal turnout is greater (1.53 versus 0.97 percentage point). There is no heterogeneity in total turnout in $t+1$. But substitution from in-person to postal turnout is still larger in smaller municipalities (1.03 and -0.99 versus 0.52 and -0.63).

similar to the baseline estimation (Figure 3).[34] For the bottom tercile, there is no reduction in in-person turnout. Hence, on average, only former abstainers who now vote by mail are affected. For the middle tercile, substitution is the highest with point estimates of 1.25 and −1.25 for postal and in-person turnout, respectively. The top tercile has, again, very similar coefficients to the baseline estimation. Moving to quintiles, heterogeneities are very similar (Appendix Figure D5).[35] So overall, we find stronger increases in total turnout for smaller municipalities. This is in line with the social pressure hypothesis, assuming that social pressure is higher in smaller municipalities (Funk, 2010). However, in contrast to Funk (2010), we find that high social pressure in combination with lower postal voting costs leads to higher total turnout. Moreover, we find high substitution from in-person to postal voting in mid-sized municipalities. For larger municipalities, we find moderate effects on total turnout and with respect to substitution from in-person to postal voting.

There is some correlation between past turnout and smaller municipalities and party vote share for the CSU (Appendix Table E2). Hence, a further possibility, besides changed party preferences, is that the CSU profits from the changes in the composition of the electorate. However, the observed correlation merely suggests potential compositional effects if the average voter in high-turnout or small municipalities is systematically affected. Yet, it does not speak about the affected marginal voters.

4.3.2 The Distribution of Information Costs: Heterogeneity by Socio-Economic Characteristics

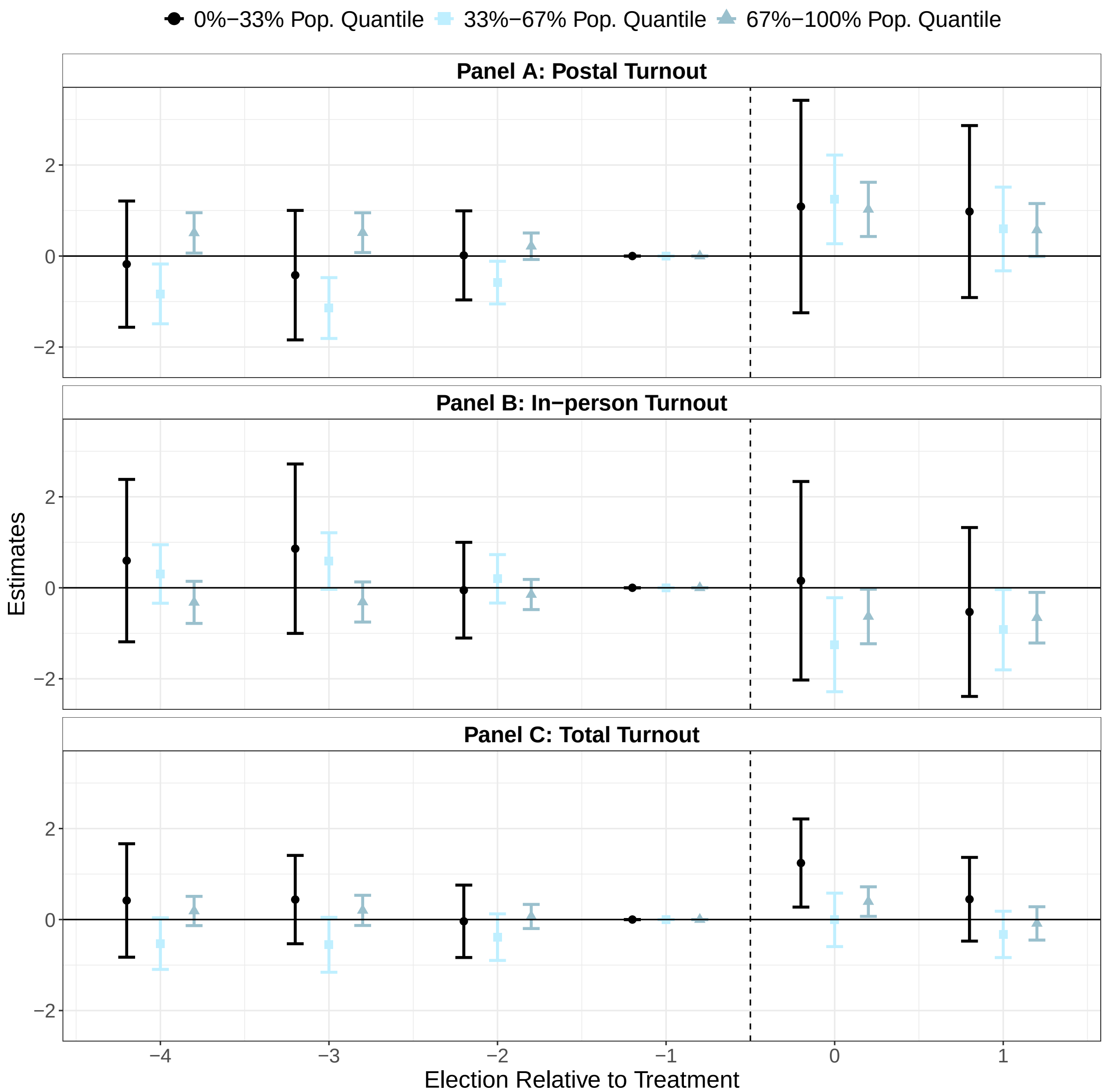
We hypothesize that the higher the information costs associated with an unfamiliar voting mode, the stronger the expected treatment effect. Thus, we examine whether varying socio-economic compositions across municipalities result in differential treatment effect sizes. We explore heterogeneity in voting behavior across age groups, unemployment status, gender, and the share of households with children, by transforming these characteristics to z-scores (i.e., mean zero and unitary standard deviation).

Age We show heterogeneity across age groups in Figure 7. Since all values for the age groups are from 2018, individuals aged 15–17 years old partly did not vote in the 2020 Runoff and did not vote at all in previous elections. In this group, we see no differential effects, as there is presumably no voting mode standard for them yet (Panel A). This indicates that the all-postal runoff affects someone who has no prior electoral experience similarly to the average population. For age groups that have been eligible to vote since at least the 2018 State Election, there seems to be an age gradient, indicating that the more recently one became eligible to vote, the weaker

[34] Here, we also have the highest density of treated municipalities (Appendix Figure D1). This also explains the larger confidence intervals for the bottom tercile.

[35] The cutoffs are at 1098, 1634, 2643, and 4496 eligible voters. We find the largest increase in total turnout for the bottom quintile (2.30 percentage points). For the next quintile, we find very strong substitution but not increase in total turnout. For the middle quintile, there are no significant estimates but sizable point estimates for the substitution from in-person to postal voting. For the two top quintiles, we find estimates similar to the baseline estimation.

Figure 6: Heterogeneity by Population Size: Terciles



Notes: The figure presents event study results based on Equation 3 for postal, in-person, and total turnout (0-100). The reference election $(t-1)$ is the 2018 State Election and it is controlled for the population as in Figure 3. The population quantiles are based on the number of eligible voters constant in 2018. Confidence intervals are drawn at the 95 percent level using standard errors clustered at the municipality level. The point estimates and standard errors underlying the results are shown in Appendix Table E9.

the substitution effect of the forced experimentation with postal voting due to the all-postal runoff. While for the 30–59 age group, there are no differential turnout effects (Panel D), the 60+ age group exhibits a stronger substitution effect from in-person to postal voting (Panel E). This suggests that older voters are more strongly affected by the treatment and have had the highest information costs before. For 18–19-year-olds, who were also already eligible to vote in 2018, the substitution from in-person to postal voting is weaker (Panel B). Similar results are shown for the 20–29 age group (Panel C). This suggests that individuals who recently established a preferred voting method are less inclined to switch as both groups might be well informed about possible modes of voting.[36] Interestingly, coefficients are rather similar between $t+0$ and $t+1$. Hence, adjustments are similar for all age groups.

The age gradient is also reflected in the total turnout effects: The stronger the substitution, the lower the effect on total turnout. But only for the age group 20–29, there is a positive and marginally significant effect in $t+0$. Though lacking statistical significance, for the oldest age group, the differential total turnout point estimate is negative. For all other age groups that have voted before, differential total effects are positive or very close to zero. It can be seen as an indication that younger "lazy voters" who were less informed about voting modes also abstained more before the all-postal runoff and that these react to the treatment while the better informed already vote using their preferred voting mode.

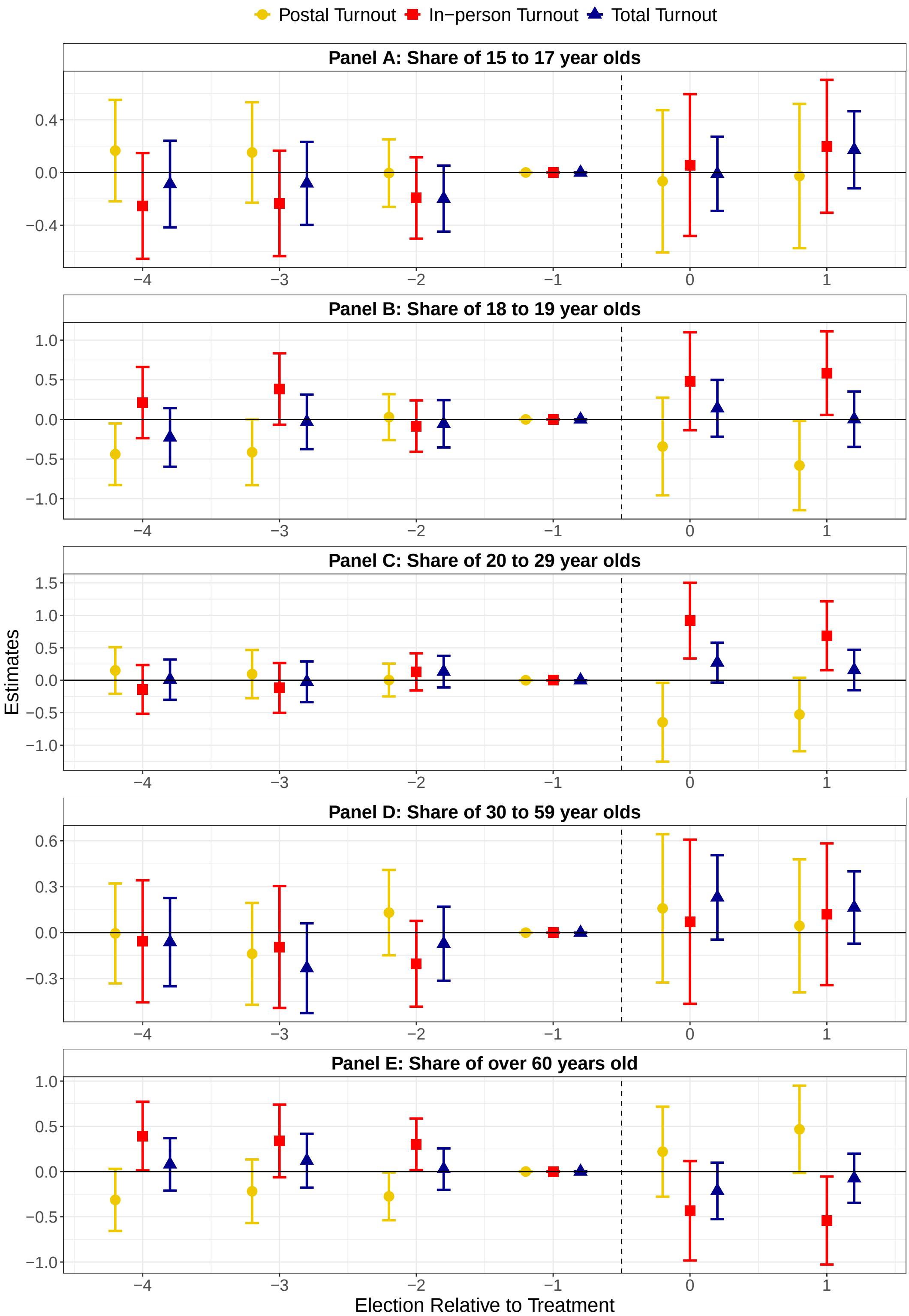
Unemployment Heterogeneity by the share of unemployed in a municipality is depicted in Panel A of Appendix Figure D6. The share of unemployed individuals, presumably correlated with average income and education levels, has weak differential effects on total turnout. In municipalities with a higher share of unemployed, the positive baseline effect on total turnout in $t+0$ is mitigated. However, the estimate is only statistically significant at the 10 percent level. The observed mitigation is driven by a slightly, though statistically insignificant, lower postal and larger in-person turnout effect compared to the average baseline effect. This indicates that for this group, there might be only a substitution from postal voting to in-person voting, but not from former abstainers. In the subsequent election $(t+1)$, no statistically significant estimates are observed for the triple-difference-in-differences estimates.

Gender We observe different effects for women and men. In municipalities with a larger share of women, there is an even larger decrease in in-person voting, which comes with only a slightly and statistically insignificant higher increase in postal voting in $t+0$ (Panel B of Appendix Figure D6). Hence, while the substitution from in-person to postal voting is more pronounced for women, the substitution from former abstainers is weaker. Estimates in $t+1$ are very similar. However, it should be noted that only the in-person turnout coefficients (in $t+0$ and $t+1$) and the total turnout coefficient (in $t+0$) are statistically significant. These results specifically align with the heterogeneities of the highest age group. Noting the strong correlation between these two variables makes it hard to attribute the finding only to the gender (Appendix Table E2).

Households with Children A difference in the share of households with children in a municipality does not alter the effect of the all-postal election on subsequent voting behavior (Panel C of Appendix Figure D6). Our interest in this demographic variable stems from the hypothesis that parents might prioritize family needs differently, potentially affecting their voting behavior. However, it does not.

[36] Recall that postal voting has been possible for a long time and can be requested easily online since 2009.

Figure 7: Heterogeneities by Age



Notes: The figure presents triple-difference estimates based on Equation 3 for postal, in-person, and total turnout (0-100). Panel captions refer to Z_i from Equation 3 and are transformed to z-scores (i.e., mean zero and unitary standard deviation). The reference election $(t-1)$ is the 2018 State Election and it is controlled for the population as in Figure 3. Confidence intervals are drawn at the 95 percent level using standard errors clustered at the municipality level. The point estimates and standard errors underlying the results are shown in Appendix Table E12.

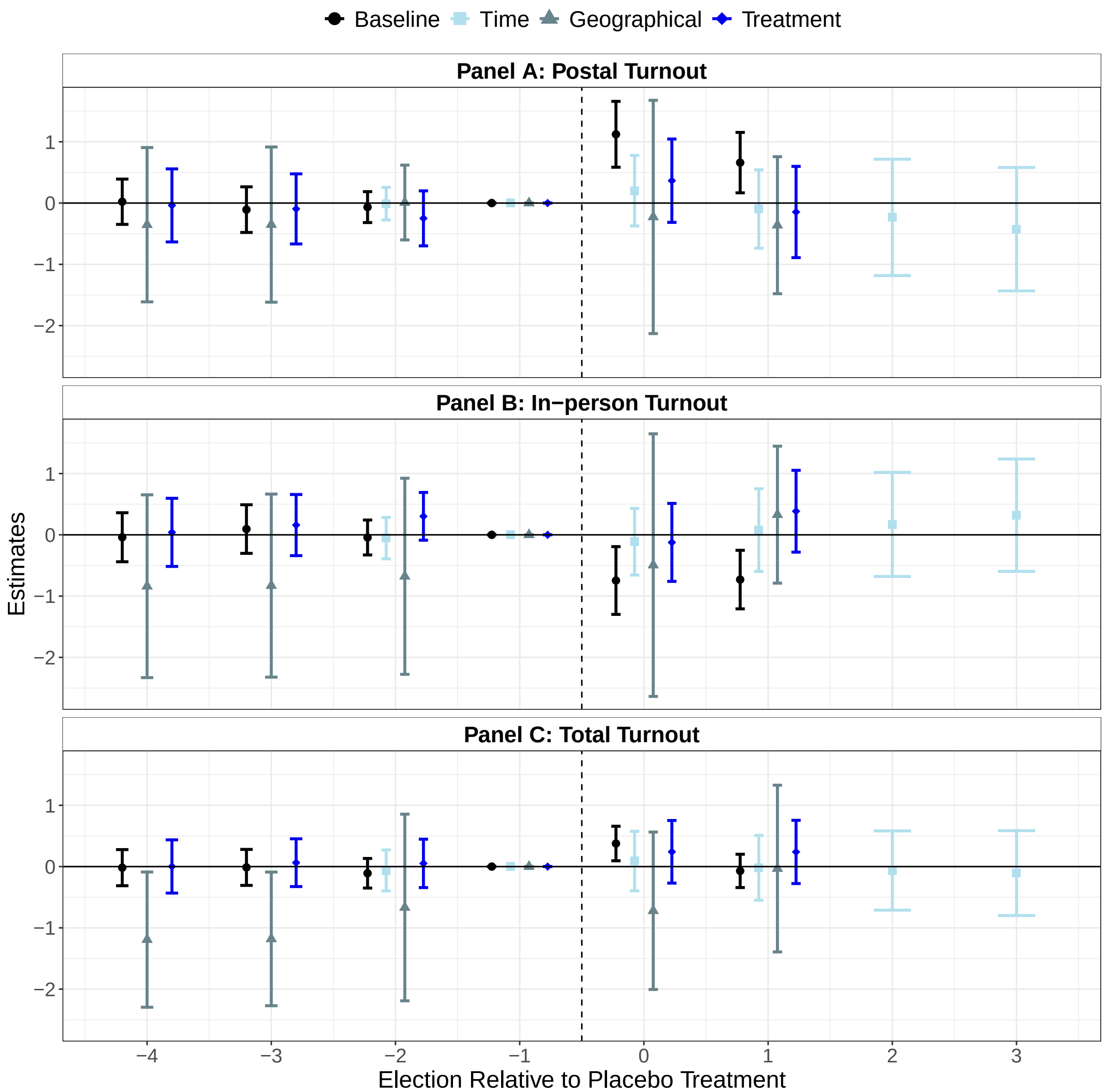
4.4 Placebo Tests

We assess the validity of the treatment effect observed in the previous analyses by applying the same event study estimation to a placebo treatment. Our identification relies on the parallel trends assumption. However, we cannot test this assumption directly. By changing the time, the place, and the treatment, we show that only the unique circumstances of the all-postal runoffs are the cause of the shown effects. We first re-set the treatment to the previous mayoral elections in 2014, where there was no all-postal runoff. We then re-set the treatment to a different state, Hesse, where there were also mayoral elections in late 2020 and early 2021, during the second lockdown, but no all-postal runoff. Next, we estimate on the counties in Bavaria with local elections in 2020 but which had a county commissioner runoff, such that the whole county is experiencing an all-postal election, not only the treated municipalities with a mayoral runoff. Last, we randomize the treatment across municipalities within a county using the randomization inference method by Heß (2017).

Time Placebo One might be worried that the estimated effects in Figure 3 originate not from the all-postal runoff but are the result of a runoff in general, with the idea in mind that any additional voting experience might have lasting impacts on the turnout. First, we want to highlight that this cannot explain the particular pattern of a stronger increase in postal voting. To address this further, we conduct a placebo exercise to confirm that this is not a concern. Specifically, we shift the treatment to the local election held six years earlier, in 2014, and assign the placebo treatment to municipalities that experienced a runoff in that election. In 2014, the runoff followed the same voting procedure as the first round, i.e., there was no all-postal election in the runoff. We exclude municipalities that had a county commissioner runoff in 2014 or 2020, as well as any treated municipalities in 2020. A county commissioner runoff in 2014 would still mean an additional election for the placebo control group. Excluding municipalities with a county commissioner runoff in 2020 and that were treated in 2020 ensures that in the later estimates, there is no contamination from the actual treatment. The results of the placebo event study, depicted in specification *Time* of Figure 8, do not reveal any significant pre-treatment trends. As the placebo treatment originates from the 2014 Local Elections, only two elections remain prior to this event in our sample (the 2013 State Election and the 2013 Federal Election). As expected, the estimates are close to zero and lack statistical significance in all four post-treatment periods and in all three outcomes. Therefore, the placebo test strengthens the conclusion that the treatment effect in the original analyses is due to the introduction of one-time all-postal ballots and not due to the runoff itself.

Geographical Placebo Further, one might be worried that the observed effects are due to the election being held during a pandemic. We already provide some counter evidence by estimating with only off-cycle elections in the control group (at the end of the robustness section in Appendix C). However, we can also show that the effects are lacking when there is a local election during the pandemic, but without an all-postal runoff. For this purpose, we repeat

Figure 8: Event Study: Placebos



Notes: The figure presents event study results based on Equation 2 for postal, in-person, and total turnout (0-100). The specifications are as follows: *Baseline* are the main results, shown in Figure 3. *Time* are time placebo results. The reference election ($t-1$) is the 2013 Federal Election. The placebo treatment is defined as a mayoral runoff in the 2014 Local Elections. The sample excludes municipalities that had a county commissioner runoff in 2014 or 2020, as well as any treated municipalities in 2020. *Geographical* is a geographical placebo test for the 2020/2021 Local Elections in Hesse. The reference election ($t-1$) is the 2018 State Election in Hesse. *Treatment* shows the results of a treatment placebo. Only counties in Bavaria that had a county commissioner runoff in 2020 are considered. The reference election ($t-1$) is the 2018 State Election. In all specifications, it is controlled for the population as in Figure 3. Confidence intervals are drawn at the 95 percent level using standard errors clustered at the municipality level. The point estimates and standard errors underlying the results are shown in Appendix Table E13.

the estimation in a different state. We shift our analysis to Hesse, where mayoral elections scheduled between April and October 2020 were postponed to November 2020 at the earliest or held together with the local elections in March 2021.[37] As they did not take place at the beginning of the COVID-19 pandemic, they were not held as all-postal elections. If the treatment effect from our analysis is from the all-postal aspect of the Bavarian elections, we do not expect any significant effects of a mayoral runoff. Specification *Geographical* of Figure 8 presents the results. They confirm these expectations. There are no statistically significant deviations after the placebo treatment. As the sample comprises only 78 municipalities, the coefficients might potentially lack statistical significance due to a lack of power. Therefore, it is important to note the *negative* point estimate for total turnout in $t+0$. All other coefficients are close to zero and have huge confidence intervals.

Treatment Placebo The setting in which the mayor and county commissioner are elected in the same local elections allows us to estimate a placebo at the same time and in the same state as the actual treatment. Again, we assign the placebo treatment to municipalities with a mayoral runoff. But this time, we estimate only on municipalities with an all-postal runoff in the county commissioner election. In this specification, the control group also experiences an all-postal election. We thus expect no differences between the treatment and control group. Accordingly, specification *Treatment* of Figure 8 shows estimates close to zero, before and after the treatment, that lack statistical significance. Thus, this placebo indicates no significant treatment effects, suggesting that the observed increase in turnout was not specifically associated with the mayoral runoff but the all-postal runoffs.

Randomization Inference Finally, we employ a randomized inference approach, as described by Heß (2017). This method involves 1,000 permutations, holding constant the number of treated municipalities within a county to assess whether the results could be attributed to random chance. The randomized inference results in Appendix Table E14 demonstrate significant treatment effects consistent with the original analysis, increasing the evidence that the estimated effects are indeed statistically significant. This approach helps to rule out the possibility that the results were driven by random variation rather than the actual treatment effect of the all-postal runoffs.

5 Conclusion

Using a quasi-experimental event study framework, we find that a one-time all-postal mayoral runoff in Bavarian municipalities increased total turnout by 0.4 percentage point in the first subsequent election. While this increase is only transitory, a substitution from in-person to postal voting of 0.7 percentage point persists across the next two subsequent elections. Consistent with

[37] A notable difference is that these elections took place closer to the 2021 Federal Election, which was held on September 26. To ensure comparability, we restrict the sample to those municipalities that hold the mayoral elections after November 2020 but before the federal election in September 2021. Further, we exclude independent cities and cities with special status, as well as the municipality of *Westertal*, which was founded by a merger in 2020. The federal elections are the same, and the state elections were held in the same years as in Bavaria (2013, 2018, and 2023).

the Porter Hypothesis (Porter, 1996), our findings show that policies altering decision-making costs can have lasting effects and highlight the long-term impact of forced experimentation. The estimated effects remain robust across different model specifications and are supported by placebo tests.

The decrease of the information costs is not large enough to increase total turnout permanently. The transitory increase stems from misperceived postal voting costs, as the all-postal election came without requesting postal ballots. When realizing the actual postal voting costs, postal turnout and total turnout decrease. However, in some subgroups, there are persistent positive shifts in total turnout. This holds particularly true for municipalities with higher past total turnout. On the other hand, this also implies a total turnout decrease in municipalities with lower past total turnout. Further heterogeneity analyses by past turnout but also by the municipality size indicate that social pressure increases the effect size. Information costs are particularly high for older eligible voters, leading to a higher substitution in treated municipalities with a higher share of more than 60-year-olds. However, for this group, total turnout effects are lower than in average municipalities. As total turnout deviations are larger in age groups where the substitution is lower, this might indicate that the treatment effect on total turnout is higher for eligible voters who were less informed about voting modes ("lazy voters").

Finally, we find persistent partisan effects. Increases in the vote share of the largest conservative party (CSU) are found on average for postal turnout and stem primarily from former in-person voters of other right-wing parties. However, we cannot rule out compositional drivers from the shifts in total turnout differing by municipality characteristics.

Our results may lead to important policy implications. First, even though postal voting is easily accessible and Germany has a long tradition of postal voting, information costs are still high, especially for older eligible voters, who are hindered from casting their ballot. Thus, targeting eligible voters with high information costs can increase turnout. Second, in surroundings with low social pressure, even lowering voting costs can have negative implications for total turnout. Thus, in such municipalities, programs on the value of elections might be important. Finally, partisan consequences should be considered, specifically if the incumbent party profits from increased turnout. However, in this case, the incumbent might profit due to the post-pandemic situation and gain primarily from other right-wing parties.

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Online Appendix

Should I Mail or Should I Go:
Voting Behavior After a One-Time All-Postal Election
by *Marius Kröper* and *Valentin Lindlacher*

A Electoral Systems

Bavarian State Electoral System Elections for the Bavarian State Parliament (*Landtag*) follow a mixed-member proportional representation system. Voters have two votes: the first (*Erststimme*) is for a direct candidate in their constituency (*Stimmkreis*), elected through a first-past-the-post system, whereas the second (*Zweitstimme*) is cast for a party list within one of Bavaria's seven administrative regions (*Regierungsbezirke*). The composition of the Landtag is determined proportionally based on the total of both votes received by parties across these regions. A party must receive at least five percent of the total valid votes statewide to enter the Landtag.

German Federal Electoral System The German Federal Parliament (*Bundestag*) is elected through a personalized proportional representation system, also using two votes per voter. The first vote elects a candidate in single-member constituencies using a first-past-the-post system, whereas the second vote determines the proportional distribution of seats among parties based on federal state party lists. To enter the Bundestag, a party must either secure at least five percent of second votes nationwide or win a minimum of three constituency seats.

B Implicit First Stage

We show turnout effects for elections subsequent to the 2020 Local Elections. We assume that some eligible voters get into contact with postal voting for the first time during this all-postal election and interpret this as an implicit first stage. This happens as some eligible voters voted by mail who either would not have voted in the absence of an all-postal election or would not have voted by mail. Besides having some eligible voters who had never voted by mail before but who voted at the polling place, some eligible voters abstained before experimenting with postal voting as the actual postal voting process was facilitated, i.e., the postal voting documents were sent without request, and thus costs of voting are reduced.

We do not have individual-level data to show directly that some eligible voters voted for the first time by mail in the 2020 Runoff. However, we analyze turnout on the aggregated level.[38] Empirically, we compare the differences between the first and second round total turnout in 2020 with the differences between the first and second round total turnout in previous years, as proposed by Frank et al. (2023) and applying otherwise the same empirical specification as in our main setting (Equation 2):

$$\begin{aligned} Turnout_{ictb} = \zeta\, Runoff_b + \tau(Election\,2020_t \times Runoff_b) \\ + \log(pop_i^{2018})\rho_t + \vartheta_i + \vartheta_{ct} + \upsilon_{ictb}, \end{aligned} \tag{B1}$$

where $Turnout_{ibct}$ is the total turnout in municipality i in election t ($t \in \{2002, 2008, 2014, 2020\}$) in round b ($b \in \{1, 2\}$). Further, $Runoff_b$ is a dummy for a mayoral runoff and represents interacted with the indicator for the 2020 Mayoral Election, the variable of interest. Identical to the main estimation (Equation 2), we control for the time-invariant log population, $\log(pop_i^{2018})$, measured as the number of eligible voters in 2018, interacted with election-specific coefficients ρ_t. To account for time-invariant differences influencing total turnout, we include municipality fixed effects (ϑ_i). To account for common shocks in counties—such as the COVID-19 pandemic or rain—we include election×county fixed effects (ϑ_{ct}).

The results are shown in Table B1. Column (1) shows the results of all mayoral elections in Bavaria since 2002. The results indicate that the total turnout in the 2020 Runoff was 8.3 percentage points higher than in ordinary runoffs due to the exclusive postal vote and the automatically sent ballots. Comparable and also statistically significant results are obtained if the sample is restricted to municipalities we use in the main analysis (Column 2). Column (3) shows a slightly larger main effect. Here, the sample is restricted to municipalities that had a runoff in both of the last two mayoral elections. Overall, this exercise indicates that the all-postal runoff was different from usual runoffs in terms of turnout and, thus, probably made some eligible voters experiment with postal voting.

[38] With the data available, we can only investigate total turnout, as for local elections, municipalities do not report postal and in-person votes differently. However, an increase in total turnout also implies an increase in postal turnout, as in the runoff, only postal voting was available.

Table B1: Implicit First Stage

Total Turnout	(1)	(2)	(3)
Runoff	-1.532***	-0.8962***	-3.659***
	(0.1843)	(0.2165)	(0.4539)
Election 2020 × Runoff	8.254***	8.072***	9.086***
	(0.2566)	(0.3151)	(0.5177)
Observations	8,566	6,145	475
R^2	0.81689	0.81162	0.93433
Municipality FE	✓	✓	✓
Year-County FE	✓	✓	✓

Notes: The table presents regression results based on Equation B1 using different samples. The data comprises mayoral elections in Bavaria between 2002 and 2020. Column (1) considers all mayoral elections between 2002 and 2020 in all Bavarian municipalities (excluding independent cities). Column (2) restricts the sample to municipalities we use in the main analysis, i.e., we additionally disregard counties where there was a runoff in the 2020 County Commissioner Election. Column (3) additionally considers only municipalities with a runoff in the 2014 and in the 2020 Mayoral Elections. It is controlled for the log number of eligible voters held constant to 2018. Standard errors are clustered at the municipality level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

C Robustness Checks

C.1 Close Elections

In the main specification, the estimation sample contains all Bavarian municipalities, excluding independent cities and those with a runoff for the county commissioner. To assess the robustness of our findings, we limit the sample to municipalities with close first-round mayoral elections in 2020, ensuring that our results are not driven by extreme electoral outcomes. We drop municipalities where a runoff was either very unlikely or very likely. Therefore, we narrow the sample to include only municipalities with relatively close first-round results in the 2020 Mayoral Election. While the treatment and control group become more comparable, this comes at the expense of statistical power. Table C1 shows the estimates for the sample with bandwidths ranging from 6 to 10 percentage points. The cutoff is set at 50 percent of the votes, determining whether a runoff is necessary. Compared to the main specification, the 10-percentage-point specification includes only 2,430 observations, approximately 28 percent of the main sample (8,742 observations). In $t+0$, results remain statistically significant for postal and total turnout. For in-person turnout, despite similar point estimates as of the baseline, the estimate is only statistically significant at the 10 percent level. In $t+1$, point estimates are similar to the baseline but lack statistical significance.

For lower thresholds, the positive total turnout effect in $t+0$ is found for all values (significant at the 5 percent level between 7 and 9.5 percentage points). The positive postal turnout effect in $t+0$ is found for all values between 7 and 10 percentage points (significant at the 5 percent level). The negative in-person turnout effect in $t+0$ is found for values of 8.5 percentage points and higher (statistically significant, at least at the 10 percent level). For these specifications, but the 10 percentage point one, it also remains in $t+1$. Overall, estimates are close to the baseline but sometimes lack statistical significance due to smaller sample sizes.

Table C1: Robustness: Bandwidth Election Share of the First Rounds' Winners

Panel A: Postal Turnout	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treatment $(t-4)$	0.0987	0.0554	0.0846	0.0525	0.1902	0.0125	0.0161	-0.0436	-0.0478
	(0.3650)	(0.3345)	(0.3250)	(0.3161)	(0.3029)	(0.2941)	(0.2863)	(0.2765)	(0.2728)
Treatment $(t-3)$	-0.1662	-0.2166	-0.1511	-0.2179	-0.1206	-0.1820	-0.1486	-0.1831	-0.1599
	(0.3806)	(0.3474)	(0.3386)	(0.3261)	(0.3079)	(0.2951)	(0.2860)	(0.2757)	(0.2751)
Treatment $(t-2)$	-0.0048	0.1762	0.2695	0.1988	0.2428	0.1626	0.0964	0.0866	-0.0120
	(0.2538)	(0.2349)	(0.2338)	(0.2245)	(0.2085)	(0.2057)	(0.2009)	(0.1920)	(0.1942)
Treatment $(t+0)$	0.7845	0.7395	1.003**	1.055**	1.169***	1.350***	1.262***	1.292***	1.223***
	(0.5208)	(0.4905)	(0.4769)	(0.4530)	(0.4415)	(0.4238)	(0.4132)	(0.4032)	(0.3953)
Treatment $(t+1)$	0.1255	0.1052	0.3075	0.2752	0.3984	0.5545	0.5307	0.5673	0.5544
	(0.4937)	(0.4618)	(0.4474)	(0.4309)	(0.4235)	(0.4051)	(0.3932)	(0.3821)	(0.3727)
R^2	0.97970	0.97926	0.97865	0.97766	0.97650	0.97644	0.97655	0.97662	0.97686
Panel B: In-person Turnout									
Treatment $(t-4)$	-0.0975	-0.1805	-0.1164	-0.1093	-0.1523	0.1058	0.0767	0.0598	0.0493
	(0.3795)	(0.3482)	(0.3298)	(0.3165)	(0.2953)	(0.3128)	(0.3055)	(0.2917)	(0.2895)
Treatment $(t-3)$	0.4028	0.2290	0.3164	0.3112	0.2802	0.4075	0.3447	0.3170	0.2345
	(0.3812)	(0.3424)	(0.3281)	(0.3198)	(0.2968)	(0.3109)	(0.3034)	(0.2942)	(0.2889)
Treatment $(t-2)$	0.1900	0.0385	-0.0106	0.0990	0.0304	0.0427	0.0396	-0.0040	0.0662
	(0.2905)	(0.2767)	(0.2687)	(0.2573)	(0.2427)	(0.2273)	(0.2208)	(0.2141)	(0.2095)
Treatment $(t+0)$	-0.3433	-0.3206	-0.5156	-0.5132	-0.6722	-0.8384*	-0.7956*	-0.8301**	-0.7674*
	(0.5435)	(0.5160)	(0.4932)	(0.4765)	(0.4605)	(0.4364)	(0.4265)	(0.4171)	(0.4063)
Treatment $(t+1)$	-0.4734	-0.4631	-0.5116	-0.4322	-0.5246	-0.6533*	-0.6139*	-0.6505*	-0.5732
	(0.4785)	(0.4422)	(0.4272)	(0.4228)	(0.4026)	(0.3827)	(0.3722)	(0.3618)	(0.3516)
R^2	0.96951	0.96959	0.96920	0.96786	0.96736	0.96728	0.96746	0.96732	0.96791
Panel C: Total Turnout									
Treatment $(t-4)$	0.0013	-0.1250	-0.0319	-0.0568	0.0379	0.1182	0.0928	0.0161	0.0015
	(0.3073)	(0.2892)	(0.2895)	(0.2772)	(0.2709)	(0.2565)	(0.2468)	(0.2378)	(0.2305)
Treatment $(t-3)$	0.2366	0.0124	0.1653	0.0934	0.1596	0.2255	0.1961	0.1339	0.0746
	(0.2888)	(0.2800)	(0.2878)	(0.2729)	(0.2577)	(0.2449)	(0.2365)	(0.2322)	(0.2271)
Treatment $(t-2)$	0.1852	0.2147	0.2590	0.2978	0.2732	0.2053	0.1360	0.0826	0.0543
	(0.2344)	(0.2312)	(0.2246)	(0.2149)	(0.2009)	(0.1870)	(0.1820)	(0.1840)	(0.1791)
Treatment $(t+0)$	0.4412*	0.4189*	0.4874**	0.5420**	0.4970**	0.5120**	0.4659**	0.4622**	0.4553**
	(0.2605)	(0.2506)	(0.2439)	(0.2394)	(0.2245)	(0.2123)	(0.2045)	(0.1993)	(0.1941)
Treatment $(t+1)$	-0.3479	-0.3579	-0.2042	-0.1569	-0.1262	-0.0988	-0.0832	-0.0832	-0.0189
	(0.2891)	(0.2722)	(0.2593)	(0.2567)	(0.2432)	(0.2256)	(0.2195)	(0.2106)	(0.2048)
R^2	0.97513	0.97447	0.97338	0.97153	0.97161	0.97178	0.97207	0.97197	0.97200
Specification	BW 6p.p.	BW 6.5p.p.	BW 7p.p.	BW 7.5p.p.	BW 8p.p.	BW 8.5p.p.	BW 9p.p.	BW 9.5p.p.	BW 10p.p.
Observations	1,464	1,620	1,764	1,872	1,992	2,148	2,226	2,328	2,430
Municipality FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Election-County FE	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: The table presents event study results based on Equation 2 for postal, in-person, and total turnout (0-100) using only municipalities where the first round of the mayoral race in 2020 was close, i.e., within a bandwidth of between 6 and 10 percentage points, respectively. The reference election $(t-1)$ is the 2018 State Election and it is controlled for the population as in Figure 3. Standard errors are clustered at the municipality level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

C.2 Clustering

In our baseline, we cluster at the level of municipalities, which is the level of the treatment. The county is the next higher administrative unit, and local election administrators report to the county offices. One might be concerned that model errors are correlated within counties as these have important tasks in organizing elections, such as the preparation of electoral registers or the dispatch of election notifications. We, therefore, increase the level of clustering to the county (Column 2 of Table C2). Standard errors are only marginally larger when clustering at this level. Furthermore, two-way clusters at the level of municipalities (to account for error correlation over time) and at the level of counties for each election (to account for within-county-election correlation) are slightly smaller (Column 3) than clustered standard errors at the county level. Following MacKinnon et al. (2023), we also check the robustness of our results using wild bootstrapped clustered standard errors in Columns (4) and (5) of Table C2 at the level of municipalities and counties, respectively. Statistically significant results remain statistically significant at least at the 5 percent level when bootstrapping standard errors at the level of municipalities or counties.

Table C2: Robustness: Standard Errors

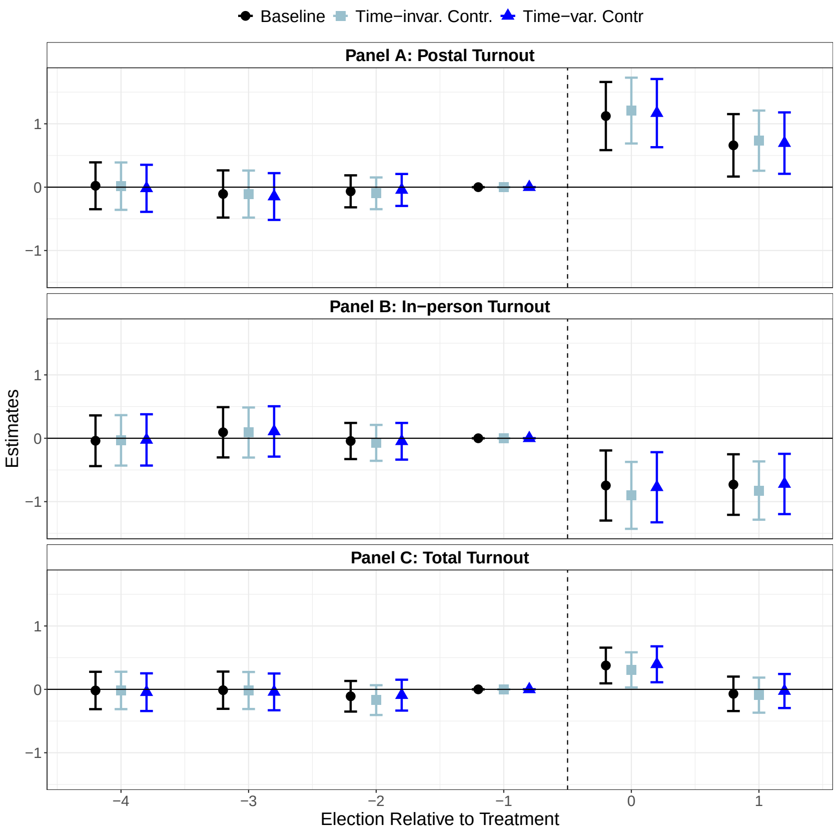
Panel A: Postal Turnout	(1)	(2)	(3)	(4)	(5)
Treatment $(t-4)$	0.0218	0.0218	0.0218	0.0218	0.0218
	(0.1885)	(0.1953)	(0.1946)	[0.901]	[0.899]
Treatment $(t-3)$	-0.1075	-0.1075	-0.1075	-0.1075	-0.1075
	(0.1898)	(0.2001)	(0.2020)	[0.586]	[0.586]
Treatment $(t-2)$	-0.0658	-0.0658	-0.0658	-0.0658	-0.0658
	(0.1286)	(0.1465)	(0.1610)	[0.611]	[0.673]
Treatment $(t+0)$	1.122***	1.122***	1.122***	1.122***	1.122***
	(0.2741)	(0.2997)	(0.2807)	[<0.001]	[<0.001]
Treatment $(t+1)$	0.6601***	0.6601**	0.6601**	0.6601**	0.6601**
	(0.2512)	(0.2783)	(0.2701)	[0.013]	[0.015]
R^2	0.96980	0.96980	0.96980	0.96980	0.96980
Panel B: In-person Turnout					
Treatment $(t-4)$	-0.0396	-0.0396	-0.0396	-0.0396	-0.0396
	(0.2040)	(0.1889)	(0.2033)	[0.831]	[0.846]
Treatment $(t-3)$	0.0941	0.0941	0.0941	0.0941	0.0941
	(0.2022)	(0.2098)	(0.2094)	[0.663]	[0.642]
Treatment $(t-2)$	-0.0433	-0.0433	-0.0433	-0.0433	-0.0433
	(0.1454)	(0.1359)	(0.1707)	[0.780]	[0.744]
Treatment $(t+0)$	-0.7456***	-0.7456**	-0.7456**	-0.7456***	-0.7456**
	(0.2819)	(0.3240)	(0.2990)	[0.009]	[0.027]
Treatment $(t+1)$	-0.7305***	-0.7305**	-0.7305***	-0.7305***	-0.7305**
	(0.2436)	(0.2904)	(0.2687)	[0.003]	[0.014]
R^2	0.95905	0.95905	0.95905	0.95905	0.95905
Panel C: Total Turnout					
Treatment $(t-4)$	-0.0178	-0.0178	-0.0178	-0.0178	-0.0178
	(0.1503)	(0.1512)	(0.1526)	[0.920]	[0.896]
Treatment $(t-3)$	-0.0134	-0.0134	-0.0134	-0.0134	-0.0134
	(0.1498)	(0.1620)	(0.1553)	[0.925]	[0.934]
Treatment $(t-2)$	-0.1091	-0.1091	-0.1091	-0.1091	-0.1091
	(0.1231)	(0.1326)	(0.1251)	[0.384]	[0.412]
Treatment $(t+0)$	0.3765***	0.3765**	0.3765***	0.3765**	0.3765**
	(0.1436)	(0.1408)	(0.1398)	[0.010]	[0.013]
Treatment $(t+1)$	-0.0705	-0.0705	-0.0705	-0.0705	-0.0705
	(0.1386)	(0.1454)	(0.1513)	[0.604]	[0.611]
R^2	0.95402	0.95402	0.95402	0.95402	0.95402
Standard Error	Munic.	County	TW Munic. + County-Elect.	Munic. Bootstrap	County Bootstrap
Number of clusters	1331	53	1331+318	1331	53
Observations	7,986	7,986	7,986	7,986	7,986
Municipality FE	✓	✓	✓	✓	✓
Election-County FE	✓	✓	✓	✓	✓

Notes: The table presents event study results based on Equation 2 for postal, in-person, and total turnout (0-100) using different standard error calculation methods: Column (1) is the baseline and standard errors are clustered at the municipality level. In Column (2), standard errors are clustered at the county level. Column (3) uses two-way clustered standard errors at the level of municipalities and county×elections. Standard errors are reported in parentheses. Column (4) uses wild cluster bootstrap at the municipality level and Column (5) at the county level. p-values from wild bootstrap clustering are reported in square brackets. We use Rademacher weights and 1000 replications. The reference election $(t-1)$ is the 2018 State Election and it is controlled for the population as in Figure 3. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

C.3 Controls

Additional Controls In the main specification, we only control for the log number of eligible voters held constant to the 2018 State Election, the last election before the treatment, interacted with election-specific coefficients. To test the robustness of the main specification, we add further time-invariant controls, holding them also constant to 2018 (Figure C1). Additional controls are the share of unemployed, the share of females, the share of households with children, and the share of 16 age groups (under 3, 3–5, 6–9, 10–14, 15–17, 18–19, 20–24, 25–29, 30–34, 35–39, 40–44, 45–49, 50–54, 55–59, 60–64, 65–74 years old, with over 74 years old as the reference group) in a municipality. We then also use these controls in a time-variant manner.[39] In both specifications, the results remain unchanged.

Figure C1: Additional Controls

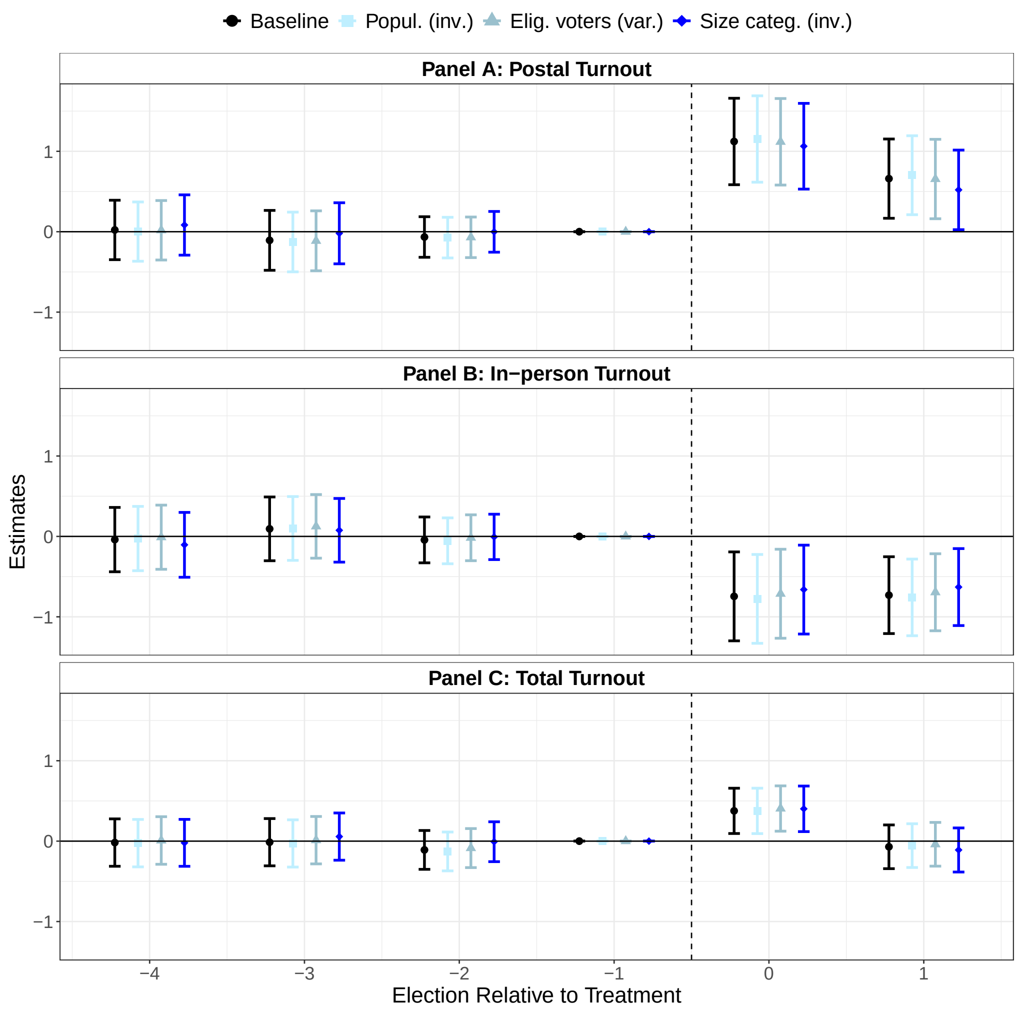


Notes: The figure presents event study results based on Equation 2 for postal, in-person, and total turnout (0-100) using different sets of controls. *Baseline* repeats the main results (Figure 3). *Time-invar. Contr.* uses the following additional controls, held constant to 2018: the share of unemployed, the share of females, the share of households with children, and the share of 16 age groups (under 3, 3–5, 6–9, 10–14, 15–17, 18–19, 20–24, 25–29, 30–34, 35–39, 40–44, 45–49, 50–54, 55–59, 60–64, 65–74 years old, with over 74 years old as the reference group). *Time-var. Contr.* uses the same additional covariates, allowing them to vary over time, except the share of households with children. The point estimates and standard errors underlying the results are shown in Table E15. Confidence intervals are drawn at the 95 percent level using standard errors clustered at the municipality level. The reference election ($t-1$) is the 2018 State Election.

[39] For the time-variant controls, we drop the share of households with children because it is not available for all time periods.

Population Measure In the main specification, we control for the log number of eligible voters, held constant to the 2018 State Election, the last election before the treatment, and interacted with election-specific coefficients. To further demonstrate robustness, we replace this variable with other population variables (Figure C2). First, we use the total population residing in a municipality, regardless of nationality or age, which is also held constant to 2018. Next, we allow the number of eligible voters to vary over time. Finally, we use constant-size classes that split the sample into quintiles. The size categories are constructed by splitting the sample based on the 2018 population measured as the number of eligible voters (thresholds: 1098, 1634, 2643, and 4496 eligible voters). In all specifications, the results remain robust.

Figure C2: Population Controls



Notes: The figure presents event study results based on Equation 2 for postal, in-person, and total turnout (0-100), using different population measures. *Baseline* repeats the main results (Figure 3). *Popul. (inv.)* uses the total population residing in a municipality, held constant to 2018. *Elig. voters (var.)* uses the number of eligible voters, allowing for variation over time. *Size categ. (inv.)* uses size categories, splitting the sample into quintiles based on the 2018 population measured as the number of eligible voters (thresholds: 1098, 1634, 2643, and 4496 eligible voters). The point estimates and standard errors underlying the results are shown in Table E16. Confidence intervals are drawn at the 95 percent level using standard errors clustered at the municipality level. The reference election $(t-1)$ is the 2018 State Election.

C.4 Matching on Observables

We conduct various matching procedures to ensure the observational comparability of the treatment and control group, namely local matching, propensity score matching (PSM), Mahalanobis distance matching, and Entropy Balancing as proposed by Hainmueller (2012). We re-estimate our baseline estimation (Equation 2) using each matching method and present the findings in Figure C3.

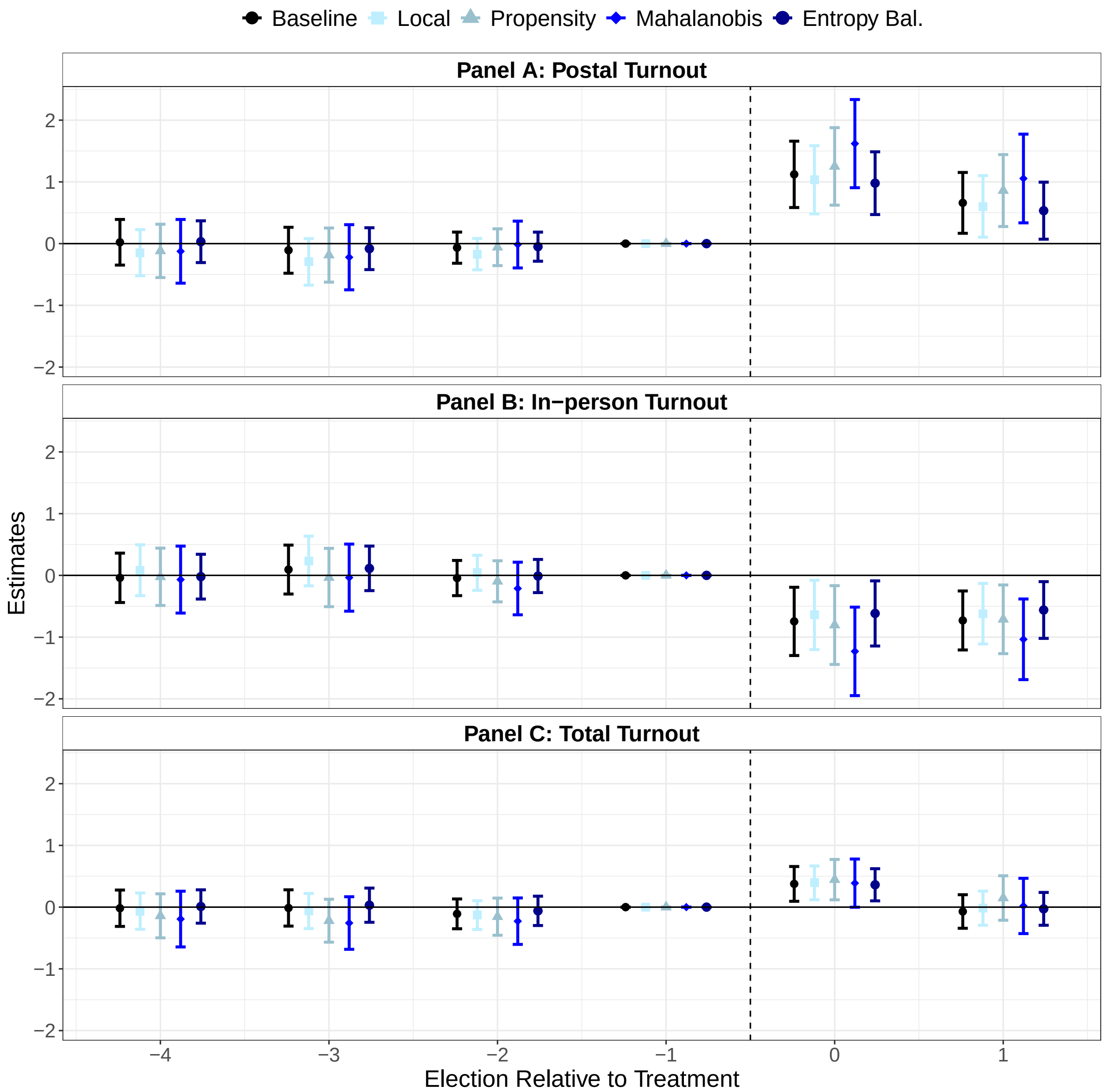
Local Matching First, we use a local matching approach based on geographical distances to ensure that control group municipalities far away from any treated municipality do not drive the effect, as municipalities closer to each other may be more similar. We identify untreated municipalities within the same county with a maximal border-to-border distance of 5km to a treated municipality. The remaining sample is depicted in Figure C4. We match all 205 treated units to 866 (out of 1252) untreated units. All treatment effects are close to the baseline estimation and remain statistically significant.

Propensity Score Matching We conduct a propensity score matching procedure to estimate the propensity of having a one-time all-postal election. This propensity is calculated using a probit regression based on the 2018 log population, measured as the number of eligible voters, within a county. The matched sample is obtained using 1:1 nearest neighbor matching without replacement. In this process, the 205 treated units are matched to an equal number of control units. The estimates derived from the matched sample confirm our original results.

Mahalanobis Matching We check the robustness of our findings by matching treated and untreated units based on the Mahalanobis distance. This method evaluates similarity by measuring the proximity of units' covariates in vector space. We calculate distances using the same covariates as previously mentioned and employ a 1:1 nearest neighbor matching with replacement to create the matched sample. In this process, the 205 treated municipalities are matched to 159 control group municipalities. Following Colmer et al. (2024), we adjust standard errors for two-way clustering to account for potential bias introduced by matching on covariates. The first cluster is at the municipality level, and the second cluster is at the control group$\times$election level. Estimates for postal and in-person turnout tend to increase. The effect for total turnout in $t+0$ remains marginally statistically significant. The lower precision can partly be attributed to the smaller sample size as 1093 municipalities are dropped from the control group.

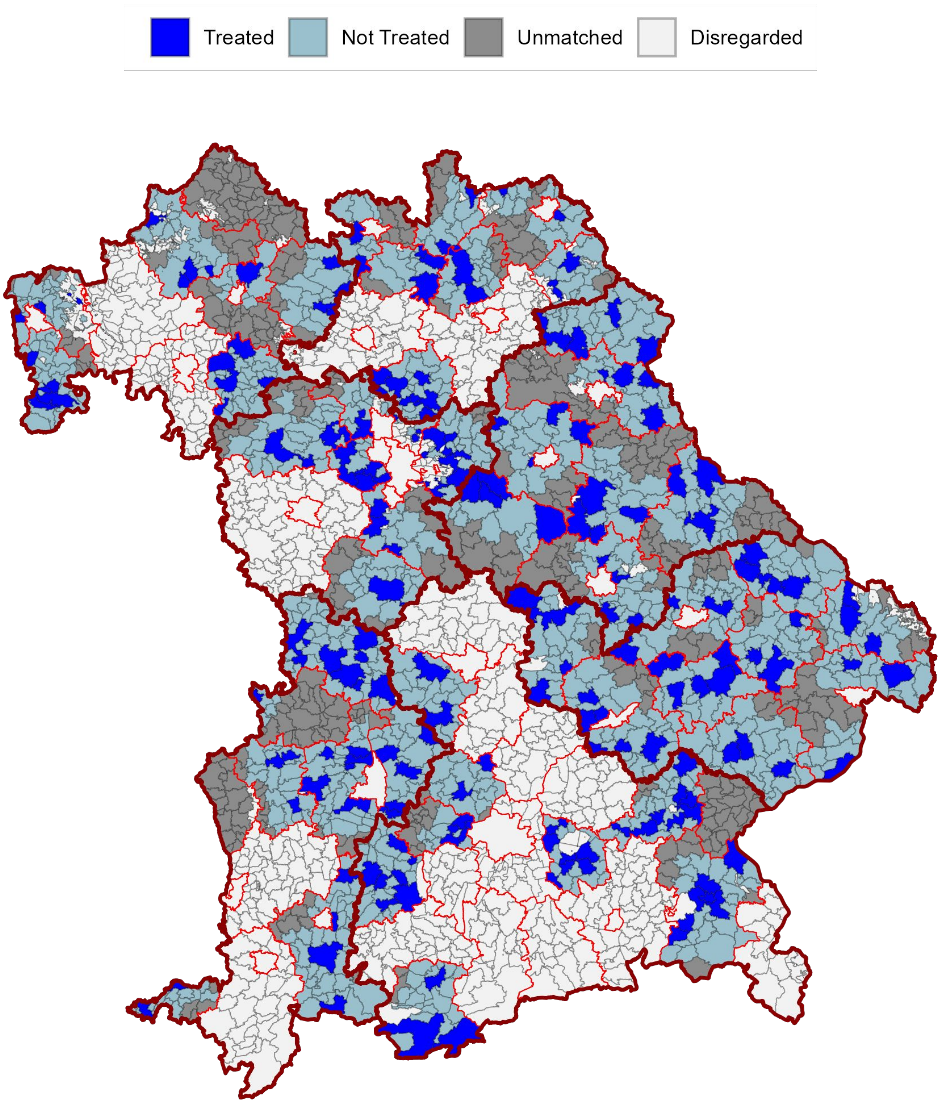
Entropy Balancing We implement the entropy balancing approach proposed by Hainmueller (2012). This method offers the advantage of not truncating the sample. Instead, it assigns weights to the control group to balance the treatment and control groups across the covariate distributions. Specifically, we balance the means and variances of the 2018 log population, measured as the number of eligible voters, within a county. We use the resulting entropy weights in the event study regressions. The results remain.

Figure C3: Event Study: Matching on Observables



Notes: The figure presents event study results based on Equation 2 for postal, in-person, and total turnout (0-100) using different matching approaches. *Baseline* repeats the main results (Figure 3). *Local* considers only municipalities in the control group that are located within the same county and at a border-to-border distance of at most 5km to treated municipalities. *Propensity Score* employs 1:1 nearest neighbor matching on propensity scores, which are computed from the pre-treatment population and the exact match on the county. *Mahalanobis* employs 1:1 nearest neighbor matching based on the Mahalanobis distance using the same characteristics. *Entropy Balancing* uses weights from Hainmueller (2012). The reference election $(t-1)$ is the 2018 State Election and it is controlled for the population as in Figure 3. For the Mahalanobis distance matching approach, we cluster at the municipality and control group×election level, following Colmer et al. (2024). For the other approaches, we cluster at the municipality level. Confidence intervals are drawn at the 95 percent level. The point estimates and standard errors underlying the results are shown in Table E17.

Figure C4: Local Matching

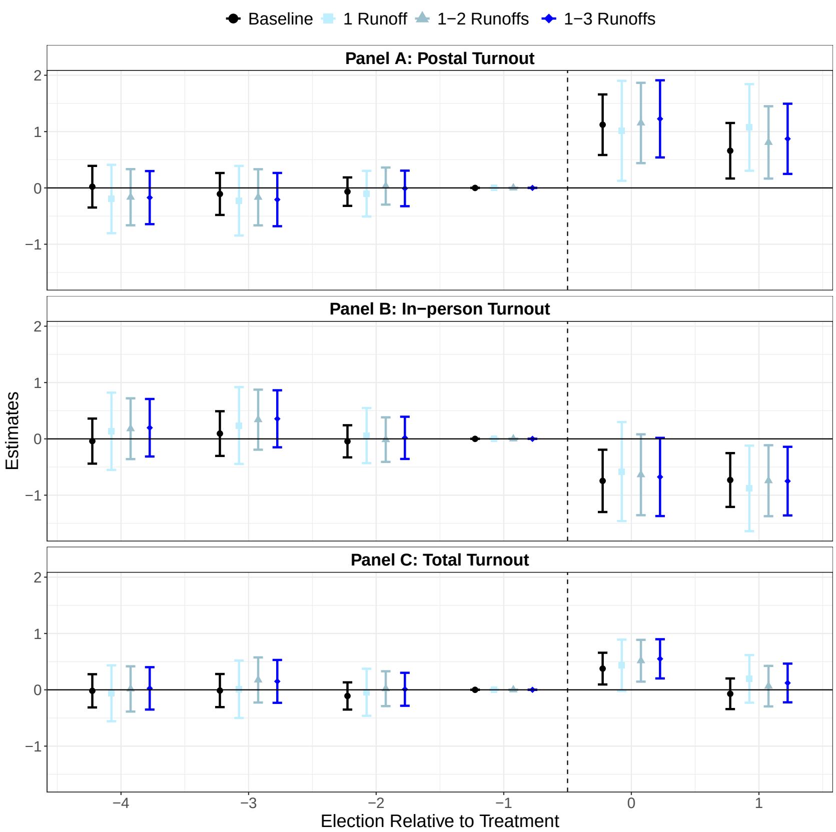


Notes: The figure presents a map of Bavaria containing municipalities used in the local matching approach. Only treated municipalities and control group municipalities that are at most 5km away from the border of a treated municipality within the same county are included. Thick red lines show government district boundaries, thin red lines show county boundaries and grey lines show municipality boundaries.

C.5 Number of Runoffs

We restrict the sample to municipalities with respect to the number of runoffs in the last three mayoral elections. This ensures that the result is not driven by treated municipalities with frequent runoffs or control group municipalities being far from having ever a runoff. Results are presented in Figure C5. *1 Runoff* considers only municipalities with exactly one runoff. This means that the treatment group did not have any runoff in the last three mayoral elections other than the treatment-defining election. *1–2 Runoffs* and *1–3 Runoffs* consider municipalities with at least one and at most two or three runoffs in the last three mayoral elections, respectively.[40] All specifications show robust estimates compared to the baseline results with respect to the increase in total and postal turnout. Only for in-person turnout in $t + 0$ the estimates for *1–2 Runoffs* and *1–3 Runoffs* are only marginally significant, and for *1 Runoff*, significance is lacking. However, the point estimates remain close to the baseline.

Figure C5: Number of Runoffs



Notes: The figure presents event study results based on Equation 2 for postal, in-person, and total turnout (0-100) using samples restricted by the minium and maximal number of runoffs in the last three mayoral elections. *Baseline* repeats the main results shown in Figure 3. *1 Runoff* considers only municipalities with exactly one runoff. *1–2 Runoffs* and *1–3 Runoffs* consider municipalities with at least one and at most two and three runoffs, respectively. The reference election $(t - 1)$ is the 2018 State Election and it is controlled for the population as in Figure 3. Confidence intervals are drawn at the 95 percent level using standard errors clustered at the municipality level. The point estimates and standard errors underlying the results are shown in Table E18.

[40] The number of municipalities by treatment is distributed as follows: *1 Runoff* (113 in the treatment and 240 in the control group); *1–2 Runoffs* (188 and 265); *1–3 Runoffs* (205 and 268).

C.6 Off-cycle Elections

To validate our findings further, we examine municipalities that did not participate in the mayoral elections in March 2020, which define the treatment, but had an off-cycle election prior to this date. This allows for a cleaner comparison. Specifically, as they did not vote in March 2020, they did not experiment with postal voting due to health issues and caution during the very early phase of the pandemic.

Table C3 provides an overview of runoffs (Panel A) and off-cycle elections (Panel B) in Bavaria. Within our sample (*No county [commissioner] runoff in 2020*), between the on-cycle elections in 2014 and 2020, there were 84 off-cycle elections. The cleanest comparison would be a control group of off-cycle municipalities with a runoff. However, this sample is very small, containing only 13 municipalities. Therefore, we also estimate a specification using all 84 off-cycle municipalities.

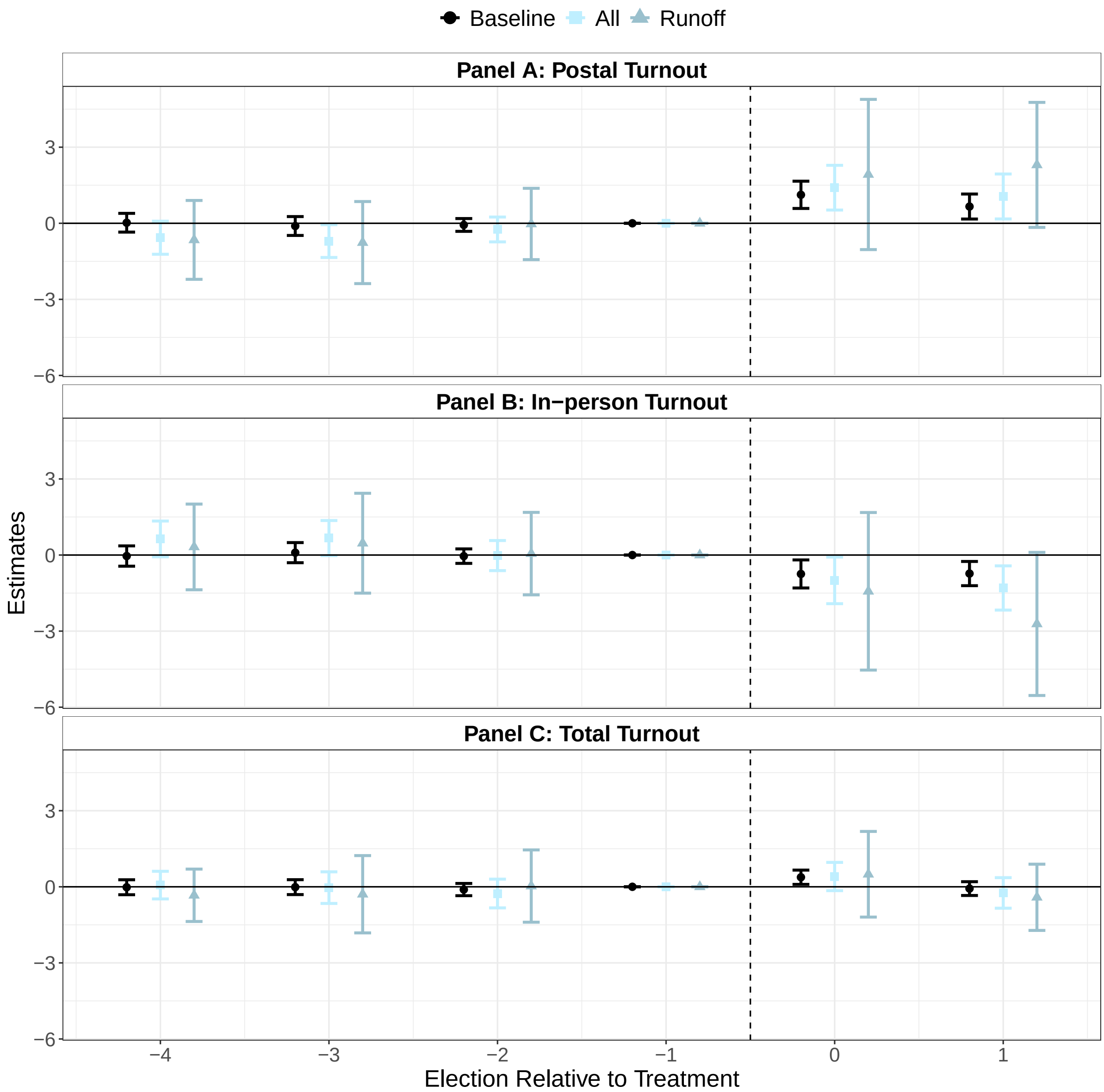
Table C3: Bavarian Local Elections by Runoff and Date

Panel A: 2020 Runoff Elections				
All	Mayor only	County only	Mayor & County	No runoff
2031	205	485	89	1252
Panel B: Off-cycle Elections				
	No county runoff in 2020		All municipalities	
Period	All	Runoff	All	Runoff
After Mar 30, 2014	84	13	125	24

Notes: The table presents an overview of municipalities in Bavaria, excluding independent cities. Panel A shows the number of municipalities by runoff category for the 2020 Local Elections (none, mayor, and/or county commissioner). Panel B shows the number of municipalities that had an off-cycle election, i.e., those that did not vote on the regular local election date in 2020. On March 30, 2014, the previous on-cycle local elections were held.

Figure C6 presents the results. *All* includes all municipalities with off-cycle mayoral elections between the on-cycle elections of 2014 and 2020. The analysis reveals larger effects compared to the main specification. However, due to lower precision, their difference to the main specification is not statistically significant. Therefore, the larger total turnout coefficient also lacks statistical significance. A further restriction of the sample to municipalities that had a runoff off-cycle election between 2014 and 2020 yields similar results, which, however, lose significance due to the small sample size (*Runoff*).

Figure C6: Off-Cycle Control Group



Notes: The figure presents event study results based on Equation 2 for postal, in-person, and total turnout (0-100) using municipalities as the control group where mayoral elections were held off-cycle (and no runoff in the 2020 county commissioner elections). *All* considers all municipalities as the control group with an off-cycle election between the on-cycle elections in 2014 and 2020. *Runoff* further restricts the control group to municipalities that had a runoff between the on-cycle elections in 2014 and 2020. The reference election ($t-1$) is the 2018 State Election. All specifications include the 2018 population as a time-invariant covariate. Confidence intervals are drawn at the 95 percent level using standard errors clustered at the municipality level. The point estimates and standard errors underlying the results are shown in Table E19.

C.7 Robust Inference Approach

To assess the robustness of our main findings to deviations from the parallel trends assumption, we apply the framework of Rambachan and Roth (2023), which constructs confidence intervals under two types of restrictions. First, *Relative Magnitude Bounds* (RM) constrain post-treatment deviations to be no larger than a multiple M^{RM} of the largest pre-treatment violation. Second, *Smoothness Restrictions* (SD) bound changes in the slope of differential trends across periods via a second-difference parameter M^{SD}, expressed in multiples of the coefficient's standard error.

Table C4 reports 95% confidence intervals under both approaches. In Panel A, RM-based bounds for $M^{RM} \in \{0, 0.5, 1\}$ show that effects on postal and in-person turnout remain statistically significant even when allowing for sizable post-treatment deviations. For total turnout, the $t+0$ estimate is robust up to $M^{RM} = 0.5$ but becomes insignificant at $M^{RM} = 1$, while the $t+1$ estimate includes zero even at $M^{RM} = 0$, reflecting its original insignificance.

Panel B displays bounds under increasing smoothness relaxations ($M^{SD} \in \{0, 0.25, 0.5, 0.75, 1.00\}\times$ s.e.). In-person turnout is robust in $t+0$, with estimates remaining significant even under the most flexible assumptions. in $t+1$, however, results become insignificant at $M^{SD} = 0.5\times$ s.e. Postal turnout is more sensitive in $t+0$, as the effect becomes insignificant at $M^{SD} = 0.75\times$ s.e., while the $t+1$ estimate remains significant throughout. For total turnout, the $t+0$ effect holds under strict linearity and mild smoothness, but becomes insignificant at $M^{SD} \geq 0.5\times$ s.e.; the $t+1$ estimate remains insignificant at all levels.

These results highlight the robustness of the postal and in-person turnout effects, with only modest sensitivity in the results, and limited robustness for total turnout. This is expected given the original $t+1$ coefficient for total turnout was already statistically insignificant. Importantly, our upper bound for M^{SD} is deliberately conservative, allowing for slope changes up to one standard error between periods.

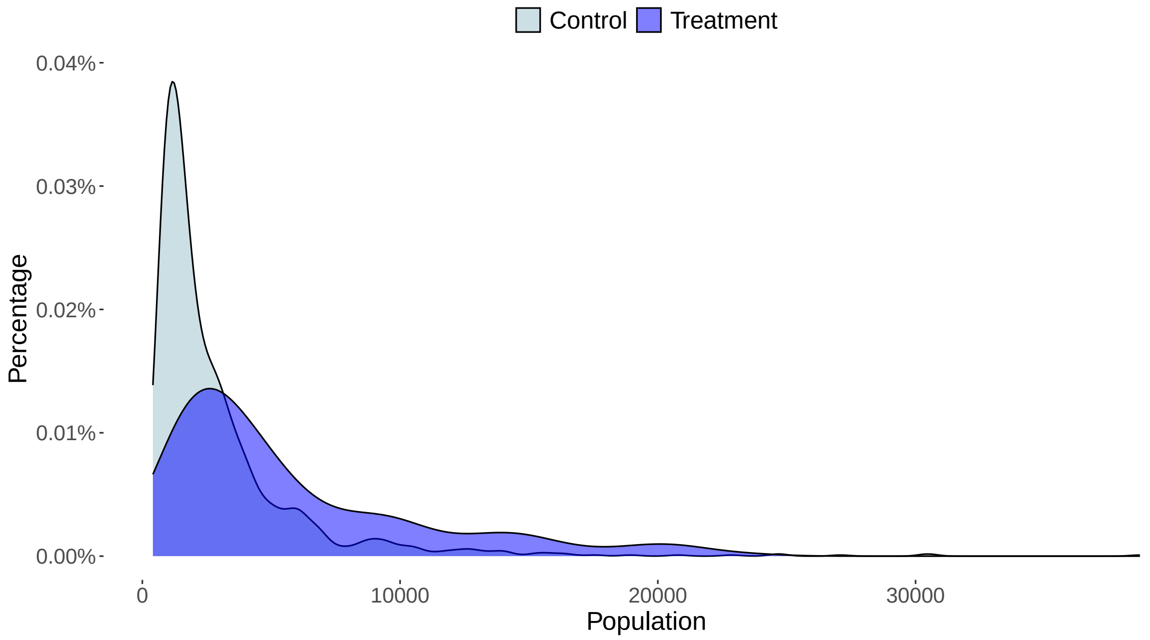
Table C4: Robust Inference Approach

Panel A: Relative Magnitude Bounds						
Turnout	Postal		In-person		Total	
Period	t+0	t+1	t+0	t+1	t+0	t+1
$M^{RM} = 0$	(0.576, 1.663)	(0.166, 1.162)	(-1.304, -0.186)	(-1.214, -0.249)	(0.089, 0.658)	(-0.347, 0.203)
$M^{RM} = 0.5$	(0.521, 1.707)	(0.106, 1.202)	(-1.360, -0.119)	(-1.273, -0.180)	(0.003, 0.704)	(-0.419, 0.247)
$M^{RM} = 1$	(0.466, 1.750)	(0.045, 1.252)	(-1.451, -0.006)	(-1.380, -0.054)	(-0.129, 0.796)	(-0.541, 0.341)
Panel B: Smoothness Restrictions						
Turnout	Postal		In-person		Total	
Period	t+0	t+1	t+0	t+1	t+0	t+1
M^{SD}=0	(0.727, 1.794)	(0.247, 1.242)	(-1.402, -0.272)	(-1.312, -0.310)	(0.159, 0.675)	(-0.340, 0.241)
$M^{SD} = 0.25 \times$ s.e.	(0.532, 1.763)	(0.082, 1.189)	(-1.428, -0.114)	(-1.335, -0.192)	(0.087, 0.721)	(-0.390, 0.263)
$M^{SD} = 0.50 \times$ s.e.	(0.386, 1.730)	(-0.012, 1.207)	(-1.494, -0.010)	(-1.373, -0.078)	(-0.015, 0.786)	(-0.464, 0.304)
$M^{SD} = 0.75 \times$ s.e.	(0.326, 1.787)	(-0.071, 1.259)	(-1.595, 0.027)	(-1.469, -0.049)	(-0.108, 0.826)	(-0.534, 0.330)
$M^{SD} = 1.00 \times$ s.e.	(0.261, 1.851)	(-0.130, 1.319)	(-1.665, 0.087)	(-1.542, -0.006)	(-0.182, 0.852)	(-0.597, 0.359)

Notes: The table reports robust 95% confidence intervals for post-treatment coefficients under assumptions about deviations from parallel trends, following Rambachan and Roth (2023). Panel A reports bounds under relative magnitude (RM) restrictions, where M^{RM} scales the largest pre-treatment deviation. Panel B shows results under smoothness restrictions (SD), where M^{SD} bounds the change in the slope of differential trends between periods. Confidence intervals are shown for each outcome and post-treatment period ($t+0$, $t+1$). Intervals that include zero indicate sensitivity to potential violations.

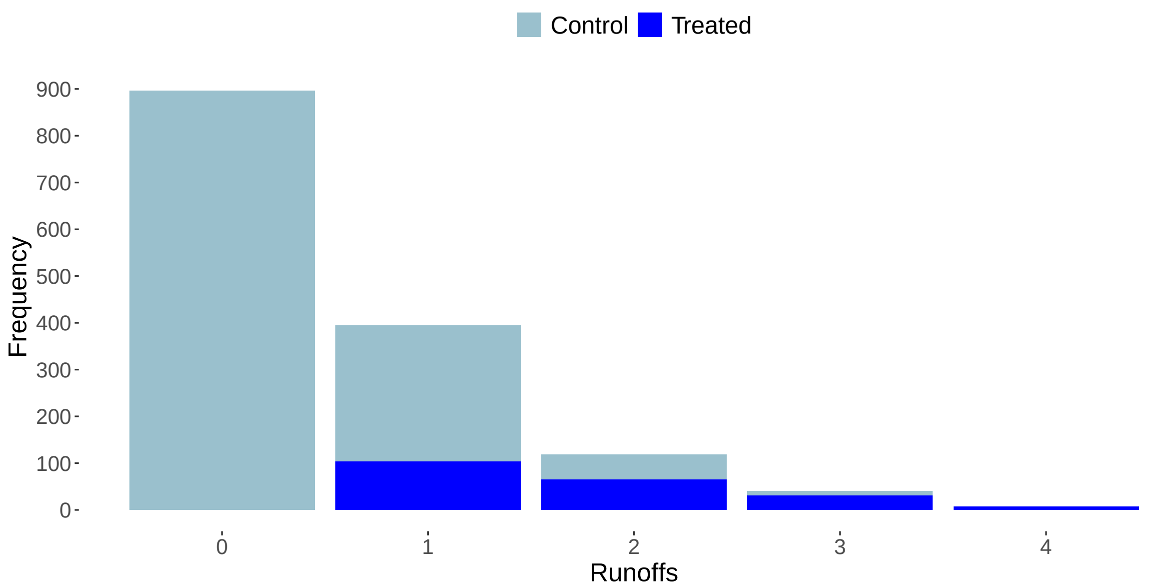
D Figures

Figure D1: Density of Population by Treatment



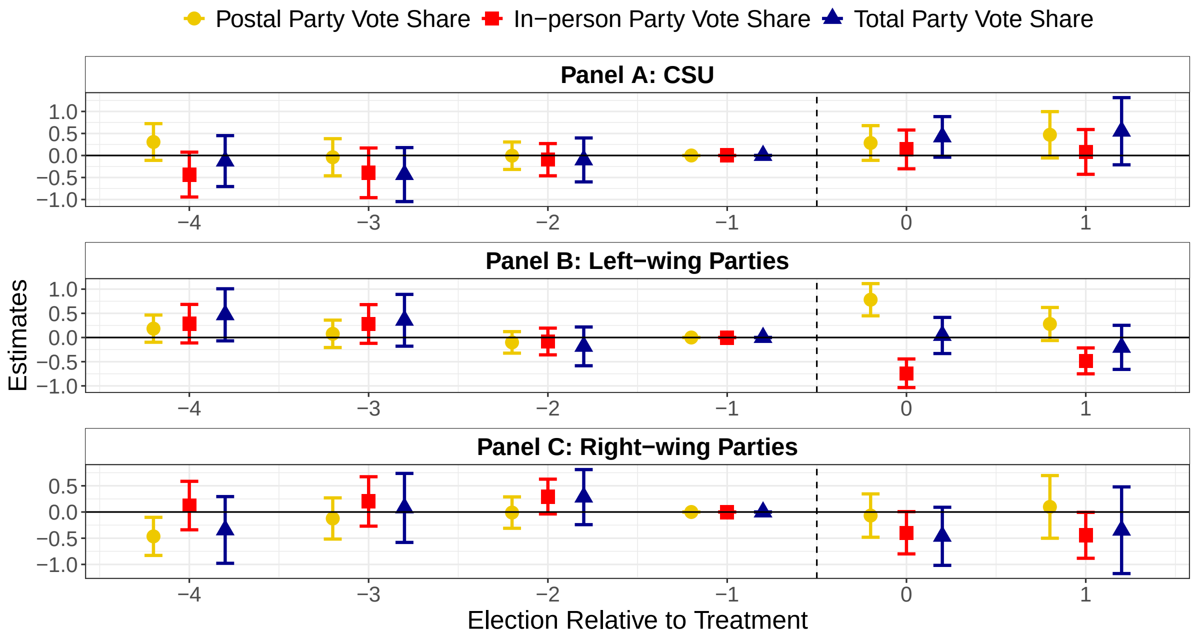
Notes: The figure shows a density plot depicting the distribution of the population variable for the treatment and control groups, representing the relative concentration of observations across the range of population values.

Figure D2: Number of Runoffs in the Last Four Elections



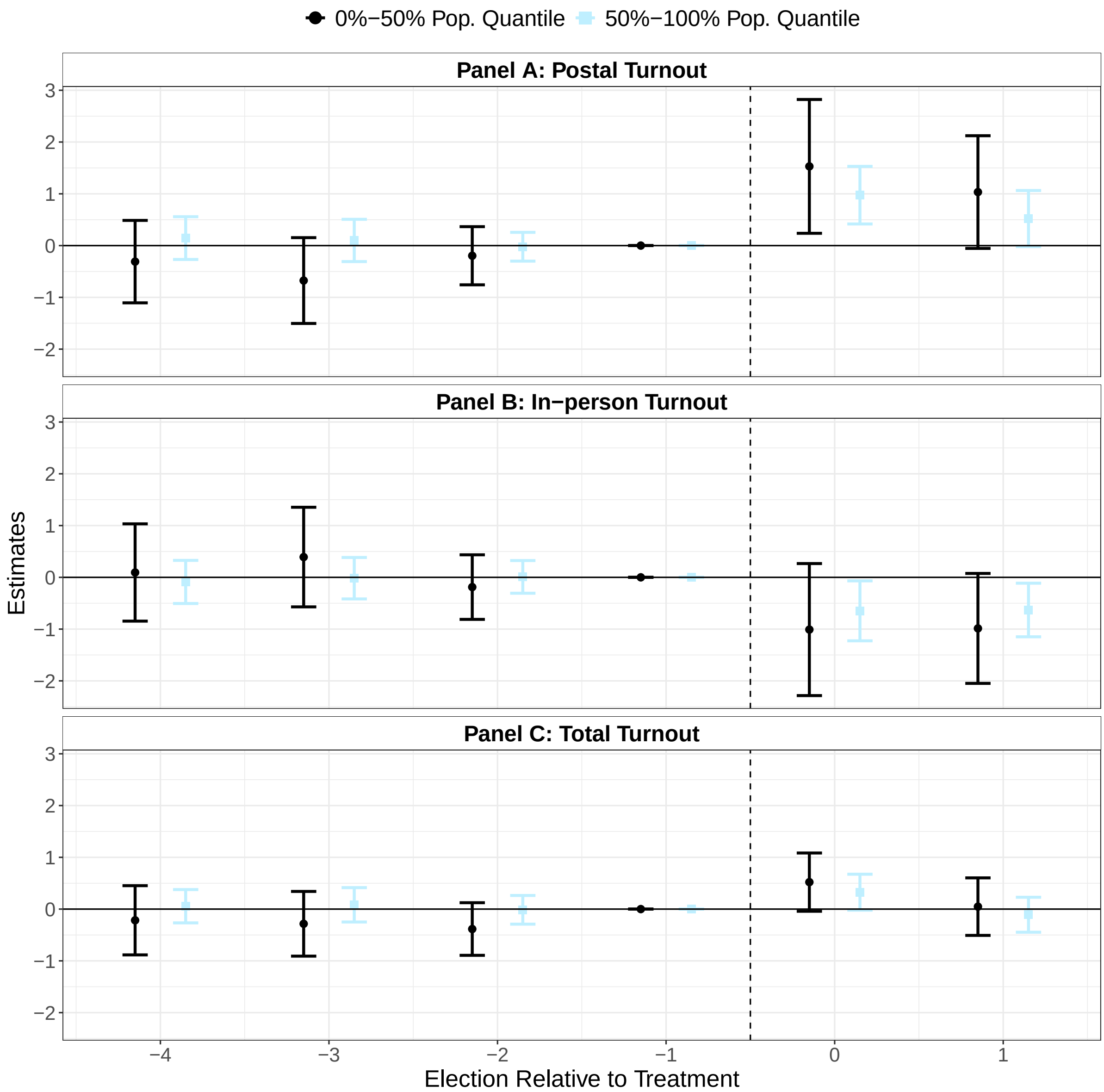
Notes: The figure shows the number of runoffs per municipality for Bavarian mayoral elections for the last four mayoral elections.

Figure D3: Election Results (Cast Votes)



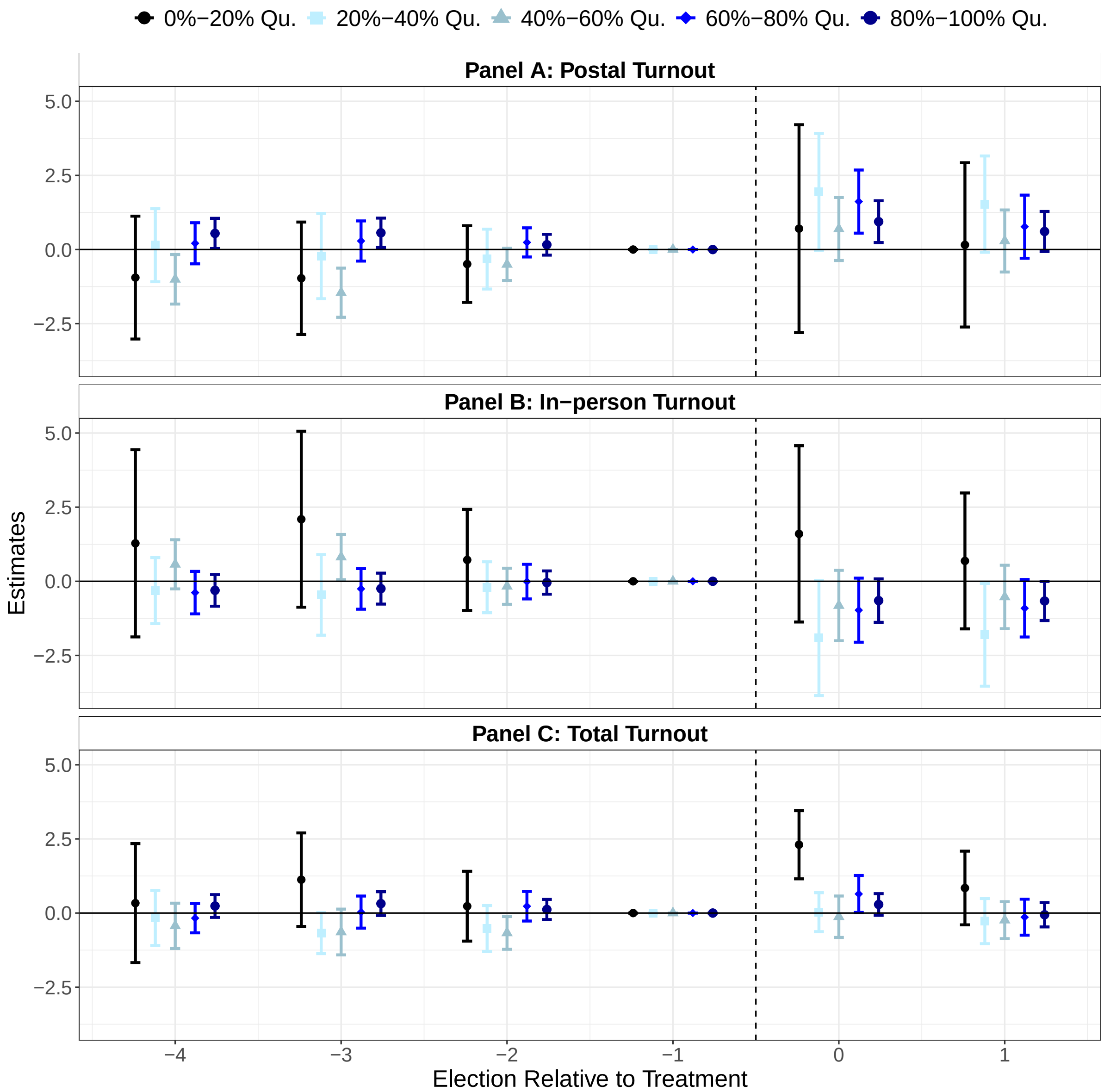
Notes: The figure presents event study results based on Equation 2 for postal vote share, the in-person vote share, and the total vote share (0-100) – each as a share of each as a share of votes cast – of the party *Christlich Soziale Union* (CSU, Panel A), and of left- and right-wing parties (Panel B and C). Left-wing parties include *Die Linke* (Linke), the *Sozialdemokratische Partei Deutschland* (SPD), and *Bündnis 90/Die Grüne* (Greens); right-wing parties include *Freie Wähler* (FW), the *Alternative für Deutschland* (AfD), and the *Freie Demokratische Partei* (FDP). The *Christlich Soziale Union* (CSU) is not included in the aggregates. The reference election ($t-1$) is the 2018 State Election and it is controlled for the population as in Figure 3. Confidence intervals are drawn at the 95 percent level using standard errors clustered at the municipality level. The point estimates and standard errors underlying the results are shown in Appendix Table E6.

Figure D4: Heterogeneity by Population Size: Median



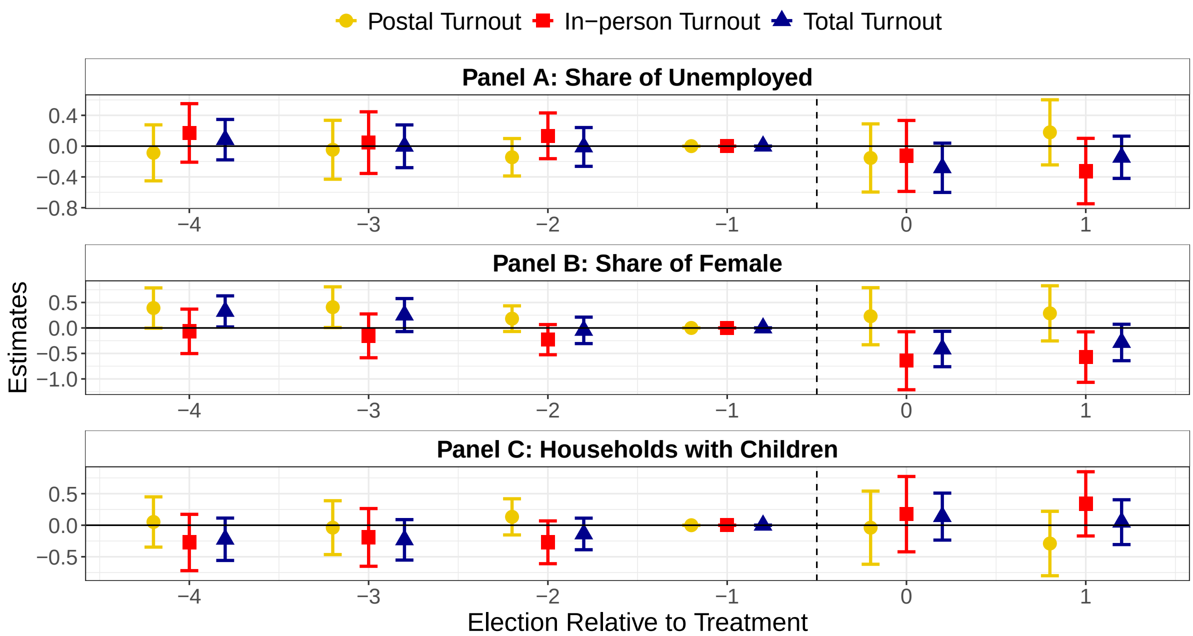
Notes: The figure presents event study results based on Equation 3 for postal, in-person, and total turnout (0-100). The reference election $(t-1)$ is the 2018 State Election and it is controlled for the population as in Figure 3. The Population Quantiles are based on the number of eligible voters constant in 2018. Standard errors are clustered at the municipality level and reported in parentheses. The reference election $(t-1)$ is the 2018 State Election and it is controlled for the population as in Figure 3. Confidence intervals are drawn at the 95 percent level using standard errors clustered at the municipality level. The point estimates and standard errors underlying the results are shown in Appendix Table E8.

Figure D5: Heterogeneity by Population Size: Quintiles



Notes: The figure presents event study results based on Equation 3 for postal, in-person, and total turnout (0-100). The reference election $(t-1)$ is the 2018 State Election and it is controlled for the population as in Figure 3. The Population Quantiles are based on the number of eligible voters constant in 2018. Standard errors are clustered at the municipality level and reported in parentheses. The reference election $(t-1)$ is the 2018 State Election and it is controlled for the population as in Figure 3. Confidence intervals are drawn at the 95 percent level using standard errors clustered at the municipality level. The point estimates and standard errors underlying the results are shown in Appendix Table E10.

Figure D6: Heterogeneities Across Socio-Economic Characteristics



Notes: The figure presents triple-difference estimates based on Equation 3 for postal, in-person, and total turnout (0-100). Panel captions refer to Z_i from Equation 3 and are transformed to z-scores (i.e., mean zero and unitary standard deviation). The reference election $(t-1)$ is the 2018 State Election and it is controlled for the population as in Figure 3. Confidence intervals are drawn at the 95 percent level using standard errors clustered at the municipality level. The point estimates and standard errors underlying the results are shown in Appendix Table E11.

E Tables

Table E1: Summary Statistics

Panel A: Full Sample								
Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Median	Pctl(75)	Max
Total Turnout (p.p.)	1,457	74.610	5.112	53.463	71.459	74.931	78.238	95.664
Postal Turnout (p.p.)	1,457	28.317	5.677	0.000	24.578	28.213	32.235	49.671
In-person Turnout (p.p.)	1,457	46.293	6.672	23.273	41.786	46.595	50.912	67.783
Elig. Voters (#)	1,457	3,332.650	3,717.260	409	1,225	2,040	3,874	38,703
Population (#)	1,457	4,391.392	5,185.257	465	1,549	2,599	4,974	58,707
Share Female (%)	1,457	0.497	0.013	0.386	0.490	0.498	0.505	0.536
Unemployment (%)	1,457	0.012	0.004	0.002	0.009	0.012	0.014	0.030
HH. with Children (%)	1,457	0.000	1.000	−1.231	−0.761	−0.269	0.538	4.870
Age 15–17 (%)	1,457	0.030	0.005	0.010	0.028	0.030	0.033	0.052
Age 18–19 (%)	1,457	0.022	0.004	0.009	0.020	0.022	0.024	0.041
Age 20–29 (%)	1,457	0.111	0.013	0.063	0.103	0.110	0.118	0.227
Age 30–59 (%)	1,457	0.425	0.018	0.328	0.413	0.424	0.437	0.506
Age 60+ (%)	1,457	0.275	0.033	0.176	0.253	0.274	0.295	0.468
Panel B: Treatment Group								
Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Median	Pctl(75)	Max
Total Turnout (p.p.)	205	73.869	5.430	57.238	70.154	74.477	77.644	84.349
Postal Turnout (p.p.)	205	28.999	4.474	19.002	25.853	28.521	32.236	44.481
In-person Turnout (p.p.)	205	44.870	6.048	24.865	40.064	46.001	49.425	57.711
Elig. Voters (#)	205	5,799.063	5,057.134	471	2,272	4,053	8,090	23,555
Population (#)	205	7,832.263	7,085.726	583	2,850	5,093	10,607	32,171
Share Female (%)	205	0.500	0.011	0.468	0.493	0.499	0.508	0.532
Unemployment (%)	205	0.014	0.004	0.005	0.011	0.013	0.016	0.027
HH. with Children (%)	205	−0.585	0.626	−1.217	−1.050	−0.773	−0.313	3.112
Age 15–17 (%)	205	0.030	0.004	0.015	0.028	0.030	0.033	0.043
Age 18–19 (%)	205	0.022	0.003	0.015	0.020	0.021	0.023	0.036
Age 20–29 (%)	205	0.110	0.012	0.073	0.102	0.110	0.117	0.163
Age 30–59 (%)	205	0.423	0.019	0.328	0.412	0.422	0.435	0.473
Age 60–99 (%)	205	0.278	0.034	0.176	0.256	0.277	0.295	0.468
Panel C: Control Group								
Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Median	Pctl(75)	Max
Total Turnout (p.p.)	1,252	74.731	5.050	53.463	71.671	75.013	78.323	95.664
Postal Turnout (p.p.)	1,252	28.205	5.844	0.000	24.332	28.114	32.235	49.671
In-person Turnout (p.p.)	1,252	46.526	6.742	23.273	42.066	46.708	51.208	67.783
Elig. Voters (#)	1,252	2,928.804	3,279.003	409	1,149.8	1,857	3,409.2	38,703
Population (#)	1,252	3,827.990	4,565.926	465	1,460	2,356	4,339.5	58,707
Share Female (%)	1,252	0.496	0.013	0.386	0.489	0.497	0.505	0.536
Unemployment (%)	1,252	0.012	0.004	0.002	0.009	0.011	0.014	0.030
HH. with Children (%)	1,252	0.096	1.017	−1.231	−0.692	−0.163	0.666	4.870
Age 15–17 (%)	1,252	0.031	0.005	0.010	0.028	0.030	0.033	0.052
Age 18–19 (%)	1,252	0.022	0.004	0.009	0.020	0.022	0.024	0.041
Age 20–29 (%)	1,252	0.111	0.014	0.063	0.103	0.111	0.119	0.227
Age 30–59 (%)	1,252	0.425	0.018	0.361	0.414	0.425	0.437	0.506
Age 60–99 (%)	1,252	0.275	0.033	0.182	0.252	0.273	0.295	0.427

Notes: The table presents summary statistics for the entire sample and split by treatment and control group for selected variables used in the analyses. All values are from 2018 and percentages are shares of the total population.

Table E2: Correlation Matrix

	Past Turnout	CSU	Female Share	Unemployed Share	Elig. Voters	Population Terciles	Households w. Childr.	Age Groups 15–17	18–19	20–29	30–59	60–99
Past Turnout	1.00	0.21	-0.17	-0.52	-0.33	-0.33	0.32	0.16	0.12	-0.10	0.23	-0.25
CSU	0.21	1.00	-0.21	-0.21	-0.26	-0.27	0.32	0.10	0.08	0.07	-0.10	0.01
Female Share	-0.17	-0.21	1.00	0.31	0.32	0.31	-0.38	-0.13	-0.17	-0.32	-0.31	0.40
Unemployed Share	-0.52	-0.21	0.31	1.00	0.34	0.32	-0.36	-0.24	-0.18	-0.01	-0.33	0.42
Elig. Voters	-0.33	-0.26	0.32	0.34	1.00	0.64	-0.58	-0.14	-0.09	0.08	-0.18	0.11
Pop. Terciles	-0.33	-0.27	0.31	0.32	0.64	1.00	-0.84	-0.17	-0.10	0.03	-0.14	0.13
Hh. w. Childr.	0.32	0.32	-0.38	-0.36	-0.58	-0.84	1.00	0.23	0.14	0.06	0.10	-0.20
Age Group 15–17	0.16	0.10	-0.13	-0.24	-0.14	-0.17	0.23	1.00	0.32	0.02	0.10	-0.36
Age Group 18–19	0.12	0.08	-0.17	-0.18	-0.09	-0.10	0.14	0.32	1.00	0.18	0.09	-0.34
Age Group 20–29	-0.10	0.07	-0.32	-0.01	0.08	0.03	0.06	0.02	0.18	1.00	0.00	-0.45
Age Group 30–59	0.23	-0.10	-0.31	-0.33	-0.18	-0.14	0.10	0.10	0.09	0.00	1.00	-0.70
Age Group 60+	-0.25	0.01	0.40	0.42	0.11	0.13	-0.20	-0.36	-0.34	-0.45	-0.70	1.00

Notes: The table shows Pearson pairwise correlations. All values, including *Past Turnout*, are from 2018, the first pre-treatment period. CSU represents the vote share of the conservative party. *Population Terciles* divide the sample into three equally sized categories based on the number of eligible voters, assigning each category a rank from one to three. Although not a continuous variable, its Pearson correlation is reported for completeness. *Female Share*, *Unemployed Share*, *Households w. Childr.*, and age group variables are expressed as proportions of the total population.

Table E3: Main Specification

	Postal Turnout (1)	In-person Turnout (2)	Total Turnout (3)
Treatment $(t-4)$	0.0218	-0.0396	-0.0178
	(0.1885)	(0.2040)	(0.1503)
Treatment $(t-3)$	-0.1075	0.0941	-0.0134
	(0.1898)	(0.2022)	(0.1498)
Treatment $(t-2)$	-0.0658	-0.0433	-0.1091
	(0.1286)	(0.1454)	(0.1231)
Treatment $(t+0)$	1.122***	-0.7456***	0.3765***
	(0.2741)	(0.2819)	(0.1436)
Treatment $(t+1)$	0.6601***	-0.7305***	-0.0705
	(0.2512)	(0.2436)	(0.1386)
Population $(t-4)$	1.160***	-0.8467***	0.3133***
	(0.0974)	(0.0945)	(0.0836)
Population $(t-3)$	1.185***	-0.3337***	0.8512***
	(0.1032)	(0.0933)	(0.0943)
Population $(t-2)$	0.5945***	0.3148***	0.9093***
	(0.0728)	(0.0732)	(0.0733)
Population $(t+0)$	-1.304***	1.476***	0.1718*
	(0.1309)	(0.1230)	(0.1018)
Population $(t+1)$	-1.751***	1.314***	-0.4367***
	(0.1252)	(0.1165)	(0.1016)
Observations	8,742	8,742	8,742
R^2	0.96980	0.95905	0.95402
Municipality FE	✓	✓	✓
Election-County FE	✓	✓	✓

Notes: The table presents event study results based on Equation 2 for postal, in-person, and total turnout (0-100). The reference election $(t-1)$ is the 2018 State Election and it is controlled for the population as in Figure 3. Standard errors are clustered at the municipality level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table E4: Difference in Coefficients (t-Test)

	Postal Turnout (1)	In-person Turnout (2)	Total Turnout (3)
$\hat{\beta}^1 - \hat{\beta}^0$	-0.4226**	0.0445	-0.3781***
	(0.1767)	(0.1558)	(0.1228)

Notes: The table presents t-test results, testing the difference between the post-treatment point estimates of the main results Figure 3, *Treatment (t+0)* and *Treatment (t+1)*. Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table E5: Party Outcomes (Elig. Voters)

Panel A: CSU Share	Postal Turnout (1)	In-person Turnout (2)	Total Turnout (3)
Treatment $(t-4)$	0.1905	-0.1733	0.0172
	(0.1374)	(0.1549)	(0.1991)
Treatment $(t-3)$	-0.0169	-0.2012	-0.2181
	(0.1351)	(0.1755)	(0.2012)
Treatment $(t-2)$	-0.0486	-0.2119	-0.2605
	(0.1113)	(0.1317)	(0.1759)
Treatment $(t+0)$	0.2748*	0.0248	0.2996*
	(0.1523)	(0.1649)	(0.1686)
Treatment $(t+1)$	0.3176	-0.0183	0.2993
	(0.1949)	(0.1850)	(0.2741)
R^2	0.89324	0.94694	0.90724
Panel B: Left-wing Parties			
Treatment $(t-4)$	0.0670	0.0904	0.1575
	(0.0890)	(0.1240)	(0.1660)
Treatment $(t-3)$	0.0212	0.1178	0.1390
	(0.0873)	(0.1292)	(0.1665)
Treatment $(t-2)$	-0.0325	-0.0370	-0.0695
	(0.0837)	(0.1017)	(0.1477)
Treatment $(t+0)$	0.7230***	-0.4953***	0.2277
	(0.1346)	(0.1122)	(0.1397)
Treatment $(t+1)$	0.2191*	-0.3648***	-0.1457
	(0.1242)	(0.0986)	(0.1634)
R^2	0.94410	0.95018	0.95580
Panel C: Right-wing Parties			
Treatment $(t-4)$	-0.2953**	0.0735	-0.2218
	(0.1209)	(0.1571)	(0.2099)
Treatment $(t-3)$	-0.0803	0.1287	0.0485
	(0.1374)	(0.1635)	(0.2310)
Treatment $(t-2)$	0.0124	0.1967	0.2091
	(0.1064)	(0.1239)	(0.1942)
Treatment $(t+0)$	0.0376	-0.2761*	-0.2385
	(0.1660)	(0.1486)	(0.2168)
Treatment $(t+1)$	0.0366	-0.3515**	-0.3149
	(0.2324)	(0.1626)	(0.3212)
R^2	0.94906	0.93384	0.95731
Observations	8,670	8,670	8,670
Municipality FE	✓	✓	✓
Election-County FE	✓	✓	✓

Notes: The table presents event study results based on Equation 2 for postal vote share, the in-person vote share, and the total vote share (0-100)—each as a share of eligible voters—of the party *Christlich Soziale Union* (CSU) (Panel A), and left- and right-wing parties (Panel B and C). Left-wing parties include *Die Linke* (Linke), the *Sozialdemokratische Partei Deutschland* (SPD), and *Bündnis 90/Die Grüne* (Greens); right-wing parties include *Freie Wähler* (FW), the *Alternative für Deutschland* (AfD), and the *Freie Demokratische Partei* (FDP). The *Christlich Soziale Union* (CSU) is not included in the aggregates. The reference election $(t-1)$ is the 2018 State Election and it is controlled for the population as in Figure 3. Standard errors are clustered at the municipality level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table E6: Party Outcomes (Cast Votes)

Panel A: CSU Share	Postal Turnout (1)	In-person Turnout (2)	Total Turnout (3)
Treatment $(t-4)$	0.3065	-0.4337*	-0.1272
	(0.2127)	(0.2596)	(0.2948)
Treatment $(t-3)$	-0.0400	-0.3938	-0.4339
	(0.2148)	(0.2874)	(0.3125)
Treatment $(t-2)$	-0.0056	-0.0954	-0.1010
	(0.1589)	(0.1864)	(0.2543)
Treatment $(t+0)$	0.2838	0.1378	0.4216*
	(0.2010)	(0.2244)	(0.2350)
Treatment $(t+1)$	0.4701*	0.0810	0.5511
	(0.2682)	(0.2593)	(0.3888)
R^2	0.86709	0.95743	0.94130
Panel B: Left-wing Parties			
Treatment $(t-4)$	0.1833	0.2860	0.4693*
	(0.1435)	(0.2030)	(0.2745)
Treatment $(t-3)$	0.0760	0.2798	0.3558
	(0.1444)	(0.2039)	(0.2729)
Treatment $(t-2)$	-0.1009	-0.0823	-0.1831
	(0.1135)	(0.1408)	(0.2043)
Treatment $(t+0)$	0.7814***	-0.7392***	0.0422
	(0.1698)	(0.1507)	(0.1904)
Treatment $(t+1)$	0.2793	-0.4829***	-0.2036
	(0.1740)	(0.1366)	(0.2323)
R^2	0.92942	0.94767	0.94486
Panel C: Right-wing Parties			
Treatment $(t-4)$	-0.4651**	0.1231	-0.3421
	(0.1852)	(0.2358)	(0.3245)
Treatment $(t-3)$	-0.1235	0.2016	0.0780
	(0.2004)	(0.2403)	(0.3356)
Treatment $(t-2)$	-0.0107	0.2948*	0.2841
	(0.1527)	(0.1693)	(0.2678)
Treatment $(t+0)$	-0.0679	-0.3958*	-0.4638
	(0.2108)	(0.2056)	(0.2826)
Treatment $(t+1)$	0.0976	-0.4451**	-0.3475
	(0.3048)	(0.2231)	(0.4215)
R^2	0.94904	0.92657	0.95448
Observations	8,670	8,670	8,670
Municipality FE	✓	✓	✓
Election-County FE	✓	✓	✓

Notes: The table presents event study results based on Equation 2 for postal vote share, the in-person vote share, and the total vote share (0-100)—each as a share of votes cast—of the party *Christlich Soziale Union* (CSU) (Panel A), and left- and right-wing parties (Panel B and C). Left-wing parties include *Die Linke* (Linke), the *Sozialdemokratische Partei Deutschland* (SPD), and *Bündnis 90/Die Grüne* (Greens); right-wing parties include *Freie Wähler* (FW), the *Alternative für Deutschland* (AfD), and the *Freie Demokratische Partei* (FDP). The *Christlich Soziale Union* (CSU) is not included in the aggregates. The reference election $(t-1)$ is the 2018 State Election and it is controlled for the population as in Figure 3. Standard errors are clustered at the municipality level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table E7: Heterogeneity by Past Turnout

	Postal Turnout (1)	In-person Turnout (2)	Total Turnout (3)
Treatment $(t-4)$	0.0221	-0.0457	-0.0236
	(0.1894)	(0.2049)	(0.1481)
Treatment $(t-3)$	-0.1013	0.0932	-0.0081
	(0.1926)	(0.2030)	(0.1425)
Treatment $(t-2)$	-0.0474	-0.0626	-0.1099
	(0.1297)	(0.1368)	(0.1025)
Treatment $(t+0)$	1.182***	-0.7729***	0.4091***
	(0.2739)	(0.2470)	(0.1169)
Treatment $(t+1)$	0.6826***	-0.7174***	-0.0347
	(0.2523)	(0.2230)	(0.1285)
Turnout '2018 (sc.)×Treatment $(t-4)$	0.0269	-0.0650	-0.0381
	(0.1611)	(0.1670)	(0.1351)
Turnout '2018 (sc.)×Treatment $(t-3)$	0.0816	0.0004	0.0820
	(0.1667)	(0.1688)	(0.1396)
Turnout '2018 (sc.)×Treatment $(t-2)$	0.1839	-0.1426	0.0413
	(0.1205)	(0.1260)	(0.1092)
Turnout '2018 (sc.)×Treatment $(t+0)$	0.5420**	-0.1631	0.3788**
	(0.2414)	(0.1934)	(0.1571)
Turnout '2018 (sc.)×Treatment $(t+1)$	0.2008	0.1932	0.3940***
	(0.2373)	(0.1985)	(0.1518)
Observations	8,742	8,742	8,742
R^2	0.97025	0.96334	0.95842
Municipality FE	✓	✓	✓
Election-County FE	✓	✓	✓

Notes: The table presents event study results based on Equation 3 for postal, in-person, and total turnout (0-100). The reference election $(t-1)$ is the 2018 State Election and it is controlled for the population as in Figure 3. The past turnout is held constant to 2018 and is scaled with a mean of 0 and a standard deviation of 1. Standard errors are clustered at the municipality level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table E8: Heterogeneity by Population Size: Median

Panel A: 0%-50% Pop. Quantile	Postal Turnout (1)	In-person Turnout (2)	Total Turnout (3)
Treatment $(t-4)$	-0.3099	0.0938	-0.2161
	(0.4059)	(0.4789)	(0.3408)
Treatment $(t-3)$	-0.6754	0.3917	-0.2837
	(0.4224)	(0.4904)	(0.3185)
Treatment $(t-2)$	-0.1964	-0.1885	-0.3849
	(0.2864)	(0.3180)	(0.2591)
Treatment $(t+0)$	1.530**	-1.009	0.5207*
	(0.6588)	(0.6500)	(0.2869)
Treatment $(t+1)$	1.034*	-0.9862*	0.0475
	(0.5547)	(0.5413)	(0.2833)
R^2	0.96984	0.95908	0.95411
Panel B: 50%-100% Pop. Quantile			
Treatment $(t-4)$	0.1446	-0.0890	0.0556
	(0.2105)	(0.2124)	(0.1640)
Treatment $(t-3)$	0.0993	-0.0166	0.0827
	(0.2081)	(0.2041)	(0.1691)
Treatment $(t-2)$	-0.0223	0.0086	-0.0137
	(0.1417)	(0.1609)	(0.1412)
Treatment $(t+0)$	0.9736***	-0.6472**	0.3263*
	(0.2836)	(0.2953)	(0.1772)
Treatment $(t+1)$	0.5220*	-0.6309**	-0.1089
	(0.2762)	(0.2641)	(0.1721)
R^2	0.96984	0.95908	0.95411
Observations	8,742	8,742	8,742
Municipality FE	✓	✓	✓
Election-County FE	✓	✓	✓

Notes: The table presents event study results based on Equation 3 for postal, in-person, and total turnout (0-100). The reference election $(t-1)$ is the 2018 State Election and it is controlled for the population as in Figure 3. The population quantiles are based on the number of eligible voters constant in 2018. Standard errors are clustered at the municipality level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table E9: Heterogeneity by Population Size: Terciles

Panel A: 0%-33% Pop. Quantile	Postal Turnout (1)	In-person Turnout (2)	Total Turnout (3)
Treatment $(t-4)$	-0.1780	0.5974	0.4194
	(0.7071)	(0.9099)	(0.6356)
Treatment $(t-3)$	-0.4204	0.8593	0.4389
	(0.7252)	(0.9490)	(0.4948)
Treatment $(t-2)$	0.0143	-0.0527	-0.0384
	(0.4989)	(0.5361)	(0.4057)
Treatment $(t+0)$	1.088	0.1546	1.242**
	(1.190)	(1.112)	(0.4938)
Treatment $(t+1)$	0.9775	-0.5314	0.4460
	(0.9633)	(0.9461)	(0.4688)
R^2	0.96984	0.95910	0.95409
Panel B: 33%-67% Pop. Quantile			
Treatment $(t-4)$	-0.8323**	0.3034	-0.5289*
	(0.3349)	(0.3276)	(0.2888)
Treatment $(t-3)$	-1.143***	0.5876*	-0.5554*
	(0.3409)	(0.3171)	(0.3074)
Treatment $(t-2)$	-0.5837**	0.1969	-0.3868
	(0.2390)	(0.2713)	(0.2608)
Treatment $(t+0)$	1.245**	-1.251**	-0.0067
	(0.4969)	(0.5266)	(0.2994)
Treatment $(t+1)$	0.5946	-0.9210**	-0.3264
	(0.4689)	(0.4501)	(0.2594)
R^2	0.96990	0.95914	0.95408
Panel C: 67%-100% Pop. Quantile			
Treatment $(t-4)$	0.5083**	-0.3203	0.1880
	(0.2260)	(0.2357)	(0.1638)
Treatment $(t-3)$	0.5136**	-0.3122	0.2014
	(0.2227)	(0.2247)	(0.1691)
Treatment $(t-2)$	0.2151	-0.1477	0.0674
	(0.1485)	(0.1688)	(0.1353)
Treatment $(t+0)$	1.025***	-0.6305**	0.3948**
	(0.3038)	(0.3057)	(0.1656)
Treatment $(t+1)$	0.5723*	-0.6573**	-0.0849
	(0.2963)	(0.2835)	(0.1867)
R^2	0.96990	0.95917	0.95408
Observations	8,742	8,742	8,742
Municipality FE	✓	✓	✓
Election-County FE	✓	✓	✓

Notes: The table presents event study results based on Equation 3 for postal, in-person, and total turnout (0-100). The reference election $(t-1)$ is the 2018 State Election and it is controlled for the population as in Figure 3. The population quantiles are based on the number of eligible voters constant in 2018. Standard errors are clustered at the municipality level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table E10: Heterogeneity by Population Size: Quintiles

Panel A: 0%-20% Pop. Quantile	Postal Turnout (1)	In-person Turnout (2)	Total Turnout (3)
Treatment $(t-4)$	-0.9463	1.281	0.3350
	(1.056)	(1.610)	(1.023)
Treatment $(t-3)$	-0.9686	2.093	1.125
	(0.9662)	(1.513)	(0.8044)
Treatment $(t-2)$	-0.4894	0.7206	0.2313
	(0.6593)	(0.8692)	(0.6003)
Treatment $(t+0)$	0.7046	1.600	2.304***
	(1.787)	(1.516)	(0.5863)
Treatment $(t+1)$	0.1575	0.6873	0.8448
	(1.413)	(1.168)	(0.6336)
R^2	0.96982	0.95911	0.95411
Panel B: 20%-40% Pop. Quantile			
Treatment $(t-4)$	0.1471	-0.3146	-0.1674
	(0.6282)	(0.5672)	(0.4733)
Treatment $(t-3)$	-0.2203	-0.4582	-0.6785*
	(0.7331)	(0.6931)	(0.3520)
Treatment $(t-2)$	-0.3223	-0.2000	-0.5224
	(0.5146)	(0.4382)	(0.3952)
Treatment $(t+0)$	1.943*	-1.914*	0.0294
	(1.006)	(0.9894)	(0.3350)
Treatment $(t+1)$	1.532*	-1.804**	-0.2722
	(0.8279)	(0.8840)	(0.3883)
R^2	0.96984	0.95911	0.95405
Panel C: 40%-60% Pop. Quantile			
Treatment $(t-4)$	-1.004**	0.5720	-0.4319
	(0.4257)	(0.4225)	(0.3893)
Treatment $(t-3)$	-1.456***	0.8167**	-0.6389
	(0.4226)	(0.3884)	(0.3936)
Treatment $(t-2)$	-0.5018*	-0.1689	-0.6707**
	(0.2771)	(0.3102)	(0.2813)
Treatment $(t+0)$	0.6933	-0.8172	-0.1239
	(0.5425)	(0.6056)	(0.3562)
Treatment $(t+1)$	0.2881	-0.5281	-0.2400
	(0.5334)	(0.5457)	(0.3179)
R^2	0.96986	0.95910	0.95406
Panel D: 60%-80% Pop. Quantile			
Treatment $(t-4)$	0.2106	-0.3826	-0.1719
	(0.3532)	(0.3655)	(0.2524)
Treatment $(t-3)$	0.2879	-0.2556	0.0323
	(0.3452)	(0.3494)	(0.2761)
Treatment $(t-2)$	0.2405	-0.0102	0.2302
	(0.2504)	(0.2980)	(0.2545)
Treatment $(t+0)$	1.617***	-0.9739*	0.6434**
	(0.5428)	(0.5511)	(0.3173)
Treatment $(t+1)$	0.7703	-0.9091*	-0.1388
	(0.5429)	(0.4945)	(0.3097)
R^2	0.96985	0.95908	0.95409
Panel E: 80%-100% Pop. Quantile			
Treatment $(t-4)$	0.5435**	-0.3059	0.2377
	(0.2597)	(0.2727)	(0.1956)
Treatment $(t-3)$	0.5650**	-0.2469	0.3182
	(0.2524)	(0.2662)	(0.2045)
Treatment $(t-2)$	0.1626	-0.0427	0.1200
	(0.1791)	(0.2000)	(0.1738)
Treatment $(t+0)$	0.9403***	-0.6511*	0.2892
	(0.3600)	(0.3736)	(0.1860)
Treatment $(t+1)$	0.6081*	-0.6649**	-0.0569
	(0.3444)	(0.3365)	(0.2095)
R^2	0.96990	0.95911	0.95409
Observations	8,742	8,742	8,742
Municipality FE	✓	✓	✓
Election-County FE	✓	✓	✓

Notes: The table presents event study results based on Equation 3 for postal, in-person, and total turnout (0-100). The reference election $(t-1)$ is the 2018 State Election and it is controlled for the population as in Figure 3. The population quantiles are based on the number of eligible voters constant in 2018. Standard errors are clustered at the municipality level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table E11: Heterogeneity by Socio-Economic Characteristics

Panel A: Unemployment	Postal Turnout (1)	In-person Turnout (2)	Total Turnout (3)
Unempl.×Treatment $(t-4)$	-0.0874	0.1714	0.0840
	(0.1855)	(0.1939)	(0.1338)
Unempl.×Treatment $(t-3)$	-0.0473	0.0452	-0.0021
	(0.1951)	(0.2041)	(0.1416)
Unempl.×Treatment $(t-2)$	-0.1447	0.1340	-0.0106
	(0.1239)	(0.1516)	(0.1285)
Unempl.×Treatment $(t+0)$	-0.1538	-0.1276	-0.2814*
	(0.2257)	(0.2350)	(0.1632)
Unempl.×Treatment $(t+1)$	0.1790	-0.3240	-0.1450
	(0.2156)	(0.2165)	(0.1400)
R^2	0.97088	0.96107	0.95435
Panel B: Share Female			
Female Share×Treatment $(t-4)$	0.3913*	-0.0664	0.3249**
	(0.2013)	(0.2221)	(0.1553)
Female Share×Treatment $(t-3)$	0.4076**	-0.1549	0.2527
	(0.2034)	(0.2190)	(0.1652)
Female Share×Treatment $(t-2)$	0.1824	-0.2296	-0.0472
	(0.1282)	(0.1508)	(0.1322)
Female Share×Treatment $(t+0)$	0.2301	-0.6431**	-0.4130**
	(0.2853)	(0.2900)	(0.1769)
Female Share×Treatment $(t+1)$	0.2867	-0.5715**	-0.2848
	(0.2757)	(0.2524)	(0.1820)
R^2	0.96997	0.95949	0.95423
Panel C: Hh. w. Children			
Hh. w. Children×Treatment $(t-4)$	-0.6739**	0.4847	-0.1892
	(0.2799)	(0.3431)	(0.2451)
Hh. w. Children×Treatment $(t-3)$	-0.8007***	0.6339*	-0.1668
	(0.2985)	(0.3494)	(0.2180)
Hh. w. Children×Treatment $(t-2)$	-0.2507	-0.0903	-0.3410*
	(0.2279)	(0.2626)	(0.1841)
Hh. w. Children×Treatment $(t+0)$	-0.0351	0.3201	0.2850
	(0.5054)	(0.4595)	(0.2073)
Hh. w. Children×Treatment $(t+1)$	-0.0726	0.1276	0.0549
	(0.5177)	(0.4160)	(0.2586)
R^2	0.96993	0.95918	0.95410
Observations	8,742	8,742	8,742
Municipality FE	✓	✓	✓
Election-County FE	✓	✓	✓

Notes: The table presents event study results based on Equation 3 for postal, in-person, and total turnout (0-100). The reference election $(t-1)$ is the 2018 State Election and it is controlled for the population as in Figure 3. The socio-economic covariates were held constant to 2018 and scaled with a mean of 0 and a standard deviation of 1. Standard errors are clustered at the municipality level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table E12: Heterogeneity by Age

Panel A: Share of 15 to 17 year old	Postal Turnout (1)	In-person Turnout (2)	Total Turnout (3)
15-17yo×Treatment $(t-4)$	0.1657	-0.2538	-0.0881
	(0.1961)	(0.2042)	(0.1676)
15-17yo×Treatment $(t-3)$	0.1519	-0.2347	-0.0828
	(0.1942)	(0.2039)	(0.1604)
15-17yo×Treatment $(t-2)$	-0.0047	-0.1934	-0.1981
	(0.1305)	(0.1574)	(0.1277)
15-17yo×Treatment $(t+0)$	-0.0667	0.0563	-0.0104
	(0.2751)	(0.2741)	(0.1435)
15-17yo×Treatment $(t+1)$	-0.0266	0.1986	0.1720
	(0.2788)	(0.2569)	(0.1490)
R^2	0.96998	0.95920	0.95410
Panel B: Share of 18 to 19 year old			
18-19yo×Treatment $(t-4)$	-0.4392**	0.2120	-0.2273
	(0.1979)	(0.2287)	(0.1884)
18-19yo×Treatment $(t-3)$	-0.4139*	0.3832*	-0.0306
	(0.2115)	(0.2295)	(0.1753)
18-19yo×Treatment $(t-2)$	0.0289	-0.0843	-0.0554
	(0.1474)	(0.1653)	(0.1521)
18-19yo×Treatment $(t+0)$	-0.3415	0.4814	0.1399
	(0.3140)	(0.3149)	(0.1825)
18-19yo×Treatment $(t+1)$	-0.5808**	0.5838**	0.0030
	(0.2875)	(0.2688)	(0.1780)
R^2	0.96987	0.95915	0.95413
Panel C: Share of 20 to 29 year old			
20-29yo×Treatment $(t-4)$	0.1506	-0.1413	0.0093
	(0.1827)	(0.1914)	(0.1581)
20-29yo×Treatment $(t-3)$	0.0948	-0.1179	-0.0232
	(0.1893)	(0.1956)	(0.1595)
20-29yo×Treatment $(t-2)$	0.0036	0.1288	0.1323
	(0.1289)	(0.1460)	(0.1242)
20-29yo×Treatment $(t+0)$	-0.6469**	0.9187***	0.2717*
	(0.3093)	(0.2972)	(0.1563)
20-29yo×Treatment $(t+1)$	-0.5275*	0.6851**	0.1575
	(0.2883)	(0.2705)	(0.1591)
R^2	0.96991	0.95921	0.95415
Panel D: Share of 30 to 59 year old			
30-59yo×Treatment $(t-4)$	-0.0050	-0.0566	-0.0616
	(0.1665)	(0.2032)	(0.1470)
30-59yo×Treatment $(t-3)$	-0.1384	-0.0937	-0.2320
	(0.1696)	(0.2030)	(0.1496)
30-59yo×Treatment $(t-2)$	0.1308	-0.2033	-0.0725
	(0.1421)	(0.1428)	(0.1233)
30-59yo×Treatment $(t+0)$	0.1587	0.0715	0.2303
	(0.2470)	(0.2732)	(0.1407)
30-59yo×Treatment $(t+1)$	0.0445	0.1199	0.1643
	(0.2215)	(0.2360)	(0.1204)
R^2	0.96992	0.95913	0.95436
Panel E: Share of over 60 year old			
over 60yo×Treatment $(t-4)$	-0.3131*	0.3926**	0.0795
	(0.1750)	(0.1932)	(0.1476)
over 60yo×Treatment $(t-3)$	-0.2181	0.3382*	0.1201
	(0.1792)	(0.2046)	(0.1516)
over 60yo×Treatment $(t-2)$	-0.2741**	0.3013**	0.0272
	(0.1346)	(0.1454)	(0.1168)
over 60yo×Treatment $(t+0)$	0.2200	-0.4334	-0.2134
	(0.2537)	(0.2801)	(0.1590)
over 60yo×Treatment $(t+1)$	0.4673*	-0.5414**	-0.0741
	(0.2460)	(0.2479)	(0.1387)
R^2	0.97033	0.95947	0.95441
Observations	8,742	8,742	8,742
Municipality FE	✓	✓	✓
Election-County FE	✓	✓	✓

Notes: The table presents event study results based on Equation 3 for postal, in-person, and total turnout (0-100). The reference election $(t-1)$ is the 2018 State Election and it is controlled for the population as in Figure 3. The socio-economic covariates are held constant to 2018 and are scaled with a mean of 0 and a standard deviation of 1. Standard errors are clustered at the municipality level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table E13: Placebos

Panel A: Postal Turnout	(1)	(2)	(3)	(4)
Treatment $(t-4)$	0.0218		-0.3531	-0.0383
	(0.1885)		(0.6323)	(0.3034)
Treatment $(t-3)$	-0.1075		-0.3512	-0.0953
	(0.1898)		(0.6360)	(0.2912)
Treatment $(t-2)$	-0.0658	-0.0094	0.0088	-0.2497
	(0.1286)	(0.1353)	(0.3064)	(0.2285)
Treatment $(t+0)$	1.122***	0.2025	-0.2267	0.3650
	(0.2741)	(0.2934)	(0.9560)	(0.3463)
Treatment $(t+1)$	0.6601***	-0.0972	-0.3613	-0.1458
	(0.2512)	(0.3259)	(0.5616)	(0.3791)
Treatment $(t+2)$		-0.2339		
		(0.4835)		
Treatment $(t+3)$		-0.4263		
		(0.5130)		
R^2	0.96980	0.97202	0.98587	0.96620
Panel B: In-person Turnout				
Treatment $(t-4)$	-0.0396		-0.8388	0.0402
	(0.2040)		(0.7491)	(0.2831)
Treatment $(t-3)$	0.0941		-0.8290	0.1590
	(0.2022)		(0.7503)	(0.2541)
Treatment $(t-2)$	-0.0433	-0.0534	-0.6765	0.3019
	(0.1454)	(0.1726)	(0.8035)	(0.1984)
Treatment $(t+0)$	-0.7456***	-0.1125	-0.4943	-0.1235
	(0.2819)	(0.2772)	(1.076)	(0.3239)
Treatment $(t+1)$	-0.7305***	0.0770	0.3289	0.3850
	(0.2436)	(0.3436)	(0.5613)	(0.3399)
Treatment $(t+2)$		0.1695		
		(0.4326)		
Treatment $(t+3)$		0.3202		
		(0.4670)		
R^2	0.95905	0.96043	0.98446	0.95851
Panel C: Total Turnout				
Treatment $(t-4)$	-0.0178		-1.192**	0.0019
	(0.1503)		(0.5541)	(0.2212)
Treatment $(t-3)$	-0.0134		-1.180**	0.0637
	(0.1498)		(0.5477)	(0.1985)
Treatment $(t-2)$	-0.1091	-0.0628	-0.6677	0.0522
	(0.1231)	(0.1705)	(0.7649)	(0.2012)
Treatment $(t+0)$	0.3765***	0.0900	-0.7210	0.2414
	(0.1436)	(0.2475)	(0.6451)	(0.2602)
Treatment $(t+1)$	-0.0705	-0.0202	-0.0324	0.2391
	(0.1386)	(0.2693)	(0.6834)	(0.2627)
Treatment $(t+2)$		-0.0644		
		(0.3294)		
Treatment $(t+3)$		-0.1061		
		(0.3526)		
R^2	0.95402	0.95876	0.97910	0.93121
Observations	8,742	5,466	468	3,540
Specification	Baseline	Time	Geographical	Treatment
Municipality FE	✓	✓	✓	✓
Election-County FE	✓		✓	✓
Election-County FE		✓		

Notes: The table presents event study results based on Equation 2 for postal, in-person, and total turnout (0-100). Columns (1) are the main results, shown in Figure 3. Columns (2) are time placebo results. The reference election $(t-1)$ is the 2013 Federal Election. The placebo treatment is defined as a runoff in the 2014 Local Elections. The sample excludes municipalities that had a county commissioner runoff in 2014 or 2020, as well as any treated municipalities in 2020.Column (3) is a geographical placebo test for the 2020/2021 Local Elections in Hesse. The reference election $(t-1)$ is the 2018 State Election in Hesse. Column (4) shows the results of a treatment placebo. Only counties in Bavaria that had a county commissioner runoff in 2020 are considered. The reference election $(t-1)$ is the 2018 State Election. In all specifications, it is controlled for the population as in Figure 3. Standard errors are clustered at the municipality level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table E14: Main Results with Randomized Inference

	Postal Turnout (1)	In-person Turnout (2)	Total Turnout (3)
Treatment $(t-4)$	0.0218	-0.0396	-0.0178
	[0.913]	[0.859]	[0.911]
Treatment $(t-3)$	-0.1075	0.0941	-0.0134
	[0.654]	[0.666]	[0.948]
Treatment $(t-2)$	-0.0658	-0.0433	-0.1091
	[0.677]	[0.774]	[0.413]
Treatment $(t+0)$	1.122***	-0.7456***	0.3765**
	[<0.001]	[0.004]	[0.040]
Treatment $(t+1)$	0.6601**	-0.7305***	-0.0705
	[0.011]	[0.006]	[0.688]
Observations	8,742	8,742	8,742
Municipality FE	✓	✓	✓
Election-County FE	✓	✓	✓

Notes: The table presents event study results based on Equation 2 for postal, in-person, and total turnout (0-100). The reference election $(t-1)$ is the 2018 State Election and it is controlled for the population as in Figure 3. In the randomized samples, the number of treated municipalities within a county is held constant. p-values, computed using randomized inference with 1,000 permutations following Heß (2017), are reported in brackets and denoted as *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table E15: Additional Controls

Panel A: Postal Turnout	(1)	(2)
Treatment $(t-4)$	-0.0185	0.0160
	(0.1893)	(0.1906)
Treatment $(t-3)$	-0.1477	-0.1089
	(0.1881)	(0.1892)
Treatment $(t-2)$	-0.0440	-0.0969
	(0.1285)	(0.1276)
Treatment $(t+0)$	1.168***	1.208***
	(0.2743)	(0.2648)
Treatment $(t+1)$	0.6952***	0.7338***
	(0.2470)	(0.2420)
R^2	0.97077	0.97217
Panel B: In-person Turnout		
Treatment $(t-4)$	-0.0260	-0.0332
	(0.2063)	(0.2028)
Treatment $(t-3)$	0.1077	0.0908
	(0.2025)	(0.2014)
Treatment $(t-2)$	-0.0476	-0.0727
	(0.1475)	(0.1444)
Treatment $(t+0)$	-0.7731***	-0.9012***
	(0.2820)	(0.2692)
Treatment $(t+1)$	-0.7213***	-0.8252***
	(0.2424)	(0.2343)
R^2	0.95949	0.96242
Panel C: Total Turnout		
Treatment $(t-4)$	-0.0445	-0.0173
	(0.1516)	(0.1504)
Treatment $(t-3)$	-0.0400	-0.0182
	(0.1479)	(0.1489)
Treatment $(t-2)$	-0.0916	-0.1696
	(0.1241)	(0.1198)
Treatment $(t+0)$	0.3954***	0.3073**
	(0.1445)	(0.1414)
Treatment $(t+1)$	-0.0261	-0.0914
	(0.1369)	(0.1411)
R^2	0.95566	0.95623
Controls	Invariant	Varying
Observations	8,742	8,742
Municipality FE	✓	✓
Election-County FE	✓	✓

Notes: The table presents event study results based on Equation 2 for postal, in-person, and total turnout (0-100) using reduced samples. Columns (1) uses the following additional controls, held constant to 2018: the share of unemployed, the share of females, the share of households with children, and the share of 16 age groups (under 3, 3–5, 6–9, 10–14, 15–17, 18–19, 20–24, 25–29, 30–34, 35–39, 40–44, 45–49, 50–54, 55–59, 60–64, 65–74 years old, with over 74 years old as the reference group). Column (2) uses the same additional covariates but the share of households with children, allowing them to vary over time. The reference election $(t-1)$ is the 2018 State Election and it is controlled for the population as in Figure 3. Standard errors are clustered at the municipality level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table E16: Population Controls

Panel A: Postal Turnout	(1)	(2)	(3)	(4)
Treatment $(t-4)$	0.0218	0.0014	0.0179	0.0835
	(0.1885)	(0.1878)	(0.1885)	(0.1912)
Treatment $(t-3)$	-0.1075	-0.1278	-0.1133	-0.0197
	(0.1898)	(0.1892)	(0.1900)	(0.1936)
Treatment $(t-2)$	-0.0658	-0.0735	-0.0698	-0.0014
	(0.1286)	(0.1289)	(0.1286)	(0.1292)
Treatment $(t+0)$	1.122***	1.153***	1.118***	1.063***
	(0.2741)	(0.2740)	(0.2743)	(0.2718)
Treatment $(t+1)$	0.6601***	0.7025***	0.6553***	0.5202**
	(0.2512)	(0.2503)	(0.2517)	(0.2524)
R^2	0.96980	0.97000	0.96980	0.96939
Panel B: In-person Turnout				
Treatment $(t-4)$	-0.0396	-0.0266	-0.0099	-0.1046
	(0.2040)	(0.2039)	(0.2034)	(0.2056)
Treatment $(t-3)$	0.0941	0.0991	0.1251	0.0761
	(0.2022)	(0.2023)	(0.2017)	(0.2019)
Treatment $(t-2)$	-0.0433	-0.0548	-0.0166	-0.0060
	(0.1454)	(0.1455)	(0.1455)	(0.1439)
Treatment $(t+0)$	-0.7456***	-0.7770***	-0.7128**	-0.6613**
	(0.2819)	(0.2816)	(0.2821)	(0.2818)
Treatment $(t+1)$	-0.7305***	-0.7584***	-0.6945***	-0.6301***
	(0.2436)	(0.2430)	(0.2442)	(0.2440)
R^2	0.95905	0.95920	0.95900	0.95879
Panel C: Total Turnout				
Treatment $(t-4)$	-0.0178	-0.0252	0.0080	-0.0212
	(0.1503)	(0.1504)	(0.1508)	(0.1488)
Treatment $(t-3)$	-0.0134	-0.0287	0.0118	0.0565
	(0.1498)	(0.1498)	(0.1503)	(0.1498)
Treatment $(t-2)$	-0.1091	-0.1283	-0.0864	-0.0074
	(0.1231)	(0.1231)	(0.1237)	(0.1264)
Treatment $(t+0)$	0.3765***	0.3760***	0.4055***	0.4018***
	(0.1436)	(0.1439)	(0.1437)	(0.1444)
Treatment $(t+1)$	-0.0705	-0.0559	-0.0392	-0.1099
	(0.1386)	(0.1387)	(0.1387)	(0.1398)
R^2	0.95402	0.95413	0.95401	0.95369
Specification	Baseline	Popul. (inv.)	Elig. voters (var.)	Size categ. (inv.)
Observations	8,742	8,742	8,742	8,742
Municipality FE	✓	✓	✓	✓
Election-County FE	✓	✓	✓	✓

Notes: The table presents event study results based on Equation 2 for postal, in-person, and total turnout (0-100) using reduced samples. Columns (1) are the main results shown in Figure 3s. Column (2) uses the total population residing in a municipality, held constant to 2018. Column (3) uses the number of eligible voters, allowing for variation over time. Column (4) uses size categories, splitting the sample into five equal groups based on the 2018 population measured as the number of eligible voters (thresholds: 1098, 1634, 2643, and 4496 eligible voters). The reference election $(t-1)$ is the 2018 State Election and it is controlled for the population as in Figure 3. Standard errors are clustered at the municipality level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table E17: Robustness Matching

Panel A: Postal Turnout	(1)	(2)	(3)	(4)
Treatment $(t-4)$	-0.1472	-0.1178	-0.1250	0.0311
	(0.1907)	(0.2196)	(0.2624)	(0.1724)
Treatment $(t-3)$	-0.2958	-0.1854	-0.2208	-0.0821
	(0.1924)	(0.2231)	(0.2683)	(0.1728)
Treatment $(t-2)$	-0.1706	-0.0584	-0.0148	-0.0496
	(0.1296)	(0.1516)	(0.1928)	(0.1202)
Treatment $(t+0)$	1.034***	1.251***	1.620***	0.9789***
	(0.2815)	(0.3193)	(0.3633)	(0.2592)
Treatment $(t+1)$	0.6034**	0.8596***	1.055***	0.5335**
	(0.2540)	(0.2965)	(0.3654)	(0.2356)
R^2	0.97257	0.97829	0.97812	0.97615
Panel B: In-person Turnout				
Treatment $(t-4)$	0.0831	-0.0232	-0.0683	-0.0208
	(0.2102)	(0.2364)	(0.2759)	(0.1849)
Treatment $(t-3)$	0.2328	-0.0347	-0.0367	0.1136
	(0.2053)	(0.2402)	(0.2764)	(0.1840)
Treatment $(t-2)$	0.0418	-0.0960	-0.2127	-0.0105
	(0.1449)	(0.1692)	(0.2168)	(0.1370)
Treatment $(t+0)$	-0.6408**	-0.8051**	-1.231***	-0.6167**
	(0.2863)	(0.3246)	(0.3646)	(0.2690)
Treatment $(t+1)$	-0.6205**	-0.7117**	-1.036***	-0.5613**
	(0.2501)	(0.2831)	(0.3323)	(0.2344)
R^2	0.96255	0.97088	0.97154	0.96667
Panel C: Total Turnout				
Treatment $(t-4)$	-0.0641	-0.1410	-0.1933	0.0102
	(0.1500)	(0.1813)	(0.2296)	(0.1377)
Treatment $(t-3)$	-0.0630	-0.2201	-0.2575	0.0316
	(0.1449)	(0.1769)	(0.2166)	(0.1412)
Treatment $(t-2)$	-0.1289	-0.1544	-0.2275	-0.0602
	(0.1185)	(0.1531)	(0.1914)	(0.1211)
Treatment $(t+0)$	0.3934***	0.4457***	0.3887*	0.3622***
	(0.1394)	(0.1663)	(0.1986)	(0.1327)
Treatment $(t+1)$	-0.0171	0.1479	0.0191	-0.0278
	(0.1409)	(0.1832)	(0.2277)	(0.1356)
R^2	0.96287	0.97713	0.97755	0.97379
Procedure	Local	Propensity	Mahalanobis	Entropy Bal.
Standard-Errors	Municipality	Municipality	Municipality & Control Group-Election	Municipality
Observations	6,426	2,460	2,184	8,742
Municipality FE	✓	✓	✓	✓
Election-County FE	✓	✓	✓	✓

Notes: The table presents event study results based on Equation 2 for postal, in-person, and total turnout (0-100) using different matching approaches. Column (1) considers only municipalities in the control group close to treated municipalities within the same county (see Figure C4). Columns (2) uses nearest neighbor matching based on propensity score, and Column (3) based on Mahalanobis distance. Column (4) uses weights from Hainmueller (2012). The reference election $(t-1)$ is the 2018 State Election and it is controlled for the population as in Figure 3. In all specifications, standard errors are clustered at the municipality level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table E18: Number of Runoffs

Panel A: Postal Turnout	(1)	(2)	(3)
Treatment $(t-4)$	-0.1965	-0.1649	-0.1719
	(0.3087)	(0.2535)	(0.2399)
Treatment $(t-3)$	-0.2269	-0.1662	-0.2069
	(0.3142)	(0.2535)	(0.2406)
Treatment $(t-2)$	-0.1015	0.0331	-0.0089
	(0.2058)	(0.1675)	(0.1606)
Treatment $(t+0)$	1.014**	1.153***	1.226***
	(0.4512)	(0.3628)	(0.3485)
Treatment $(t+1)$	1.074***	0.8079**	0.8718***
	(0.3909)	(0.3268)	(0.3171)
R^2	0.97599	0.97564	0.97555
Panel B: In-person Turnout			
Treatment $(t-4)$	0.1348	0.1803	0.1975
	(0.3489)	(0.2745)	(0.2598)
Treatment $(t-3)$	0.2368	0.3409	0.3564
	(0.3466)	(0.2712)	(0.2576)
Treatment $(t-2)$	0.0581	-0.0134	0.0181
	(0.2488)	(0.2013)	(0.1907)
Treatment $(t+0)$	-0.5802	-0.6369*	-0.6762*
	(0.4470)	(0.3653)	(0.3532)
Treatment $(t+1)$	-0.8794**	-0.7429**	-0.7502**
	(0.3862)	(0.3206)	(0.3100)
Panel C: Total Turnout			
Treatment $(t-4)$	-0.0617	0.0154	0.0256
	(0.2522)	(0.2044)	(0.1914)
Treatment $(t-3)$	0.0099	0.1748	0.1495
	(0.2596)	(0.2037)	(0.1937)
Treatment $(t-2)$	-0.0434	0.0197	0.0093
	(0.2124)	(0.1574)	(0.1489)
Treatment $(t+0)$	0.4341*	0.5161***	0.5500***
	(0.2323)	(0.1888)	(0.1771)
Treatment $(t+1)$	0.1943	0.0650	0.1216
	(0.2149)	(0.1831)	(0.1749)
R^2	0.96890	0.97164	0.97216
Sample	1 Runoff	1-2 Runoffs	1-3 Runoffs
Observations	2,118	2,718	2,838
Municipality FE	✓	✓	✓
Election-County FE	✓	✓	✓

Notes: The table presents event study results based on Equation 2 for postal, in-person, and total turnout (0-100) using samples restricted by the minimum and maximal number of runoffs in the last three mayoral elections. Column (1) considers only municipalities with exactly one runoff. Columns (2) and (3) consider municipalities with at least one and at most two and three runoffs, respectively. The reference election $(t-1)$ is the 2018 State Election and it is controlled for the population as in Figure 3. Standard errors are clustered at the municipality level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table E19: Off-Cycle Control Group

Panel A: Postal Turnout	(1)	(2)
Treatment $(t-4)$	-0.5693*	-0.6547
	(0.3314)	(0.7890)
Treatment $(t-3)$	-0.7018**	-0.7604
	(0.3285)	(0.8208)
Treatment $(t-2)$	-0.2438	-0.0268
	(0.2491)	(0.7133)
Treatment $(t+0)$	1.404***	1.924
	(0.4481)	(1.503)
Treatment $(t+1)$	1.057**	2.302*
	(0.4502)	(1.250)
R^2	0.98064	0.98121
Panel B: In-person Turnout		
Treatment $(t-4)$	0.6356*	0.3198
	(0.3584)	(0.8571)
Treatment $(t-3)$	0.6686*	0.4668
	(0.3521)	(0.9987)
Treatment $(t-2)$	-0.0210	0.0556
	(0.3014)	(0.8250)
Treatment $(t+0)$	-0.9990**	-1.430
	(0.4671)	(1.576)
Treatment $(t+1)$	-1.299***	-2.716*
	(0.4430)	(1.431)
R^2	0.97202	0.97350
Panel C: Total Turnout		
Treatment $(t-4)$	0.0663	-0.3349
	(0.2767)	(0.5240)
Treatment $(t-3)$	-0.0333	-0.2936
	(0.3168)	(0.7732)
Treatment $(t-2)$	-0.2648	0.0288
	(0.2879)	(0.7230)
Treatment $(t+0)$	0.4050	0.4943
	(0.2831)	(0.8567)
Treatment $(t+1)$	-0.2416	-0.4141
	(0.3059)	(0.6625)
R^2	0.97930	0.98342
Off-Cycle	All	Runoff
Observations	1,734	1,308
Municipality FE	✓	✓
Election-County FE	✓	✓

Notes: The table presents event study results based on Equation 2 for postal, in-person, and total turnout (0-100) using municipalities as the control group where mayoral elections were held off-cycle (and no runoff in the 2020 county commissioner elections). Column (1) considers all municipalities as the control group with an off-cycle election between the on-cycle elections in 2014 and 2020. Column (2) further restricts the control group to those that had a runoff between the on-cycle elections in 2014 and 2020. The reference election $(t-1)$ is the 2018 State Election. The sample is restricted to counties with at least one observation in the control and treatment group. All specifications include the 2018 population as time-invariant covariate. Standard errors are clustered at the municipality level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.