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

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Should I Mail or Should I Go:

Voting Behavior After a One-Time All-Postal Election*

Marius Kröper† Valentin Lindlacher‡

Abstract

We investigate how reducing information costs through forced experimentation with postal voting, while holding administrative rules fixed, affects subsequent voting behavior. Leveraging a natural experiment during Bavaria's 2020 Mayoral Elections and drawing on municipality-level administrative data spanning seven federal and state elections (2013—2025), we employ an event study design. We find a transitory increase in total turnout of 0.4 percentage points in the first election after the treatment, one and a half years later, and a persistent substitution from in-person to postal voting even five years after the treatment. Municipalities with a higher turnout in the past show larger effects. Investigating the distribution of information costs shows an age gradient, with the highest information costs in the oldest municipalities. The conservative governing party gains from higher postal turnout and other right-wing parties' in-person voters.

Keywords: Postal voting, Voter turnout, Local elections, Information costs, COVID-19

JEL-Codes: D72, 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, 1991). While Porter (1991) 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 three high-salience general elections until the 2025 Federal Election. 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 5 years after the treatment (in $t+1$ and $t+2$, the 2023 State and 2025 Federal Elections, respectively). 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 (*Christlich Soziale Union*), 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 other right-wing parties exhibit mirrored changes in their vote shares relative to the CSU. However, increases in the CSU's total vote share last only for two elections, might not be separable from the pandemic, and potentially stem from compositional effects as well.

Importantly, the administrative voting rules and procedures remained unchanged before or after the treatment across the seven general elections, four federal and three state elections, which we analyze between 2013 and 2025. Postal voting has long been readily available and widely used, possibly reducing the treatment effect compared to a setting with no postal voting option.[2] This is a specific feature differing from the literature on the introduction of postal

[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, were additionally 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] 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,

voting (e.g., Funk, 2010; Hodler et al., 2015), and implies that many eligible voters did not face information costs of postal voting even prior to the treatment.[3] Taken together with the one-time nature of the policy shift, these factors make Bavaria a uniquely well-suited setting to isolate voting effects of a reduction in information costs of 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 affects voter participation.[4] 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. Importantly, our analysis focuses on general elections after the all-postal election. Although the first post-treatment election took place during the pandemic, pandemic-related health concerns and messaging should affect treated and control voters similarly, so the observed post-runoff differences align with a reduction in information costs rather than pandemic-specific influences.

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

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 in-person voting. All over Germany, postal voting is a common practice and easily accessible (by simple request) for all eligible voters (similar to the UK, Canada, and parts of the US).

[3] Information costs arise due to unfamiliarity with postal voting. They are removed after having voted by mail at least once.

[4] Cantoni and Pons (2021) find no overall effect of stricter ID laws on voter turnout, though notable heterogeneities for socio-economic groups. The US Supreme Court's decision in *Shelby County v. Holder* (2013), which ended federal VRA preclearance, reduced Black turnout relative to white turnout (Billings et al., 2024). Pettigrew (2021) 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 and early voting (Alipour and Lindlacher, 2025; Clinton et al., 2021; Tomkins et al., 2023). For an extensive overview, see Cantoni et al. (2025). Beyond administrative policies, targeted outreach increases voter participation across different settings (Marx et al., 2021; Pons and Liegey, 2019; Gerber et al., 2003), and individual characteristics such as education and overconfidence also affect participation (Milligan et al., 2004; Ortoleva and Snowberg, 2015).

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 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. 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 and 2025). 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.[5]

During the all-postal runoff, some voters experience postal voting for the first time, reducing information costs of postal voting for them in future elections. In subsequent elections, the administrative costs of postal voting return to their initial levels, while administrative costs of in-person voting remain constant across all 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 from those of regular elections. They had a reduction in their information costs and in their perceived costs of requesting and casting the postal ballot. 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. While the information costs remain eliminated and the costs of casting the postal ballot remain unchanged, additional costs are now introduced for the request of the postal ballot. 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. Subsequently, such as in the 2025 Federal Election, voting costs remain constant, and we, therefore, do not expect changes in turnout.

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 ($t + 0$), one and a half years after the treatment.[6] This increase results from a stronger increase in postal turnout in comparison

[5] 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 March 2020 but in earlier years.

[6] This is comparable to the turnout increase of two additional early voting days or one additional newspaper in the US (Kaplan and Yuan, 2020; Gentzkow et al., 2011), or of a 5 percent increase in opening hours of polling

to the reduction in in-person turnout (1.1 versus −0.7 percentage points). In the 2023 State Election ($t+1$), three and a half years after the treatment, a 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. In the 2025 State Election ($t+2$), five years after the treatment, estimates are very similar to the previous election ($t+1$). A postal turnout increase of 0.9 is mirrored by an in-person turnout decline of 0.8, leading to no alterations in total turnout. 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. 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 therefore return by postal voting only in the next election, we rationalize our finding by a second adjustment 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. Hence, 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 adds 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.[7] We reject the habit formation hypothesis, as shifted voting costs lead to statistically significant deviations in the first election after the treatment, contrary to the prediction that voters would adhere to prior habits. We also find effects in the

places in Austria (Potrafke and Roesel, 2020). The effect sizes are similar to those reported in Alipour and Lindlacher (2025), albeit 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.

[7] Fujiwara et al. (2016) finds a lasting turnout reduction after a shock affecting only voting costs once. 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).

subsequent election, when postal voting costs are (re-)adjusted, but not in the third election, when costs remain constant.[8] This aligns with our theoretical considerations on adjusted voting costs but contrasts with the hypothesis that voting is habit forming.

We investigate which parties profit from the higher turnout.[9] For left-wing parties, results are similar to the turnout effects, though the coefficients are smaller in magnitude: There is only a temporary increase in the parties' vote share of 0.2 percentage points, which lacks statistical significance, and persistently substitution in the mode of voting by less than half a percentage point. The largest conservative party (CSU) gains up to 0.60 percentage points in its total vote share in $t + 1$. While there is no loss in the in-person vote share, the increase in the postal vote share is about the size of other right-wing parties' losses in their in-person vote share. We rationalize the total vote share increase with changed party preferences. Right-wing voters give credit to the CSU for how well they managed the COVID-19 pandemic and specifically that they were able to retain the local elections. However, this might be strongly associated with the pandemic and no generalizable implication of lowered information costs. As the salience of COVID-19 diminishes over time, these electoral gains appear to dissipate accordingly. In $t+2$, the gains for the CSU attenuate, with the estimated effect approaching zero. Nevertheless, there is a persistent increase in their postal vote share. Additionally, heterogeneous effects might change the composition of the electorate, potentially favoring the CSU as well.

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

[8] Having similar coefficients between the 2023 State and 2025 Federal Elections and a decline in total turnout between the 2021 and 2025 Federal Elections, the results cannot be explained by the slightly less salient state election.

[9] 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.

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 seven elections for the German Federal Parliament *(Bundestag)* and the Bavarian State Parliament (*Landtag*), which were held between 2013 and 2025. All elections in our analysis follow the principles of proportional representation but differ in electoral rules.[10] 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. 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.[11] 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. Postal ballots can be submitted to any mailbox without postage.[12] Postal voting must be requested anew for each election: there is no permanent postal voting register in Bavaria, and postal ballot papers are not sent automatically, regardless of their previous voting mode. The 2020 all-postal runoff, in which ballot papers were sent automatically to all eligible voters, was therefore a clear one-time deviation from the standard procedure.

While we investigate electoral consequences in general elections, the treatment originates from the mayoral elections held in March 2020, where mayors must secure an absolute majority.

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

[11] 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.

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

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.[13] 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.[14] 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 were held two weeks after the first round on March 29 in accordance with standard procedure and following the adjustment of electoral law 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.

The 2021 Federal Election, held on September 26, still took place under Germany's special legal status ("epidemic situation of national scope"), which expired on November 25, two months later. In Bavaria, the 7-day incidence was rising and relatively high at 87.0 per 100,000; however, with 61.1 percent of residents fully vaccinated, infections were concentrated among the unvaccinated and the occupancy of intensive care beds was not critical.[15] Nationwide COVID-19 measures lapsed on April 7, 2023, about six months before the 2023 State Election. Figure 1 in Subsection 2.2 shows a spike in the postal share in 2021 and elevated levels in 2023 relative to 2025.[16]

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.[17] The data contains the number of eligible voters and (party) votes and provides this information separately by polling places and postal precincts.[18] We aggregate the data to the municipality level for two reasons: First, polling places are not

[13] 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.

[14] A nationwide lockdown, including a stay-at-home order and a ban on gatherings of more than two people, was implemented on March 22 and remained in effect until May 6, 2020.

[15] These figures and resulting policy measures were reported and implemented at the county level.

[16] Such county- and election-specific shocks are absorbed by election×county fixed effects and should not induce differences between treatment and control municipalities.

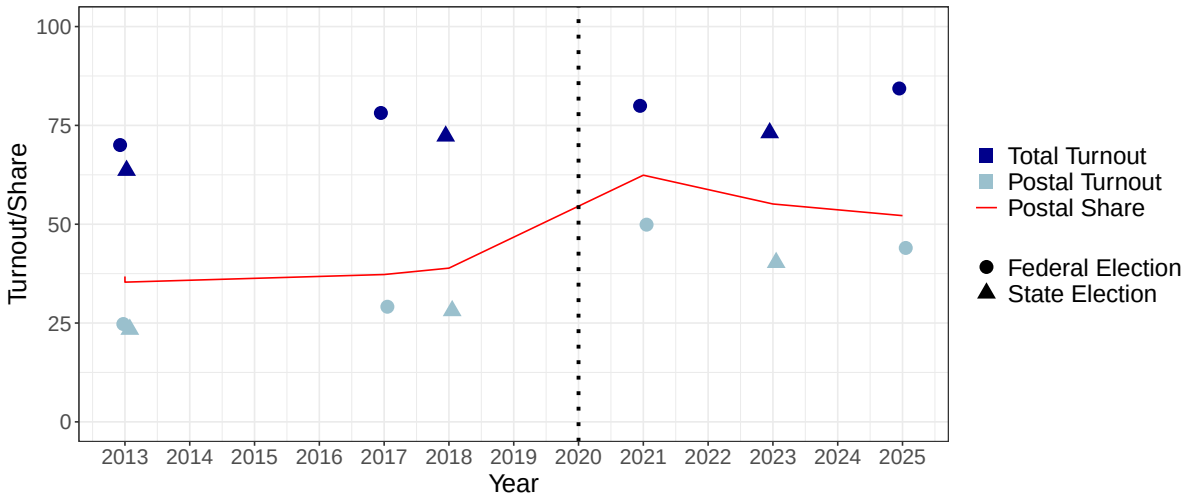
[17] https://www.bundeswahlleiterin.de/ and https://www.statistik.bayern.de/wahlen/landeswahlleitung/, both accessed on 15.05.2024.

[18] 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.

necessarily nested within postal precincts. Thus, municipalities are the smallest level at which we can analyze total turnout and party vote shares. Second, precinct boundaries are regularly adjusted between elections, whereas municipality boundaries change less frequently, providing a time-constant unit of observation.[19] 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.[20] The dotted vertical line depicts the 2020 Mayoral Elections, which are decisive for the treatment.[21] In federal elections, the total turnout is slightly higher than in state elections, and total turnout increased between all elections of the same type. The postal voting share increased from 35.3 percent in 2013 to 52.2 percent in 2025, with a spike of 62.4 percent in 2021 during COVID-19.[22]

Figure 1: Turnout by Election Type in Bavaria since 2013



Notes: The figure presents postal and total turnout (0-100) for the four federal and three 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.

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.[23] We use this information to define the treated municipalities—those

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

[20] Federal elections are held every four years, while state elections are held every five years. In 2013, both a federal election and a state election were conducted, occurring one week apart on September 15 and September 22, respectively. The 2025 Federal Election was advanced to February 23 as a snap election, following the early collapse of the ruling coalition.

[21] 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.

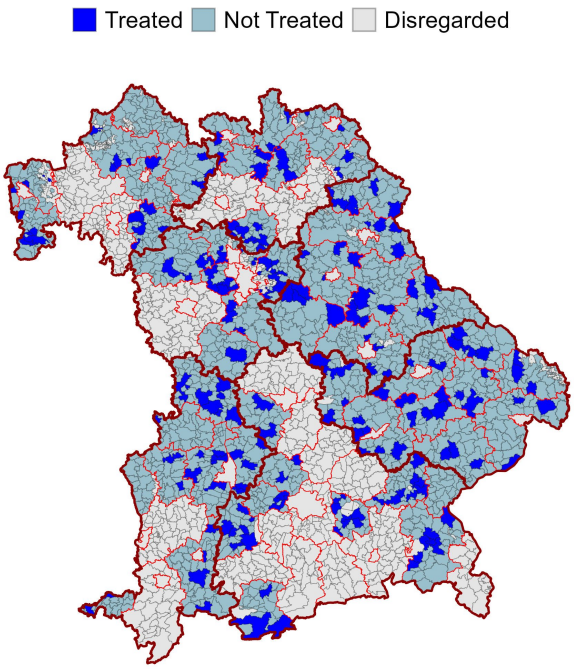
[22] With increasing COVID-19 infection rates and rather low vaccination rates, during the 2021 Federal Election and, to a lesser extent, the 2023 State Election, authorities and major media highlighted postal voting as a convenient and low-contact option.

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

with a mayoral runoff on March 29, 2020—as well as to construct the estimation sample. The estimation sample is restricted to municipalities located in counties where no county commissioner runoff was held 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.[24] Moreover, independent cities, being identical to the county, are removed from the estimation sample as they contain no within-county variation.[25]

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 were 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.

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).

Runoffs are relatively rare 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

[24] We use them later in a placebo exercise.
[25] We estimate using election×county fixed effects. Hence, only variation within counties is used.

more than one runoff. For the treatment group, one runoff is the modal case; indeed, for half of the municipalities (103 out of 205), the 2020 Runoff was their first in any of the at least previous 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 with a high frequency of runoffs.

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 perceived costs of postal voting.[26] While in-person voting costs remain constant across all elections, postal voting costs change in a stepwise manner: initially, information costs are eliminated due to the forced experimentation with postal voting; only in the following election do treated individuals encounter the actual costs associated with requesting a postal ballot. 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

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

where $P_{i\tau}$ represents the individual's i net payoff from voting in period τ. 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 $C_{i\tau}^p$, respectively. If $P_{i\tau} < 0$ individual i abstains. Contrarily, if $P_{i\tau} \geq 0$ and $C_i^b \leq C_{i\tau}^p$, she turns out at the polling place, otherwise she votes by mail. For in-person voting, the costs C_i^b include transportation to the polling place or congestion at the polling place. The costs of postal voting ($C_{i\tau}^p$) can be separated into transportation costs to the postal box ($\bar{C}_i$), costs of requesting the postal ballot ($C_i^{request}$), and information costs due to unfamiliarity with postal voting (C_i^{info}). While $\mathbb{1}_{i\tau}^{\tilde{p}}$ is equal to one if an individual voted by mail in a *regular* election, $\mathbb{1}_{i\tau}^{p}$ is equal to one if an individual ever voted by mail (including the all-postal runoff).

The forced experimentation with postal voting due to the all-postal runoff 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 of postal voting. This shift induces two effects: Some in-person voters switch to postal voting, and

[26] The model is formulated at the individual level, but our data are aggregated at the municipality level. We therefore interpret the predictions in terms of average behavior across individuals within municipalities.

some abstainers, who previously considered postal voting too costly, participate. Consequently, postal and total turnout increase, with a larger increase in 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 pre-treatment election $(t-1)$, leads to a second adjustment 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 $C_i^{request} > C_i^{info}$.

As voting costs remain constant after the second adjustment, we do not expect further turnout alterations. The largest effects should be observed among individuals with high initial information costs, such as elderly voters. 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:

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

where Y_{ict} is postal, in-person, or total turnout and party vote shares in municipality i in election t, where $t = e - 5$ (with election $e \in \{1, 2, ..., 7\}$ 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. It represents the variable of interest when interacted with the (relative) election indicators. In the main specification, we only control for the time-invariant log population, $\log(pop_i^{2018})$, measured as the number of eligible voters for the 2018 State Election, interacted with election-specific coefficients δ_t to flexibly absorb any size-related and time-invariant factors.[27] 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 (α_{ct}). Hence, we compare municipalities of similar sizes within the same county. The election×county fixed effects account for differences in election-specific voting behavior. These include distinctions between federal and state elections, the competitiveness of races (e.g., expected close contests; Bursztyn et al., 2024), broader trends

[27] 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.

in voting modes, and increasing political polarization (particularly the growing relevance of the AfD, *Alternative für Deutschland*).[28] They also absorb the post–COVID-19 rise in postal voting associated with health concerns, elite messaging, and media coverage encouraging mail voting as a safer alternative. Moreover, eliminating variation across counties accounts for differences in COVID-19 incidence and policy measures (reported and implemented at the county level), whether a county is directly contested for parliamentary seats, and election-day weather. In Appendix C, we provide further robustness checks using different approaches to match on observables and local matching. Finally, we estimate Equation 2 using OLS and 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.[29] However, we show that parallel pre-trends hold for the three types of turnout and for party vote shares. We address potential selection into runoffs by estimating cross-sectional OLS specifications in Appendix Table E2. Making it comparable to our main specification (Equation 2), we include eligible voters as population control and county fixed effects. Of fourteen municipality characteristics, only one, the share of residents aged 20–29, is statistically significant at conventional levels (p-value of 0.046).[30] Notably, including these covariates yields only a negligible change in the R^2. 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.[31]

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.[32] 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

[28] Founded in 2013, the party did not contest that year's Bavarian state election and narrowly missed the 5 percent threshold for entering the federal parliament by 0.3 percentage points. It has been represented in the federal parliament since 2017 and in the Bavarian state parliament since 2018.

[29] 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).

[30] Each column in Appendix Table E2 reports a separate regression. Apart from turnout (taken from the 2018 State Election), all municipality characteristics are measured as of 31 December 2019, i.e., close to the runoff in March 2020.

[31] We shrink the sample instead of estimating an RDD as we are primarily interested in the dynamic effects across all three subsequent elections.

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

a runoff in the 2020 County Commissioner Elections as a placebo sample. Thus, we can show that it is not Bavaria or the beginning of the COVID-19 pandemic but the all-postal election.

Equation 2 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 of 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. The estimated effect is positive and, with about about 8 percentage points, sizable (Appendix Table B1). Further details are provided in Appendix B.

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.[33]

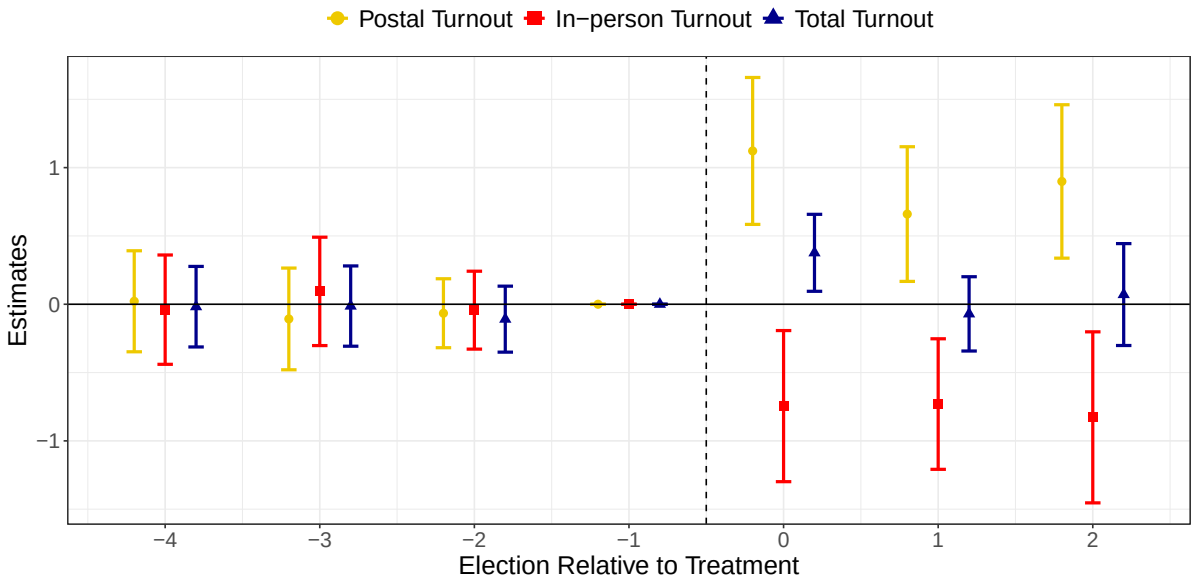
For all pre-treatment periods, estimates are very close to zero and lack statistical significance for all three outcomes. The absence of pre-trends lends credibility to the assumption of parallel trends.[34] In the first election after the treatment, the 2021 Federal Election, we observe a statistically 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 in 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. the postal turnout estimate shrinks by about half a percentage point to an estimate of 0.66. In the 2025 Federal Election, five years after the treatment and one and a half years after the 2023 State Election, the total turnout effect remains at zero (0.07). The point estimates for postal and in-person turnout basically remain (0.90 and -0.83).[35]

[33] 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 E4.

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

[35] We only observe the municipalities' aggregate effects. We cannot rule out ecological fallacy, i.e., that some eligible voters turned from in-person voting to abstaining, while others moved from postal voting to in-person voting.

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 E4.

The increase in total turnout of 0.38 percentage points in the first election after the treatment, the 2021 Federal Election, is comparable to the turnout increase of two additional early voting days in the US (Kaplan and Yuan, 2020), of one additional newspaper around the turn of the twentieth century in the US (Gentzkow et al., 2011), or of a 5 percent increase in opening hours of polling places in Austria (Potrafke and Roesel, 2020). It is also large enough to offset the negative effects of a polling place relocation in Munich, Germany (Alipour and Lindlacher, 2025). Thus, albeit transitory, the total turnout effect of the forced experimentation with postal voting is sizable.

For the postal turnout effect, direct comparisons are less readily available. However, studies examining substitution effects due to polling place changes report similar magnitudes. Alipour and Lindlacher (2025) find that increased in-person voting costs lead to an increase in postal turnout of about 0.61 percentage points, accompanied by a decrease in in-person turnout of 1.0 percentage points. Similarly for the US, Tomkins et al. (2023) report that polling place changes result in a 0.76 percentage points increase in postal and absentee voting, with a corresponding 1.3 percentage points decrease in in-person voting, and Clinton et al. (2021) find that a polling place change leads to a complete substitution towards early in-person voting of 0.7 percentage points in North Carolina. In contrast, studies on the introduction of all-postal voting in the US report larger increases in (postal) turnout, typically about 2 to 4 percentage points (Thompson et al., 2020; Barber and Holbein, 2020; Gerber et al., 2013). This is unsurprising, as all registered voters in those settings automatically receive a ballot, and these estimates, as

noted by Thompson et al. (2020), "might provide an upper bound on the effect" of postal voting policies.

One might find these effects surprising for several reasons. First, the forced experimentation took place during a lower-salience local election, which typically attracts lower turnout than general elections. As a result, fewer eligible voters may have been exposed to the treatment. Second, the policy change was a one-time intervention, suggesting that its effects may be smaller than those observed in settings with repeated or sustained exposure to all-postal voting. Third, the shift was prompted by the COVID-19 pandemic, meaning that even voters in the control group may have opted to vote by mail for the first time, potentially attenuating the treatment contrast. Finally, Bavaria already had relatively high rates of postal voting prior to the intervention, leaving a smaller pool of voters without prior postal voting experience. Taken together, these factors suggest that the observed effects are likely lower-bound estimates of the true potential impact of reducing barriers to postal voting.

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 correlated 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 or potential changes in the municipalities' population composition, 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 turnout is 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 former 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, as before, if they wished to vote by mail. Consequently,

the cost changes associated with directly mailing postal ballots in the runoffs might distort the perceived postal voting costs, and the behavior in period $t+1$ can be seen as ongoing experimentation.

In theory, one could attribute these effects to differences in the election type (federal versus state). However, at least three points speak against this interpretation. First, in the 2025 Federal Election, there is no effect on total turnout. Given the absence of ongoing experimentation, it is unsurprising that coefficients remain unchanged in comparison to the 2023 State Election. Second, while the coefficient on in-person turnout does not change between $t+0$ and $t+1$, the drop in total turnout stems from a reduction in the effect on postal turnout. There is no compelling explanation for why former abstainers would vote by mail in the first federal election but not in a state election. Third, election fixed effects account for average turnout differences across federal and state elections, thereby mitigating the risk of confounding by election type.

We can reject habit formation as a mechanism of the altered total turnout. Instead, consistent with rational choice models of electoral turnout, eligible voters adjust their voting costs twice. Moreover, we would observe a persistent increase in total turnout, also in the 2023 State and 2025 Federal Elections ($t+1$ and $t+2$), if voting were habit forming. Instead, this effect is only transitory. In contrast to Fujiwara et al. (2016), where a *one-time* cost shock has lasting effects, our setting involves a one-time policy shift that *permanently* alters the information costs of postal voting. Yet, once these costs stabilize, from $t+1$ to $t+2$, there are also no changes in the voting behavior. This supports the interpretation that it is the structure of voting costs, not habit, that drives the observed dynamics.

It should be noted, however, that we observe only aggregate effects at the municipal level. We cannot rule out that the altered turnout in the previous election ($t+0$) indirectly affects turnout in subsequent elections ($t+1$ and $t+2$). It is possible that some voters who vote at the polling place in $t+0$ notice lower-than-expected turnout at the polling place and subsequently choose to abstain from voting.[36] At the same time, some voters who vote by mail in $t+0$ might return to in-person voting due to the aforementioned adjustments in postal voting costs. We will investigate this further when analyzing heterogeneities by past turnout as a proxy for social pressure.

4.2 Party Outcomes

Next, we examine whether the all-postal runoff 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 ballots centrally.[37] This allows us to differentiate between shifts from in-person to postal vote shares across parties, providing a clearer understanding than an analysis of aggregated party-level shifts.

[36] This potential mechanism is similar to the one Funk (2010) finds.

[37] 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.

We first show estimation results for 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.[38] In the political spectrum, the CSU is classified as center-right (Jolly et al., 2022). First, 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 shares prior to the treatment. In the first election after the treatment ($t+0$), the CSU gains 0.30 percentage points (marginally statistically significant). In comparison to the main effects from Figure 3, this is about three quarters. This indicates that some but not all former abstainers voted for the CSU. Decomposing this effect, it stems almost exclusively from an increase in the postal vote share of 0.27 percentage points and from a basically unchanged in-person vote share. By contrast, our main results show postal turnout rising while in-person voting falls only modestly. In the subsequent election ($t+1$), the effect on the total vote share increases to 0.60 percentage points and is statistically significant at the 1 percent level. It increases due to both an increased postal and in-person vote share. This contrasts with the effects on turnout, where the total turnout effect disappears due to a decrease in the postal turnout effect. In the 2025 Federal Election ($t+2$), there is still a marginally significant increase in the postal vote share of 0.29 percentage points but no effect on the total vote share. This is similar to the effects on turnout. The coefficient is about a third of the effect size. Hence, effects on the party preferences are less lasting, but also CSU voters switch from in-person to postal voting.

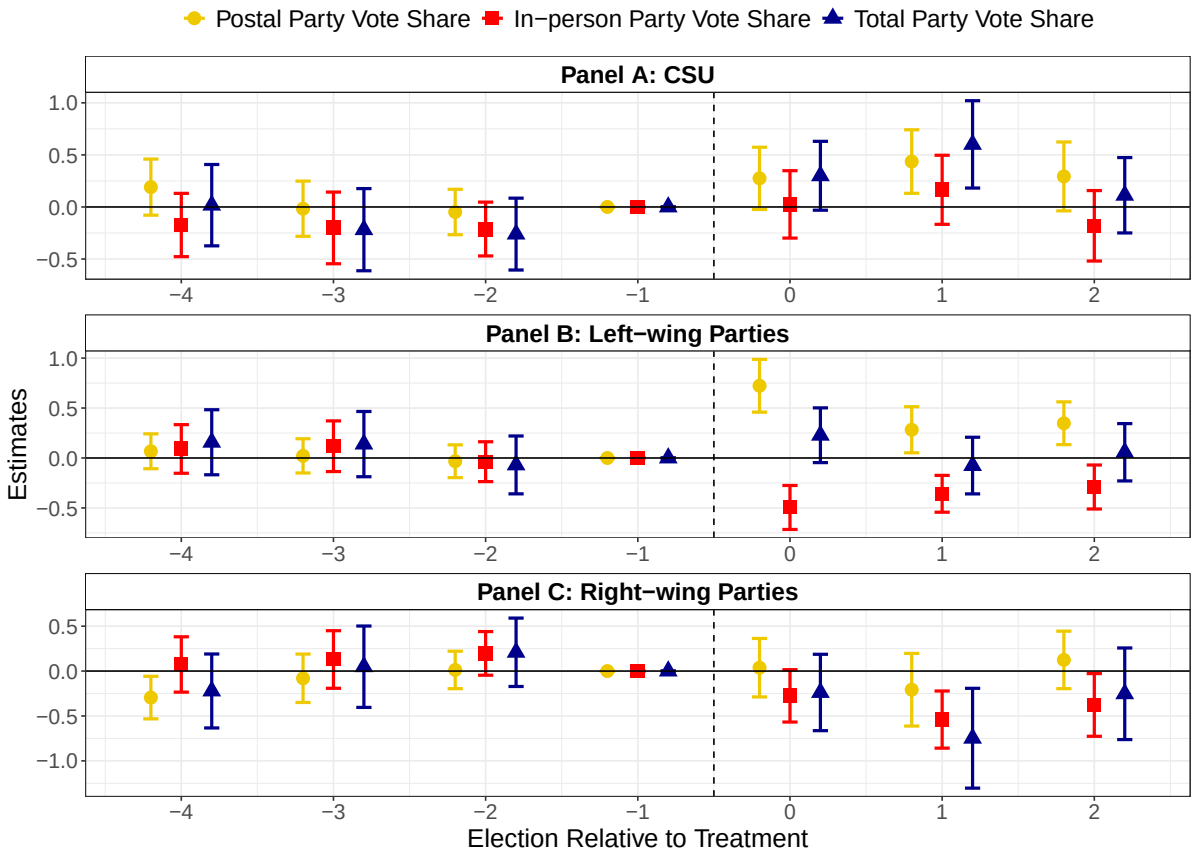
These changes closely mirror the changes in the other right-wing parties' vote shares (Panel C). In the first election after the treatment ($t+0$), there is basically no effect on postal voting. However, there is still a marginally statistically significant reduction in the in-person vote share of 0.28 percentage points, which also translates to a (statistically insignificant) drop in the total vote share of 0.24 percentage points. In the subsequent election ($t+1$), both their postal and in-person vote shares are decreased, such that their total vote share coefficient decreases to a loss of 0.75 percentage points. In the 2025 Federal Election ($t+2$), only a negative effect on the in-person vote share of 0.38 percentage points remains. This indicates that while some in-person CSU voters switched to postal voting, some other right-wing in-person voters kept voting at the polling place but switched to voting for the CSU.

The CSU was part of the German as well as the Bavarian Government in 2020 during the pandemic. Additionally, it held most mayoral and county commissioner offices. Thus, some right-wing voters might have switched to the CSU to reward its continuation of the electoral process during the pandemic, especially since other right-wing parties advocated different pandemic policies.[39] It is likely that these voters aligned more closely with the CSU's stricter COVID-19 policies, particularly as they experienced the tangible consequences of these policies through the all-postal election. Hence, the results suggest that the CSU benefits not only from

[38] 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 Alliance 90/The Greens (*Bündnis 90/Die Grünen*) with 17.6 percent in 2018, and the SPD (*Sozialdemokratische Partei Deutschlands*) with 20.6 percent in 2013, respectively.

[39] 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.

Figure 4: Election Results (Eligible Voters)



Notes: The figure presents event study results based on Equation 2 for postal, in-person, and total vote shares (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* (The Left), the *Sozialdemokratische Partei Deutschlands* (SPD), and *Bündnis 90/Die Grünen* (Alliance 90/The Alliance 90/The 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.

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. Moreover, they do not last until the third post-treatment election. Finally, we cannot rule out that the true mechanism behind this finding is compositional effects instead of changed party preferences. In theory, it is possible that the treatment affects different eligible voters differently, such that new CSU voters turn out, while other former right-wing voters abstain. We investigate this alternative mechanism in the heterogeneity analysis (Subsection 4.3).

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 postal 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, which, however, lacks statistical significance. After adjusting postal voting costs for another time, the coefficient for the postal vote shares drops to 0.28 in

$t+1$ and to 0.35 in $t+2$, mirrored by a decrease in the in-person vote share of 0.36 in $t+1$ and 0.29 in $t+2$. Some left-wing voters return to abstaining, such that there are no lasting changes in the total vote share.

In terms of election results, i.e., party vote shares relative to cast votes, the CSU wins 0.42 percentage points in the 2021 Federal Election and 0.99 percentage points in the 2023 State Election (Appendix Figure D3). The left-wing parties show no alteration in their results. Hence, their gains are relative to the total turnout increases. Consequently, the other right-wing parties lose about the size of the CSU gains. As the Bavarian Parliament has 203 members, the difference between a counterfactual parliament without the treatment and a counterfactual parliament where all municipalities had an all-postal runoff is about two additional seats for the CSU.

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 better understand the distribution of the information costs.[40] 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, is time-invariant by construction, and takes values from 2018, the year of the last election before the treatment. The triple difference-in-differences model is specified as follows:

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

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. 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} 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

[40] Alvarez et al. (2008) document that confidence in different voting modes varies systematically with socio-economic characteristics. This motivates the assumption that information costs may likewise be heterogeneously distributed across these groups.

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.

Note that we lack exogenous variation to causally identify the sources of heterogeneity. As a result, we can only provide suggestive evidence on which groups drive the results and where information costs are particularly high. Nevertheless, Appendix Table E3 shows some correlation across covariates, which might confound the relationship and could partly explain the heterogeneity, rather than the covariate of interest, Z_i.

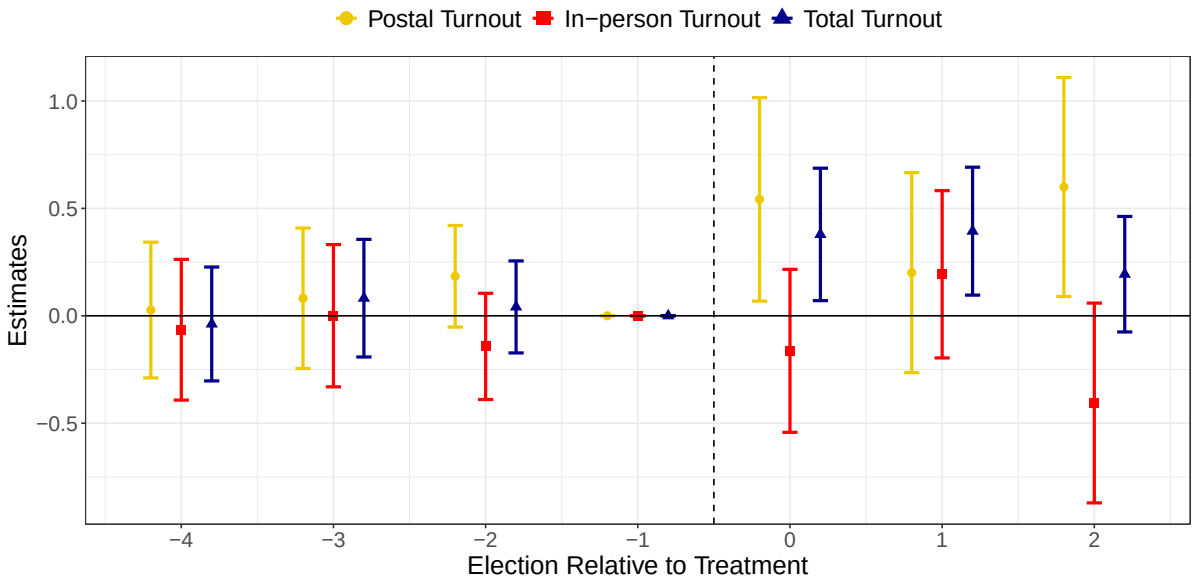
4.3.1 Social Pressure: Heterogeneity by Past Turnout and Municipality Size

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), while the deviation for in-person turnout is close to zero. The positive and statistically significant deviation for total turnout persists into the subsequent election ($t+1$). Even after five years, in the 2025 Federal Election, a positive deviation in total turnout is found, which, however, lacks statistical significance. Moreover, municipalities with higher turnout in the past show larger substitutions. There is a positive deviation for postal turnout and a (marginally statistically significant) negative deviation in in-person turnout. This is similar to the first federal election ($t + 0$). In the 2023 State Election ($t + 1$), the positive deviation in total turnout 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). 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. In municipalities with higher turnout in the past, trust in the election administration might be generally higher, or it is more likely that they will talk to someone who affirms the validity of postal 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 (μ^{τ}) 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. 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.

Regarding partisan effects, Appendix Figure D4 shows that the CSU gains more in municipalities with higher past turnout: 0.44 percentage points in $t+0$, increasing to 0.59 percentage points in $t+1$, and remaining positive at 0.36 percentage points in $t+2$. These increases are again mirrored by losses for other right-wing parties. Though they are smaller and lack statistical significance. And there are no large deviations for left-wing parties. Thus, both effects might be at play: The CSU might attract voters from other right-wing parties (party preference effect) as well as from former abstainers (compositional effect). Results are very similar when considering votes instead of eligible voters (Appendix Figure D5).

Municipality Size To examine heterogeneities 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 large (Appendix Figure D6). Nevertheless, we find slightly stronger effects in the bottom half of the sample's municipalities.[41] 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 in the middle

[41] Throughout this paragraph, we depict marginal effects for each category instead of coefficients of interaction terms. Appendix Figure D6 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 $t+1$ and $t+2$ (all coefficients are close to zero). But substitution from in-person to postal turnout is still larger in smaller municipalities in $t+1$ (1.03 and -0.99 versus 0.52 and -0.63) and $t+2$ (1.56 and -1.70 versus 0.66 and -0.51).

tercile. For the top tercile, we find a moderate increase of 0.39 percentage points, similar to the baseline estimation (Figure 3).[42] 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, similar coefficients to the baseline estimation. Estimates in $t+1$ and $t+2$ are similar but less precisely estimated. We find in all terciles a reduction from $t+0$ to $t+1$ in total turnout. Moving to quintiles, heterogeneities are similar (Appendix Figure D7).[43] 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.

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).

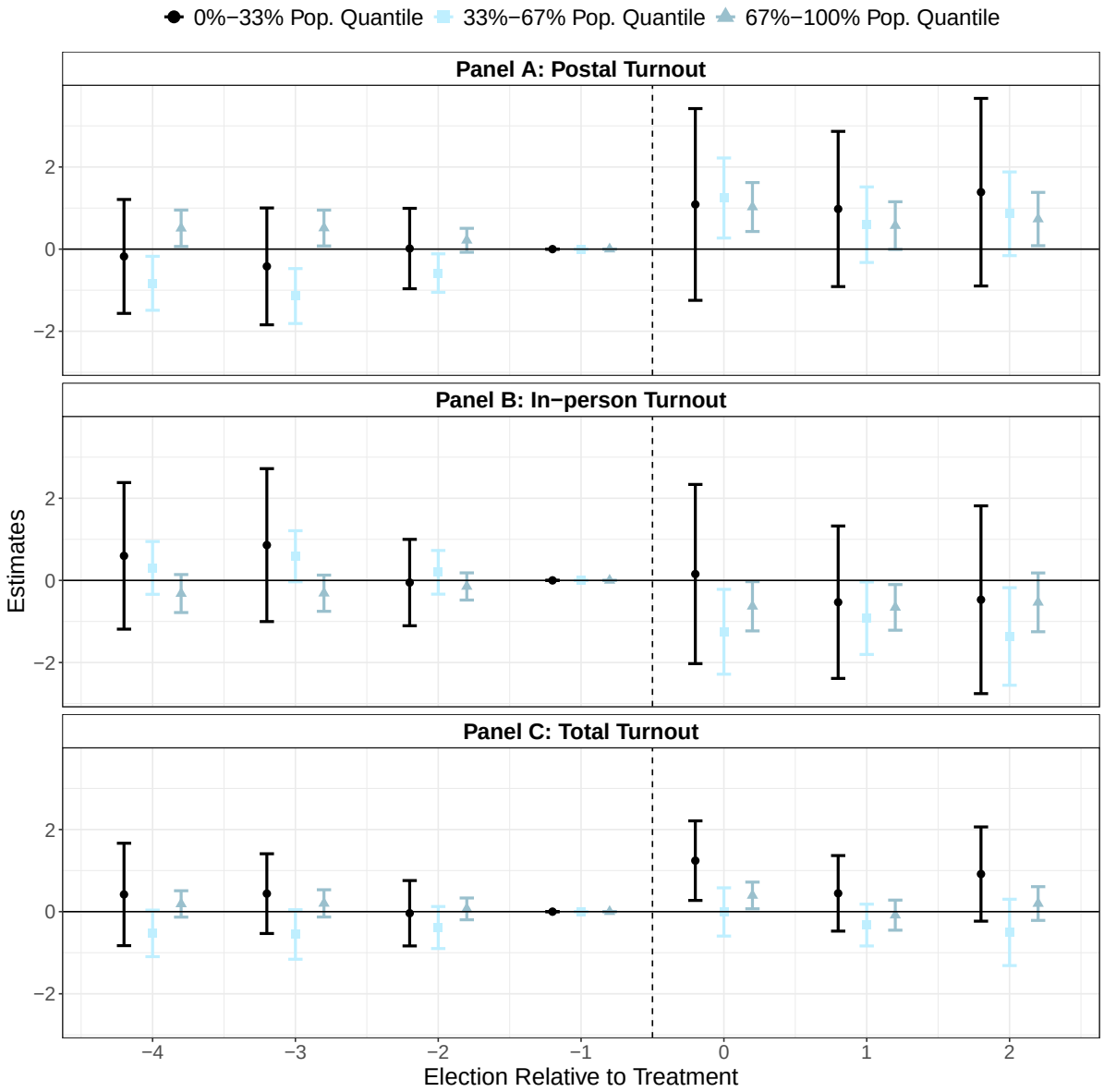
Age We show heterogeneity across age groups in Figure 7. Since all values for the age groups are from 2018, individuals aged 15–17 years old partly did not vote in the 2020 Runoff and did not vote at all in previous elections. In this group, we see no differential effects, as there is presumably no voting 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 the substitution effect of the forced experimentation with postal voting. 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

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

[43] 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 strong substitution but no increase in total turnout. The three other qunitiles show estimates similar to the baseline estimation. Only in the middle quintile, there is no increase in total turnout.

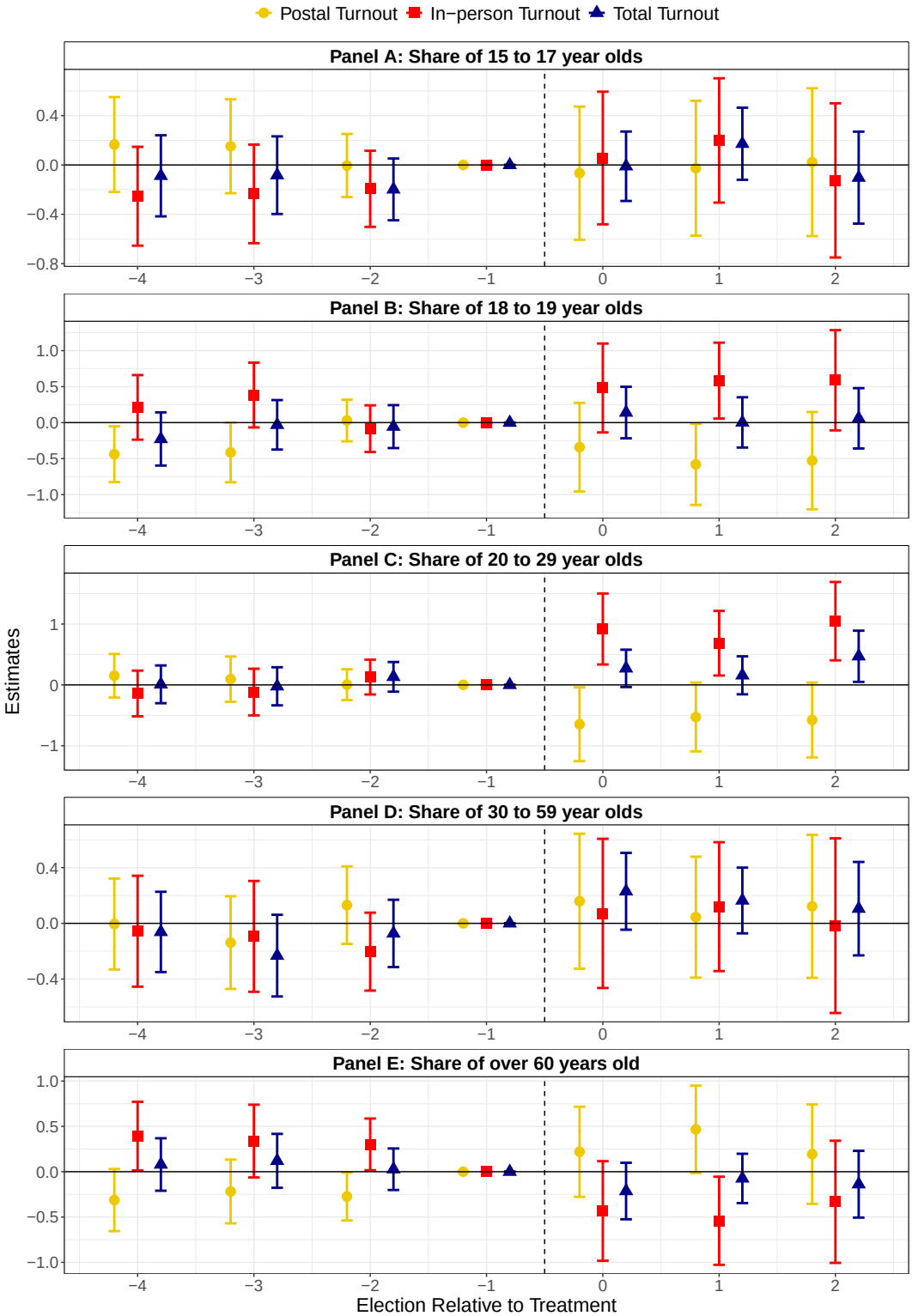
Figure 6: Heterogeneity by Population Size: Terciles



Notes: The figure presents event study results based on Equation 3 for postal, in-person, and total turnout (0-100). Marginal effects for each category, instead of coefficients of interaction terms, are shown. The reference election $(t-1)$ is the 2018 State Election and it is controlled for the population as in Figure 3. The terciles 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 E11.

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 they might be well informed about possible modes of

Figure 7: Heterogeneities by Age



Notes: The figure presents triple difference estimates (μ^{τ}) 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 E13.

voting.[44] Interestingly, coefficients are rather similar in all post-treatment periods. 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 $t+0$ and a statistically significant effect in $t+2$. 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. 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 voted using their preferred voting mode.

Unemployment Heterogeneity by the share of unemployed in a municipality is depicted in Panel A of Appendix Figure D8. 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, and in $t+2$, all coefficients are close to zero.

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, and results in a mitigation of the total turnout effect (Panel B of Figure D8). While the substitution from in-person to postal voting is more pronounced for women, the substitution from former abstainers is weaker. Estimates in all post-treatment elections are similar. However, it should be noted that the total turnout coefficient in $t+1$ lacks statistical significance. 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 E3).

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 D8). 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.

[44] Recall that postal voting has been possible since 1957 and can be requested without giving an excuse since 2008.

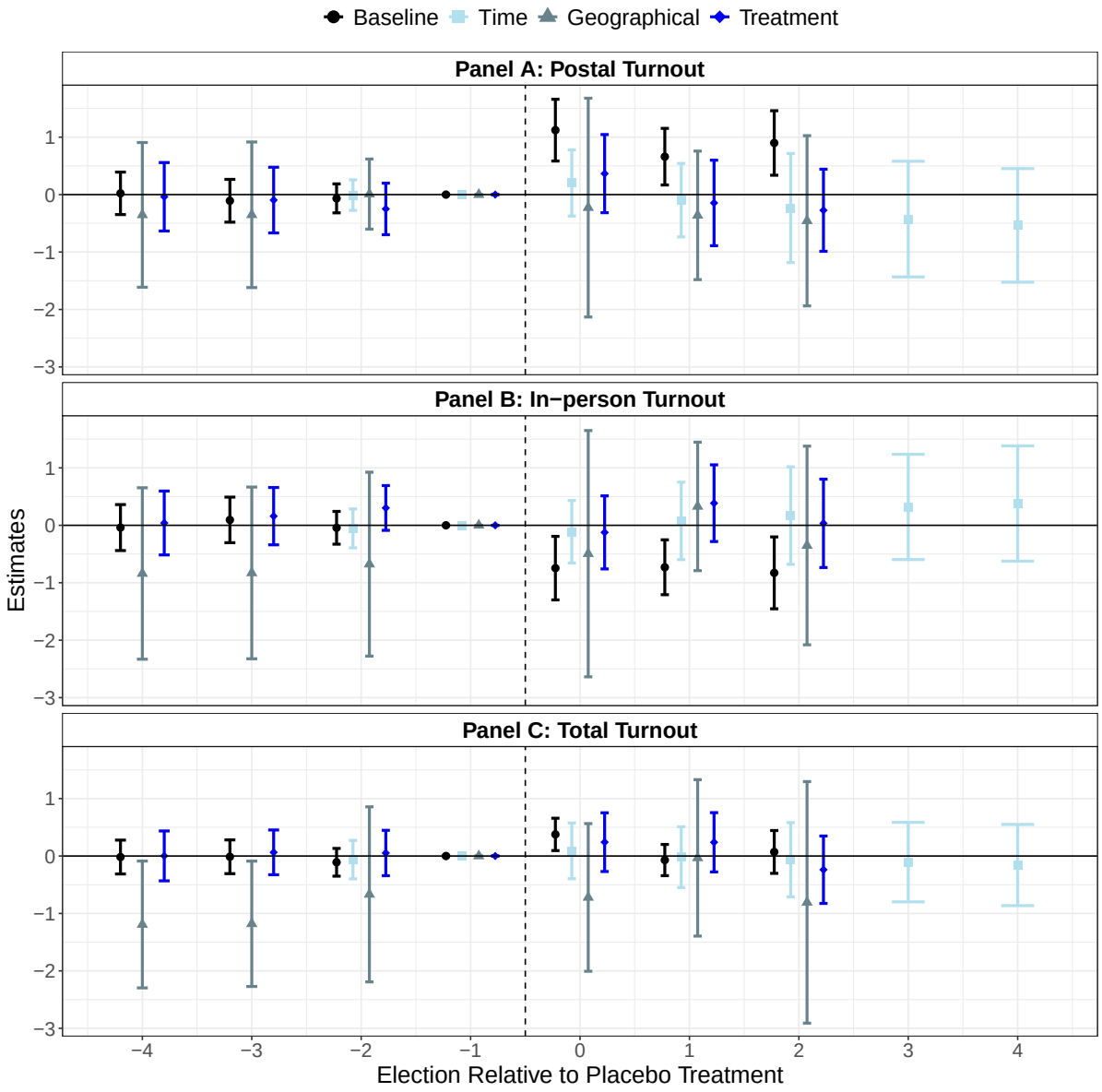
4.4 Placebo Tests

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 reset the treatment to the previous mayoral elections in 2014, where there was no all-postal runoff. We then reset 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 five 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 forced experimentation with postal voting 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 E15.

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.[45] As they did not take place at the beginning of the COVID-19 pandemic, they were not held as all-postal elections. Still, overall turnout patterns are similar to Bavaria and specifically, we observe a similar increase in the share of postal voting in 2021, decreasing again in later elections (Appendix Figure D9). 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 lack of statistical significance might be due to limited statistical power.[46] Nonetheless, it is noteworthy that the point estimates for postal and total turnout are negative in all three post-treatment periods, while estimates for in-person turnout fluctuate around zero. If anything, the negative point estimates for total turnout suggest voting fatigue (Garmann, 2017), which would also bias downward the positive estimate we find as a main result in $t+0$. Moreover, the negative point estimates for postal voting are opposite in sign to our main results, indicating that regular runoffs during the COVID-19 pandemic do not increase postal turnout.

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 in 2020. 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. 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 runoff.

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 E16 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.

[45] 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 mayoral elections after November 2020 but before the 2021 Federal Election. 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).

[46] Among the 78 municipalities in the sample, 15 held a runoff election and are thus classified as placebo-treated. The remaining 63 municipalities without a runoff serve as the control group. This is a slightly larger share of treated municipalities (19 percent versus 14 percent in the main estimation sample.

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, one and a half years after the treatment. While this increase is only transitory, a substitution from in-person to postal voting of less than a percentage point persists across the next three subsequent elections. Consistent with the Porter Hypothesis (Porter, 1991), our findings show that policies altering decision-making costs can have lasting effects and highlight the long-term impact of the forced experimentation. The estimated effects remain robust across different model specifications and are supported by placebo tests.

The decrease in 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 or with a higher share of the 20–29 age group. 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 partisan effects. Increases in the total vote share of the largest conservative party (CSU) due to an increase in its postal vote share stem primarily from former in-person voters of other right-wing parties. However, these effects are lasting only over two elections and we cannot separate them from the cause of the all-postal election, the pandemic. Also, we cannot rule out compositional effects.

Our results may lead to important policy implications. Even in Germany, where postal voting is easily accessible and has a long tradition, information costs of postal voting remain substantial, particularly for older eligible voters, thereby discouraging participation. Targeted measures aimed at reducing these costs for groups facing higher barriers could increase turnout. Moreover, we show that in a setting with relatively high baseline postal turnout and low administrative barriers, eliminating information costs of postal voting still affects voting behavior. In countries with lower, but rising, postal voting rates, such as the UK, the effects of similar interventions might be even larger. Framed as a least-likely case, our findings suggest that targeted reductions in information costs may have even greater payoffs where postal voting is less established.

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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.[47] 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 for the 2018 State Election, 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 the 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.

[47] 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 the 2020 Mayoral Elections. It is controlled for the time-invariant log number of eligible voters, held constant to the 2018 State Election. 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 2,835 observations, approximately 28 percent of the main sample (10,199 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. In $t+2$, the size of the coefficient increases slightly, but the total turnout effect remains statistically insignificant.

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 10 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, it also remains in $t+1$ and $t+2$ with the expectation of the 10 percentage point one 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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Postal Turnout									
Treatment $(t-4)$	0.0987	0.0554	0.0846	0.0525	0.1902	0.0125	0.0161	-0.0436	-0.0478
	(0.3651)	(0.3345)	(0.3251)	(0.3162)	(0.3029)	(0.2941)	(0.2863)	(0.2765)	(0.2729)
Treatment $(t-3)$	-0.1662	-0.2166	-0.1511	-0.2179	-0.1206	-0.1820	-0.1486	-0.1831	-0.1599
	(0.3807)	(0.3474)	(0.3386)	(0.3262)	(0.3079)	(0.2952)	(0.2860)	(0.2758)	(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)	(0.2058)	(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***
	(0.5209)	(0.4906)	(0.4770)	(0.4531)	(0.4416)	(0.4239)	(0.4132)	(0.4033)	(0.3953)
Treatment $(t+1)$	0.1255	0.1052	0.3075	0.2752	0.3984	0.5545	0.5307	0.5673	0.5544
	(0.4938)	(0.4619)	(0.4474)	(0.4309)	(0.4236)	(0.4052)	(0.3933)	(0.3822)	(0.3728)
Treatment $(t+2)$	0.9319*	0.8019	0.9245*	0.9159**	1.146**	1.252***	1.164***	1.216***	1.118***
	(0.5095)	(0.4905)	(0.4805)	(0.4643)	(0.4627)	(0.4478)	(0.4301)	(0.4180)	(0.4093)
R^2	0.97779	0.97736	0.97666	0.97531	0.97418	0.97403	0.97416	0.97421	0.97445
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
	(0.3796)	(0.3482)	(0.3298)	(0.3166)	(0.2953)	(0.3129)	(0.3055)	(0.2918)	(0.2896)
Treatment $(t-3)$	0.4028	0.2290	0.3164	0.3112	0.2802	0.4075	0.3447	0.3170	0.2345
	(0.3813)	(0.3424)	(0.3281)	(0.3198)	(0.2969)	(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
	(0.2906)	(0.2768)	(0.2688)	(0.2573)	(0.2427)	(0.2273)	(0.2208)	(0.2141)	(0.2096)
Treatment $(t+0)$	-0.3433	-0.3206	-0.5156	-0.5132	-0.6722	-0.8384*	-0.7956*	-0.8301**	-0.7674*
	(0.5436)	(0.5160)	(0.4933)	(0.4765)	(0.4605)	(0.4364)	(0.4266)	(0.4171)	(0.4064)
Treatment $(t+1)$	-0.4734	-0.4631	-0.5116	-0.4322	-0.5246	-0.6533*	-0.6139*	-0.6505*	-0.5732
	(0.4786)	(0.4423)	(0.4272)	(0.4228)	(0.4026)	(0.3827)	(0.3722)	(0.3618)	(0.3516)
Treatment $(t+2)$	-0.5762	-0.4620	-0.6027	-0.4909	-0.7076	-0.8870*	-0.8576*	-0.9505**	-0.8036*
	(0.5874)	(0.5683)	(0.5602)	(0.5454)	(0.5251)	(0.4980)	(0.4783)	(0.4666)	(0.4550)
R^2	0.96154	0.96147	0.96079	0.95905	0.95856	0.95845	0.95884	0.95846	0.95913
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
	(0.3074)	(0.2892)	(0.2895)	(0.2773)	(0.2710)	(0.2565)	(0.2468)	(0.2379)	(0.2305)
Treatment $(t-3)$	0.2366	0.0124	0.1653	0.0934	0.1596	0.2255	0.1961	0.1339	0.0746
	(0.2889)	(0.2801)	(0.2879)	(0.2729)	(0.2578)	(0.2449)	(0.2365)	(0.2323)	(0.2272)
Treatment $(t-2)$	0.1852	0.2147	0.2590	0.2978	0.2732	0.2053	0.1360	0.0826	0.0543
	(0.2344)	(0.2313)	(0.2246)	(0.2150)	(0.2010)	(0.1870)	(0.1821)	(0.1840)	(0.1792)
Treatment $(t+0)$	0.4412*	0.4189*	0.4874**	0.5420**	0.4970**	0.5120**	0.4659**	0.4622**	0.4553**
	(0.2606)	(0.2506)	(0.2439)	(0.2395)	(0.2245)	(0.2124)	(0.2046)	(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)	(0.2433)	(0.2256)	(0.2195)	(0.2106)	(0.2048)
Treatment $(t+2)$	0.3557	0.3399	0.3217	0.4249	0.4386	0.3649	0.3065	0.2658	0.3142
	(0.3500)	(0.3278)	(0.3395)	(0.3360)	(0.3141)	(0.2961)	(0.2841)	(0.2756)	(0.2685)
R^2	0.97642	0.97584	0.97407	0.97220	0.97256	0.97272	0.97300	0.97309	0.97312
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	1,708	1,890	2,058	2,184	2,324	2,506	2,597	2,716	2,835
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

	(1)	(2)	(3)	(4)	(5)
Panel A: Postal Turnout					
Treatment $(t-4)$	0.0218	0.0218	0.0218	0.0218	0.0218
	(0.1885)	(0.1953)	(0.2035)	[0.908]	[0.913]
Treatment $(t-3)$	-0.1075	-0.1075	-0.1075	-0.1075	-0.1075
	(0.1898)	(0.2001)	(0.2097)	[0.570]	[0.599]
Treatment $(t-2)$	-0.0658	-0.0658	-0.0658	-0.0658	-0.0658
	(0.1286)	(0.1465)	(0.1779)	[0.608]	[0.660]
Treatment $(t+0)$	1.122***	1.122***	1.122***	1.122***	1.122***
	(0.2741)	(0.2997)	(0.2792)	[<0.001]	[<0.001]
Treatment $(t+1)$	0.6601***	0.6601**	0.6601**	0.6601***	0.6601**
	(0.2512)	(0.2783)	(0.2678)	[0.009]	[0.023]
Treatment $(t+2)$	0.8988***	0.8988**	0.8988***	0.8988***	0.8988**
	(0.2862)	(0.3377)	(0.3126)	[0.002]	[0.012]
R^2	0.96761	0.96761	0.96761	0.96761	0.96761
Panel B: In-person Turnout					
Treatment $(t-4)$	-0.0396	-0.0396	-0.0396	-0.0396	-0.0396
	(0.2040)	(0.1889)	(0.2154)	[0.846]	[0.837]
Treatment $(t-3)$	0.0941	0.0941	0.0941	0.0941	0.0941
	(0.2022)	(0.2098)	(0.2187)	[0.641]	[0.661]
Treatment $(t-2)$	-0.0433	-0.0433	-0.0433	-0.0433	-0.0433
	(0.1454)	(0.1359)	(0.1934)	[0.765]	[0.755]
Treatment $(t+0)$	-0.7456***	-0.7456**	-0.7456**	-0.7456***	-0.7456**
	(0.2819)	(0.3240)	(0.2970)	[0.008]	[0.028]
Treatment $(t+1)$	-0.7305***	-0.7305**	-0.7305***	-0.7305***	-0.7305**
	(0.2436)	(0.2904)	(0.2669)	[0.003]	[0.017]
Treatment $(t+2)$	-0.8279***	-0.8279**	-0.8279**	-0.8279***	-0.8279**
	(0.3191)	(0.3810)	(0.3483)	[0.009]	[0.037]
R^2	0.94892	0.94892	0.94892	0.94892	0.94892
Panel C: Total Turnout					
Treatment $(t-4)$	-0.0178	-0.0178	-0.0178	-0.0178	-0.0178
	(0.1503)	(0.1512)	(0.1543)	[0.905]	[0.908]
Treatment $(t-3)$	-0.0134	-0.0134	-0.0134	-0.0134	-0.0134
	(0.1498)	(0.1620)	(0.1565)	[0.929]	[0.936]
Treatment $(t-2)$	-0.1091	-0.1091	-0.1091	-0.1091	-0.1091
	(0.1231)	(0.1326)	(0.1263)	[0.374]	[0.422]
Treatment $(t+0)$	0.3765***	0.3765**	0.3765***	0.3765***	0.3765**
	(0.1436)	(0.1408)	(0.1363)	[0.009]	[0.011]
Treatment $(t+1)$	-0.0705	-0.0705	-0.0705	-0.0705	-0.0705
	(0.1386)	(0.1454)	(0.1458)	[0.610]	[0.635]
Treatment $(t+2)$	0.0710	0.0710	0.0710	0.0710	0.0710
	(0.1901)	(0.1852)	(0.1895)	[0.708]	[0.708]
R^2	0.95911	0.95911	0.95911	0.95911	0.95911
SE cluster	Munic.	County	TW Munic. + County-Elect.	Munic. Bootstrap	County Bootstrap
Observations	10,199	10,199	10,199	10,199	10,199
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. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

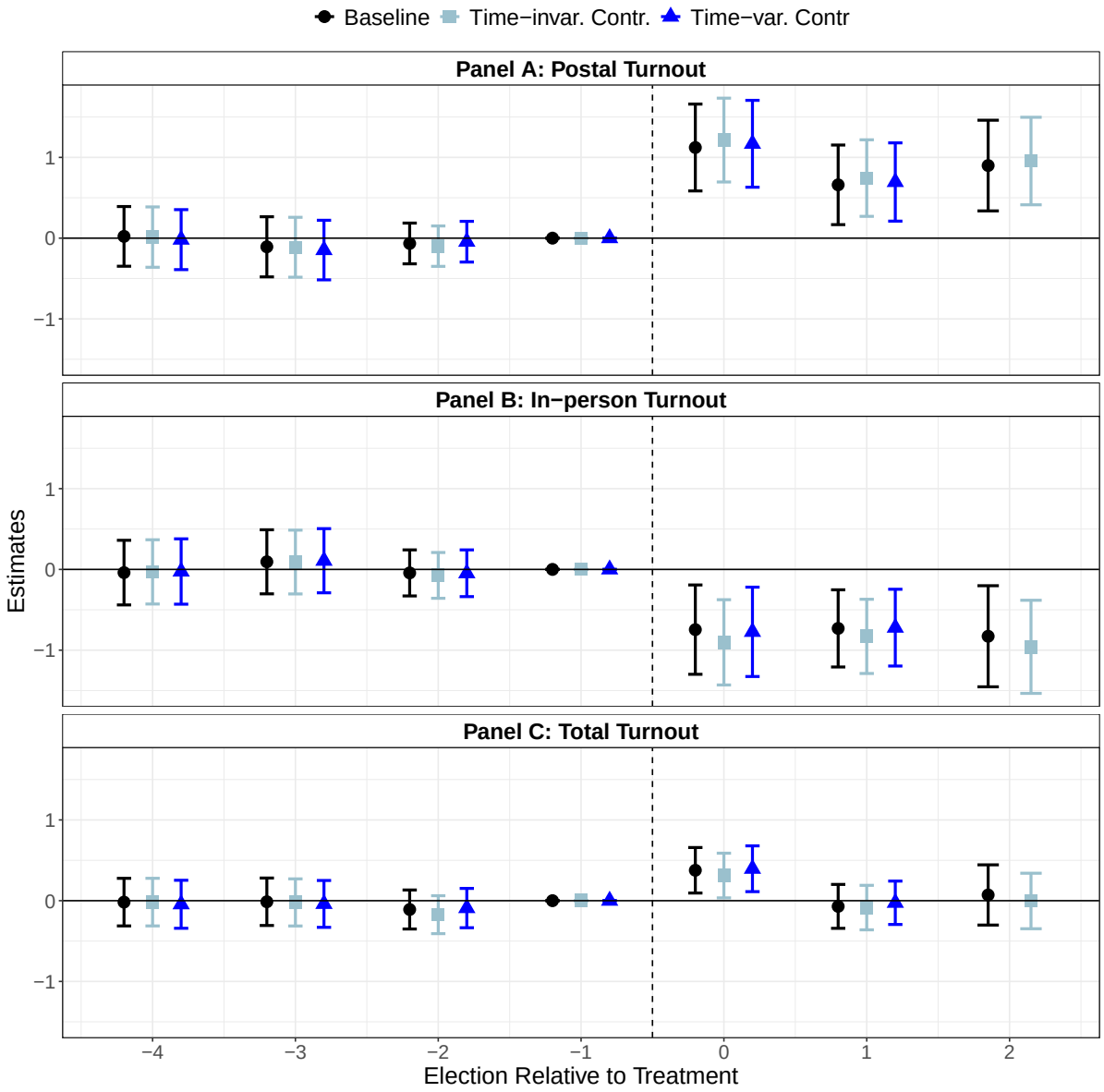
C.3 Controls

Additional Controls In the main specification, we only control for the time-invariant 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 by accounting for unobserved heterogeneity, 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.[48] In both specifications, the results remain unchanged.

Population Measure In the main specification, we control for the time-invariant 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.

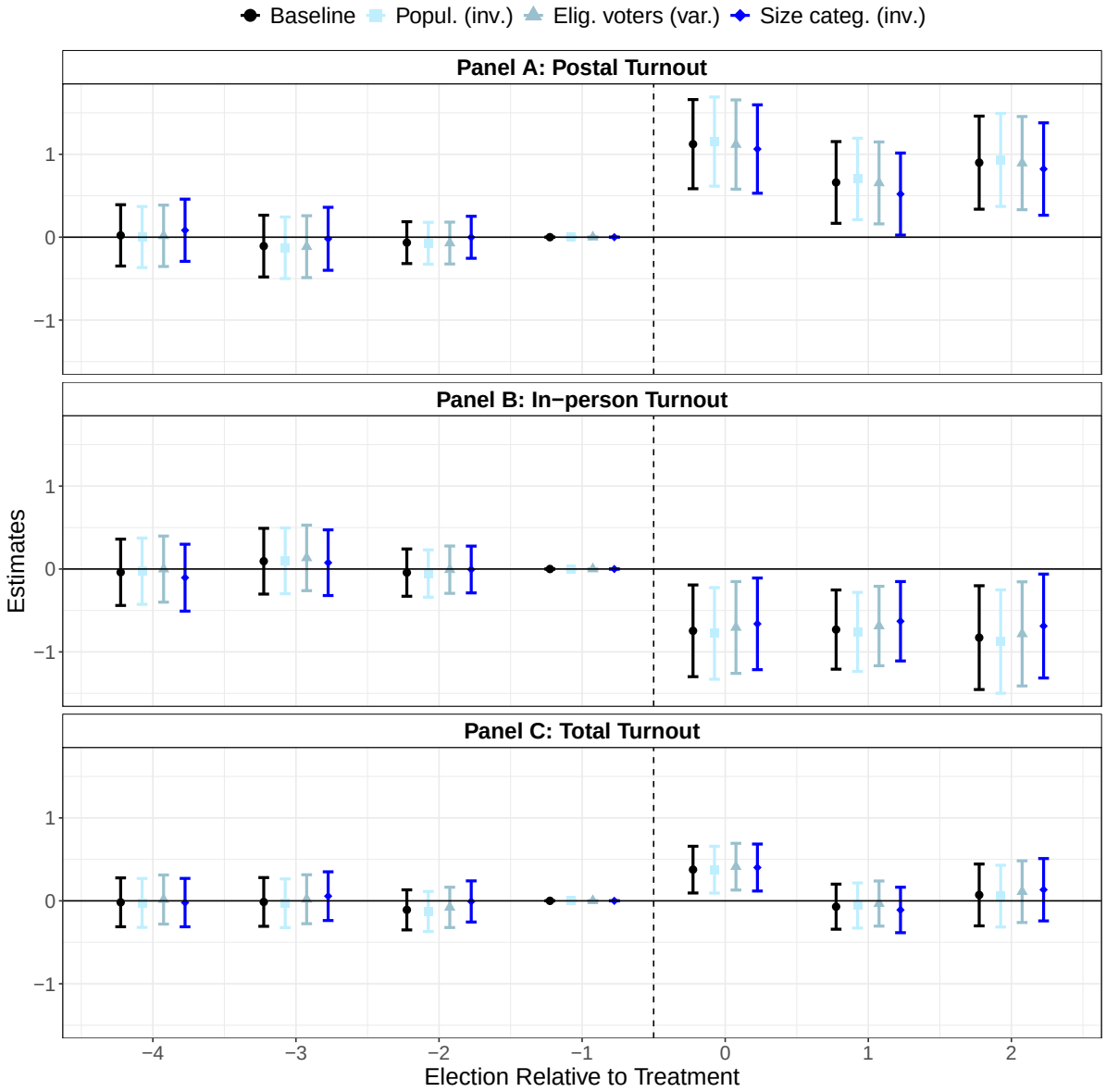
[48] For the time-variant controls, values for 2025 ($t+2$) are not available yet. Additionally, we drop the share of households with children because it is not available for all time periods.

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. Values for 2025 $(t + 2)$ are not yet available. The point estimates and standard errors underlying the results are shown in Table E17. 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.

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 E18. 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.

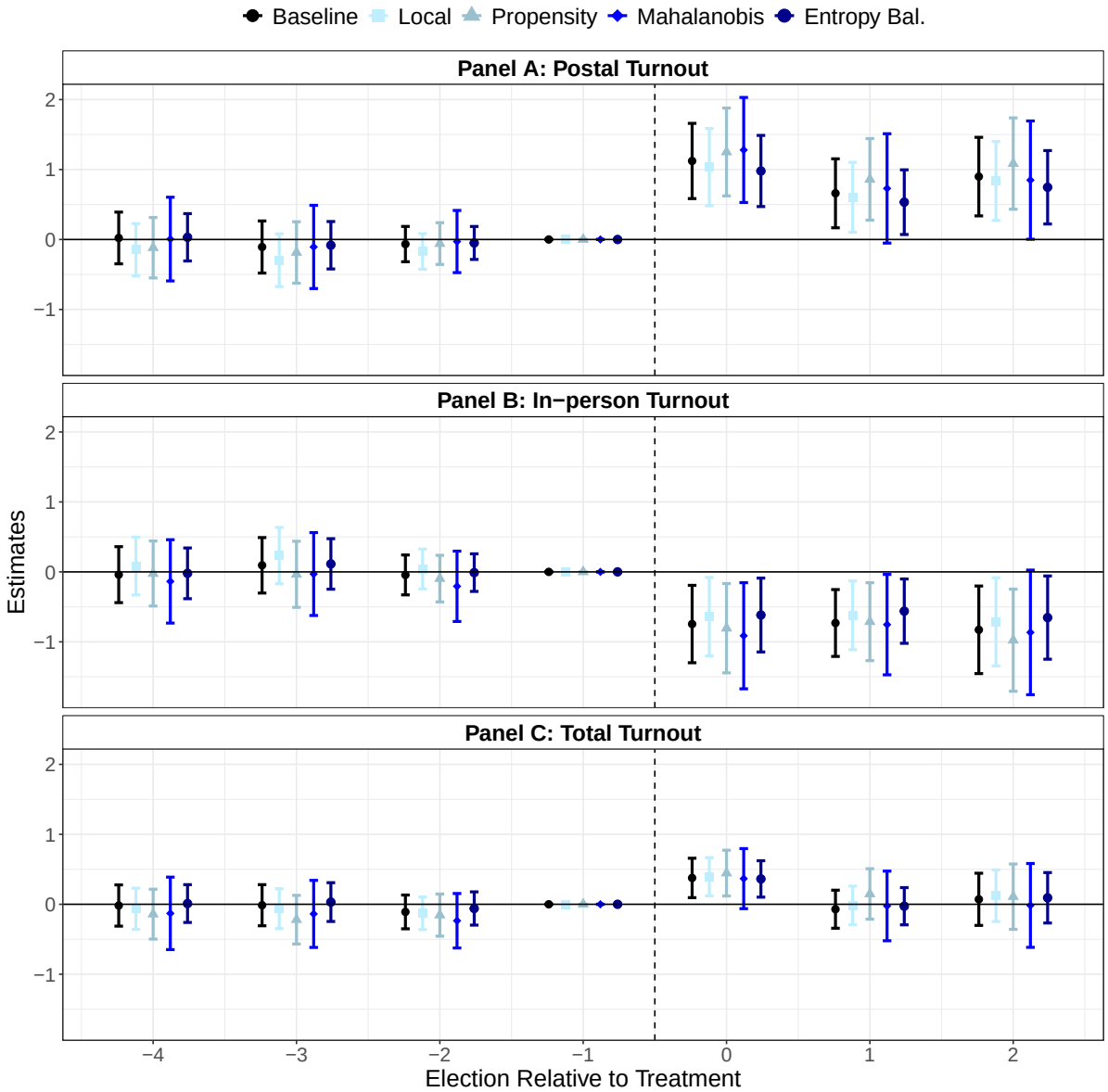
Propensity Score Matching We conduct a propensity score matching procedure to estimate the propensity of having a one-time all-postal election. This propensity is calculated using a probit regression based on the log population, measured as the number of eligible voters for the 2018 State Election, within 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. (2025), we adjust standard errors for two-way clustering to account for potential bias introduced by matching on covariates. The first cluster is at the level of the match, and the second cluster is at the control group×election level. Estimates for postal and in-person turnout remain. However, estimates for postal voting in $t+1$ and for in-person voting in $t+2$ are only marginally significant. 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 log population, measured

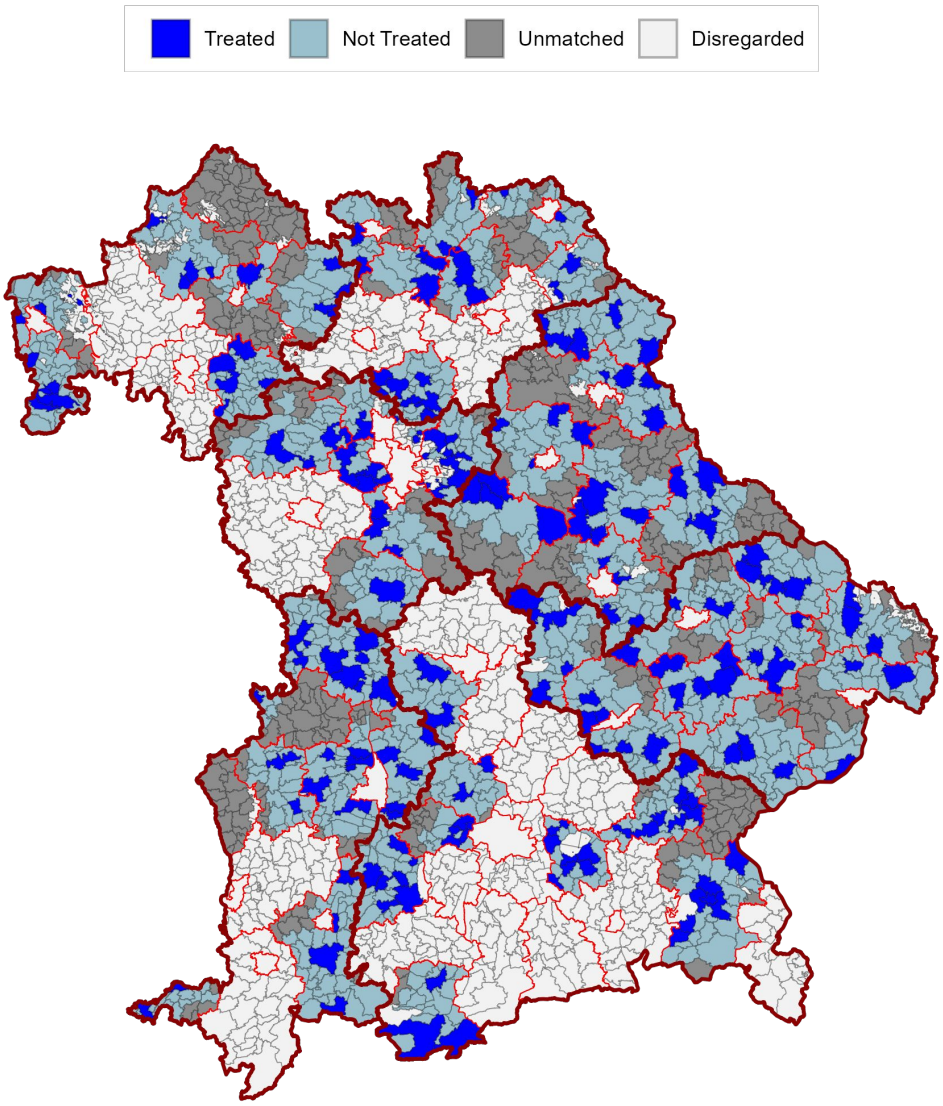
as the number of eligible voters for the 2018 State Election, within a county. We use the resulting entropy weights in the event study regressions. The results remain.

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. Mahalanobis matching adjusts standard errors for two-way clustering at the level of the matched pair and the control group×election level, following Colmer et al. (2025). 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 E19.

Figure C4: Local Matching



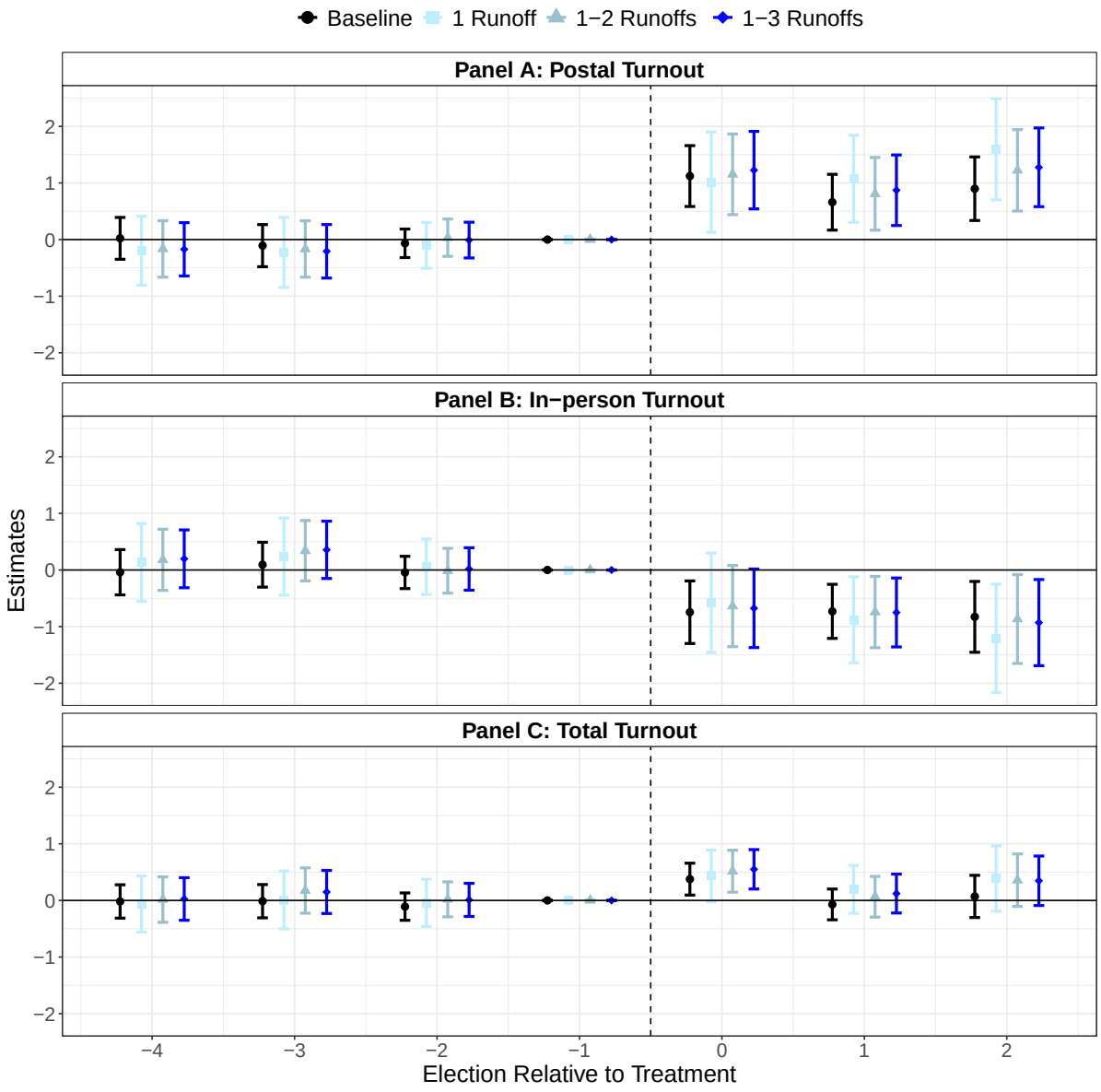
Notes: The figure presents a map of Bavaria containing municipalities used in the local matching approach. Only treated municipalities and control group municipalities that are at most 5km away from the border of a treated municipality within the same county are included. Thick red lines show government 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.[49] 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.

[49] 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).

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 minimum and maximum 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 E20.

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 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 small, containing only 13 municipalities. Therefore, we also estimate a specification using all 84 off-cycle municipalities.

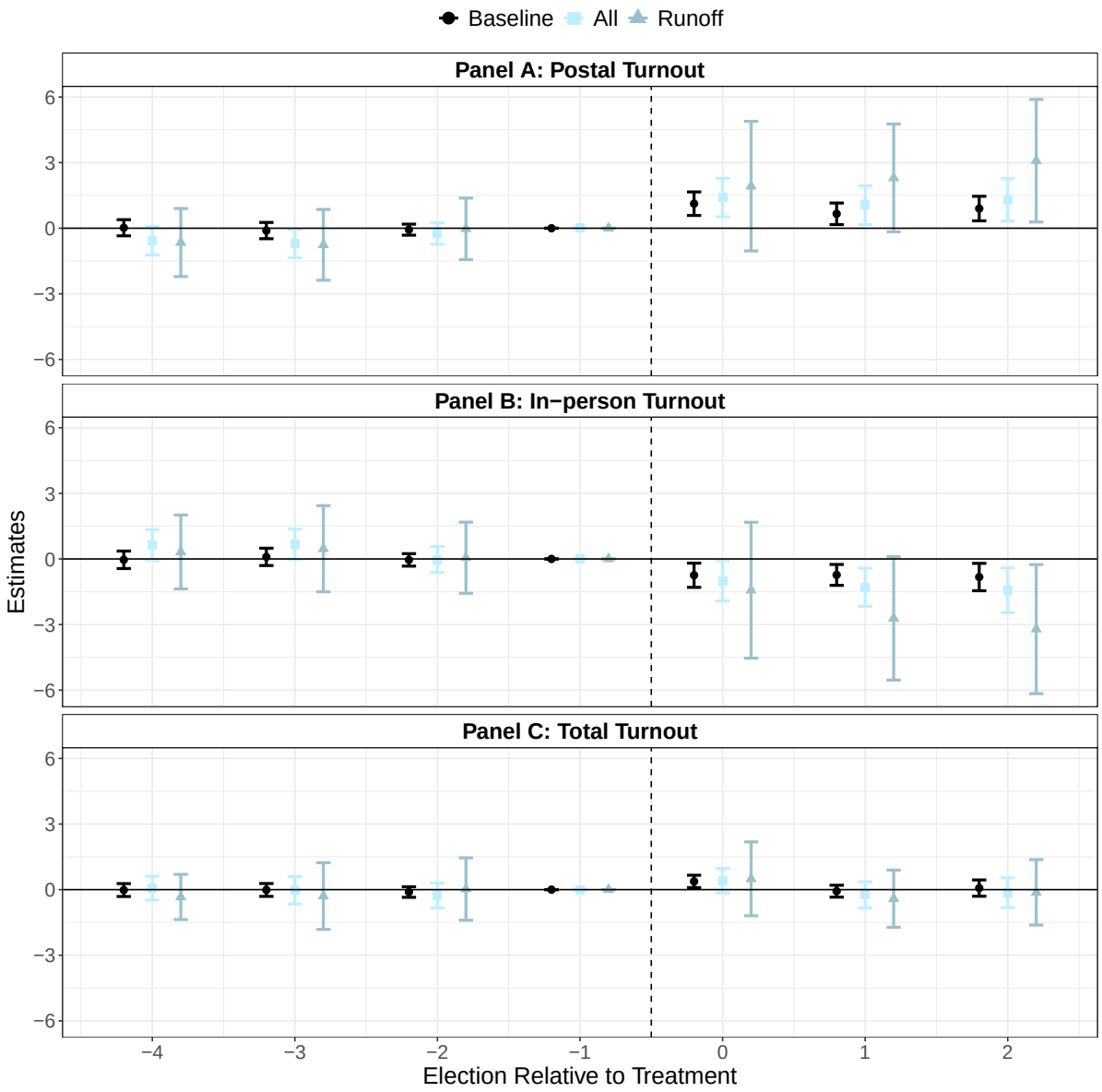
Table C3: Bavarian Local Elections by Runoff and Date

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, with even larger coefficients, 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 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 E21.

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}.

Table C4 presents 95% confidence intervals for postal turnout, in-person turnout, and total turnout under both frameworks. Each panel reports RM-based bounds for $M^{RM} \in \{0, 0.5, 1\}$ and SD-based bounds for $M^{SD} \in \{0, 0.025, 0.05\}$ for three post-treatment periods ($t+0$, $t+1$, and $t+2$). In Panel A, results for postal turnout remain statistically significant under all RM and SD values. The lower bounds remain well above zero even under the most flexible assumptions, indicating strong robustness. In Panel B, the negative treatment effect on in-person turnout is also robust. Even at $M^{RM} = 1$ and $M^{SD} = 0.05$, all confidence intervals exclude zero. In Panel C, the increase in total turnout at $t+0$ is sensitive to more flexible assumptions. The estimate is robust up to $M^{RM} = 0.5$ but becomes insignificant at $M^{RM} = 1$ However, the effect is robust at $M^{SD} = 0.05$.

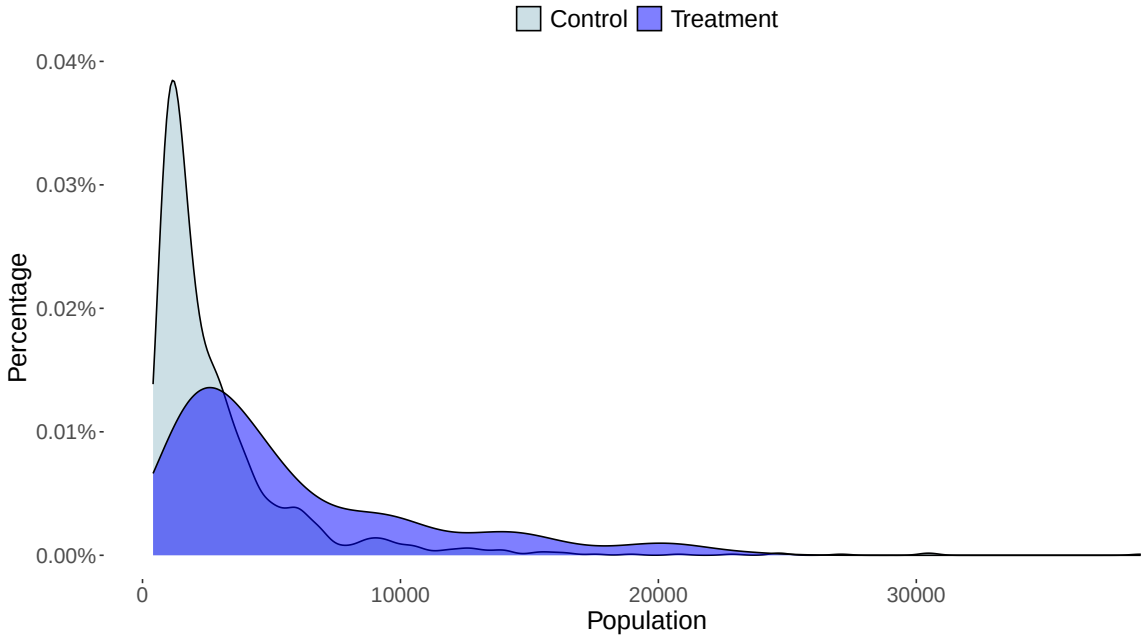
Table C4: Robust Inference Approach

Relative Magnitude Bounds				Smoothness Restrictions			
M^{RM}	$t+0$	$t+1$	$t+2$	M^{SD}	$t+0$	$t+1$	$t+2$
	(1)	(2)	(3)		(4)	(5)	(6)
Panel A: Postal Turnout							
0	(0.576, 1.663)	(0.166, 1.162)	(0.327, 1.461)	0	(0.727, 1.794)	(0.247, 1.242)	(0.441, 1.567)
0.5	(0.521, 1.707)	(0.106, 1.202)	(0.281, 1.507)	0.025	(0.673, 1.778)	(0.206, 1.229)	(0.400, 1.553)
1	(0.466, 1.750)	(0.045, 1.252)	(0.223, 1.553)	0.05	(0.571, 1.748)	(0.113, 1.193)	(0.314, 1.524)
Panel B: In-person Turnout							
0	(-1.304, -0.186)	(-1.214, -0.249)	(-1.463, -0.198)	0	(-1.402, -0.272)	(-1.312, -0.310)	(-1.544, -0.300)
0.5	(-1.360, -0.119)	(-1.273, -0.180)	(-1.514, -0.134)	0.025	(-1.406, -0.241)	(-1.317, -0.282)	(-1.547, -0.267)
1	(-1.451, -0.006)	(-1.380, -0.054)	(-1.604, -0.045)	0.05	(-1.419, -0.174)	(-1.330, -0.221)	(-1.561, -0.199)
Panel C: Total Turnout							
0	(0.089, 0.658)	(-0.347, 0.203)	(-0.308, 0.445)	0	(0.159, 0.675)	(-0.340, 0.241)	(-0.222, 0.482)
0.5	(0.003, 0.704)	(-0.419, 0.247)	(-0.392, 0.476)	0.025	(0.118, 0.701)	(-0.369, 0.253)	(-0.256, 0.500)
1	(-0.129, 0.796)	(-0.541, 0.341)	(-0.506, 0.544)	0.05	(0.046, 0.748)	(-0.423, 0.282)	(-0.324, 0.537)

Notes: The table reports 95% confidence intervals for post-treatment coefficients under deviations from the parallel trends assumption, based on the framework of Rambachan and Roth (2023). Each panel corresponds to one outcome variable: postal turnout (Panel A), in-person turnout (Panel B), and total turnout (Panel C). For each outcome, we report bounds under two types of restrictions: Relative Magnitude (RM) bounds (Columns 1 through 3), where M^{RM} scales the largest observed pre-treatment deviation, and Smoothness (SD) restrictions (Columns 4 through 6), where M^{SD} limits the change in the slope of differential trends across periods. Confidence intervals are shown for the three post-treatment periods ($t+0$, $t+1$, and $t+2$). Intervals that include zero indicate sensitivity to potential violations.

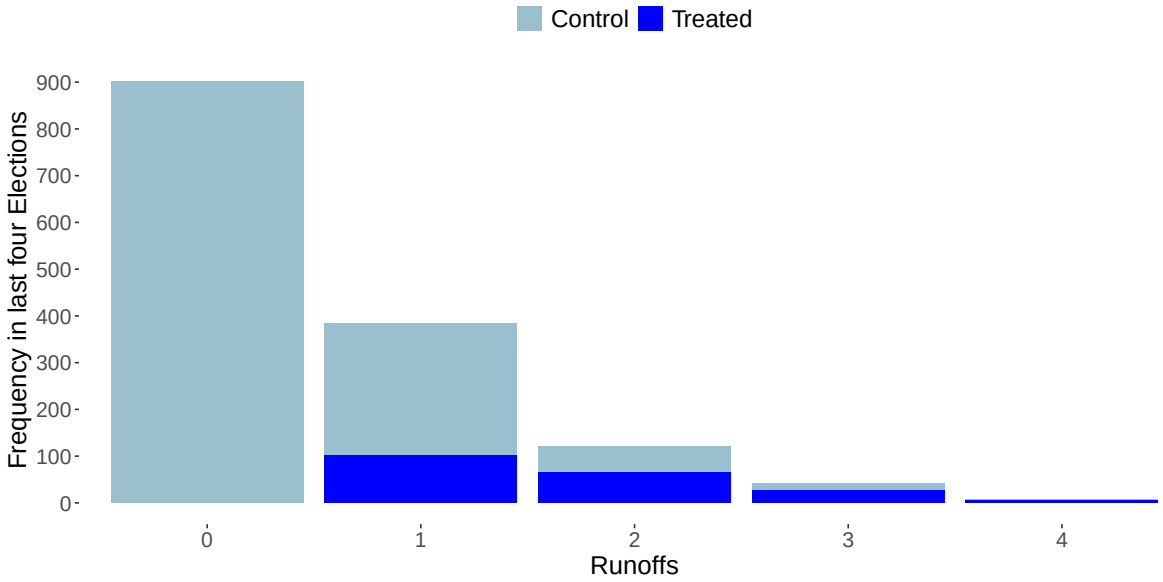
D Figures

Figure D1: Density of Population by Treatment



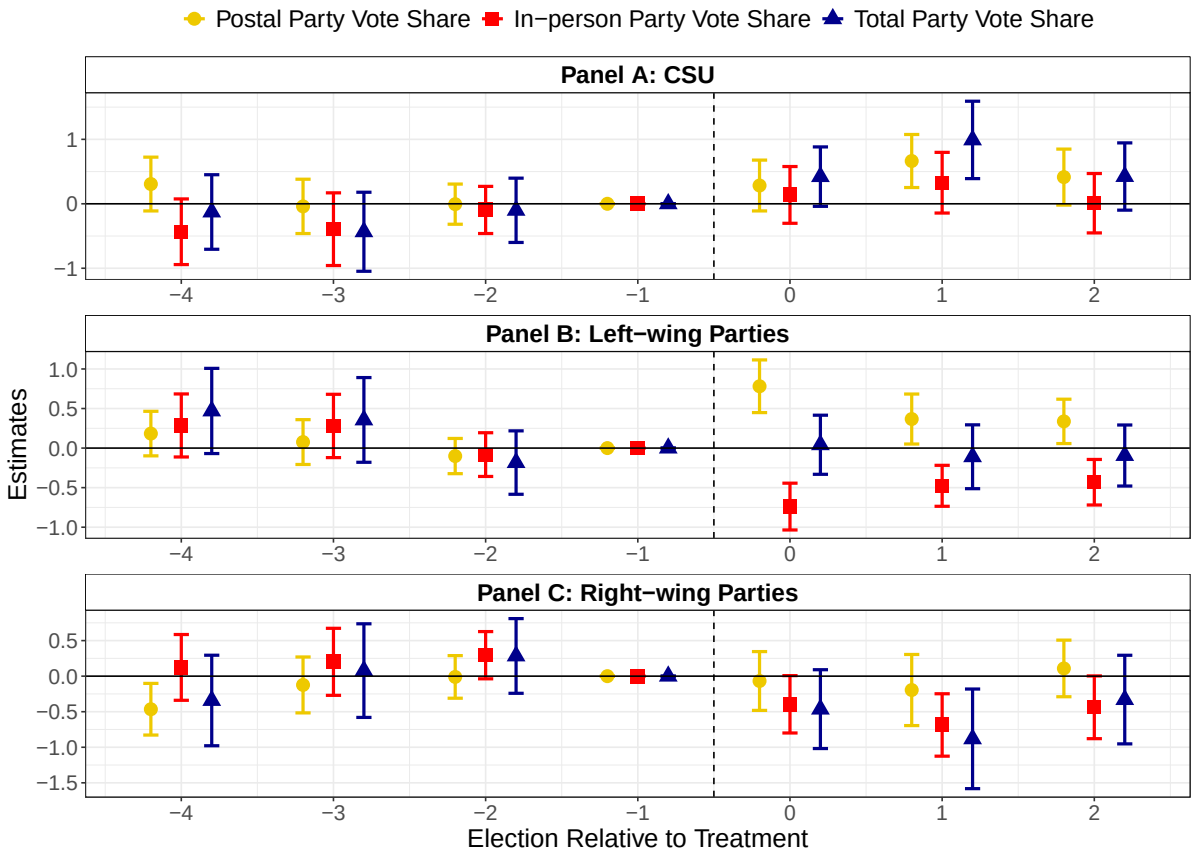
Notes: The figure shows a density plot depicting the distribution of the population variable for the treatment and control groups, representing the relative concentration of observations across the range of population values.

Figure D2: Number of Runoffs in the Last Four Elections



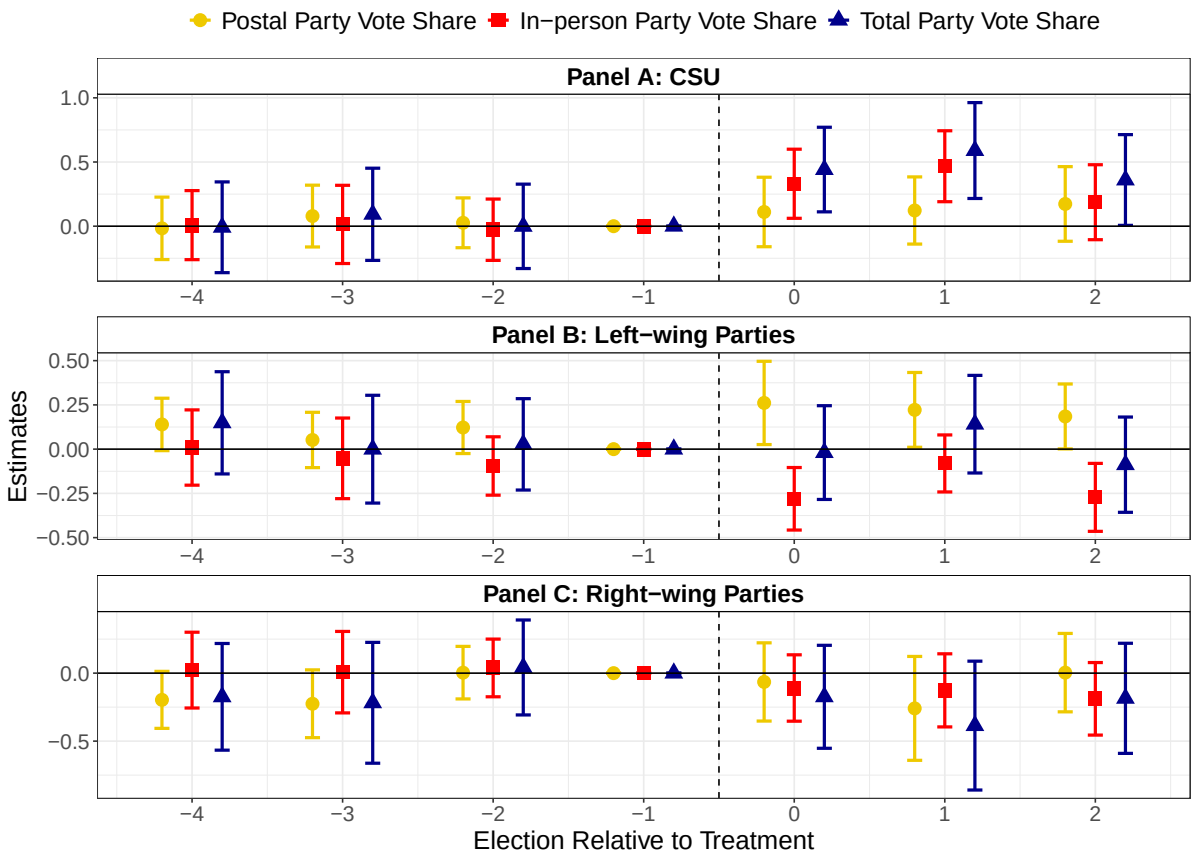
Notes: The figure shows the number of runoffs per municipality for Bavarian mayoral elections for the last four mayoral elections.

Figure D3: Election Results (Cast Votes)



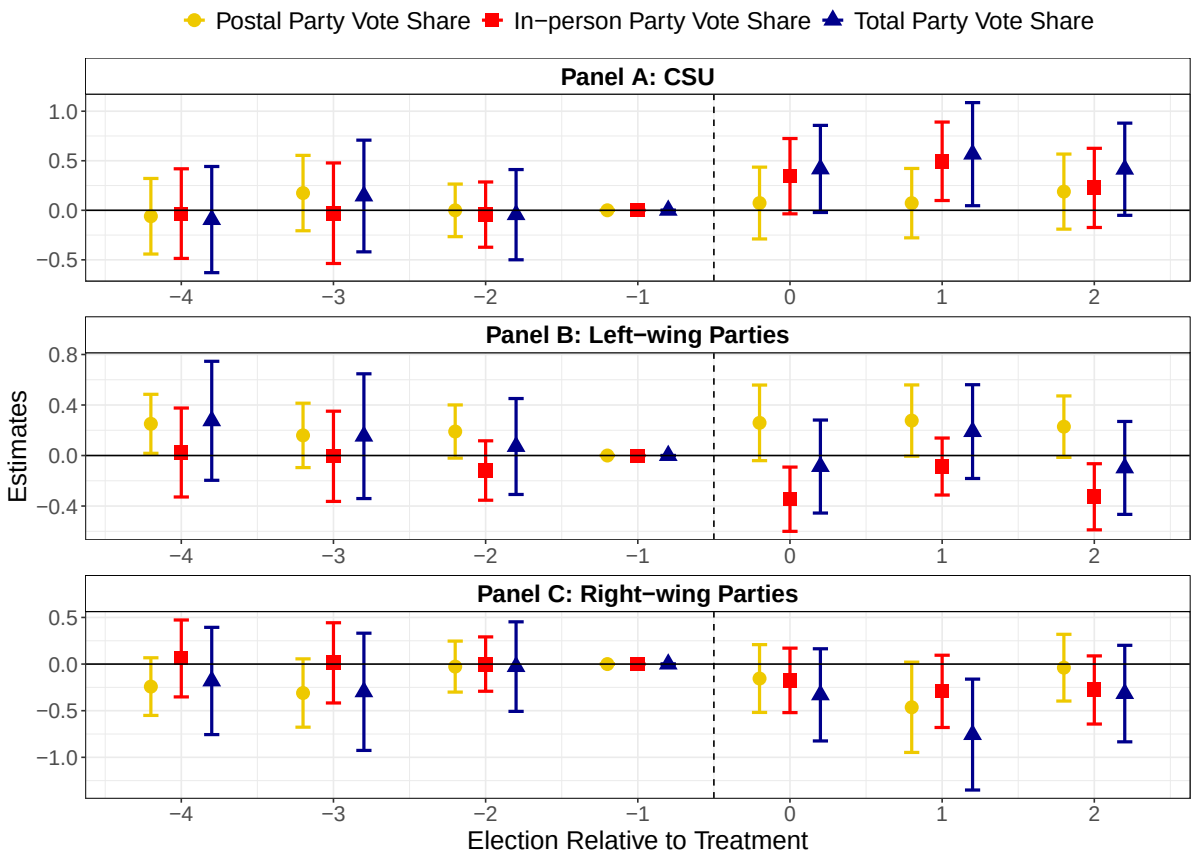
Notes: The figure presents event study results based on Equation 2 for postal, in-person, and total vote shares (0-100)—each as a share of each as a share of votes cast—of the party *Christlich Soziale Union* (CSU, Panel A), and of left- and other right-wing parties (Panel B and C). Left-wing parties include *Die Linke* (The Left), the *Sozialdemokratische Partei Deutschlands* (SPD), and *Bündnis 90/Die Grüne* (Alliance 90/The Greens); other 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 E6.

Figure D4: Party Heterogeneity by Past Turnout (Elig. Voters)



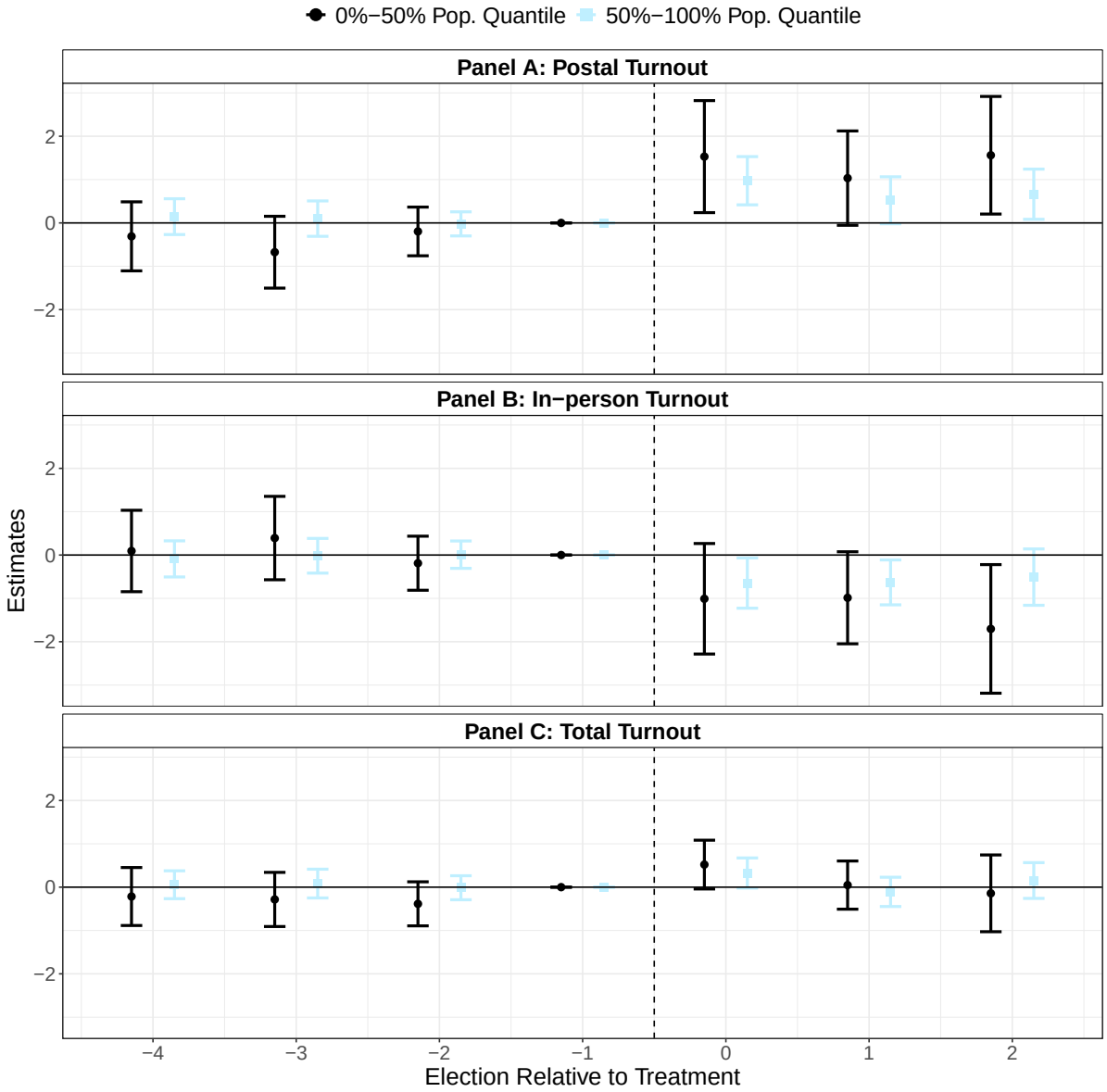
Notes: The figure presents triple difference estimates (μ^{τ}) based on Equation 3 for postal, in-person, and total vote shares (0-100)—each as a share of each as a share of eligible voters—of the party *Christlich Soziale Union* (CSU, Panel A), and of left- and other right-wing parties (Panel B and C). Left-wing parties include *Die Linke* (The Left), the *Sozialdemokratische Partei Deutschlands* (SPD), and *Bündnis 90/Die Grüne* (Alliance 90/The Greens); other 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 E8.

Figure D5: Party Heterogeneity by Past Turnout (Cast Votes)



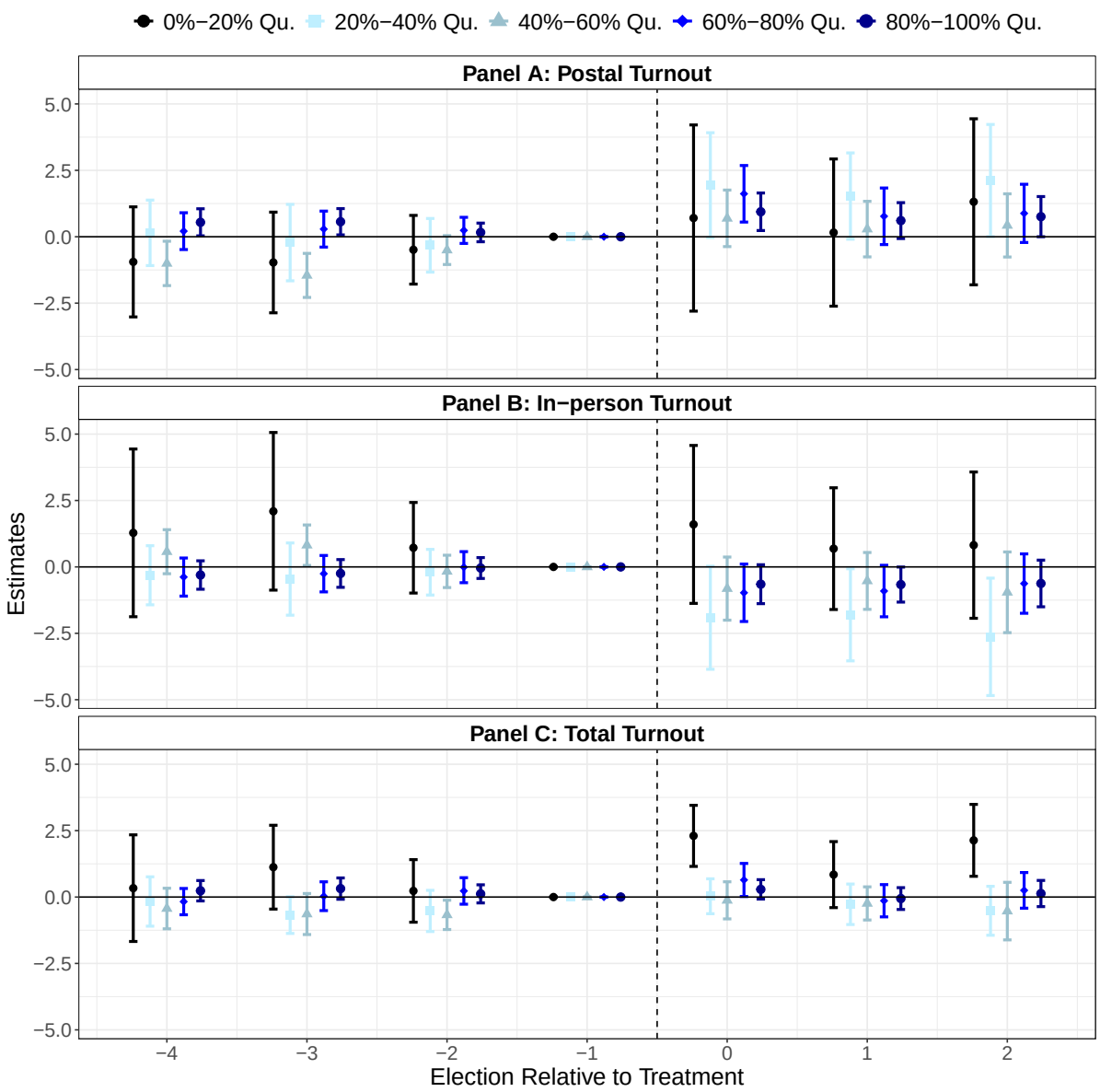
Notes: The figure presents triple difference estimates (μ^{τ}) based on Equation 3 for postal, in-person, and total vote shares (0-100)—each as a share of each as a share of of votes cast—of the party *Christlich Soziale Union* (CSU, Panel A), and of left- and other right-wing parties (Panel B and C). Left-wing parties include *Die Linke* (The Left), the *Sozialdemokratische Partei Deutschlands* (SPD), and *Bündnis 90/Die Grüne* (Alliance 90/The Greens); other 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 E9.

Figure D6: Heterogeneity by Population Size: Median



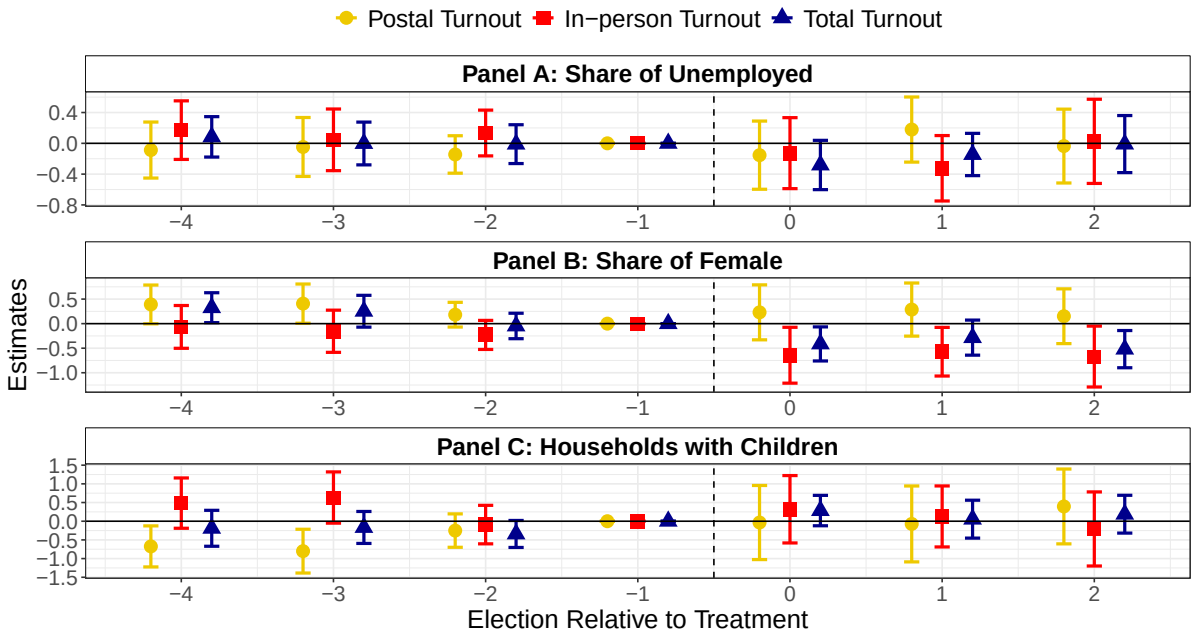
Notes: The figure presents event study results based on Equation 3 for postal, in-person, and total turnout (0-100). Marginal effects for each category, instead of coefficients of interaction terms, are shown. The reference election $(t-1)$ is the 2018 State Election and it is controlled for the population as in Figure 3. The median is based on the number of eligible voters constant in 2018. Standard errors are clustered at the municipality level and reported in parentheses. 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 D7: Heterogeneity by Population Size: Quintiles



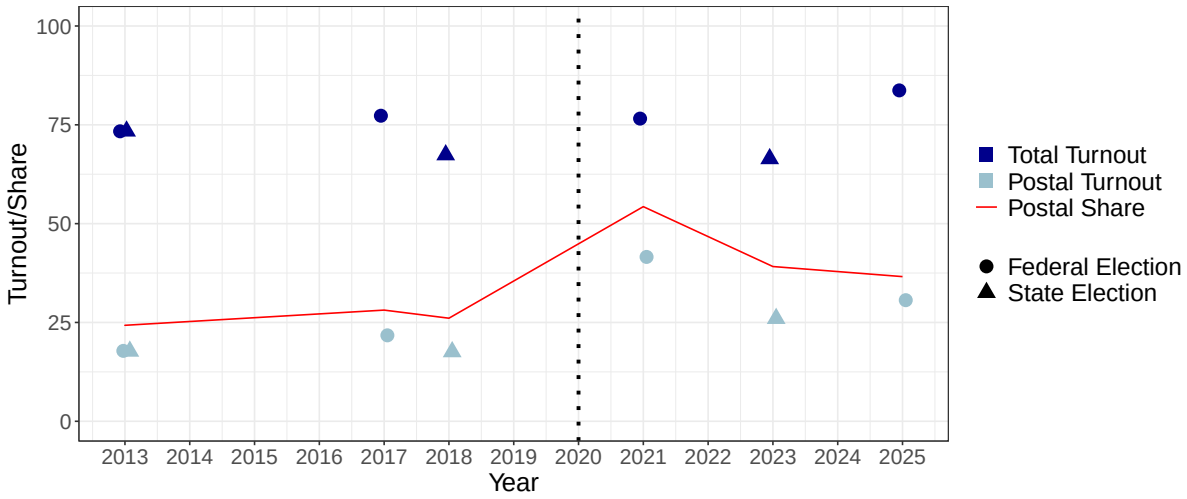
Notes: The figure presents event study results based on Equation 3 for postal, in-person, and total turnout (0-100). Marginal effects for each category, instead of coefficients of interaction terms, are shown. The reference election ($t-1$) is the 2018 State Election and it is controlled for the population as in Figure 3. The quintiles are based on the number of eligible voters constant in 2018. Standard errors are clustered at the municipality level and reported in parentheses. 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.

Figure D8: Heterogeneities Across Socio-Economic Characteristics



Notes: The figure presents triple difference estimates (μ^{τ}) 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 E14.

Figure D9: Turnout by Election Type in Hesse since 2013



Notes: The figure presents postal and total turnout (0-100) for the four federal and three state elections included in the Hesse sample. The placebo treatment is defined by the 2021 Mayoral Elections. The turnout for the 2021 Local Elections is not considered in the analyses.

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 to 17 (%)	1,457	0.030	0.005	0.010	0.028	0.030	0.033	0.052
Age 18 to 19 (%)	1,457	0.022	0.004	0.009	0.020	0.022	0.024	0.041
Age 20 to 29 (%)	1,457	0.111	0.013	0.063	0.103	0.110	0.118	0.227
Age 30 to 59 (%)	1,457	0.425	0.018	0.328	0.413	0.424	0.437	0.506
Age 60 to 99 (%)	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 to 17 (%)	205	0.030	0.004	0.015	0.028	0.030	0.033	0.043
Age 18 to 19 (%)	205	0.022	0.003	0.015	0.020	0.021	0.023	0.036
Age 20 to 29 (%)	205	0.110	0.012	0.073	0.102	0.110	0.117	0.163
Age 30 to 59 (%)	205	0.423	0.019	0.328	0.412	0.422	0.435	0.473
Age 60 to 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 to 17 (%)	1,252	0.031	0.005	0.010	0.028	0.030	0.033	0.052
Age 18 to 19 (%)	1,252	0.022	0.004	0.009	0.020	0.022	0.024	0.041
Age 20 to 29 (%)	1,252	0.111	0.014	0.063	0.103	0.111	0.119	0.227
Age 30 to 59 (%)	1,252	0.425	0.018	0.361	0.414	0.425	0.437	0.506
Age 60 to 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, the last pre-treatment period, and percentages are shares of the total population.

Table E2: Balance Table

	Treatment														
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Eligible Voters (log)	0.1250***	0.0954***	0.1168***	0.1245***	0.1202***	0.1245***	0.1230***	0.1229***	0.1511***	0.1250***	0.1267***	0.1249***	0.1265***	0.1240***	0.1234***
	(0.0132)	(0.0232)	(0.0143)	(0.0133)	(0.0147)	(0.0140)	(0.0133)	(0.0137)	(0.0270)	(0.0132)	(0.0132)	(0.0132)	(0.0132)	(0.0133)	(0.0132)
Inhabitants		0.1972													
		(0.1590)													
Pop. Density ($1/km^2$)			0.0219												
			(0.0170)												
Postal Turnout (p.p.)				0.0016											
				(0.0017)											
In-person Turnout (p.p.)					-0.0016										
					(0.0017)										
Total Turnout (p.p.)						-0.0002									
						(0.0022)									
Unemployment (%)							-0.0143								
							(0.0102)								
Share Female (%)								0.0048							
								(0.0088)							
HH. with Children (%)									0.0250						
									(0.0171)						
Age 0 to 14 (%)										0.0007					
										(0.0098)					
Age 15 to 17 (%)											0.0132				
											(0.0082)				
Age 18 to 19 (%)												-0.0013			
												(0.0078)			
Age 20 to 29 (%)													-0.0167**		
													(0.0084)		
Age 30 to 59 (%)														-0.0037	
														(0.0109)	
Age 60 to 99 (%)															0.0085
															(0.0122)
Observations	1,457	1,457	1,457	1,457	1,457	1,457	1,457	1,457	1,457	1,457	1,457	1,457	1,457	1,457	1,457
R^2	0.13273	0.13446	0.13416	0.13315	0.13319	0.13273	0.13386	0.13286	0.13378	0.13273	0.13406	0.13274	0.13440	0.13281	0.13305
County FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: The table reports cross-sectional balance tests with the treatment indicator as the dependent variable. Covariates are drawn from the 2018 State Election (number of eligible voters and postal, in-person, and total turnout) and from 31 December 2019 (number of inhabitants, unemployment rate, share of females, share of households with children, and the shares of the population aged 0—14, 15-–17, 18-–19, 20-–29, 39—59, and 60+). All variables are standardized to have mean 0 and standard deviation 1. Each single regression includes the log number of eligible voters and county fixed effects, analogous to Equation 2.

Table E3: 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 last pre-treatment period. *CSU* represents the vote share of the largest conservative party, CSU. *Population Terciles* divide the sample into three equally sized groups 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 E4: 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)
Treatment $(t+2)$	0.8988***	-0.8279***	0.0710
	(0.2862)	(0.3191)	(0.1901)
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)	(0.0734)
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)
Population $(t+2)$	-1.222***	1.975***	0.7528***
	(0.1378)	(0.1400)	(0.1070)
Observations	10,199	10,199	10,199
R^2	0.96761	0.94892	0.95911
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 E5: Party Outcomes (Elig. Voters)

	Postal Party Vote Share (1)	In-person Party Vote Share (2)	Total Party Vote Share (3)
Panel A: CSU Share			
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 $(t+1)$	0.4362***	0.1649	0.6011***
	(0.1556)	(0.1691)	(0.2138)
Treatment $(t+2)$	0.2933*	-0.1809	0.1124
	(0.1686)	(0.1725)	(0.1845)
R^2	0.90855	0.94353	0.91549
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 $(t+1)$	0.2825**	-0.3585***	-0.0759
	(0.1176)	(0.0940)	(0.1446)
Treatment $(t+2)$	0.3478***	-0.2905***	0.0573
	(0.1091)	(0.1123)	(0.1463)
R^2	0.94508	0.94800	0.95847
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 $(t+1)$	-0.2078	-0.5402***	-0.7480***
	(0.2062)	(0.1622)	(0.2833)
Treatment $(t+2)$	0.1242	-0.3772**	-0.2530
	(0.1631)	(0.1779)	(0.2600)
R^2	0.95329	0.93464	0.96228
Observations	10,115	10,115	10,115
Municipality FE	✓	✓	✓
Election-County FE	✓	✓	✓

Notes: The table presents event study results based on Equation 2 for postal, in-person, and total vote shares (0-100)—each as a share of eligible voters—of the party *Christlich Soziale Union* (CSU) (Panel A), and left- and other right-wing parties (Panel B and C). Left-wing parties include *Die Linke* (The Left), the *Sozialdemokratische Partei Deutschlands* (SPD), and *Bündnis 90/Die Grüne* (Alliance 90/The Greens); other 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. 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)

	Postal Party Vote Share (1)	In-person Party Vote Share (2)	Total Party Vote Share (3)
Panel A: CSU Share			
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)	(0.1865)	(0.2543)
Treatment $(t+0)$	0.2838	0.1378	0.4216*
	(0.2010)	(0.2244)	(0.2350)
Treatment $(t+1)$	0.6639***	0.3272	0.9911***
	(0.2098)	(0.2399)	(0.3060)
Treatment $(t+2)$	0.4143*	0.0090	0.4233
	(0.2215)	(0.2349)	(0.2660)
R^2	0.87166	0.95626	0.94471
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 $(t+1)$	0.3672**	-0.4768***	-0.1096
	(0.1610)	(0.1320)	(0.2057)
Treatment $(t+2)$	0.3375**	-0.4315***	-0.0939
	(0.1429)	(0.1467)	(0.1966)
R^2	0.92946	0.94683	0.94760
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 $(t+1)$	-0.1951	-0.6865***	-0.8816**
	(0.2549)	(0.2237)	(0.3571)
Treatment $(t+2)$	0.1086	-0.4380*	-0.3294
	(0.2030)	(0.2249)	(0.3176)
R^2	0.95403	0.92633	0.95988
Observations	10,115	10,115	10,115
Municipality FE	✓	✓	✓
Election-County FE	✓	✓	✓

Notes: The table presents event study results based on Equation 2 for postal, in-person, and total vote shares (0-100)—each as a share of votes cast—of the party *Christlich Soziale Union* (CSU) (Panel A), and left- and other right-wing parties (Panel B and C). Left-wing parties include *Die Linke* (The Left), the *Sozialdemokratische Partei Deutschlands* (SPD), and *Bündnis 90/Die Grüne* (Alliance 90/The Greens); other 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. 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)
Treatment $(t+2)$	0.9625***	-0.8823***	0.0802
	(0.2894)	(0.2761)	(0.1361)
Turnout×Treatment $(t-4)$	0.0269	-0.0650	-0.0381
	(0.1611)	(0.1670)	(0.1351)
Turnout×Treatment $(t-3)$	0.0816	0.0004	0.0820
	(0.1667)	(0.1688)	(0.1396)
Turnout×Treatment $(t-2)$	0.1839	-0.1426	0.0413
	(0.1205)	(0.1260)	(0.1092)
Turnout×Treatment $(t+0)$	0.5420**	-0.1631	0.3788**
	(0.2414)	(0.1934)	(0.1572)
Turnout×Treatment $(t+1)$	0.2008	0.1932	0.3940***
	(0.2373)	(0.1985)	(0.1518)
Turnout×Treatment $(t+2)$	0.5995**	-0.4059*	0.1936
	(0.2599)	(0.2371)	(0.1370)
Observations	10,199	10,199	10,199
R^2	0.96800	0.95469	0.96518
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 the 2018 State Election and is scaled (i.e., mean zero and unitary standard deviation). Standard errors are clustered at the municipality level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table E8: Party Heterogeneity by Past Turnout (Elig. Voters)

	Postal Party Vote Share (1)	In-person Party Vote Share (2)	Total Party Vote Share (3)
Panel A: CSU Share			
Turnout×Treatment $(t-4)$	-0.0167	0.0082	-0.0085
	(0.1240)	(0.1371)	(0.1802)
Turnout×Treatment $(t-3)$	0.0788	0.0138	0.0926
	(0.1227)	(0.1553)	(0.1831)
Turnout×Treatment $(t-2)$	0.0264	-0.0275	-0.0011
	(0.0989)	(0.1218)	(0.1676)
Turnout×Treatment $(t+0)$	0.1107	0.3305**	0.4412***
	(0.1380)	(0.1372)	(0.1680)
Turnout×Treatment $(t+1)$	0.1223	0.4671***	0.5894***
	(0.1334)	(0.1409)	(0.1904)
Turnout×Treatment $(t+2)$	0.1732	0.1867	0.3599**
	(0.1482)	(0.1489)	(0.1802)
R^2	0.90930	0.94744	0.91847
Panel B: Left-wing Parties			
Turnout×Treatment $(t-4)$	0.1396*	0.0091	0.1487
	(0.0755)	(0.1084)	(0.1472)
Turnout×Treatment $(t-3)$	0.0518	-0.0520	-0.0002
	(0.0797)	(0.1161)	(0.1552)
Turnout×Treatment $(t-2)$	0.1222	-0.0951	0.0271
	(0.0750)	(0.0841)	(0.1316)
Turnout×Treatment $(t+0)$	0.2611**	-0.2805***	-0.0194
	(0.1199)	(0.0900)	(0.1350)
Turnout×Treatment $(t+1)$	0.2218**	-0.0808	0.1410
	(0.1077)	(0.0821)	(0.1406)
Turnout×Treatment $(t+2)$	0.1847**	-0.2725***	-0.0878
	(0.0935)	(0.0979)	(0.1372)
R^2	0.94679	0.94900	0.95990
Panel C: Right-wing Parties			
Turnout×Treatment $(t-4)$	-0.1965*	0.0223	-0.1742
	(0.1069)	(0.1420)	(0.2000)
Turnout×Treatment $(t-3)$	-0.2253*	0.0071	-0.2181
	(0.1271)	(0.1527)	(0.2265)
Turnout×Treatment $(t-2)$	0.0035	0.0382	0.0418
	(0.0985)	(0.1081)	(0.1780)
Turnout×Treatment $(t+0)$	-0.0647	-0.1093	-0.1740
	(0.1466)	(0.1244)	(0.1930)
Turnout×Treatment $(t+1)$	-0.2592	-0.1266	-0.3858
	(0.1947)	(0.1370)	(0.2415)
Turnout×Treatment $(t+2)$	0.0037	-0.1889	-0.1851
	(0.1470)	(0.1359)	(0.2064)
R^2	0.95370	0.93840	0.96366
Observations	10,115	10,115	10,115
Municipality FE	✓	✓	✓
Election-County FE	✓	✓	✓

Notes: The table presents triple difference estimates (μ^{τ}) based on Equation 3 for postal, in-person, and total vote shares (0-100)—each as a share of eligible voters—of the party *Christlich Soziale Union* (CSU) (Panel A), and left- and other right-wing parties (Panel B and C). Left-wing parties include *Die Linke* (The Left), the *Sozialdemokratische Partei Deutschlands* (SPD), and *Bündnis 90/Die Grüne* (Alliance 90/The Greens); other 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. The past turnout is held constant to the 2018 State Election and is scaled (i.e., mean zero and unitary standard deviation). Standard errors are clustered at the municipality level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table E9: Party Heterogeneity by Past Turnout (Cast Votes)

	Postal Party Vote Share (1)	In-person Party Vote Share (2)	Total Party Vote Share (3)
Panel A: CSU Share			
Turnout×Treatment $(t-4)$	-0.0602	-0.0335	-0.0938
	(0.1944)	(0.2306)	(0.2731)
Turnout×Treatment $(t-3)$	0.1738	-0.0296	0.1442
	(0.1939)	(0.2586)	(0.2874)
Turnout×Treatment $(t-2)$	-0.0007	-0.0437	-0.0443
	(0.1353)	(0.1679)	(0.2321)
Turnout×Treatment $(t+0)$	0.0731	0.3442*	0.4173*
	(0.1847)	(0.1938)	(0.2241)
Turnout×Treatment $(t+1)$	0.0721	0.4945**	0.5666**
	(0.1785)	(0.2019)	(0.2652)
Turnout×Treatment $(t+2)$	0.1885	0.2259	0.4144*
	(0.1932)	(0.2037)	(0.2372)
R^2	0.87323	0.95659	0.94524
Panel B: Left-wing Parties			
Turnout×Treatment $(t-4)$	0.2511**	0.0238	0.2750
	(0.1191)	(0.1795)	(0.2402)
Turnout×Treatment $(t-3)$	0.1594	-0.0062	0.1532
	(0.1300)	(0.1821)	(0.2518)
Turnout×Treatment $(t-2)$	0.1901*	-0.1187	0.0714
	(0.1073)	(0.1199)	(0.1936)
Turnout×Treatment $(t+0)$	0.2587*	-0.3459***	-0.0872
	(0.1526)	(0.1297)	(0.1877)
Turnout×Treatment $(t+1)$	0.2767*	-0.0872	0.1895
	(0.1439)	(0.1150)	(0.1893)
Turnout×Treatment $(t+2)$	0.2286*	-0.3267**	-0.0981
	(0.1240)	(0.1335)	(0.1876)
R^2	0.93159	0.94953	0.95175
Panel C: Right-wing Parties			
Turnout×Treatment $(t-4)$	-0.2416	0.0238	-0.2416
	(0.1576)	(0.1795)	(0.1576)
Turnout×Treatment $(t-3)$	-0.3105*	-0.0062	-0.3105*
	(0.1867)	(0.1821)	(0.1867)
Turnout×Treatment $(t-2)$	-0.0270	-0.1187	-0.0270
	(0.1394)	(0.1199)	(0.1394)
Turnout×Treatment $(t+0)$	-0.1551	-0.3459***	-0.1551
	(0.1853)	(0.1297)	(0.1853)
Turnout×Treatment $(t+1)$	-0.4634*	-0.0872	-0.4634*
	(0.2468)	(0.1150)	(0.2468)
Turnout×Treatment $(t+2)$	-0.0386	-0.3267**	-0.0386
	(0.1824)	(0.1335)	(0.1824)
R^2	0.95439	0.94953	0.95439
Observations	10,115	10,115	10,115
Municipality FE	✓	✓	✓
Election-County FE	✓	✓	✓

Notes: The table presents triple difference estimates (μ^{τ}) based on Equation 3 for postal, in-person, and total vote shares (0-100)—each as a share of votes cast—of the party *Christlich Soziale Union* (CSU) (Panel A), and left- and other right-wing parties (Panel B and C). Left-wing parties include *Die Linke* (The Left), the *Sozialdemokratische Partei Deutschlands* (SPD), and *Bündnis 90/Die Grüne* (Alliance 90/The Greens); other 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. The past turnout is held constant to the 2018 State Election and is scaled (i.e., mean zero and unitary standard deviation). 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: Median

	Postal Turnout (1)	In-person Turnout (2)	Total Turnout (3)
Panel A: 0%-50% Population Quantile			
Treatment $(t-4)$	-0.3099	0.0938	-0.2161
	(0.4059)	(0.4789)	(0.3408)
Treatment $(t-3)$	-0.6754	0.3917	-0.2837
	(0.4225)	(0.4905)	(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
	(0.5548)	(0.5414)	(0.2833)
Treatment $(t+2)$	1.561**	-1.704**	-0.1427
	(0.6917)	(0.7562)	(0.4514)
R^2	0.96767	0.94899	0.95918
Panel B: 50%-100% Population 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)	(0.2042)	(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)
Treatment $(t+2)$	0.6624**	-0.5093	0.1531
	(0.2951)	(0.3315)	(0.2104)
R^2	0.96767	0.94899	0.95918
Observations	10,199	10,199	10,199
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). Marginal effects for each category, instead of coefficients of interaction terms, are shown. The reference election $(t-1)$ is the 2018 State Election and it is controlled for the population as in Figure 3. The median is 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 Population Size: Terciles

	Postal Turnout (1)	In-person Turnout (2)	Total Turnout (3)
Panel A: 0%-33% Population Quantile			
Treatment $(t-4)$	-0.1780	0.5974	0.4194
	(0.7072)	(0.9099)	(0.6356)
Treatment $(t-3)$	-0.4204	0.8593	0.4389
	(0.7253)	(0.9491)	(0.4949)
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
	(0.9634)	(0.9462)	(0.4689)
Treatment $(t+2)$	1.387	-0.4707	0.9159
	(1.164)	(1.166)	(0.5850)
R^2	0.96766	0.94897	0.95917
Panel B: 33%-67% Population Quantile			
Treatment $(t-4)$	-0.8323**	0.3034	-0.5289*
	(0.3350)	(0.3276)	(0.2889)
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
	(0.4690)	(0.4501)	(0.2594)
Treatment $(t+2)$	0.8603*	-1.365**	-0.5051
	(0.5193)	(0.6056)	(0.4117)
R^2	0.96771	0.94905	0.95915
Panel C: 67%-100% Population 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)	(0.2248)	(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)	(0.3058)	(0.1656)
Treatment $(t+1)$	0.5723*	-0.6573**	-0.0849
	(0.2963)	(0.2835)	(0.1867)
Treatment $(t+2)$	0.7325**	-0.5346	0.1978
	(0.3308)	(0.3648)	(0.2095)
R^2	0.96771	0.94908	0.95915
Observations	10,199	10,199	10,199
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). Marginal effects for each category, instead of coefficients of interaction terms, are shown. The reference election $(t-1)$ is the 2018 State Election and it is controlled for the population as in Figure 3. The terciles 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 E12: Heterogeneity by Population Size: Quintiles

	Postal Turnout (1)	In-person Turnout (2)	Total Turnout (3)
Panel A: 0%-20% Population Quantile			
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)	(0.6004)
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)	(0.6337)
Treatment $(t+2)$	1.316	0.8194	2.135***
	(1.593)	(1.404)	(0.6902)
R^2	0.96762	0.94898	0.95919
Panel B: 20%-40% Population Quantile			
Treatment $(t-4)$	0.1471	-0.3146	-0.1674
	(0.6283)	(0.5673)	(0.4733)
Treatment $(t-3)$	-0.2203	-0.4582	-0.6785*
	(0.7331)	(0.6932)	(0.3520)
Treatment $(t-2)$	-0.3223	-0.2000	-0.5224
	(0.5147)	(0.4382)	(0.3952)
Treatment $(t+0)$	1.943*	-1.914*	0.0294
	(1.006)	(0.9895)	(0.3350)
Treatment $(t+1)$	1.532*	-1.804**	-0.2722
	(0.8279)	(0.8840)	(0.3883)
Treatment $(t+2)$	2.116**	-2.633**	-0.5173
	(1.076)	(1.127)	(0.4696)
R^2	0.96767	0.94905	0.95913
Panel C: 40%-60% Population Quantile			
Treatment $(t-4)$	-1.004**	0.5720	-0.4319
	(0.4258)	(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)	(0.3563)
Treatment $(t+1)$	0.2881	-0.5281	-0.2400
	(0.5334)	(0.5457)	(0.3179)
Treatment $(t+2)$	0.4267	-0.9561	-0.5294
	(0.6063)	(0.7744)	(0.5525)
R^2	0.96767	0.94899	0.95914
Panel D: 60%-80% Population 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)	(0.3495)	(0.2761)
Treatment $(t-2)$	0.2405	-0.0102	0.2302
	(0.2505)	(0.2980)	(0.2545)
Treatment $(t+0)$	1.617***	-0.9739*	0.6434**
	(0.5428)	(0.5512)	(0.3173)
Treatment $(t+1)$	0.7703	-0.9091*	-0.1388
	(0.5430)	(0.4945)	(0.3097)
Treatment $(t+2)$	0.8799	-0.6276	0.2523
	(0.5586)	(0.5698)	(0.3425)
R^2	0.96768	0.94902	0.95916
Panel E: 80%-100% Population Quantile			
Treatment $(t-4)$	0.5435**	-0.3059	0.2377
	(0.2597)	(0.2728)	(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)	(0.1861)
Treatment $(t+1)$	0.6081*	-0.6649**	-0.0569
	(0.3444)	(0.3366)	(0.2095)
Treatment $(t+2)$	0.7567**	-0.6238	0.1329
	(0.3855)	(0.4477)	(0.2514)
R^2	0.96773	0.94904	0.95916
Observations	10,199	10,199	10,199
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). Marginal effects for each category, instead of coefficients of interaction terms, are shown. The reference election $(t-1)$ is the 2018 State Election and it is controlled for the population as in Figure 3. The quintiles 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 E13: Heterogeneity by Age

	Postal Turnout (1)	In-person Turnout (2)	Total Turnout (3)
Panel A: Share of 15 to 17 year old			
$Z\times$Treatment $(t-4)$	0.1657	-0.2538	-0.0881
	(0.1961)	(0.2042)	(0.1676)
$Z\times$Treatment $(t-3)$	0.1519	-0.2347	-0.0828
	(0.1942)	(0.2039)	(0.1604)
$Z\times$Treatment $(t-2)$	-0.0047	-0.1934	-0.1981
	(0.1305)	(0.1574)	(0.1277)
$Z\times$Treatment $(t+0)$	-0.0667	0.0563	-0.0104
	(0.2751)	(0.2741)	(0.1435)
$Z\times$Treatment $(t+1)$	-0.0266	0.1986	0.1720
	(0.2788)	(0.2569)	(0.1490)
$Z\times$Treatment $(t+2)$	0.0224	-0.1252	-0.1028
	(0.3056)	(0.3187)	(0.1901)
R^2	0.96780	0.94911	0.95917
Panel B: Share of 18 to 19 year old			
$Z\times$Treatment $(t-4)$	-0.4392**	0.2120	-0.2273
	(0.1979)	(0.2287)	(0.1884)
$Z\times$Treatment $(t-3)$	-0.4139*	0.3832*	-0.0306
	(0.2115)	(0.2295)	(0.1753)
$Z\times$Treatment $(t-2)$	0.0289	-0.0843	-0.0554
	(0.1474)	(0.1653)	(0.1521)
$Z\times$Treatment $(t+0)$	-0.3415	0.4814	0.1399
	(0.3140)	(0.3149)	(0.1825)
$Z\times$Treatment $(t+1)$	-0.5808**	0.5838**	0.0030
	(0.2875)	(0.2688)	(0.1780)
$Z\times$Treatment $(t+2)$	-0.5286	0.5885*	0.0599
	(0.3448)	(0.3550)	(0.2136)
R^2	0.96767	0.94903	0.95919
Panel C: Share of 20 to 29 year old			
$Z\times$Treatment $(t-4)$	0.1505	-0.1413	0.0093
	(0.1827)	(0.1914)	(0.1581)
$Z\times$Treatment $(t-3)$	0.0948	-0.1179	-0.0231
	(0.1892)	(0.1956)	(0.1595)
$Z\times$Treatment $(t-2)$	0.0036	0.1287	0.1323
	(0.1289)	(0.1460)	(0.1241)
$Z\times$Treatment $(t+0)$	-0.6467**	0.9184***	0.2716*
	(0.3092)	(0.2972)	(0.1563)
$Z\times$Treatment $(t+1)$	-0.5274*	0.6849**	0.1575
	(0.2882)	(0.2705)	(0.1590)
$Z\times$Treatment $(t+2)$	-0.5769*	1.047***	0.4701**
	(0.3131)	(0.3284)	(0.2144)
R^2	0.96773	0.94917	0.95935
Panel D: Share of 30 to 59 year old			
$Z\times$Treatment $(t-4)$	-0.0050	-0.0566	-0.0616
	(0.1664)	(0.2031)	(0.1470)
$Z\times$Treatment $(t-3)$	-0.1383	-0.0936	-0.2320
	(0.1696)	(0.2030)	(0.1496)
$Z\times$Treatment $(t-2)$	0.1308	-0.2032	-0.0724
	(0.1420)	(0.1427)	(0.1233)
$Z\times$Treatment $(t+0)$	0.1587	0.0715	0.2302
	(0.2469)	(0.2732)	(0.1407)
$Z\times$Treatment $(t+1)$	0.0445	0.1198	0.1643
	(0.2214)	(0.2360)	(0.1203)
$Z\times$Treatment $(t+2)$	0.1221	-0.0166	0.1055
	(0.2617)	(0.3198)	(0.1711)
R^2	0.96773	0.94899	0.95942
Panel E: Share of over 60 year old			
$Z\times$Treatment $(t-4)$	-0.3130*	0.3925**	0.0795
	(0.1749)	(0.1931)	(0.1475)
$Z\times$Treatment $(t-3)$	-0.2180	0.3381*	0.1200
	(0.1792)	(0.2046)	(0.1516)
$Z\times$Treatment $(t-2)$	-0.2740**	0.3012**	0.0272
	(0.1346)	(0.1453)	(0.1167)
$Z\times$Treatment $(t+0)$	0.2200	-0.4333	-0.2133
	(0.2536)	(0.2800)	(0.1589)
$Z\times$Treatment $(t+1)$	0.4672*	-0.5412**	-0.0741
	(0.2459)	(0.2479)	(0.1386)
$Z\times$Treatment $(t+2)$	0.1938	-0.3321	-0.1383
	(0.2800)	(0.3435)	(0.1877)
R^2	0.96812	0.94934	0.95956
Observations	10,199	10,199	10,199
Municipality FE	✓	✓	✓
Election-County FE	✓	✓	✓

Notes: The table presents triple difference estimates (μ^{τ}) 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 (i.e., mean zero and unitary standard deviation). Standard errors are clustered at the municipality level and reported in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table E14: Heterogeneity by Socio-Economic Characteristics

	Postal Turnout (1)	In-person Turnout (2)	Total Turnout (3)
Panel A: Unemployment			
$Z\times$Treatment $(t-4)$	-0.0874	0.1714	0.0840
	(0.1855)	(0.1939)	(0.1338)
$Z\times$Treatment $(t-3)$	-0.0473	0.0452	-0.0021
	(0.1951)	(0.2041)	(0.1416)
$Z\times$Treatment $(t-2)$	-0.1447	0.1340	-0.0106
	(0.1239)	(0.1516)	(0.1285)
$Z\times$Treatment $(t+0)$	-0.1538	-0.1276	-0.2814*
	(0.2257)	(0.2350)	(0.1632)
$Z\times$Treatment $(t+1)$	0.1790	-0.3240	-0.1450
	(0.2156)	(0.2165)	(0.1400)
$Z\times$Treatment $(t+2)$	-0.0356	0.0258	-0.0098
	(0.2445)	(0.2787)	(0.1889)
R^2	0.96868	0.95201	0.95977
Panel B: Share Female			
$Z\times$Treatment $(t-4)$	0.3913*	-0.0664	0.3249**
	(0.2013)	(0.2221)	(0.1553)
$Z\times$Treatment $(t-3)$	0.4076**	-0.1549	0.2527
	(0.2034)	(0.2191)	(0.1652)
$Z\times$Treatment $(t-2)$	0.1824	-0.2295	-0.0472
	(0.1282)	(0.1508)	(0.1322)
$Z\times$Treatment $(t+0)$	0.2301	-0.6431**	-0.4130**
	(0.2853)	(0.2900)	(0.1769)
$Z\times$Treatment $(t+1)$	0.2867	-0.5715**	-0.2848
	(0.2757)	(0.2524)	(0.1820)
$Z\times$Treatment $(t+2)$	0.1509	-0.6701**	-0.5192***
	(0.2842)	(0.3162)	(0.1932)
R^2	0.96777	0.94944	0.95931
Panel C: Households with Children			
$Z\times$Treatment $(t-4)$	-0.6739**	0.4847	-0.1892
	(0.2799)	(0.3432)	(0.2451)
$Z\times$Treatment $(t-3)$	-0.8007***	0.6339*	-0.1668
	(0.2985)	(0.3494)	(0.2180)
$Z\times$Treatment $(t-2)$	-0.2507	-0.0903	-0.3410*
	(0.2279)	(0.2626)	(0.1841)
$Z\times$Treatment $(t+0)$	-0.0351	0.3201	0.2850
	(0.5055)	(0.4595)	(0.2073)
$Z\times$Treatment $(t+1)$	-0.0726	0.1276	0.0549
	(0.5177)	(0.4160)	(0.2586)
$Z\times$Treatment $(t+2)$	0.3952	-0.2072	0.1879
	(0.5104)	(0.5054)	(0.2578)
R^2	0.96775	0.94906	0.95917
Observations	10,199	10,199	10,199
Municipality FE	✓	✓	✓
Election-County FE	✓	✓	✓

Notes: The table presents triple difference estimates (μ^{τ}) 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 (i.e., mean zero and unitary standard deviation). Standard errors are clustered at the municipality level and reported in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table E15: Placebos

	(1)	(2)	(3)	(4)
Panel A: Postal Turnout				
Treatment $(t-4)$	0.0218		-0.3531	-0.0383
	(0.1885)		(0.6326)	(0.3035)
Treatment $(t-3)$	-0.1075		-0.3512	-0.0953
	(0.1898)		(0.6363)	(0.2912)
Treatment $(t-2)$	-0.0658	-0.0094	0.0088	-0.2497
	(0.1286)	(0.1353)	(0.3066)	(0.2285)
Treatment $(t+0)$	1.122***	0.2025	-0.2267	0.3650
	(0.2741)	(0.2934)	(0.9565)	(0.3463)
Treatment $(t+1)$	0.6601***	-0.0972	-0.3613	-0.1458
	(0.2512)	(0.3259)	(0.5619)	(0.3791)
Treatment $(t+2)$	0.8988***	-0.2339	-0.4560	-0.2728
	(0.2862)	(0.4836)	(0.7441)	(0.3640)
Treatment $(t+3)$		-0.4263		
		(0.5130)		
Treatment $(t+4)$		-0.5364		
		(0.5043)		
R^2	0.96761	0.97011	0.98373	0.96552
Panel B: In-person Turnout				
Treatment $(t-4)$	-0.0396		-0.8388	0.0402
	(0.2040)		(0.7495)	(0.2831)
Treatment $(t-3)$	0.0941		-0.8290	0.1590
	(0.2022)		(0.7507)	(0.2541)
Treatment $(t-2)$	-0.0433	-0.0534	-0.6765	0.3019
	(0.1454)	(0.1726)	(0.8040)	(0.1984)
Treatment $(t+0)$	-0.7456***	-0.1125	-0.4943	-0.1235
	(0.2819)	(0.2772)	(1.077)	(0.3240)
Treatment $(t+1)$	-0.7305***	0.0770	0.3289	0.3850
	(0.2436)	(0.3436)	(0.5616)	(0.3399)
Treatment $(t+2)$	-0.8279***	0.1695	-0.3511	0.0342
	(0.3191)	(0.4327)	(0.8685)	(0.3917)
Treatment $(t+3)$		0.3202		
		(0.4671)		
Treatment $(t+4)$		0.3791		
		(0.5113)		
R^2	0.94892	0.95103	0.98091	0.94876
Panel C: Total Turnout				
Treatment $(t-4)$	-0.0178		-1.192**	0.0019
	(0.1503)		(0.5544)	(0.2212)
Treatment $(t-3)$	-0.0134		-1.180**	0.0637
	(0.1498)		(0.5480)	(0.1985)
Treatment $(t-2)$	-0.1091	-0.0628	-0.6677	0.0522
	(0.1231)	(0.1705)	(0.7653)	(0.2012)
Treatment $(t+0)$	0.3765***	0.0900	-0.7210	0.2414
	(0.1436)	(0.2475)	(0.6455)	(0.2602)
Treatment $(t+1)$	-0.0705	-0.0202	-0.0324	0.2391
	(0.1386)	(0.2693)	(0.6838)	(0.2627)
Treatment $(t+2)$	0.0710	-0.0644	-0.8070	-0.2386
	(0.1901)	(0.3294)	(1.056)	(0.2988)
Treatment $(t+3)$		-0.1061		
		(0.3527)		
Treatment $(t+4)$		-0.1573		
		(0.3602)		
R^2	0.95911	0.96329	0.98314	0.94085
Specification	Baseline	Time	Geographical	Treatment
Observations	10,199	6,377	546	4,130
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). Column (1) are the main results, shown in Figure 3. Column (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 E16: Main Results with Randomized Inference

	Postal Turnout	In-person Turnout	Total Turnout
	(1)	(2)	(3)
Treatment $(t-4)$	0.0218	-0.0396	-0.0178
	(0.015)	(0.018)	(0.015)
	[0.913]	[0.859]	[0.911]
Treatment $(t-3)$	-0.1075	0.0941	-0.0134
	(0.025)	(0.025)	(0.012)
	[0.654]	[0.666]	[0.948]
Treatment $(t-2)$	-0.0658	-0.0433	-0.1091
	(0.024)	(0.022)	(0.026)
	[0.677]	[0.774]	[0.413]
Treatment $(t+0)$	1.122***	-0.7456***	0.3765**
	(<0.001)	(0.003)	(0.010)
	[<0.001]	[0.004]	[0.040]
Treatment $(t+1)$	0.6601**	-0.7305***	-0.0705
	(0.005)	(0.004)	(0.024)
	[0.011]	[0.006]	[0.688]
Treatment $(t+2)$	0.8988***	-0.8279***	0.0710
	(0.002)	(0.003)	(0.022)
	[0.001]	[0.003]	[0.754]
Observations	10,199	10,199	10,199
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.

Table E17: Additional Controls

	(1)	(2)
Panel A: Postal Turnout		
Treatment $(t-4)$	-0.0185	0.0129
	(0.1893)	(0.1905)
Treatment $(t-3)$	-0.1477	-0.1130
	(0.1881)	(0.1891)
Treatment $(t-2)$	-0.0440	-0.0993
	(0.1285)	(0.1276)
Treatment $(t+0)$	1.168***	1.215***
	(0.2743)	(0.2646)
Treatment $(t+1)$	0.6952***	0.7438***
	(0.2470)	(0.2416)
Treatment $(t+2)$		0.9548***
		(0.2763)
R^2	0.97077	0.96997
Panel B: In-person Turnout		
Treatment $(t-4)$	-0.0260	-0.0307
	(0.2063)	(0.2028)
Treatment $(t-3)$	0.1077	0.0913
	(0.2025)	(0.2014)
Treatment $(t-2)$	-0.0476	-0.0741
	(0.1475)	(0.1445)
Treatment $(t+0)$	-0.7731***	-0.9031***
	(0.2820)	(0.2691)
Treatment $(t+1)$	-0.7213***	-0.8293***
	(0.2424)	(0.2342)
Treatment $(t+2)$		-0.9585***
		(0.2939)
R^2	0.95949	0.95372
Panel C: Total Turnout		
Treatment $(t-4)$	-0.0445	-0.0178
	(0.1516)	(0.1504)
Treatment $(t-3)$	-0.0400	-0.0216
	(0.1479)	(0.1488)
Treatment $(t-2)$	-0.0916	-0.1734
	(0.1241)	(0.1197)
Treatment $(t+0)$	0.3954***	0.3114**
	(0.1445)	(0.1411)
Treatment $(t+1)$	-0.0261	-0.0855
	(0.1369)	(0.1405)
Treatment $(t+2)$		-0.0037
		(0.1753)
Controls	Invariant	Varying
R^2	0.95566	0.96166
Observations	8,742	10,199
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. Column (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 E18: Population Controls

	(1)	(2)	(3)	(4)
Panel A: Postal Turnout				
Treatment $(t-4)$	0.0218	0.0014	0.0168	0.0835
	(0.1885)	(0.1878)	(0.1886)	(0.1912)
Treatment $(t-3)$	-0.1075	-0.1278	-0.1145	-0.0197
	(0.1898)	(0.1892)	(0.1901)	(0.1936)
Treatment $(t-2)$	-0.0658	-0.0735	-0.0708	-0.0014
	(0.1286)	(0.1289)	(0.1287)	(0.1292)
Treatment $(t+0)$	1.122***	1.153***	1.117***	1.063***
	(0.2741)	(0.2740)	(0.2743)	(0.2718)
Treatment $(t+1)$	0.6601***	0.7025***	0.6545***	0.5202**
	(0.2512)	(0.2503)	(0.2518)	(0.2524)
Treatment $(t+2)$	0.8988***	0.9318***	0.8933***	0.8219***
	(0.2862)	(0.2861)	(0.2865)	(0.2840)
R^2	0.96761	0.96781	0.96760	0.96725
Panel B: In-person Turnout				
Treatment $(t-4)$	-0.0396	-0.0266	-0.0012	-0.1046
	(0.2040)	(0.2039)	(0.2033)	(0.2056)
Treatment $(t-3)$	0.0941	0.0991	0.1339	0.0761
	(0.2022)	(0.2023)	(0.2015)	(0.2019)
Treatment $(t-2)$	-0.0433	-0.0548	-0.0081	-0.0060
	(0.1454)	(0.1455)	(0.1455)	(0.1439)
Treatment $(t+0)$	-0.7456***	-0.7770***	-0.7052**	-0.6613**
	(0.2819)	(0.2816)	(0.2822)	(0.2818)
Treatment $(t+1)$	-0.7305***	-0.7584***	-0.6875***	-0.6301***
	(0.2436)	(0.2430)	(0.2443)	(0.2440)
Treatment $(t+2)$	-0.8279***	-0.8752***	-0.7825**	-0.6879**
	(0.3191)	(0.3178)	(0.3201)	(0.3195)
R^2	0.94892	0.94916	0.94883	0.94854
Panel C: Total Turnout				
Treatment $(t-4)$	-0.0178	-0.0252	0.0156	-0.0212
	(0.1503)	(0.1504)	(0.1510)	(0.1488)
Treatment $(t-3)$	-0.0134	-0.0287	0.0193	0.0565
	(0.1498)	(0.1498)	(0.1505)	(0.1498)
Treatment $(t-2)$	-0.1091	-0.1283	-0.0789	-0.0074
	(0.1231)	(0.1231)	(0.1239)	(0.1265)
Treatment $(t+0)$	0.3765***	0.3760***	0.4121***	0.4018***
	(0.1436)	(0.1439)	(0.1437)	(0.1445)
Treatment $(t+1)$	-0.0705	-0.0559	-0.0329	-0.1099
	(0.1386)	(0.1387)	(0.1388)	(0.1398)
Treatment $(t+2)$	0.0710	0.0565	0.1108	0.1340
	(0.1901)	(0.1900)	(0.1896)	(0.1918)
R^2	0.95911	0.95920	0.95908	0.95884
Specification	Baseline	Popul. (inv.)	Elig. voters (var.)	Size categ. (inv.)
Observations	10,199	10,199	10,199	10,199
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. Column (1) are the main results shown in Figure 3. 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 E19: Robustness Matching

	(1)	(2)	(3)	(4)
Panel A: Postal Turnout				
Treatment $(t-4)$	-0.1472	-0.1178	0.0059	0.0311
	(0.1907)	(0.2196)	(0.3034)	(0.1724)
Treatment $(t-3)$	-0.2958	-0.1854	-0.1066	-0.0821
	(0.1924)	(0.2231)	(0.3017)	(0.1728)
Treatment $(t-2)$	-0.1706	-0.0584	-0.0292	-0.0496
	(0.1296)	(0.1516)	(0.2254)	(0.1202)
Treatment $(t+0)$	1.034***	1.251***	1.280***	0.9789***
	(0.2815)	(0.3194)	(0.3807)	(0.2592)
Treatment $(t+1)$	0.6034**	0.8596***	0.7299*	0.5335**
	(0.2540)	(0.2966)	(0.3964)	(0.2356)
Treatment $(t+2)$	0.8363***	1.085***	0.8485**	0.7461***
	(0.2874)	(0.3320)	(0.4288)	(0.2674)
R^2	0.97044	0.97620	0.97615	0.97421
Panel B: In-person Turnout				
Treatment $(t-4)$	0.0831	-0.0232	-0.1360	-0.0208
	(0.2102)	(0.2364)	(0.3021)	(0.1849)
Treatment $(t-3)$	0.2328	-0.0347	-0.0309	0.1136
	(0.2053)	(0.2402)	(0.3005)	(0.1840)
Treatment $(t-2)$	0.0418	-0.0960	-0.2061	-0.0105
	(0.1449)	(0.1693)	(0.2549)	(0.1370)
Treatment $(t+0)$	-0.6408**	-0.8051**	-0.9130**	-0.6167**
	(0.2863)	(0.3246)	(0.3846)	(0.2690)
Treatment $(t+1)$	-0.6205**	-0.7117**	-0.7537**	-0.5613**
	(0.2501)	(0.2831)	(0.3640)	(0.2344)
Treatment $(t+2)$	-0.7140**	-0.9765***	-0.8648*	-0.6533**
	(0.3205)	(0.3713)	(0.4517)	(0.3033)
R^2	0.95349	0.96252	0.96405	0.95797
Panel C: Total Turnout				
Treatment $(t-4)$	-0.0641	-0.1410	-0.1300	0.0102
	(0.1500)	(0.1813)	(0.2627)	(0.1377)
Treatment $(t-3)$	-0.0630	-0.2201	-0.1375	0.0316
	(0.1449)	(0.1769)	(0.2430)	(0.1412)
Treatment $(t-2)$	-0.1289	-0.1544	-0.2353	-0.0602
	(0.1185)	(0.1531)	(0.1975)	(0.1212)
Treatment $(t+0)$	0.3934***	0.4457***	0.3666*	0.3622***
	(0.1394)	(0.1663)	(0.2177)	(0.1328)
Treatment $(t+1)$	-0.0171	0.1479	-0.0238	-0.0278
	(0.1409)	(0.1833)	(0.2525)	(0.1356)
Treatment $(t+2)$	0.1223	0.1088	-0.0163	0.0928
	(0.1881)	(0.2374)	(0.3039)	(0.1837)
R^2	0.96671	0.97765	0.97811	0.97572
Procedure	Local	Propensity	Mahalanobis	Entropy Bal.
SE cluster	Municipality	Municipality	TW Match & Cntrl. Gr.×Elctn.	Municipality
Observations	7,497	2,870	2,870	10,199
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). Column (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. Mahalanobis matching adjusts standard errors for two-way clustering at the level of the matched pair and the control group×election level, following Colmer et al. (2025), and reported in parentheses. For the other approaches, standard errors are clustered at the municipality level and reported in parentheses.

Table E20: Number of Runoffs

	(1)	(2)	(3)
Panel A: Postal Turnout			
Treatment $(t-4)$	-0.1965	-0.1649	-0.1719
	(0.3088)	(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
	(0.2059)	(0.1676)	(0.1606)
Treatment $(t+0)$	1.014**	1.153***	1.226***
	(0.4513)	(0.3628)	(0.3485)
Treatment $(t+1)$	1.074***	0.8079**	0.8718***
	(0.3909)	(0.3268)	(0.3172)
Treatment $(t+2)$	1.595***	1.223***	1.277***
	(0.4541)	(0.3662)	(0.3540)
R^2	0.97430	0.97397	0.97383
Panel B: In-person Turnout			
Treatment $(t-4)$	0.1348	0.1803	0.1975
	(0.3490)	(0.2745)	(0.2599)
Treatment $(t-3)$	0.2368	0.3409	0.3564
	(0.3466)	(0.2713)	(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*
	(0.4471)	(0.3653)	(0.3533)
Treatment $(t+1)$	-0.8794**	-0.7429**	-0.7502**
	(0.3862)	(0.3206)	(0.3100)
Treatment $(t+2)$	-1.207**	-0.8653**	-0.9296**
	(0.4862)	(0.3996)	(0.3873)
Panel C: Total Turnout			
Treatment $(t-4)$	-0.0617	0.0154	0.0256
	(0.2523)	(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
	(0.2125)	(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)
Treatment $(t+2)$	0.3885	0.3581	0.3473
	(0.2932)	(0.2358)	(0.2224)
R^2	0.97143	0.97368	0.97418
Sample	1 Runoff	1-2 Runoffs	1-3 Runoffs
Observations	2,471	3,171	3,311
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 E21: Off-Cycle Control Group

	(1)	(2)
Panel A: Postal Turnout		
Treatment $(t-4)$	-0.5693*	-0.6547
	(0.3315)	(0.7892)
Treatment $(t-3)$	-0.7018**	-0.7604
	(0.3285)	(0.8210)
Treatment $(t-2)$	-0.2438	-0.0268
	(0.2491)	(0.7135)
Treatment $(t+0)$	1.404***	1.924
	(0.4482)	(1.503)
Treatment $(t+1)$	1.057**	2.302*
	(0.4503)	(1.251)
Treatment $(t+2)$	1.296***	3.087**
	(0.4940)	(1.421)
R^2	0.97921	0.98004
Panel B: In-person Turnout		
Treatment $(t-4)$	0.6356*	0.3198
	(0.3584)	(0.8573)
Treatment $(t-3)$	0.6686*	0.4668
	(0.3521)	(0.9989)
Treatment $(t-2)$	-0.0210	0.0556
	(0.3015)	(0.8251)
Treatment $(t+0)$	-0.9990**	-1.430
	(0.4672)	(1.576)
Treatment $(t+1)$	-1.299***	-2.716*
	(0.4431)	(1.432)
Treatment $(t+2)$	-1.435***	-3.210**
	(0.5220)	(1.497)
R^2	0.96476	0.96649
Panel C: Total Turnout		
Treatment $(t-4)$	0.0663	-0.3349
	(0.2768)	(0.5241)
Treatment $(t-3)$	-0.0333	-0.2936
	(0.3168)	(0.7734)
Treatment $(t-2)$	-0.2648	0.0288
	(0.2880)	(0.7231)
Treatment $(t+0)$	0.4050	0.4943
	(0.2832)	(0.8569)
Treatment $(t+1)$	-0.2416	-0.4141
	(0.3059)	(0.6627)
Treatment $(t+2)$	-0.1382	-0.1239
	(0.3438)	(0.7594)
R^2	0.98050	0.98330
Off-Cycle	All	Runoff
Observations	2,023	1,526
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. In both specifications, it is controlled for the population as in Figure 3. Standard errors are clustered at the municipality level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.