

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

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Abstract

We investigate how forced experimentation with postal voting affects later voting behavior. In Bavaria’s 2020 Mayoral Elections, all runoff ballots were cast by mail, while subsequent elections reverted to prior rules. Using municipality-level administrative data from seven general elections (2013–2025) and an event study design, we find a transitory increase in total turnout of 0.4 percentage points in the first post-treatment election and a persistent substitution from in-person to postal voting, lasting at least five years. The largest conservative party, the CSU, temporarily gains up to 0.6 percentage points at the expense of other right-wing parties. Heterogeneity analysis shows an age gradient, with the oldest municipalities experiencing the largest increases in postal voting, consistent with larger reductions in information costs for these municipalities.

Keywords: Postal voting, Voter turnout, Information costs

JEL-Codes: D72, H70, D83

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1 Introduction

Temporary policies can have persistent effects when they reduce the information costs of an existing choice. This mechanism is related to the Porter Hypothesis, which posits that regulation can induce agents to discover lower-cost practices (Porter, 1991). Although Porter studies environmental regulation, the mechanism applies more broadly. Larcom et al. (2017) show that London Underground strikes led some commuters to discover faster routes they retained permanently. We study this mechanism in voting. In March 2020, the Bavarian State Government mandated that all runoffs in the local elections be conducted entirely by postal ballot in response to the surge in COVID-19 cases.¹ Postal ballots were sent automatically for this one election. Thereafter, voting rules reverted to their original form.

This paper investigates the impact of this one-time all-postal runoff on subsequent voting behavior in the three high-salience general elections through the 2025 Federal Election. Using municipality-level administrative data, 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 produces a transitory increase in total turnout (in $t + 0$, the 2021 Federal Election) and a persistent substitution from in-person to postal voting, even five 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 decompose 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.

¹ The most common regulatory response concerning organizing elections during the pandemic was to postpone them, as was the case, for example, in Poland, Austria, France, and Spain. The citizens of Vorarlberg, Austria, were additionally encouraged to vote by mail (Radjenovic et al., 2020). Related work studies how COVID-19 and other crises affect turnout, with mixed findings: some studies find lower turnout (Campante et al., 2024; Fernandez-Navia et al., 2021; Leromain and Vannoorenberghe, 2022; Picchio and Santolini, 2022; Gasper and Reeves, 2011; Stancea and Muntean, 2024), while Giommoni and Loumeau (2022) find that stricter COVID-19 restrictions increased participation. Baskaran et al. (2023) find that the pandemic reduced incumbency advantage and favored more competent candidates.

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

Our paper contributes to the literature on how altered voting costs affect voting behavior. There is extensive literature on how changing electoral costs through administrative policies affects voter participation.⁴ This literature primarily focuses on altered *in-person* voting costs. Findings on how administrative changes in *absentee* voting costs affect voter turnout diverge. 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,

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, 28.6 (37.3) percent of votes cast 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. Throughout 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 and are permanently eliminated 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). 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 (Bagwe et al., 2026; Alipour and Lindlacher, 2025). 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).

identifying convenience and flexibility offered by postal voting, and therefore reduced voting costs, as key factors. This effect may be attenuated where postal voting diminishes the social pressure to vote, as Funk (2010) shows for small Swiss municipalities. Moreover, unanticipated consequences of early voting may 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. Note that our analysis focuses on general elections after the all-postal election. Although the first post-treatment election coincided with the pandemic, health concerns and public messaging affected treated and control voters alike. The observed differences are therefore more consistent with a permanent reduction in information costs than with pandemic-specific influences.

The all-postal runoff in March 2020 can be interpreted 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 had never voted by mail before made 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. Frank et al. (2023) use the same policy change as we do and show that for Bavarian municipalities with more than 10,000 inhabitants, turnout was about 10 percentage points higher in the all-postal 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 approximately 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 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. Postal voting increased sharply in 2021, reaching levels that exceeded those in both 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. Within a rational choice framework (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 sole voting mode; its procedure was also simplified in this one election, as all eligible voters received postal ballot papers automatically instead of requesting them. Hence, eligible voters who came in contact with postal voting for the first time during that election may *perceive* different postal voting costs from those of regular elections. When they first request a postal ballot in the 2021 Federal Election, they may again adjust their postal voting costs, this time to a *higher* value. This predicts a lower postal turnout in the 2023 State Election in comparison to the 2021 Federal Election, but not to pre-treatment elections. Subsequently, voting costs stabilize, and we expect no further turnout changes in the 2025 Federal Election.

Our findings show meaningful and lasting effects of the experimentation with postal voting. On average, the all-postal runoff causes a transitory 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 larger increase in

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

postal turnout compared 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 vanishing effect on total turnout, while the point estimate for in-person turnout remains at -0.7 . In the 2025 Federal Election ($t+2$), five years after the treatment, estimates are similar to the previous election ($t + 1$). A postal turnout increase of 0.9 percentage points is mirrored by an in-person turnout decline of 0.8 percentage points, resulting in no alterations in total turnout. Consequently, while the switch from in-person to postal voting persists, the increase in total turnout is transitory. This persistent mode substitution of about 2 percent is large enough to be administratively relevant. A back-of-the-envelope calculation indicates that municipalities recover the additional postal voting costs by cutting only about 1 percent of polling place costs, equivalent to roughly one municipality in 26 closing a single polling place.

This pattern resembles findings from 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 is no unfamiliarity with the act of voting itself. Its abolition does not alter voting costs relative to before its introduction. However, our setting 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 a further adjustment in the second post-treatment election. While in their study some eligible voters are inattentive to a polling place relocation and miss the deadline to request mail-in ballots, turning to postal voting only in the next election, we rationalize our finding by a second adjustment of the postal voting costs.⁷

opening hours of polling 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 However, some eligible voters may have realized too late that they needed to request postal voting. These voters would also update their postal voting costs for the 2023 State Election, but would not vote by mail in the 2021 Federal Election.

One concern may be that other circumstances surrounding the all-postal runoff drive the estimated effects. We first show that the treatment status is uncorrelated with variables related to the pandemic: School closures prior to the 2020 Local Elections and COVID-19 deaths prior to the 2021 Federal Election and 2023 State Election are uncorrelated with the treatment indicator. Similarly, socio-economic characteristics lack statistical significance in cross-sectional balance regressions. Moreover, we conduct placebo tests by shifting the treatment back in time (using the 2014 Local Elections in Bavaria), to another German state (using the postponed local elections in Hesse from 2020 and 2021), and to a different treatment definition (using municipalities that had an all-postal election due to a runoff in the 2020 County Commissioner Election). In all three exercises, we find no effects.

In our setting, postal voting becomes universal for only one election. We can therefore investigate lasting changes in electoral participation and the mode of voting where administrative voting costs are constant. Our study adds to the existing body of research on habit formation in voting.⁸ We find that shifted voting costs lead to statistically significant deviations in the first election after the treatment. These deviations are difficult to reconcile with strong habit formation, which predicts that voters adhere to established routines regardless of cost changes. Instead, the evidence is more consistent with rational cost-based adjustment in a Downsian framework (Downs, 1957).

We investigate which parties benefit from the turnout changes.⁹ The largest conservative party (CSU) gains up to 0.6 percentage points in its total vote share in $t + 1$. Its in-person vote share is unchanged, but the increase in its postal vote share is comparable in magnitude to the decline in other right-wing parties' in-person vote shares.¹⁰ The

⁸ The evidence on habit formation in voting is mixed. Fujiwara et al. (2016) find a lasting turnout reduction after a shock affecting only voting costs once. Similarly, Gerber et al. (2003) and Meredith (2009) find persistent positive effects of initial mobilization. In contrast, studies on compulsory voting in Switzerland and Austria find no enduring effects on turnout after abolition (Bechtel et al., 2018; Gaebler et al., 2020).

⁹ Several studies examine the partisan consequences of higher turnout. Thompson et al. (2020), Barber and Holbein (2020), and Hodler et al. (2015) find no effect on election outcomes; Frank et al. (2023) find a positive effect on the incumbent vote in the same context as this paper.

¹⁰ 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,

CSU’s vote share gains may reflect a preference channel, i.e., right-wing voters crediting the CSU for pandemic management, or a compositional channel, whereby the treatment differentially mobilizes CSU-leaning abstainers. While the preference interpretation gains support from the observation that other right-wing parties lose in-person vote share while their postal share is unchanged, we cannot rule out compositional effects. As the salience of COVID-19 diminishes over time, the CSU’s electoral gains dissipate. In $t + 2$, the estimated effect approaches zero. Nevertheless, the increase in the CSU’s postal vote share persists.

One explanation for why a one-time exposure to universal postal voting could produce lasting changes in voting behavior is 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). Using a triple difference-in-differences strategy, we find suggestive evidence that high-turnout municipalities experience a persistently greater increase in total turnout compared to low-turnout municipalities. This result may seem surprising, as with fewer abstainers, treatment intensity should be lower. Moreover, we estimate differences between larger and smaller municipalities. For treated municipalities in the bottom tercile, we detect the largest increase in total turnout. This is consistent 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 from 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 examine the distribution of information costs by investigating heterogeneity across municipalities’ socio-economic characteristics. We detect an age gradient in the effect size: Municipalities with a higher share of elderly eligible voters (aged 60 and

which lacks statistical significance, and persistent substitution in the mode of voting by less than half a percentage point.

above) are associated with larger substitution from in-person voting to postal voting. In municipalities with a higher share of younger voters (aged 18–29), this pattern is weaker, consistent with these voters having only recently established their preferred voting mode. In contrast, municipalities with a higher share of the youngest age group (aged 15–17), who had not voted before, show no different behavior than the average. The same holds for the middle-aged group (aged 30–59). We also find suggestive evidence of heterogeneity by gender and unemployment, but no differential effects by the 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.¹¹ 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.¹² The notification also informs them about the option of postal voting.

A distinctive feature of the German electoral legislation is the widely accepted option of postal voting. Postal voting was introduced in West Germany in 1957, and since 2008, it can be requested without giving an excuse. Since 2013, it has been possible to request the

¹¹ For more information on the electoral systems, see [Appendix A](#).

¹² 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.

postal ballot online. Alternatively, it can be requested either in person, or by mail. The request has to be submitted no later than two days before the election. Postal ballots can be submitted to any mailbox without postage.¹³ 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 voters' previous voting mode.

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. If no candidate achieves the absolute majority in the first round, the two highest-ranked candidates proceed to a runoff two weeks later. Under the pandemic's unique circumstances, local elections were held in Bavaria on March 15, 2020.¹⁴ 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 regular mode despite the health risks of the pandemic ([Bayerisches Staatsministerium für Gesundheit, Pflege und Prävention \(STMGP\), 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.¹⁵ 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 created the unusual situation that all eligible voters who

¹³ 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.

¹⁴ 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 starting dates for their terms of office after the death or resignation of a mayor (county commissioner). Municipalities with an off-cycle mayoral election and no county commissioner election are exploited in robustness checks in [Appendix D](#), using them for an alternative definition of the control group.

¹⁵ 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.

wanted to vote in the runoff, explicitly also those who had never voted by postal ballot before and were possibly skeptical of it, had to vote by mail.

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.¹⁶ Nationwide COVID-19 measures lapsed on April 7, 2023, approximately six months before the 2023 State Election.

2.2 Data

We use official election statistics 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, reported separately by polling places and postal precincts.¹⁸ We aggregate the data to the municipality level for two reasons: First, polling places are not necessarily nested within postal precincts. Thus, municipalities are the smallest level at which we can analyze total turnout and party vote shares. Second, precinct boundaries are regularly adjusted between elections, whereas municipality boundaries remained unchanged throughout our sample period, providing a time-constant unit of observation. Appendix Table F1 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 2013.¹⁹ The dotted vertical line depicts the 2020 Mayoral

¹⁶ These figures and resulting policy measures were reported and implemented at the county level.

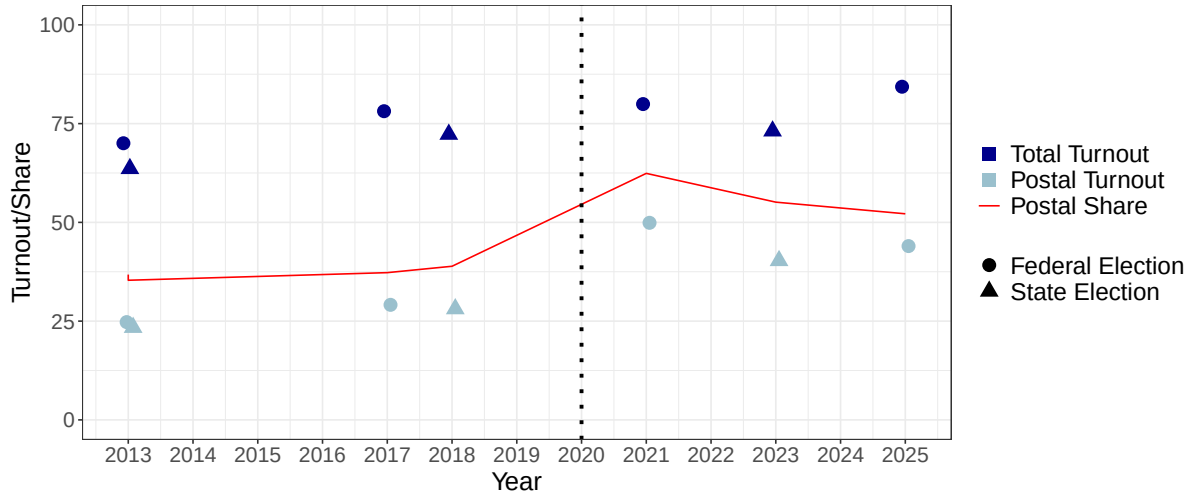
¹⁷ <https://www.bundeswahlleiterin.de/> and <https://www.statistik.bayern.de/wahlen/landeswahlleitung/>, both accessed on 15.05.2024.

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 larger.

¹⁹ 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

Elections, which are decisive for the treatment.²⁰ 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 the pandemic.²¹

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.²² We use this information to define the treated municipalities—those 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 com-

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.

²⁰ 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.

²¹ 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.

²² <https://www.statistik.bayern.de/wahlen/kommunalwahlen/bgm/>, accessed on 15.05.2023.

missioner 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, which would contaminate the treatment; we therefore exclude them.²³ Moreover, independent cities, being identical to the county, are removed from the estimation sample as they contain no within-county variation.²⁴

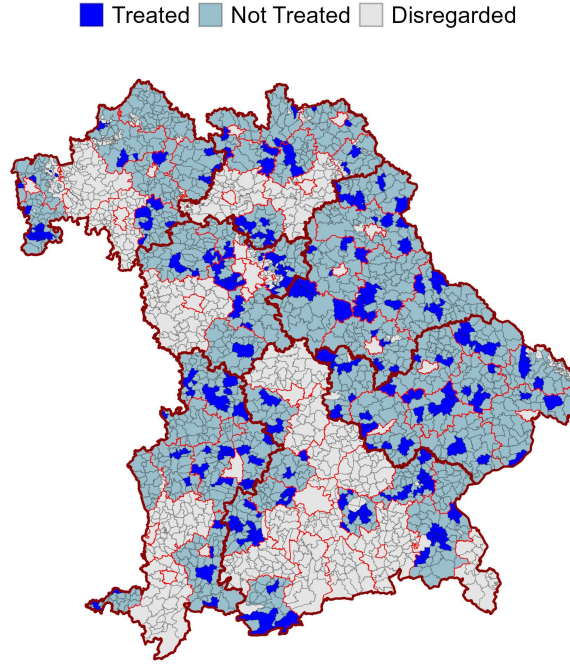
Figure 2 shows a map of all Bavarian municipalities. Government districts and county borders are delineated by thick and thin red lines, respectively, and municipality borders by grey lines. The estimation sample contains 1,457 municipalities. A runoff in the mayoral race was held in 205 municipalities. 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 county commissioner runoff in 2020 are disregarded and shown in grey.

Runoffs are relatively rare in Bavarian mayoral elections. In Appendix Figure E2, we depict the frequency of mayoral runoffs over the last four mayoral elections (through 2020) for our estimation sample. The modal case is no runoff, and there are more municipalities with only one runoff than with more than one runoff. For the treatment group, one runoff is the modal case; 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 (in every election except 2020). We provide robustness checks in Appendix D, 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.

²³ We use them later in a placebo exercise.

²⁴ We estimate using election $\times$ county fixed effects. Hence, only variation within counties is used.

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



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

3 Theoretical Considerations and Empirical Strategy

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.²⁵ 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 (τ): the pre-treatment period ($t - 1$), the first post-treatment election

²⁵ 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.

($t + 0$), and the second post-treatment election ($t + 1$). Eligible voters decide whether to participate by comparing the net benefits of voting, expressed as

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

where $P_{i\tau}$ represents the individual i 's 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. Conversely, 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 $\mathbb{1}_{i\tau}^{\bar{p}}$ 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 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 automatically in the all-postal election, $\mathbb{1}_{i,t+0}^{\bar{p}}$ remains unchanged. As a result, treated voters initially *perceive* postal voting costs to be lower than those faced by experienced postal voters in regular elections. 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 $\mathbb{1}_{i,t+1}^{\bar{p}}$ 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 relative to $t + 0$ but not relative to $t - 1$. Postal and total turnout would decrease to their original values ($t - 1$) only if $C_i^{request} > C_i^{info}$.²⁶

As voting costs remain constant after the second adjustment, we do not expect further turnout changes in $t + 2$. The largest effects should be observed among individuals with high initial information costs, such as elderly voters. These predictions guide our empirical analysis.

3.2 Main Specification

3.2.1 Event Study Design

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:

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

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 $e \in \{1, 2, \dots, 7\}$ 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

²⁶ Inattentiveness to the postal voting request requirement may also prevent some voters from voting by mail in the 2021 Federal Election (Alipour and Lindlacher, 2025). If widespread, such inattentiveness would shift postal voting from the 2021 Federal to the 2023 State Election, generating an increase between the two elections, contrary to the predictions of the adjusted voting cost theory.

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. 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 Table D2.

3.2.2 Identifying Assumptions and Exogeneity of the Treatment

The election $\times$ 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 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.

The identifying assumption for a causal interpretation is parallel trends, which cannot be tested directly.²⁹ However, we show that parallel pre-trends hold for the three types of turnout and for party vote shares. Additionally, we assess the robustness of our main

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. For robustness, we also use alternative specifications of the population control in Figure D5.

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).

findings to deviations from the parallel trends assumption ([Rambachan and Roth, 2023](#)) in Appendix [Table D4](#).

We address potential selection into runoffs by estimating cross-sectional OLS specifications in Appendix [Table F2](#). To make these specifications 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

Similarly, we show that our treatment is uncorrelated with the pandemic. First, we use early-pandemic differences that might affect voting behavior in the 2020 Local Elections. As official data on COVID-19 infection cases is only available at the county level, we use school/daycare closure records prior to the first round on March 15 from [Baskaran et al. \(2023\)](#). Appendix [Table F3](#) regresses our treatment on an indicator of whether any closure occurred and on aggregated closures per municipality. Both specifications lack statistical significance at conventional levels. Second, we use official death statistics, separated for deaths caused by COVID-19. In Appendix [Table F4](#), we show that deaths caused by COVID-19 in the 30 and 90 days prior to the 2021 Federal Election and the 2023 State Election, respectively, and accumulated until each election, are not correlated with the treatment indicator.³¹

Robustness checks are provided in [Appendix D](#). These include shrinking the sample around the margin of how the first round of the mayoral election was decided, using different approaches to match on observables and local matching, restricting the sample by the number of runoffs in the past to estimate on municipalities with a similar propensity to have a runoff, using off-cycle elections as the control group, and implementing the doubly robust difference-in-differences estimator proposed by [Sant’Anna and Zhao \(2020\)](#).³²

³⁰ Each column in Appendix [Table F2](#) reports a separate regression. Apart from the turnout variables (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 Prior to the 2025 Federal Election the pandemic was officially over and the number of registered deaths caused by COVID-19 went down to levels close to zero.

³² We shrink the sample instead of estimating an RDD as we are primarily interested in the dynamic effects across all three subsequent elections.

We conduct three placebo exercises to rule out that the estimated effects stem from runoffs per se or from holding elections during the pandemic. If the change in turnout is driven by the all-postal format rather than the runoff itself, other mayoral runoffs should produce no effect. We test this using Bavaria’s 2014 Mayoral Elections, the most recent cycle without all-postal voting, and find no effects. If, instead, the pandemic drives our results, for example, because some voters may switch to postal voting to avoid contact at polling places, we would expect similar effects in other states.³³ Hesse postponed its local elections to fall 2020 and spring 2021, roughly a year later than Bavaria’s but under similar circumstances, during Germany’s second lockdown (December 2020 to May 2021). Repeating our estimation for Hesse yields no effects. Finally, one may be concerned that the estimated effect reflects the early phase of the pandemic and not the all-postal format. We address this using Bavarian municipalities that held an all-postal election due to a runoff in the 2020 County Commissioner Elections as a placebo sample. In this sample, municipalities with a mayoral runoff do not differ in their voting behavior from those without one. Altogether, these exercises confirm that our results are driven by the all-postal election, not by the Bavarian context or the onset of the COVID-19 pandemic.

3.3 Implicit First Stage

[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-

³³ We additionally run robustness checks using off-cycle elections for the control group and controlling for time-invariant cumulative COVID-19 deaths as of the 2021 Federal Election in [Appendix D](#).

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 approximately 8 percentage points, sizable (Appendix Table B1). Further details are provided in Appendix B.

4 Results

4.1 Event Study Results

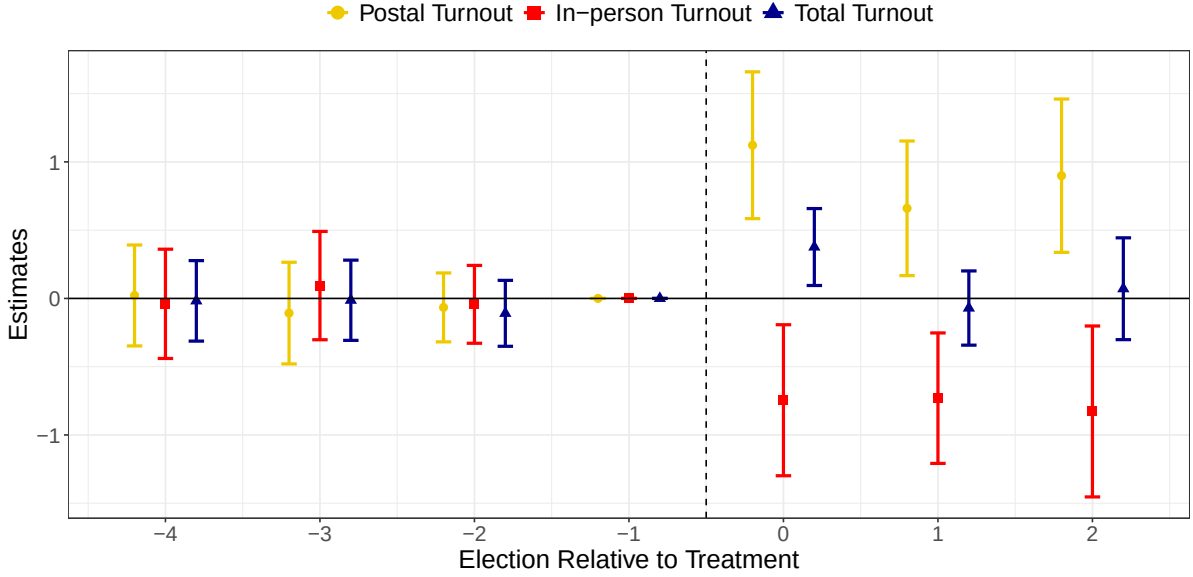
We present event study results based on Equation 2 for the effect of the all-postal runoff on total turnout, as well as postal and in-person turnout, at the municipality level in subsequent elections. Figure 3 plots the estimates for the three outcomes.³⁴

For all pre-treatment periods, estimates are close to zero and lack statistical significance for all three outcomes. The absence of pre-trends lends credibility to the assumption of parallel trends.³⁵ 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 in-person turnout of only 0.75 percentage points. In the short run, we therefore find both a substitution from in-person to postal voting and a reduction in abstention. 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 in-person turnout estimate remains at -0.73 , while the postal turnout estimate declines by approximately 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

³⁴ Total turnout is defined as the number of cast votes divided by the number of eligible voters. Postal and in-person turnout are defined analogously using the number of postal and in-person votes, respectively. Point estimates and standard errors for the main effect and the population control are shown in Appendix Table F6.

³⁵ We provide sensitivity analysis on the parallel trends assumption following Rambachan and Roth (2023) in Appendix D.

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 F6.

Election, the total turnout effect remains at zero (0.07). The point estimates for postal and in-person turnout remain similar (0.90 and -0.83).³⁶

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 due to polling place changes report similar magnitudes.

³⁶ 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.

[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. [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. 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.

We provide a back-of-the-envelope calculation of whether the estimated substitution is large enough to generate net administrative savings (details in [Appendix C](#)). Detailed election cost data are scarce, so we draw on Hamburg’s 2025 election report, which provides unusually granular expenditure breakdowns. Our point estimate implies about 30 additional postal voters per municipality in the 2025 Federal Election, raising postal-processing costs by roughly €40–€56. Since staffing and facility costs are largely fixed, offsetting these costs requires reducing polling-place provision. Given average in-person voting costs of €3,385–€5,470 per municipality, municipalities break even by cutting about 1 percent of polling place costs, equivalent to roughly one municipality in 26 closing a single polling place. With a permanent substitution five years after the treatment of about 2 percent of in-person voters (evaluated at the mean), this threshold appears readily achievable, although feasibility depends on local constraints such as municipality and precinct sizes, geography and distances, and minimum staffing requirements.

One might find our results 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 ex-

posure 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 likely represent lower-bound estimates of the true potential impact of reducing barriers to postal voting. However, generalizability may be limited in contexts where low postal voting rates reflect distrust of postal voting as a secure or legitimate method. Survey evidence suggests that confidence in mail-in voting varies systematically with partisanship and prior beliefs about electoral integrity ([Alvarez et al., 2008](#)). Where resistance to postal voting is attitudinal or ideological, forced experimentation may be less effective.

We show robustness in several dimensions in [Appendix D](#). 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 also control for deaths caused by COVID-19. 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 \(2023\)](#). To address concerns about model misspecification, we apply a doubly-robust estimation approach following [Sant’Anna and Zhao \(2020\)](#). 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 are 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 postal voting costs fall below the benefits of voting, former abstainers participate. 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 a second adjustment of perceived postal voting costs. In the 2020 Runoffs, postal ballots were sent automatically to all eligible voters. However, in subsequent elections, eligible voters had to request postal ballots, as before, if they wished to vote by mail. The automatic mailing of ballots in the runoff may therefore have distorted perceived postal voting costs downward. Behavior in period $t + 1$ can therefore be interpreted as continued experimentation. However, we cannot rule out that these effects are driven by differences in the election type (federal versus state). Still, our findings are consistent with rational choice models of electoral turnout, with eligible voters adjusting their voting costs. 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. Once these costs stabilize, voting behavior also stabilizes. This pattern is more consistent with the structure of voting costs than with strong habit formation as the primary driver of 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.³⁷ 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 investigate this further when analyzing heterogeneities by past turnout as a proxy for social pressure.

4.2 Partisan Consequences

If the treatment differentially mobilizes supporters of particular parties or shifts voters' mode of casting ballots in a party-correlated manner, the turnout effects documented above will have partisan consequences. We therefore re-estimate Equation 2 using party vote shares as dependent variables. As with turnout, we can differentiate between postal and in-person party vote shares because municipalities typically collect all postal ballots centrally.³⁸ This decomposition allows us to separate shifts across parties from shifts between voting modes within a party, providing richer variation than an analysis of aggregate party-level vote shares alone.

We first present 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.³⁹ In the political spectrum, the CSU is classified as center-right (Jolly et al., 2022). As with turnout, the absence of pre-trends in party vote shares lends further credibility to our empirical design. In the first election after the treatment ($t+0$), the CSU gains 0.30 percentage points (marginally statistically significant). Relative to the main effects from Figure 3, this is approximately three-quarters of the total turnout effect. This indicates that some

³⁷ This potential mechanism is similar to the one Funk (2010) finds.

³⁸ An allocation is not feasible for 12 municipalities, as vote counts were not reported separately by voting mode in at least one year. This applies in particular to small municipalities (<1000 eligible voters) and does not occur for any treated municipality.

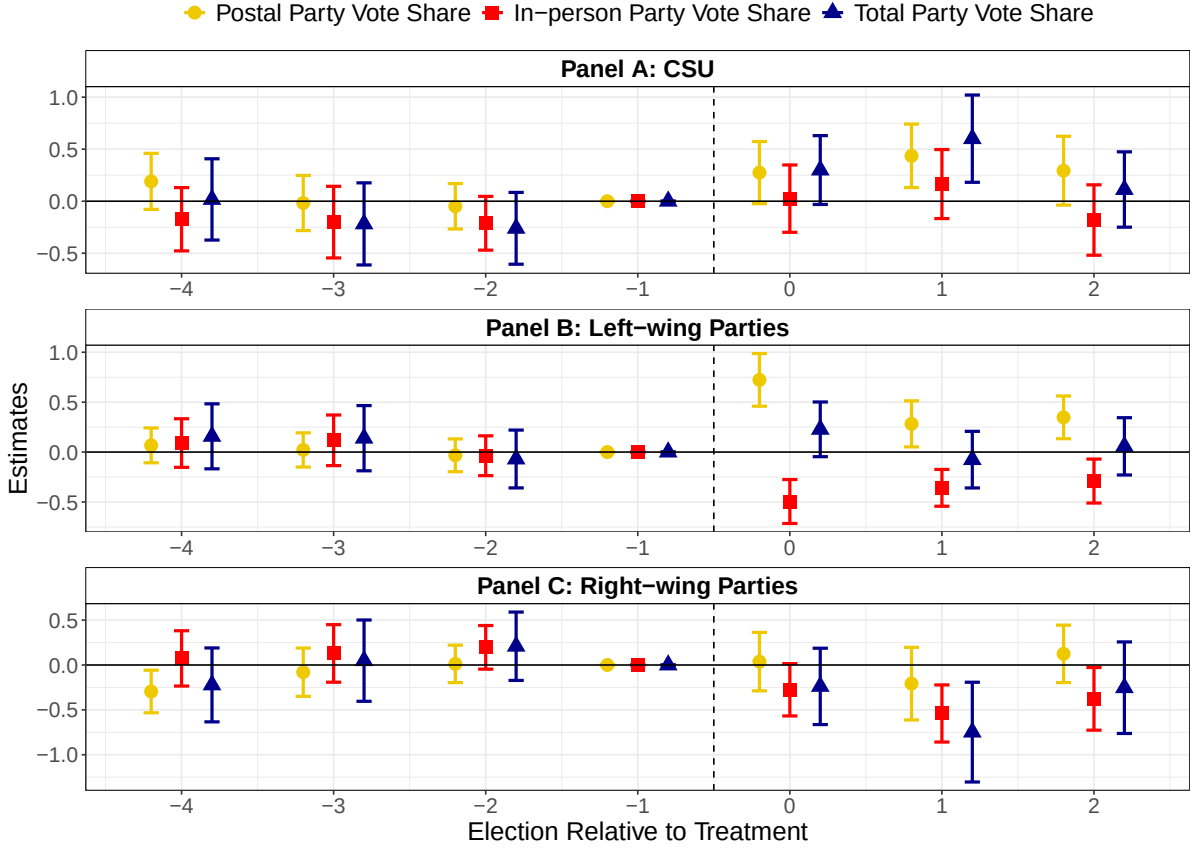
³⁹ The CSU has continuously held the office of the Bavarian Minister-President since 1957. In the most recent two state elections, the CSU won approximately 37 percent of the votes, and in 2013, the CSU even won the absolute majority of seats in the Bavarian Parliament. The next largest party was the 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.

but not all newly mobilized voters supported 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. This contrasts with our main results, where postal turnout rises while in-person turnout declines only modestly. In the subsequent election ($t+1$), the effect on the total vote share increases to 0.60 percentage points, statistically significant at the 1 percent level, driven by gains in both the postal and in-person vote shares. This contrasts with the effects on turnout, where the total turnout effect vanishes as the postal turnout effect declines. 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 approximately a third of the effect size. Hence, the partisan effects are less persistent, 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 ($t+1$), the in-person vote share declines to a negative, statistically significant estimate; the postal vote share point estimate also declines and is negative but lacks statistical significance. Together, these yield a statistically significant total vote share loss of 0.75 percentage points. By the 2025 Federal Election ($t+2$), only a negative effect on the in-person vote share of 0.38 percentage points (significant at the 5 percent level) persists. This suggests that while some in-person CSU voters switched to postal voting, some other right-wing in-person voters continued voting at the polling place but switched to the CSU.

The CSU was part of both the German federal and the Bavarian state government in 2020 and held most mayoral and county commissioner offices. Some right-wing voters may therefore have switched to the CSU to reward its role in ensuring the continuation of

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 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 F7.

the electoral process during the pandemic, particularly given that other right-wing parties advocated different pandemic policies.⁴⁰ These switchers likely aligned more closely with the CSU’s stricter COVID-19 stance, especially after experiencing its tangible consequences through the all-postal election. Hence, the results suggest that the CSU’s gains do not solely reflect the mobilization of “lazy voters” through the postal voting information treatment, as our theoretical framework predicts. Instead, they likely reflect a preference

⁴⁰ The CSU pursued a more cautious approach to pandemic management, whereas the *Freie Demokratische Partei* (FDP), *Freie Wähler* (FW), and the *Alternative für Deutschland* (AfD) favored more permissive policies.

channel, a compositional channel, or both. The preference channel, i.e., right-wing voters rewarding the CSU for pandemic management, is closely tied to the pandemic context and dissipates by $t+2$. The compositional channel, the treatment differentially mobilizing CSU-leaning abstainers at a higher rate than supporters of other right-wing parties, cannot be ruled out. We investigate this alternative mechanism in the heterogeneity analysis (Subsection 4.3).

For left-wing parties (Panel B), the pattern resembles the main effects (Figure 3) but is smaller in magnitude. In the first post-treatment election ($t+0$), an increase in the postal vote share of 0.72 percentage points is mirrored by a decrease in the in-person vote share of 0.50 percentage points, resulting in a total increase of 0.23 percentage points, which, however, lacks statistical significance. When voters adjust to the actual costs of requesting a postal ballot, the coefficient for the postal vote shares drops to 0.28 in $t+1$ and is 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 $t+2$.⁴¹ Left-wing parties exhibiting persistent substitution in the voting mode is consistent with the treatment affecting how these voters cast their ballots, and not whom they vote for.

Expressed as a share of votes cast, the CSU wins 0.42 percentage points in the 2021 Federal Election and 0.99 percentage points in the 2023 State Election (Appendix Figure E3). Left-wing parties show no statistically significant change in their vote shares relative to the total number of votes cast, implying that their gains in vote shares relative to eligible voters are proportional to the overall turnout increase. Correspondingly, other right-wing parties lose approximately the magnitude of the CSU's gains. As the Bavarian Parliament has 203 members, the difference between a counterfactual parliament in which no municipality received the treatment and one in which all municipalities had an all-postal runoff is roughly two additional seats for the CSU, under proportional allocation and assuming uniform treatment effects across all municipalities.

⁴¹ The insignificant reduction in total vote share suggests that some left-wing voters return to abstaining.

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 explore patterns consistent with social pressure (Funk, 2010; DellaVigna et al., 2017) and 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 provide suggestive evidence on the distribution of information costs.⁴² To examine these heterogeneities, we extend the event study specification 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:

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

The same controls and fixed effects as in Equation 2 are included. 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 unit 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

⁴² Alvarez et al. (2008) document that confidence in different voting modes varies systematically with socio-economic characteristics, motivating the assumption that information costs may likewise be heterogeneously distributed across these groups.

triple difference estimates μ^τ , which correspond to the differential turnout trend among treated municipalities when Z_i is increased by one standard deviation. The base effects are reported in tables in the Appendix.

We lack exogenous variation to identify the sources of heterogeneity causally. The estimates should therefore be interpreted as suggestive evidence on which groups drive the results and where information costs may be particularly high. Moreover, Appendix Table F5 shows some correlation across covariates, which may confound the relationship and partly explain the heterogeneity, not the covariate of interest, Z_i .

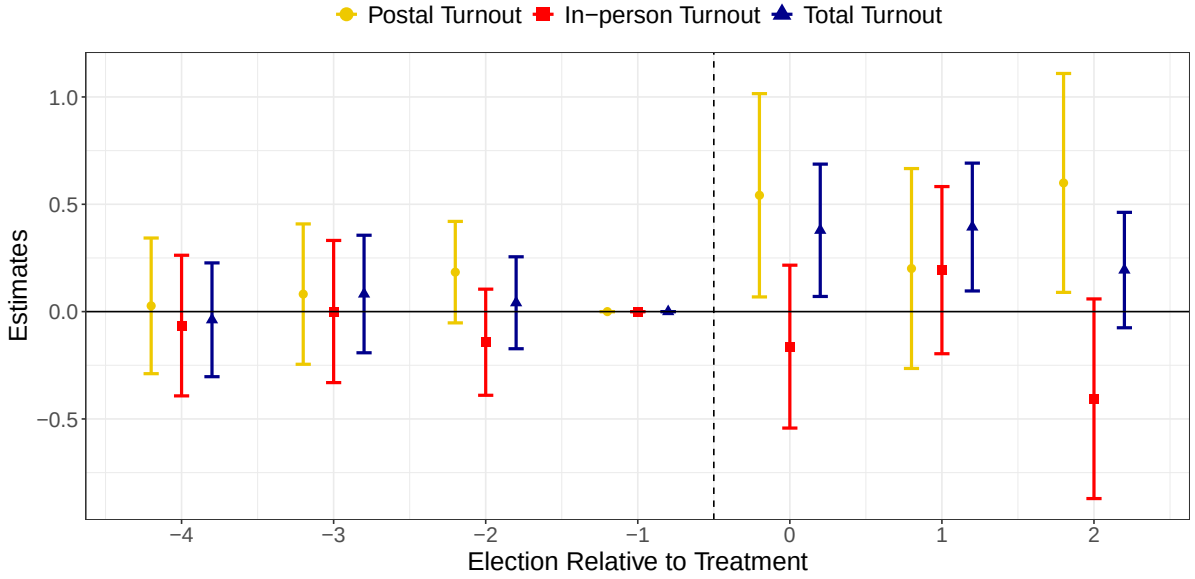
4.3.1 Social Pressure: Heterogeneity by Past Turnout and Municipality Size

Past Turnout Social pressure is an important determinant of voting behavior (Funk, 2010; DellaVigna et al., 2017). In municipalities with high turnout, non-voters may face greater social pressure to justify their lack of participation, increasing their propensity to vote. We hypothesize that the treatment effect on non-voters is larger in high-turnout municipalities. We test this by interacting the treatment indicator with the scaled turnout (i.e., mean zero and unit standard deviation) from the 2018 State Election for Z_i .

Figure 5 presents the results. In the first post-treatment period ($t + 0$), high-turnout municipalities experience a larger increase in postal and total turnout compared to low-turnout municipalities, 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 pattern differs. 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 suggests a shift from postal to in-person voting in $t + 1$ when voters adjust their postal voting costs for another time. Furthermore, social pressure in high-turnout municipalities appears to motivate voters to continue participating even if they do not persist with postal voting.

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 F9.

Several remarks are in order. First, social pressure is only one interpretation, but we consider it the most plausible for explaining why high-past-turnout municipalities show larger effects. In such municipalities, awareness of voting as a civic duty is higher (Ali and Lin, 2013), and non-voters may face greater social pressure when voting costs fall. Bangum et al. (2025) show that pro-social voters are insensitive to changes in electoral incentives; this implies that the remaining non-voters in high-civic-duty environments are disproportionately cost-sensitive and thus more responsive to the treatment. Second, this finding cannot be explained solely by reduced information costs, since the reduction in information costs should not differ systematically between high- and low-turnout municipalities. The treatment may therefore also shift the perceived benefits of voting. In

high-turnout municipalities, trust in the election administration may be higher, or voters are more likely to encounter others who affirm the validity of postal voting. Third, in high-turnout municipalities, the pool of non-voters is smaller, implying lower treatment intensity. In relative terms, the effect is therefore even larger than the absolute estimates suggest.

Regarding partisan effects, Appendix [Figure E4](#) 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 $t+2$. In $t+0$, the CSU's heterogeneous increase approximately matches the heterogeneous total turnout increase (0.38 percentage points), suggesting that the additional mobilization in high-turnout municipalities largely benefits the CSU, consistent with the compositional channel. By $t+1$, however, the CSU's increase exceeds the total turnout deviation (0.39 percentage points), and by $t+2$ the turnout heterogeneity vanishes while the CSU effect persists. This growing wedge points toward the preference channel. In high-turnout municipalities, existing voters appear to shift toward the CSU, beyond what differential mobilization alone can explain. These increases are mirrored by losses for other right-wing parties, though the latter are smaller in magnitude and lack statistical significance. There are no meaningful deviations for left-wing parties. Results are similar when considering votes instead of eligible voters (Appendix [Figure E5](#)).

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. Beginning with a median split at 2,040 eligible voters, heterogeneity by municipality size appears modest (Appendix [Figure E6](#)). Nevertheless, we find slightly stronger effects in the bottom half of the sample's municipalities.⁴³ Heterogeneity strengthens when we move to terciles, with cutoffs at 1,427 and

⁴³ Throughout this paragraph, we depict marginal effects for each category instead of coefficients of interaction terms. Appendix [Figure E6](#) 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

3,094 eligible voters, particularly 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 tercile. For the top tercile, we find a moderate increase of 0.39 percentage points, similar to the baseline estimation (Figure 3).⁴⁴ For the bottom tercile, there is no reduction in in-person turnout, implying that, 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 E7).⁴⁵ Overall, total turnout increases are largest in the smallest municipalities. This pattern is consistent with the social pressure hypothesis, assuming that social pressure is higher in smaller municipalities where mutual monitoring is easier (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

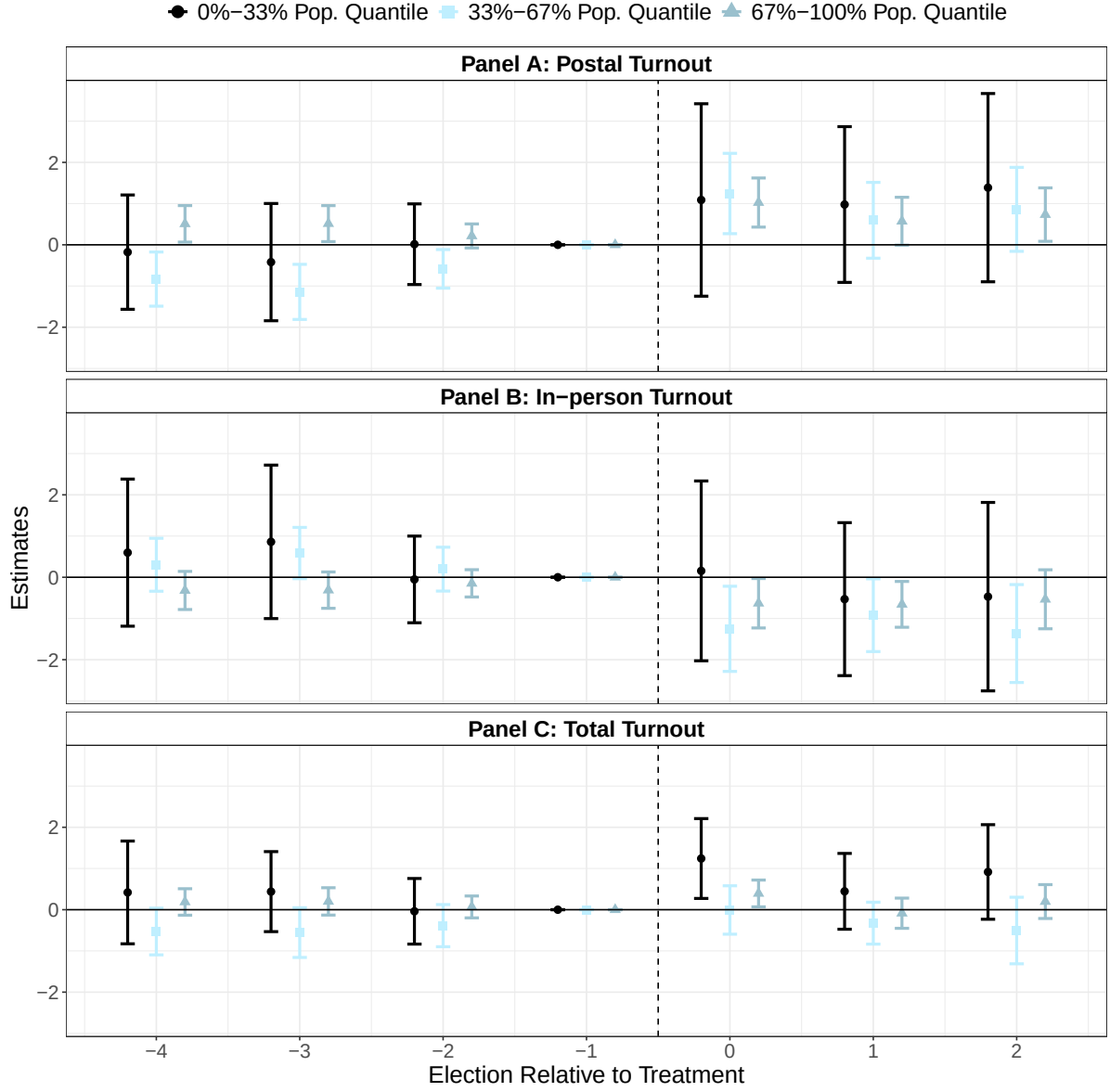
We hypothesize that the higher the information costs associated with an unfamiliar voting mode, the stronger the expected treatment effect. Thus, we examine whether varying

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).

⁴⁴ Treated municipalities are concentrated in the top tercile (Appendix Figure E1), leaving fewer treated units in the bottom tercile, which accounts for its wider confidence intervals.

⁴⁵ The cutoffs are at 1,098, 1,634, 2,643, and 4,496 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 quintiles 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 F13.

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 unit standard deviation).

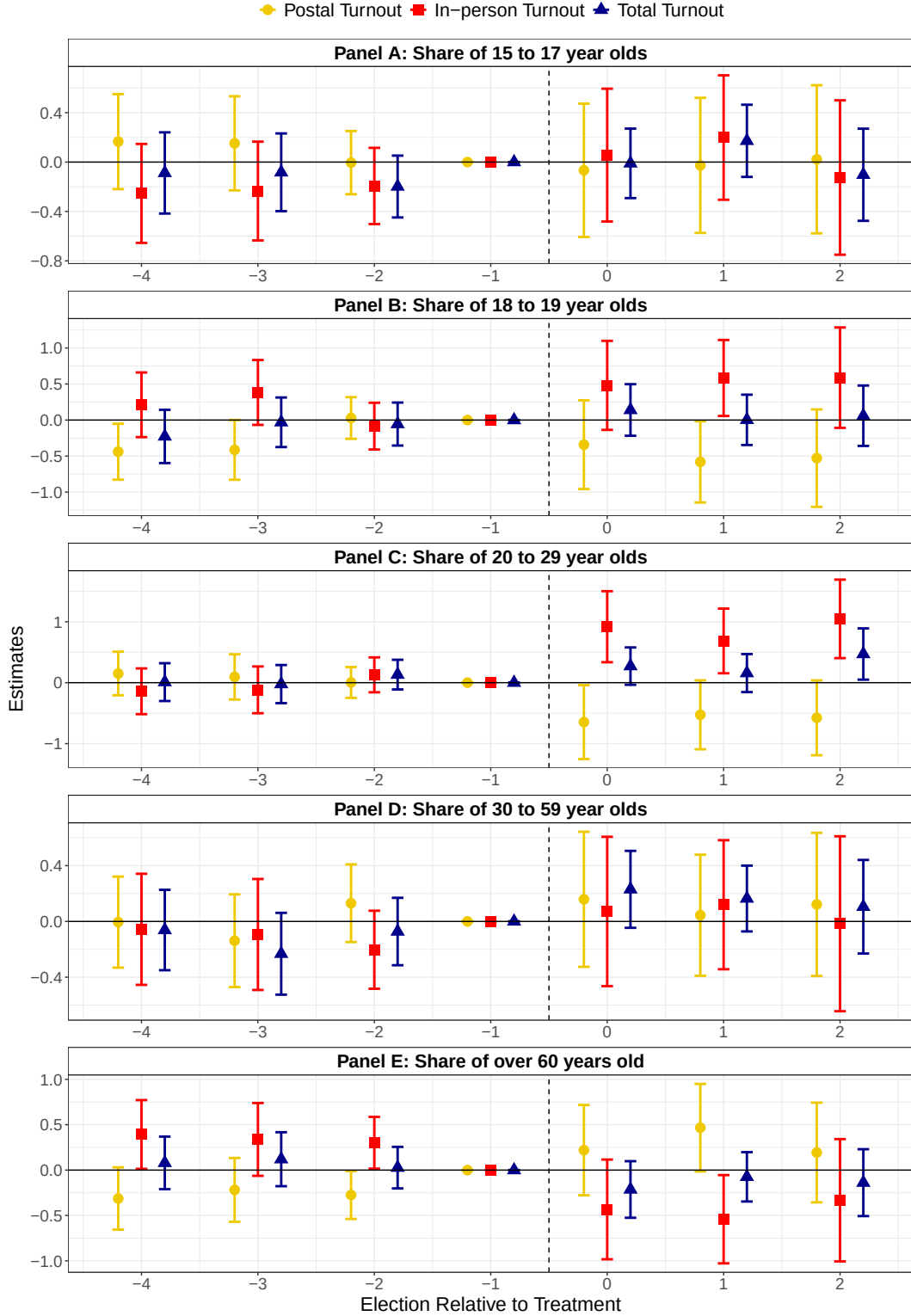
Age We show heterogeneity across age groups in Figure 7. Since all values for the age groups are from 2018, individuals aged 15–17 years old partly did not vote in the 2020 Runoff and did not vote at all in previous elections. In this group, we see no differential effects, as there is presumably no voting 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 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 from in-person to postal voting (Panel E). This is consistent with older voters being more strongly affected by the treatment and having faced higher information costs before. For 18–19-year-olds, who were also already eligible to vote in 2018, the substitution from in-person to postal voting is weaker (Panel B). Similar results are shown for the 20–29 age group (Panel C). This suggests that individuals who recently established a preferred voting method are less inclined to switch as they might be well informed about possible modes of voting.⁴⁶ 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. For the age group 20–29, there is a positive and marginally significant effect in $t+0$ and a statistically significant effect in $t+2$. For the oldest age group, the differential total turnout estimate is negative, though statistically insignificant. For all other age groups that have voted before, differential total effects are positive. One interpretation is that the treatment operates through two channels, depending on prior voting experience. Voters who already participated in person face only information costs about postal voting; the treatment reduces these, prompting mode

⁴⁶ Recall that postal voting has been possible since 1957 and can be requested without giving an excuse since 2008.

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 unit 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 F15.

substitution without changing overall participation. In contrast, for marginal infrequent voters, the treatment may lower total costs below the participation threshold, increasing turnout. The observed age gradient for total turnout is consistent with the first channel, as elderly voters are experienced participants unfamiliar with postal voting.

Unemployment Heterogeneity by the share of unemployed in a municipality is depicted in Panel A of Appendix [Figure E8](#). 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 suggests that for this group, there may 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 larger, statistically insignificant increase in postal voting, and results in a mitigation of the total turnout effect (Panel B of Appendix [Figure E8](#)). 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 solely to gender (Appendix [Table F5](#)).

Households with Children Differences in the share of households with children across municipalities do not alter the effect of the all-postal election on subsequent voting behavior (Panel C of Appendix [Figure E8](#)). We examined this variable because parents might prioritize family needs differently, potentially affecting their voting behavior. However, the results show no differential effects.

4.4 Placebo Tests

We assess the validity of our treatment effects through placebo exercises. By varying the time, the place, and the treatment definition, we show that only the unique circumstances of the all-postal runoffs produce the estimated effects. First, we shift the treatment to the previous mayoral elections in 2014, where there was no all-postal runoff. Second, we shift 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. Third, we estimate on Bavarian counties with a county commissioner runoff in 2020, where the entire county experienced an all-postal election, not only treated municipalities with a mayoral runoff. Fourth, we employ randomization inference ([Heß, 2017](#)) to assess whether the results could arise by chance.

Time Placebo One concern may be that the estimated effects in [Figure 3](#) stem not from the all-postal format but from the runoff itself, that is, any additional electoral experience may have lasting effects on turnout. We first note that this alternative explanation cannot account for the specific pattern of a stronger increase in postal voting. To address this further, we shift the treatment to the 2014 Local Elections, assigning placebo treatment to municipalities that experienced a runoff in that year. In 2014, the runoff followed the same voting procedure as the first round, i.e., there was no all-postal election. We exclude municipalities with a county commissioner runoff in 2014 or 2020, as well as municipalities treated in 2020, to avoid contamination from actual treatment exposure. The results, depicted as specification *Time* in [Figure 8](#), show no significant pre-treatment

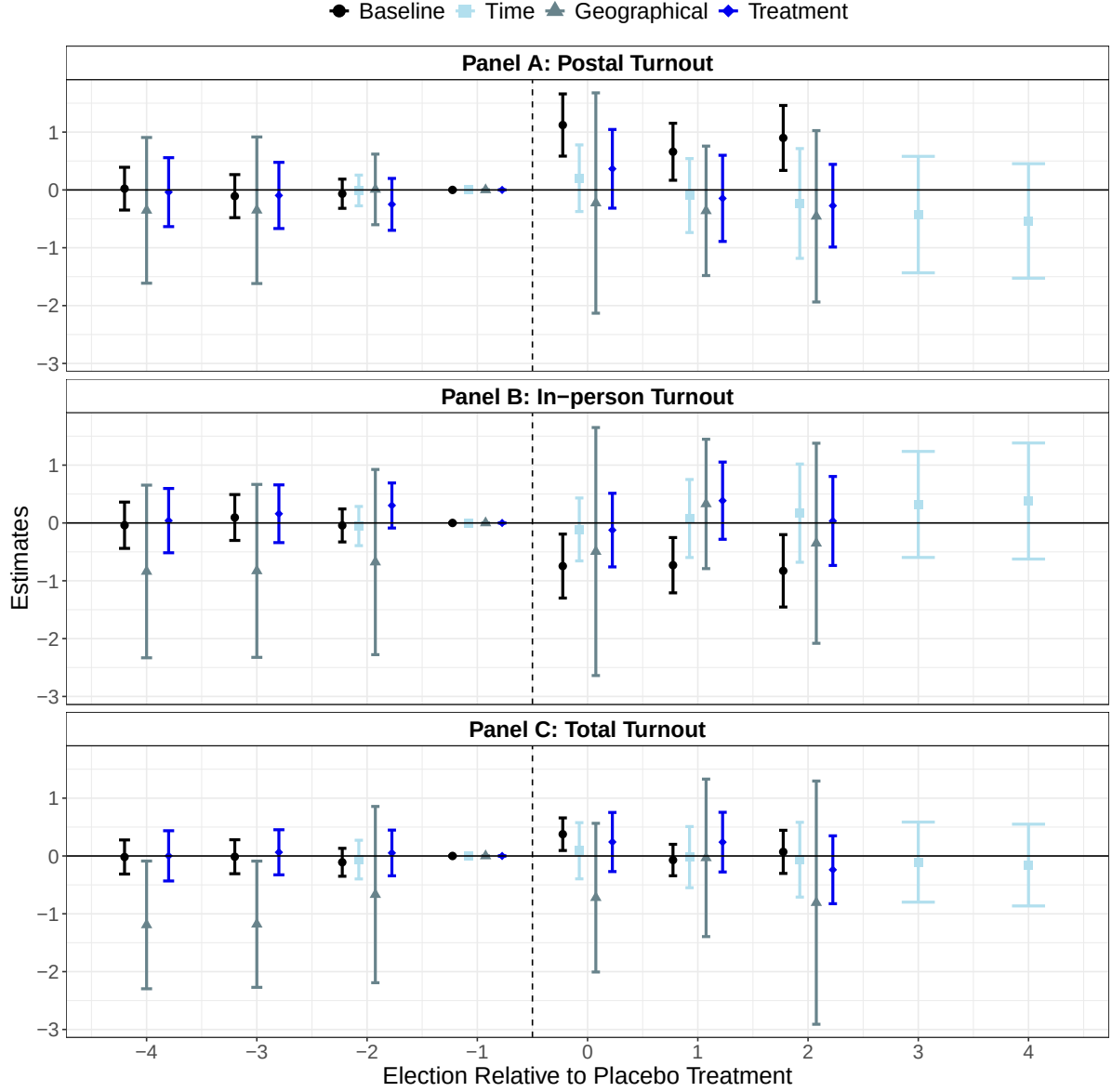
trends. As the placebo treatment originates from the 2014 Local Elections, only two pre-treatment elections remain in our sample (the 2013 State Election and the 2013 Federal Election). As expected, the estimates are close to zero and lack statistical significance in all five post-treatment periods and in all three outcomes. This placebo strengthens the conclusion that the estimated effects are attributable to the all-postal format rather than to the runoff itself.

Geographical Placebo A second concern may be that the estimated effects reflect the pandemic context rather than the all-postal format. We provide initial evidence against this by estimating with only off-cycle elections in the control group (at the end of the robustness section in [Appendix D](#)). Here, we additionally show that local elections held during the pandemic but without an all-postal runoff produce no effects. 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 2021.⁴⁷ As they did not take place at the beginning of the COVID-19 pandemic, they were not held as all-postal elections. If the treatment effect stems from the all-postal format, we expect no significant effects from a mayoral runoff in Hesse. Specification *Geographical* in [Figure 8](#) confirms this expectation. There are no statistically significant deviations after the placebo treatment. As the sample comprises only 78 municipalities, the lack of statistical significance may partly reflect limited statistical power.⁴⁸ 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

⁴⁷ 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). 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 E9](#)).

⁴⁸ Of 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 yields a slightly larger share of treated municipalities (19 percent) than in the main estimation sample (14 percent).

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 F17.

(Garmann, 2017), which would attenuate 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 Bavaria’s institutional 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. As in the main specification, we assign the (placebo) treatment to municipalities with a mayoral runoff in 2020. However, we restrict the sample to municipalities in counties with a county commissioner runoff, where the entire county experienced an all-postal election. If the estimated effect in the main specification were driven by the early phase of the pandemic rather than by the all-postal mayoral runoff, we would observe an effect in this placebo as well. The underlying assumption is that municipalities with a mayoral runoff react to the COVID-19 pandemic in their voting behavior differently from municipalities without a mayoral runoff, and that this is the same between municipalities in a county with or without a county commissioner runoff. However, if the estimated effect in the main specification actually stems from the all-postal mayoral runoff, we expect no differences between the treatment and control group as the control group also experiences an all-postal election in this placebo. Specification *Treatment* in Figure 8 shows estimates close to zero, before and after the treatment, that lack statistical significance. This placebo confirms that municipalities with a mayoral runoff in 2020 do not exhibit differential voting trends relative to those without one when both groups have already been exposed to all-postal voting via the county commissioner runoff. This is consistent with the information cost framework. Once information costs are eliminated by any all-postal election, an additional all-postal election provides no further reduction. It also rules out the possibility that some pre-existing characteristic of runoff municipalities, and not the all-postal format itself, drives the main results.

Randomization Inference Finally, we employ randomization inference ([Heß, 2017](#)). This procedure permutes the treatment indicator 1,000 times within counties, holding constant the number of treated municipalities per county. The results in Appendix [Table F18](#) confirm statistically significant treatment effects consistent with the main analysis, increasing the evidence that the estimated effects are indeed statistically significant. This rules out the possibility that the results are driven by a chance configuration of treatment assignment within counties.

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 approximately one percentage point persists across all three subsequent elections, even five years after the treatment. Consistent with the Porter Hypothesis ([Porter, 1991](#)), our findings show that policies altering decision-making costs can have lasting effects, highlighting the long-term impact of the forced experimentation with an alternative voting mode. The estimated effects are robust across different model specifications and supported by placebo tests varying the time, location, and treatment definition.

Importantly, the forced experimentation with postal voting did not produce detectable negative consequences for democratic participation. We find no evidence that total turnout declined differentially in treated municipalities in any post-treatment election, which speaks against the concern that exposure to postal voting might discourage future in-person participation without generating compensating postal turnout. The mode substitution itself carries no obvious welfare cost: postal voting is administratively cheaper per vote, and because voters could return to in-person voting in all subsequent elections, persistent switching is consistent with some voters having discovered a voting mode better

aligned with their preferences. The partisan consequences, while statistically significant, are transitory and potentially context-dependent, i.e., they dissipate as pandemic salience fades. These findings suggest that, in a context with established institutional trust and high baseline participation, one-time forced experimentation with an existing voting mode carries limited downside risk.

Our results may carry 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 may 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. In contexts where resistance to postal voting is rooted in political opposition or institutional distrust, reducing information costs alone may be insufficient to shift behavior, and complementary trust-building measures may be required.

Declaration of generative AI and AI-assisted technologies in the manuscript preparation process

During the preparation of this work, the authors used OpenAI's ChatGPT and Anthropic's Claude to assist with language editing and the development and debugging of research code. The tools were not used to generate data or to independently design or conduct the empirical analysis, interpret the results, or formulate the paper's conclusions. All AI-assisted code was independently reviewed, tested, and validated by the authors against the underlying data and research design. The authors carefully reviewed, verified, and edited all AI-assisted content as needed and take full responsibility for the accuracy, integrity, and content of the published article.

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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 for a candidate on an open 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.⁴⁹ 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.

⁴⁹ In the elections before the 2025 reform, first-vote plurality winners generally received direct mandates, while second votes determined proportional seat allocation. For the 2025 Federal Election, direct mandates were subject to second-vote coverage under the reformed electoral law.

B Implicit First Stage

The main analysis estimates the effect of the all-postal runoff on turnout in subsequent general 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.⁵⁰ 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):

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

where $Turnout_{ictb}$ 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 $\times$ 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 regular 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

⁵⁰ 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.

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.

Table B1: Implicit First Stage

Total Turnout	(1)	(2)	(3)
Runoff	-1.532*** (0.1843)	-0.8962*** (0.2165)	-3.659*** (0.4539)
Election 2020 $\times$ Runoff	8.254*** (0.2566)	8.072*** (0.3151)	9.086*** (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 Administrative Relevance and Cost Implications

This section provides a back-of-the-envelope calculation of whether the persistent substitution from in-person to postal voting observed in the 2025 Federal Election is large enough to reduce polling place provision and lower net administrative costs.

We first establish cost benchmarks for the two voting modes. Hamburg’s report on the 2025 Federal Election lists €576,000 for postal vote printing and dispatch and €220,000 for the central postal-vote office (summarized in Table C1).⁵¹ Dividing these expenditures by the 427,834 postal voters gives an average cost of €1.35 per postal vote under a narrow benchmark and €1.86 under a broader benchmark that additionally includes the central postal-vote office. For in-person voting, the report lists expenditure for polling places and election offices, election workers, and logistics. Depending on how logistics are allocated, these categories imply average direct costs of €877–€1,417 per polling place.

We apply these benchmarks to the average municipality in our estimation sample, which has 3,328 eligible voters in 2025 and operates 3.86 polling places. The estimated increase of 0.90 percentage points in postal turnout implies about 30 additional postal voters, raising postal processing costs by €40.5–€55.5. Savings on the in-person side are less immediate, because most costs arise from operating polling places. The estimated decline of 0.83 percentage points in in-person turnout implies about 28 fewer in-person voters. Since the average municipality’s polling places serve about 1,318 in-person voters, this reduces their combined load by roughly 2.1 percent.

With average direct costs of €877–€1,417 per polling place, offsetting the additional postal voting costs of €40.5–€55.5 would require consolidating about 1 percent of polling place provision under our preferred benchmark, and between 0.7 and 1.6 percent across the full range of cost assumptions. The estimated reduction in workload, therefore, exceeds the break-even requirement under all considered cost assumptions. This comparison should nevertheless be read as a benchmark. Polling places cannot be adjusted continuously, and actual savings arise only where reduced in-person demand allows municipalities to consolidate whole polling places and avoid the associated staffing, premises, equipment, and logistics costs.⁵²

⁵¹ Hamburg, *Erfahrungsbericht zur Bundestagswahl am 23. Februar 2025 und zur Wahl der Hamburgischen Bürgerschaft am 2. März 2025*: <https://www.hamburg.de/resource/blob/1090024/5b194610c14af4f568eb8c1aa37b0851/erfahrungsbericht-buewahl-und-btw-2025-data.pdf>, last accessed 20 July 2026.

⁵² As an external plausibility check, Trier’s 2025 budget implies average benchmarks of approximately €3.32 per postal vote and €2,288 per polling place. These figures rest on a different budgeting structure and should likewise be read as average costs. Although both modes are more expensive than in Hamburg, their ratio is similar. Under the Trier figures, offsetting the additional postal voting costs would require consolidating about 1.1 percent of polling-place provision. City of Trier, *Haushaltsplan 2025*: <https://www.trier.de/File/3-haushaltsplan-der-stadt-trier-fuer-das-haushaltsjahr-2025.pdf>; City of Trier, *Bundestagswahl 2025 results*: <https://www.trier.de/rathaus-politik/stadtpolitik/>

Table C1: Cost benchmarks and sample inputs

Quantity	Value	Basis
Hamburg cost benchmarks, 2025		
Postal voters	427,834	Hamburg election report
Postal-voting expenditure	€576,000–796,000	Printing and dispatch (+ central postal-vote office)
Average cost per postal vote	€1.35–1.86	Expenditure divided by postal voters
Polling places	1,267	Hamburg election report
Average direct cost per polling place	€877–1,417	Polling places and offices, election workers (+ alternative allocations of logistics)
Average Bavarian municipality in sample, 2025		
Polling places	3.86	Administrative data
Eligible voters	3,328	Administrative data
In-person voters	1,318	Administrative data

Notes: The average municipality refers to the estimation sample in 2025. The narrow postal-voting benchmark includes expenditure on printing and dispatch. The broader benchmark additionally includes the central postal-vote office. For polling places, the lower benchmark includes expenditure on polling places and election offices and election-worker compensation. The upper benchmark additionally assigns all reported logistics expenditure to in-person voting.

D Robustness Checks

D.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 D1](#) 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 point estimates similar to those in 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 exception 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 D1: 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 (0.3651)	0.0554 (0.3345)	0.0846 (0.3251)	0.0525 (0.3162)	0.1902 (0.3029)	0.0125 (0.2941)	0.0161 (0.2863)	-0.0436 (0.2765)	-0.0478 (0.2729)
Treatment ($t - 3$)	-0.1662 (0.3807)	-0.2166 (0.3474)	-0.1511 (0.3386)	-0.2179 (0.3262)	-0.1206 (0.3079)	-0.1820 (0.2952)	-0.1486 (0.2860)	-0.1831 (0.2758)	-0.1599 (0.2751)
Treatment ($t - 2$)	-0.0048 (0.2538)	0.1762 (0.2349)	0.2695 (0.2338)	0.1988 (0.2245)	0.2428 (0.2085)	0.1626 (0.2058)	0.0964 (0.2009)	0.0866 (0.1920)	-0.0120 (0.1942)
Treatment ($t + 0$)	0.7845 (0.5209)	0.7395 (0.4906)	1.003** (0.4770)	1.055** (0.4531)	1.169*** (0.4416)	1.350*** (0.4239)	1.262*** (0.4132)	1.292*** (0.4033)	1.223*** (0.3953)
Treatment ($t + 1$)	0.1255 (0.4938)	0.1052 (0.4619)	0.3075 (0.4474)	0.2752 (0.4309)	0.3984 (0.4236)	0.5545 (0.4052)	0.5307 (0.3933)	0.5673 (0.3822)	0.5544 (0.3728)
Treatment ($t + 2$)	0.9319* (0.5095)	0.8019 (0.4905)	0.9245* (0.4805)	0.9159** (0.4643)	1.146** (0.4627)	1.252*** (0.4478)	1.164*** (0.4301)	1.216*** (0.4180)	1.118*** (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 (0.3796)	-0.1805 (0.3482)	-0.1164 (0.3298)	-0.1093 (0.3166)	-0.1523 (0.2953)	0.1058 (0.3129)	0.0767 (0.3055)	0.0598 (0.2918)	0.0493 (0.2896)
Treatment ($t - 3$)	0.4028 (0.3813)	0.2290 (0.3424)	0.3164 (0.3281)	0.3112 (0.3198)	0.2802 (0.2969)	0.4075 (0.3109)	0.3447 (0.3034)	0.3170 (0.2942)	0.2345 (0.2889)
Treatment ($t - 2$)	0.1900 (0.2906)	0.0385 (0.2768)	-0.0106 (0.2688)	0.0990 (0.2573)	0.0304 (0.2427)	0.0427 (0.2273)	0.0396 (0.2208)	-0.0040 (0.2141)	0.0662 (0.2096)
Treatment ($t + 0$)	-0.3433 (0.5436)	-0.3206 (0.5160)	-0.5156 (0.4933)	-0.5132 (0.4765)	-0.6722 (0.4605)	-0.8384* (0.4364)	-0.7956* (0.4266)	-0.8301** (0.4171)	-0.7674* (0.4064)
Treatment ($t + 1$)	-0.4734 (0.4786)	-0.4631 (0.4423)	-0.5116 (0.4272)	-0.4322 (0.4228)	-0.5246 (0.4026)	-0.6533* (0.3827)	-0.6139* (0.3722)	-0.6505* (0.3618)	-0.5732 (0.3516)
Treatment ($t + 2$)	-0.5762 (0.5874)	-0.4620 (0.5683)	-0.6027 (0.5602)	-0.4909 (0.5454)	-0.7076 (0.5251)	-0.8870* (0.4980)	-0.8576* (0.4783)	-0.9505** (0.4666)	-0.8036* (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 (0.3074)	-0.1250 (0.2892)	-0.0319 (0.2895)	-0.0568 (0.2773)	0.0379 (0.2710)	0.1182 (0.2565)	0.0928 (0.2468)	0.0161 (0.2379)	0.0015 (0.2305)
Treatment ($t - 3$)	0.2366 (0.2889)	0.0124 (0.2801)	0.1653 (0.2879)	0.0934 (0.2729)	0.1596 (0.2578)	0.2255 (0.2449)	0.1961 (0.2365)	0.1339 (0.2323)	0.0746 (0.2272)
Treatment ($t - 2$)	0.1852 (0.2344)	0.2147 (0.2313)	0.2590 (0.2246)	0.2978 (0.2150)	0.2732 (0.2010)	0.2053 (0.1870)	0.1360 (0.1821)	0.0826 (0.1840)	0.0543 (0.1792)
Treatment ($t + 0$)	0.4412* (0.2606)	0.4189* (0.2506)	0.4874** (0.2439)	0.5420** (0.2395)	0.4970** (0.2245)	0.5120** (0.2124)	0.4659** (0.2046)	0.4622** (0.1993)	0.4553** (0.1941)
Treatment ($t + 1$)	-0.3479 (0.2891)	-0.3579 (0.2722)	-0.2042 (0.2593)	-0.1569 (0.2567)	-0.1262 (0.2433)	-0.0988 (0.2256)	-0.0832 (0.2195)	-0.0832 (0.2106)	-0.0189 (0.2048)
Treatment ($t + 2$)	0.3557 (0.3500)	0.3399 (0.3278)	0.3217 (0.3395)	0.4249 (0.3360)	0.4386 (0.3141)	0.3649 (0.2961)	0.3065 (0.2841)	0.2658 (0.2756)	0.3142 (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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

D.2 Clustering

In the baseline specification, 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 may 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 D2](#)). Standard errors change only marginally when clustering at this level, increasing for most coefficients. 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 D2](#) 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 D2: Robustness: Standard Errors

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

D.3 Controls

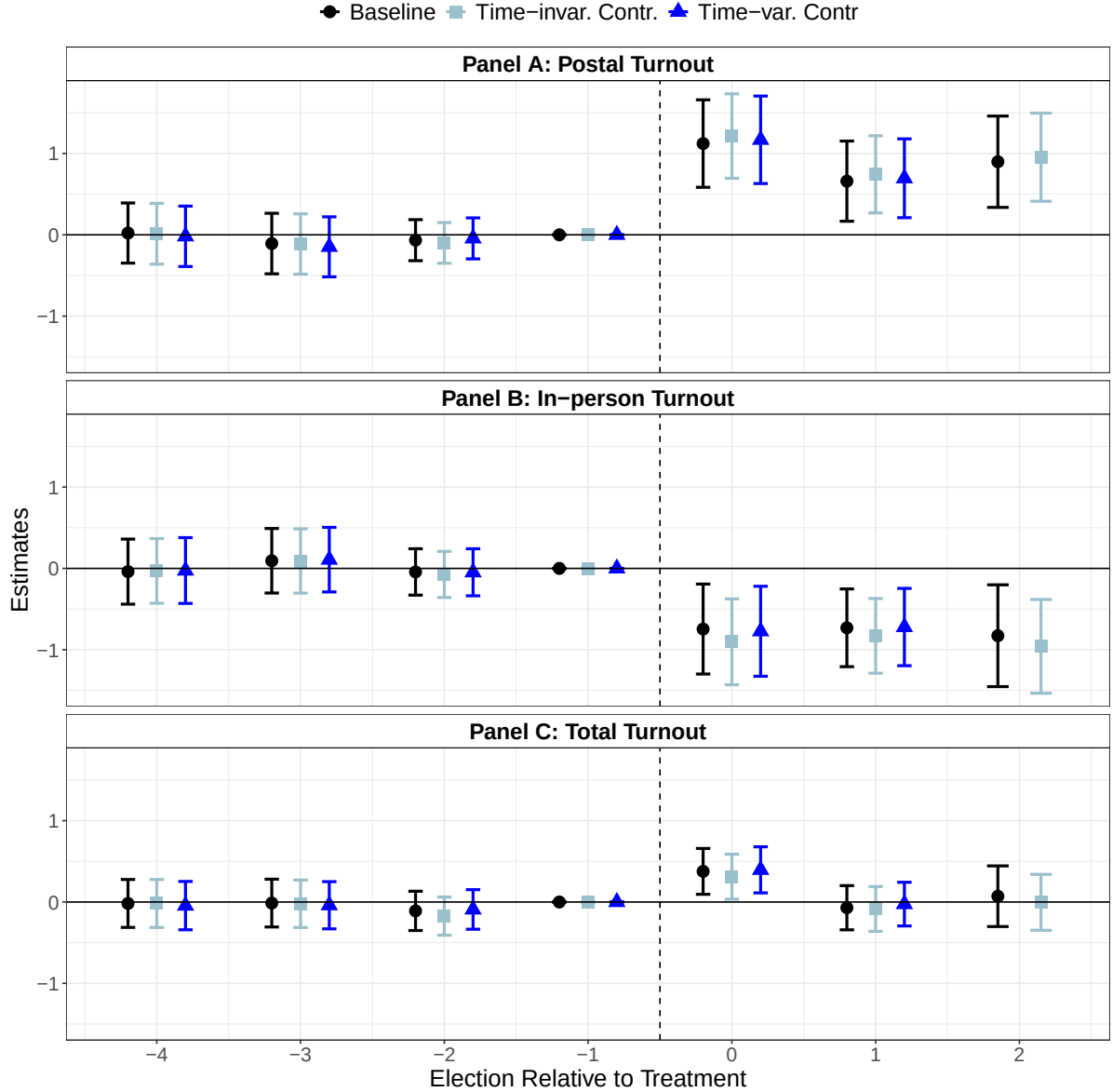
Additional Controls In the main specification, we only control for the time-invariant log number of eligible voters, held constant to the 2018 State Election, the last election before the treatment, interacted with election-specific coefficients. To test the robustness of the main specification by accounting for unobserved heterogeneity, we add further time-invariant controls, holding them also constant to 2018 (Figure D1). Additional controls are the share of unemployed, the share of women, 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.⁵³ In both specifications, the results remain unchanged.

COVID Mortality and Closure Controls To assess whether pandemic-related local conditions confound the estimated turnout effects, we add several sets of COVID controls as robustness specifications. First, we construct municipality-level COVID-19 mortality from the German Cause-of-Death Statistics, measuring deaths per 1,000 eligible voters over four windows: the 30 days before each election, the 90 days before each election, the same calendar year up to the election date, and cumulative deaths since the start of the pandemic. Figure D2 shows that the estimates remain substantively unchanged across these time-varying specifications. Second, we use the same four mortality measures but hold them fixed at their values as of the 2021 Federal Election, thereby capturing cross-sectional exposure to pandemic mortality around the first post-treatment election. The results are again robust (Figure D3). Finally, we account for early local pandemic disruption prior to the 2020 Local Elections using school and daycare closure records from Baskaran et al. (2023). Specifically, we add time-invariant controls for whether any closure record is observed in the municipality and for the total number of closures. The main results remain unchanged (Figure D4).

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 D5). 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

⁵³ 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.

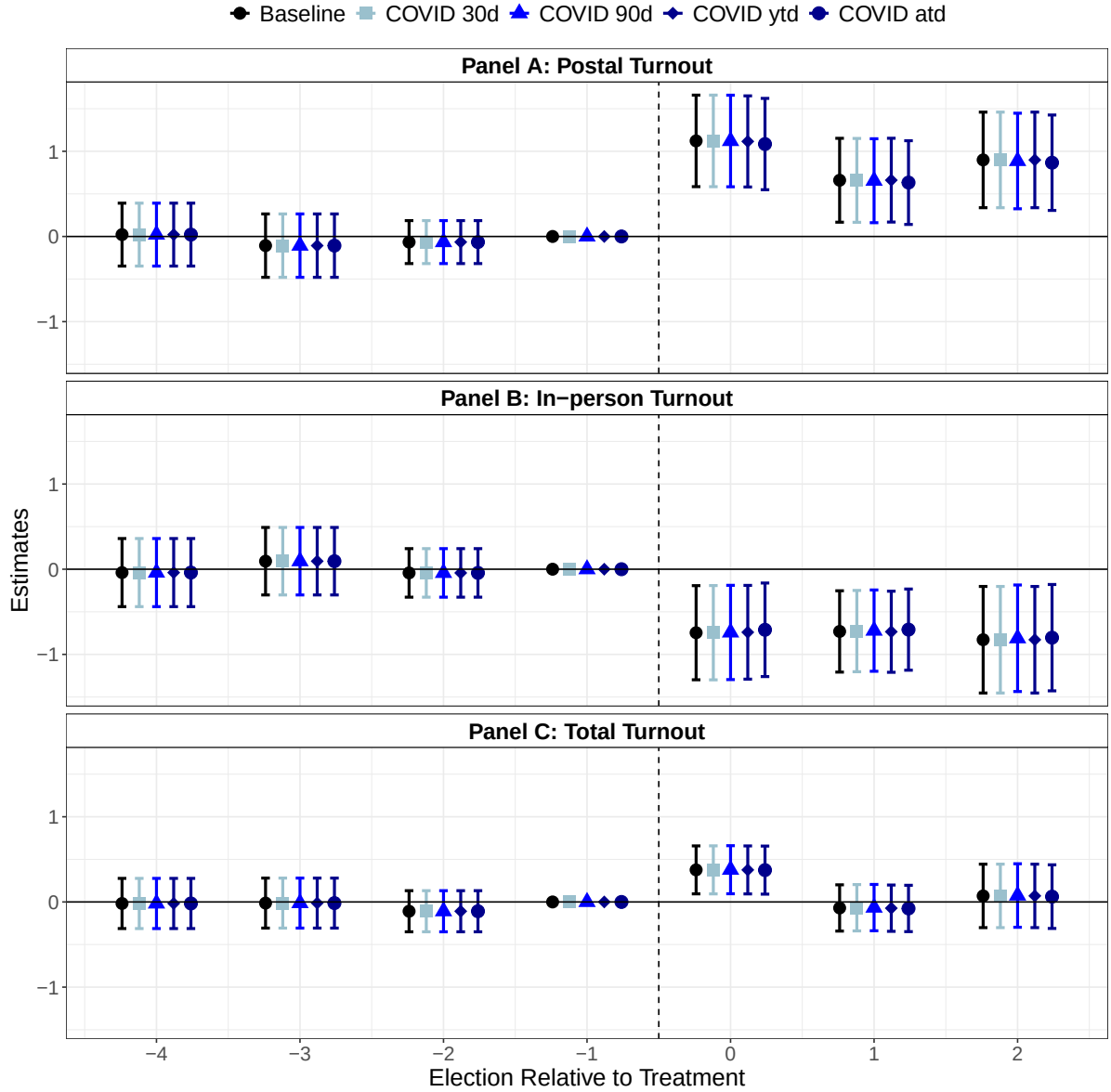
Figure D1: Additional Controls



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 women, 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 F19. 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.

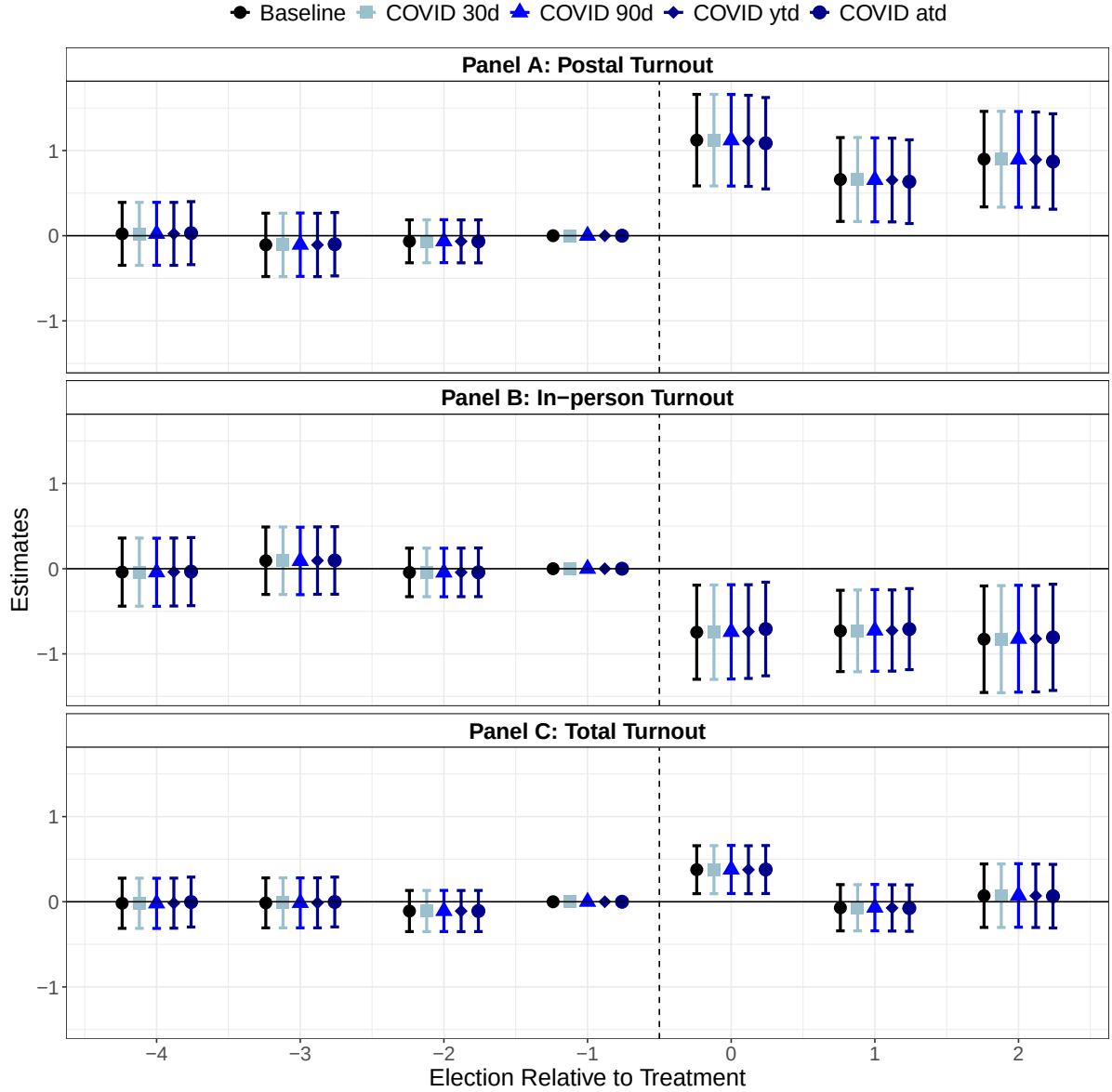
categories are constructed by splitting the sample based on the 2018 population measured as the number of eligible voters (thresholds: 1098, 1634, 2643, and 4496 eligible voters). In all specifications, the results remain robust.

Figure D2: Time-varying COVID Mortality Controls



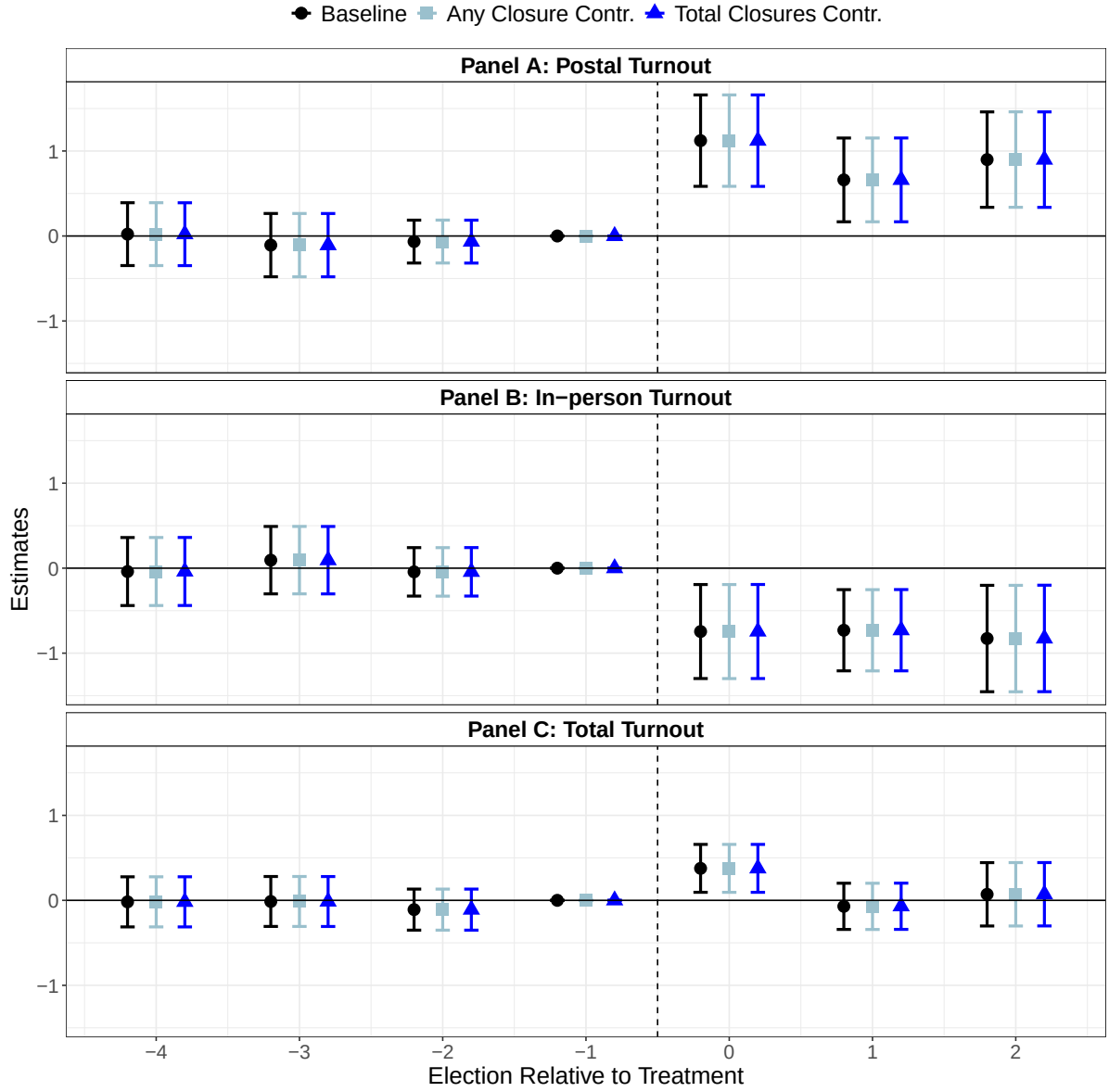
Notes: The figure presents event study results based on Equation 2 for postal, in-person, and total turnout (0–100) using different COVID mortality controls. *Baseline* repeats the main results in Figure 3. Additional specifications use the ICD COVID mortality definition and add election-specific controls for COVID deaths, measured per 1,000 eligible voters at the municipality level. *COVID 30d* uses deaths in the 30 days before each election; *COVID 90d* uses deaths in the 90 days before each election; *COVID ytd* uses deaths within the same year up to each election date; and *COVID atd* uses deaths since the start of the pandemic up to each election date. The point estimates and standard errors underlying the results are shown in Table F20. 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 D3: Time-invariant COVID Mortality Controls



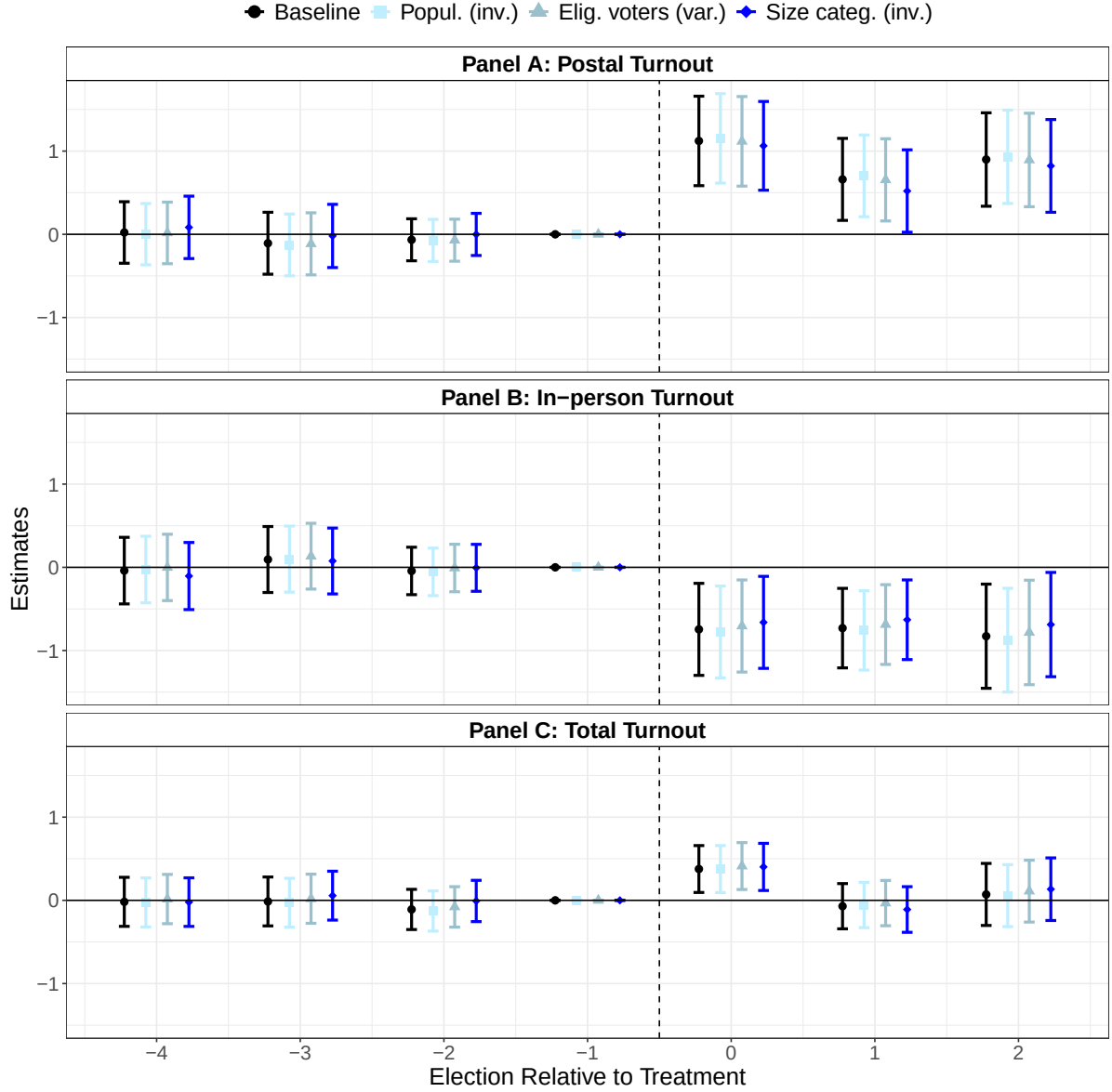
Notes: The figure presents event study results based on Equation 2 for postal, in-person, and total turnout (0–100) using different time-invariant COVID mortality controls. *Baseline* repeats the main results in Figure 3. Additional specifications use the ICD COVID mortality definition and add time-invariant controls for COVID deaths, measured per 1,000 eligible voters at the municipality level and fixed at their values as of the 2021 federal election. *COVID 30d* uses deaths in the 30 days before the 2021 federal election; *COVID 90d* uses deaths in the 90 days before the 2021 federal election; *COVID ytd* uses deaths within 2021 up to the 2021 federal election; and *COVID atd* uses deaths since the start of the pandemic up to the 2021 federal election. The point estimates and standard errors underlying the results are shown in Table F21. 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 D4: COVID-related School Closure Controls



Notes: The figure presents event study results based on [Equation 2](#) for postal, in-person, and total turnout (0–100) using different school and daycare closure controls. *Baseline* repeats the main results in [Figure 3](#). *Any Closure Contr.* flexibly controls for whether any school or daycare closure record is observed in the municipality. *Total Closures Contr.* flexibly controls for the total number of closure records aggregated at the municipality level. Sources are the school/daycare closure records from [Baskaran et al. \(2023\)](#). The point estimates and standard errors underlying the results are shown in [Table F22](#). 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 D5: 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 F23. 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.

D.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 D6](#).

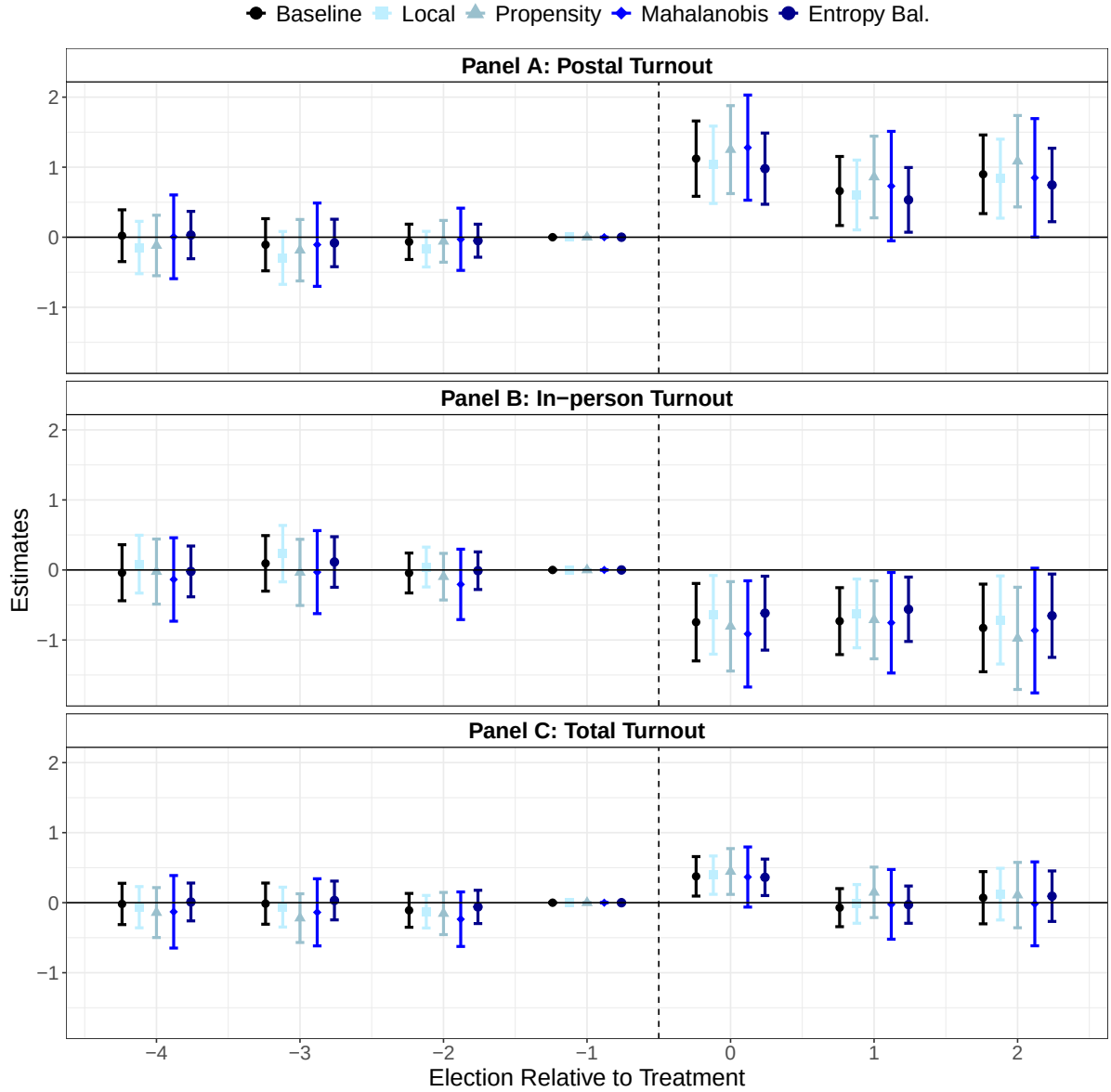
Local Matching First, we use a local matching approach based on geographical distances to ensure that control group municipalities far away from any treated municipality do not drive the effect, as municipalities closer to each other may be more similar. We identify untreated municipalities within the same county with a maximal border-to-border distance of 5km to a treated municipality. The remaining sample is depicted in [Figure D7](#). 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 $\times$ election level. Estimates for postal and in-person turnout remain significant, though postal turnout in $t + 1$ and in-person turnout in $t + 2$ are 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.

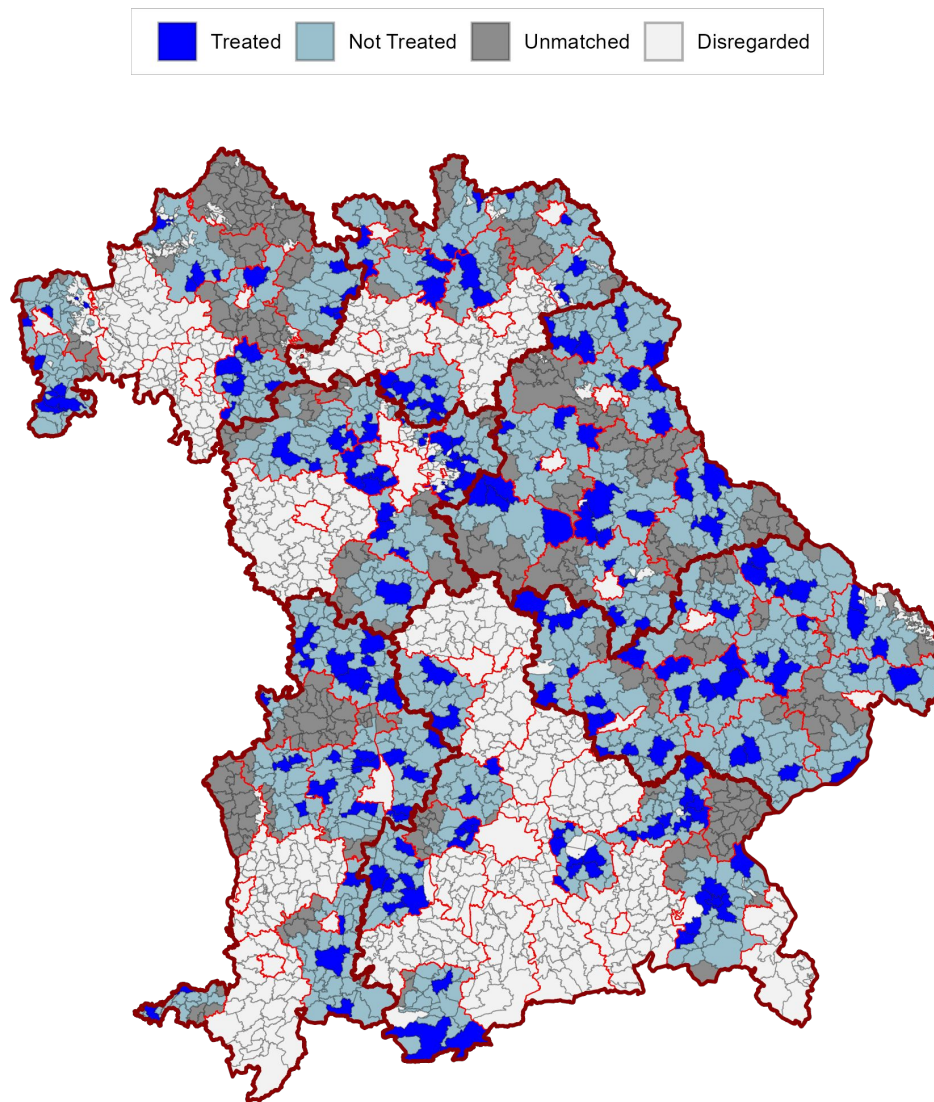
Entropy Balancing We implement the entropy balancing approach proposed by [Hainmueller \(2012\)](#). This method offers the advantage of not truncating the sample. Instead, it assigns weights to the control group to balance the treatment and control groups across the covariate distributions. Specifically, 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 unchanged.

Figure D6: 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 $\times$ 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 F24.

Figure D7: Local Matching



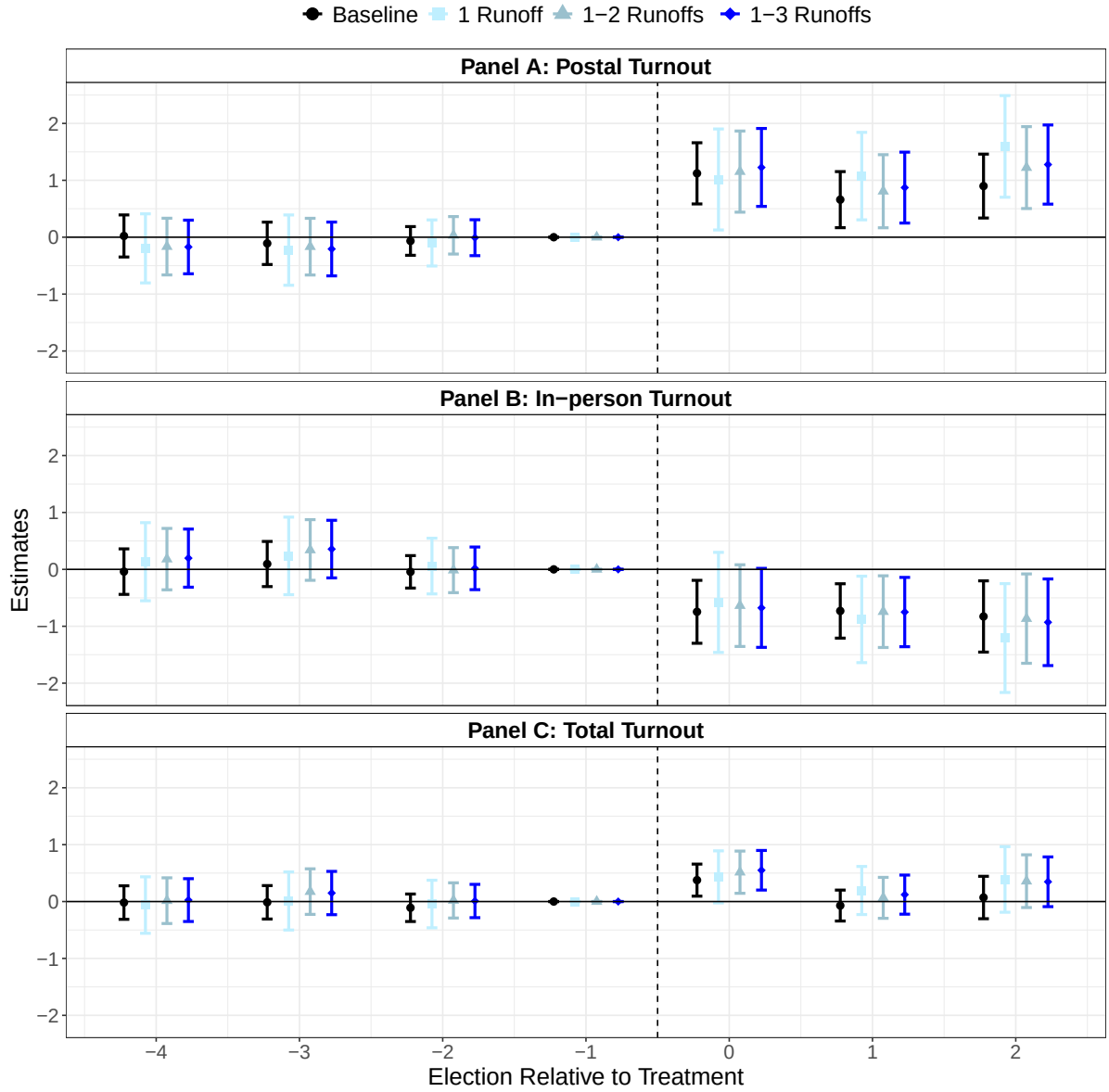
Notes: The figure presents a map of Bavaria containing municipalities used in the local matching approach. Only treated municipalities and control group municipalities that are at most 5km away from the border of a treated municipality within the same county are included. Thick red lines show government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

D.5 Number of Runoffs

We restrict the sample by the number of runoffs in the last three mayoral elections to ensure that results are not driven by treated municipalities with unusually frequent runoffs or by control municipalities that have never experienced a runoff. Results are presented in [Figure D8](#). *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.⁵⁴ All specifications show robust estimates compared to the baseline results with respect to the increase in total and postal turnout. For in-person turnout in $t + 0$, estimates under *1-2 Runoffs* and *1-3 Runoffs* are marginally significant, and under *1 Runoff*, significance is lacking. However, point estimates remain close to the baseline in all cases.

⁵⁴ 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 D8: 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 F25.

D.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 D3 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.

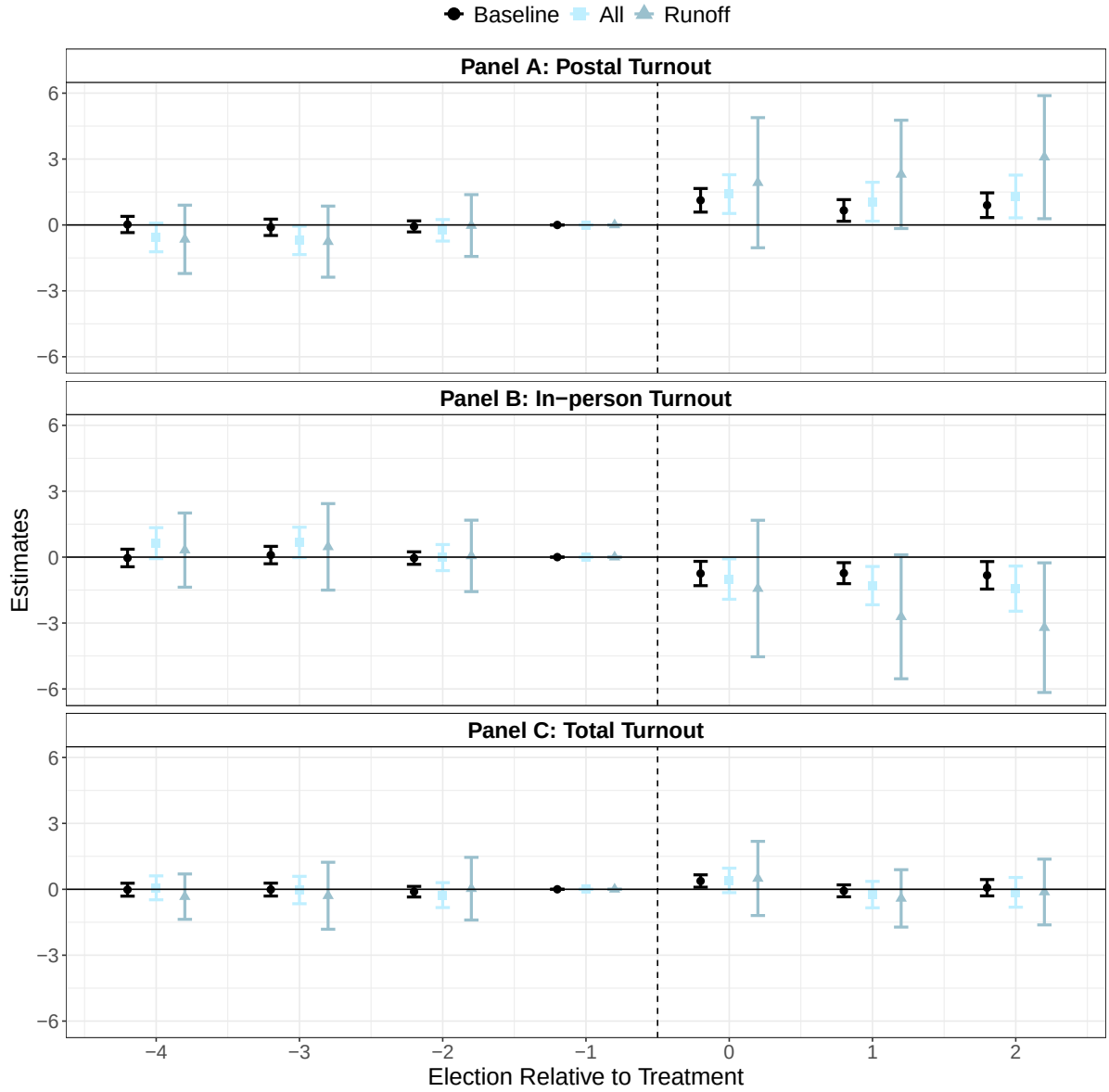
Table D3: Bavarian Local Elections by Runoff and Date

Panel A: 2020 Runoff Elections				
All	Mayor only	County only	Mayor & County	No runoff
2031	205	485	89	1252
Panel B: Off-cycle Elections				
Period	No county runoff in 2020		All municipalities	
	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 D9 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. The postal and in-person turnout effects remain statistically significant, whereas the total turnout estimate lacks statistical significance due to the smaller sample size. 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 D9: 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 F26.

D.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 D4](#) 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 D4: Robust Inference Approach

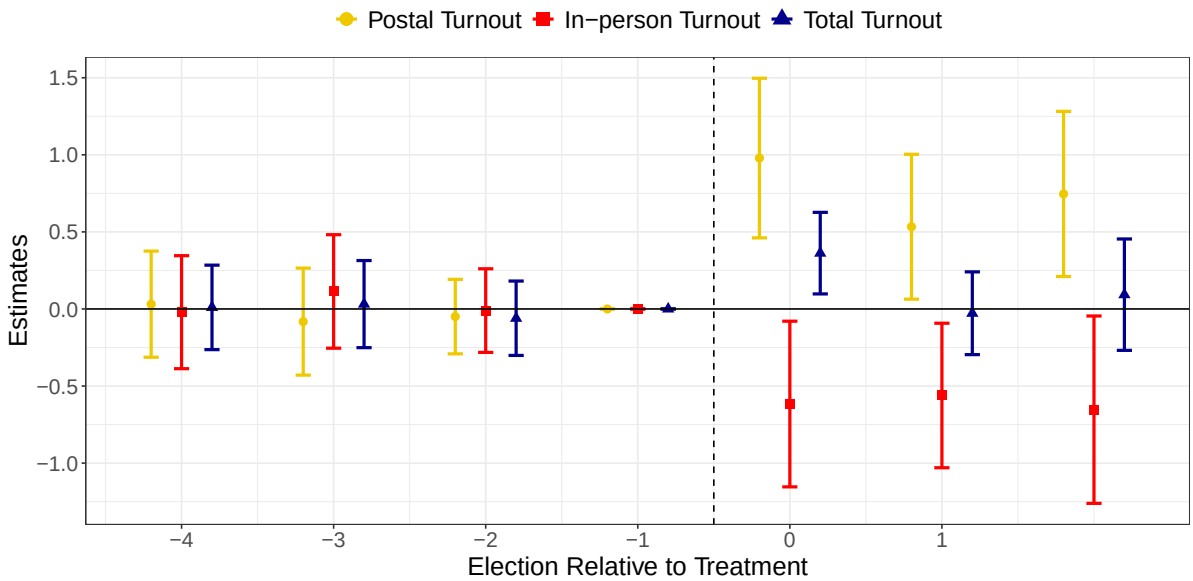
Relative Magnitude Bounds				Smoothness Restrictions			
M^{RM}	$t + 0$	$t + 1$	$t + 2$	M^{SD}	$t + 0$	$t + 1$	$t + 2$
	(1)	(2)	(3)		(4)	(5)	(6)
Panel A: Postal Turnout							
0	(0.576, 1.663)	(0.166, 1.162)	(0.327, 1.461)	0	(0.727, 1.794)	(0.247, 1.242)	(0.441, 1.567)
0.5	(0.521, 1.707)	(0.106, 1.202)	(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.8 Doubly Robust Difference-in-Differences

We implement the doubly robust difference-in-differences (DR-DiD) estimator proposed by [Sant’Anna and Zhao \(2020\)](#). The DR-DiD approach combines outcome regression with propensity-score weighting and is doubly robust in the sense that the resulting average treatment effect on the treated (ATT) is consistent if either the propensity score model or the outcome regression is correctly specified. To mirror our event study design, we estimate a sequence of two-period DR-DiD effects by comparing each election period to the reference election ($t - 1$) separately. As [Sant’Anna and Zhao \(2020\)](#) discuss in Section 2.1, the identifying framework rules out unit-specific time trends; accordingly, we focus on specifications that condition on time-invariant covariates, including county indicators and baseline population held constant at its 2018 value. The results are presented in [Figure D10](#) and are consistent with our baseline findings, suggesting that our conclusions are not driven by functional-form choices in either the propensity score or outcome model.

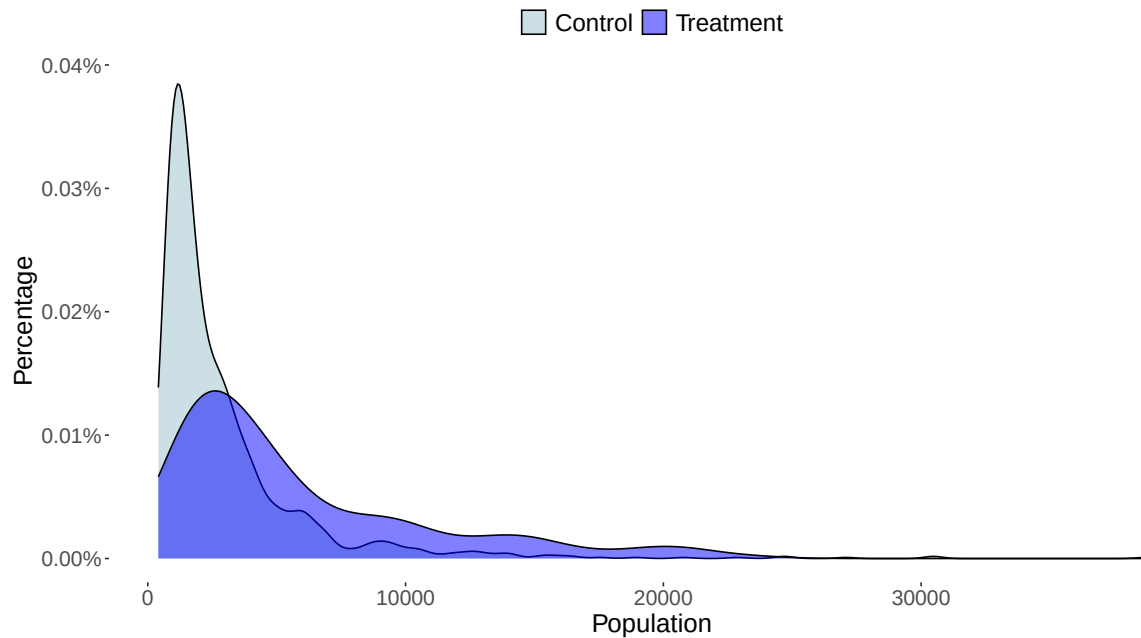
Figure D10: Doubly Robust DiD Approach



Notes: The figure presents event-time ATTs for postal, in-person, and total turnout (0-100) estimated using the doubly robust difference-in-differences estimator of [Sant’Anna and Zhao \(2020\)](#). Each point estimate is obtained from a separate two-period comparison between the reference election ($t - 1$), the 2018 State Election, and the election at event time $t + k$. All specifications condition on time-invariant covariates, including county indicators and baseline population held constant at 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 [Table F27](#).

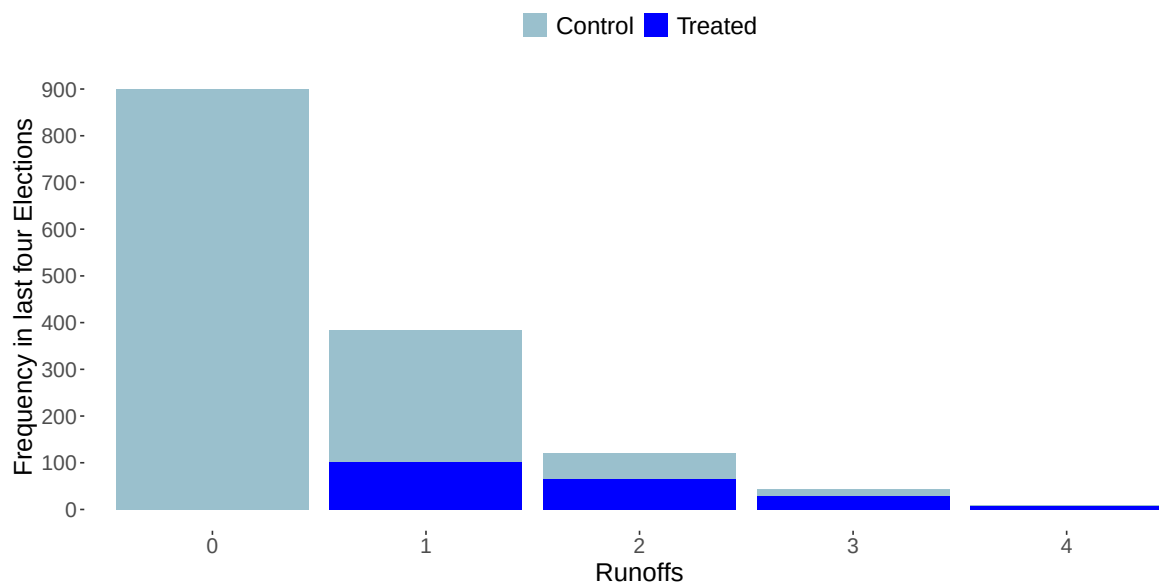
E Figures

Figure E1: Density of Population by Treatment



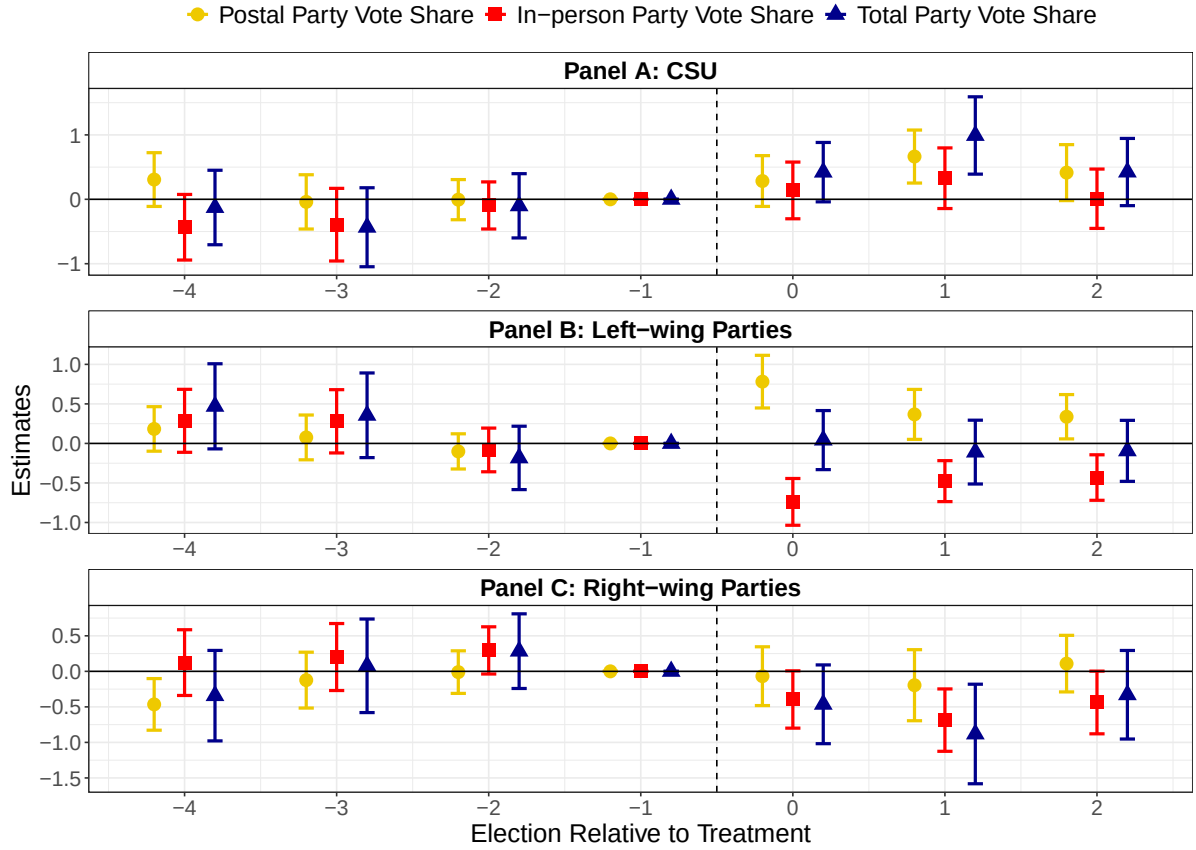
Notes: The figure shows a density plot depicting the distribution of the population variable for the treatment and control groups, representing the relative concentration of observations across the range of population values.

Figure E2: Number of Runoffs in the Last Four Elections



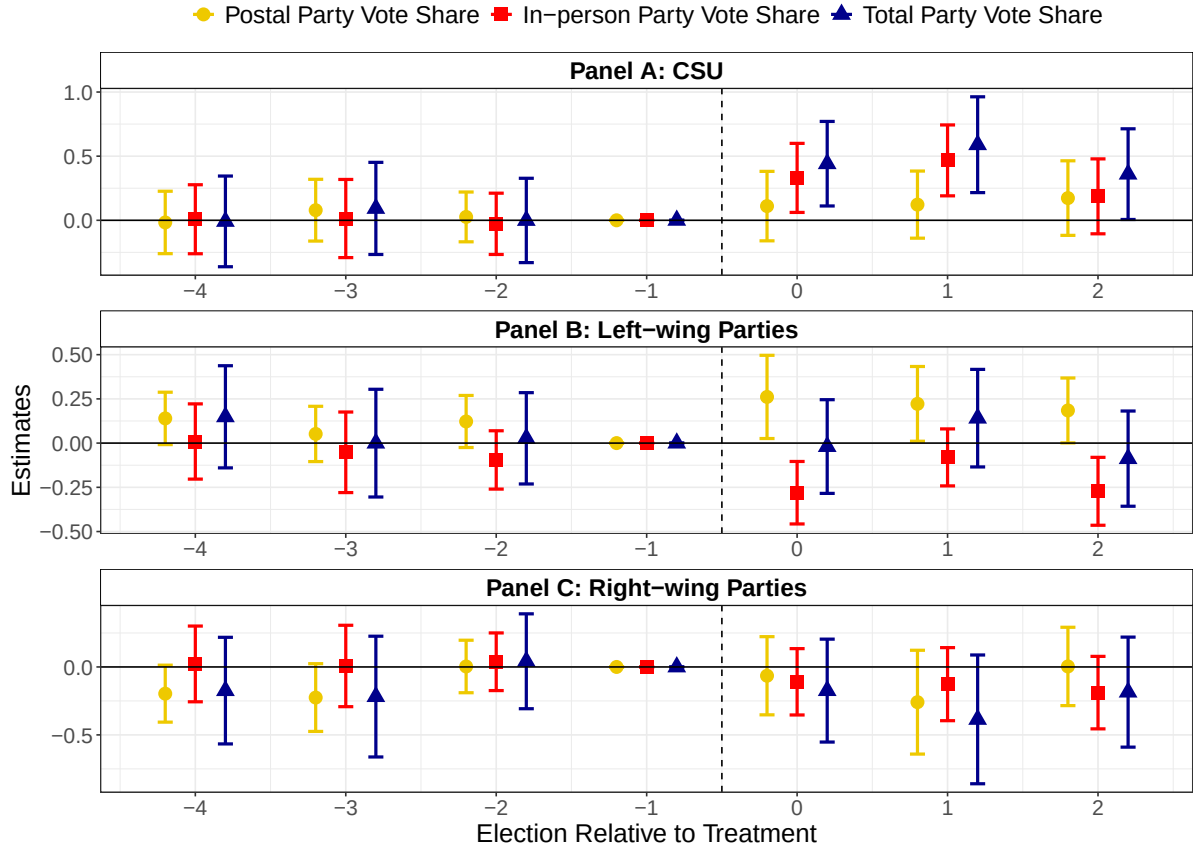
Notes: The figure shows the number of runoffs per municipality for Bavarian mayoral elections for the last four mayoral elections.

Figure E3: Election Results (Cast Votes)



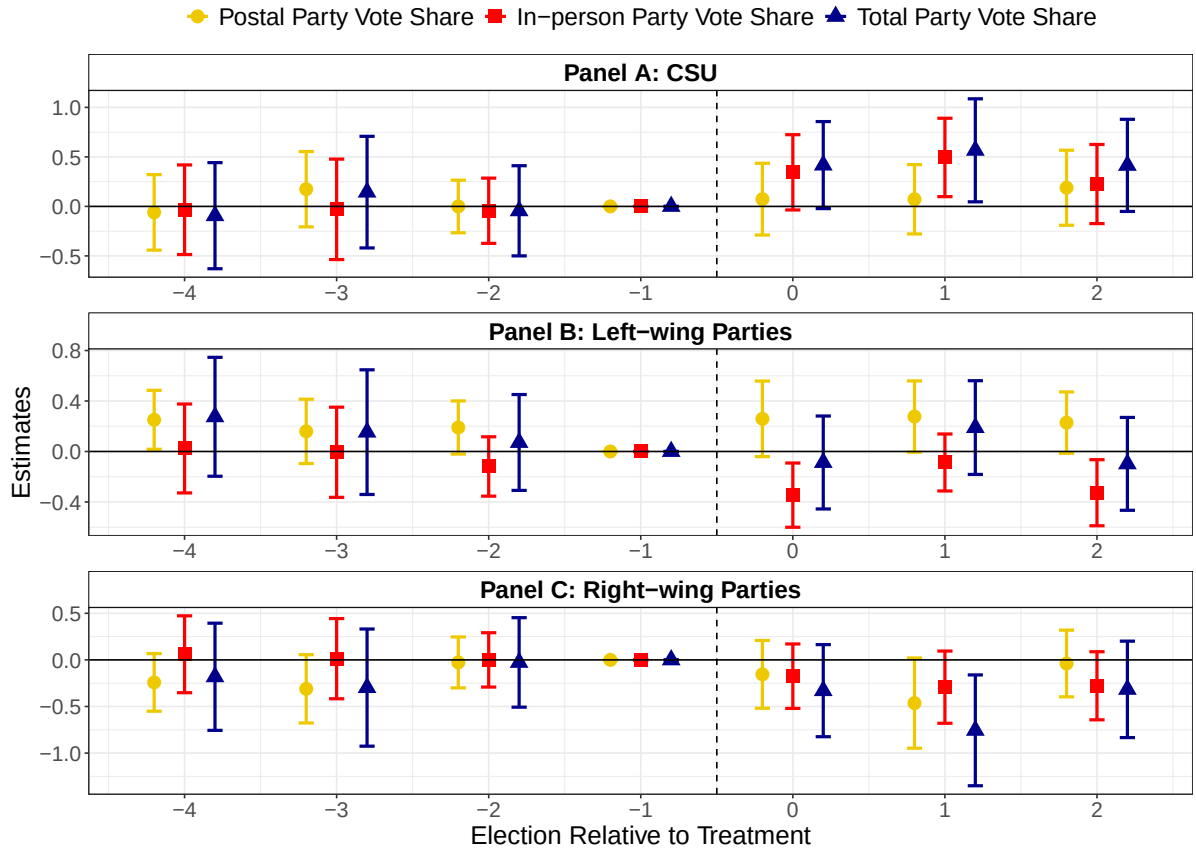
Notes: The figure presents event study results based on Equation 2 for postal, in-person, and total vote shares (0-100)—each as a share of votes cast—of the party *Christlich Soziale Union* (CSU, Panel A), and of left- and other right-wing parties (Panel B and C). Left-wing parties include *Die Linke* (The Left), the *Sozialdemokratische Partei Deutschlands* (SPD), and *Bündnis 90/Die Grünen* (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 F8.

Figure E4: Party Heterogeneity by Past Turnout (Elig. Voters)



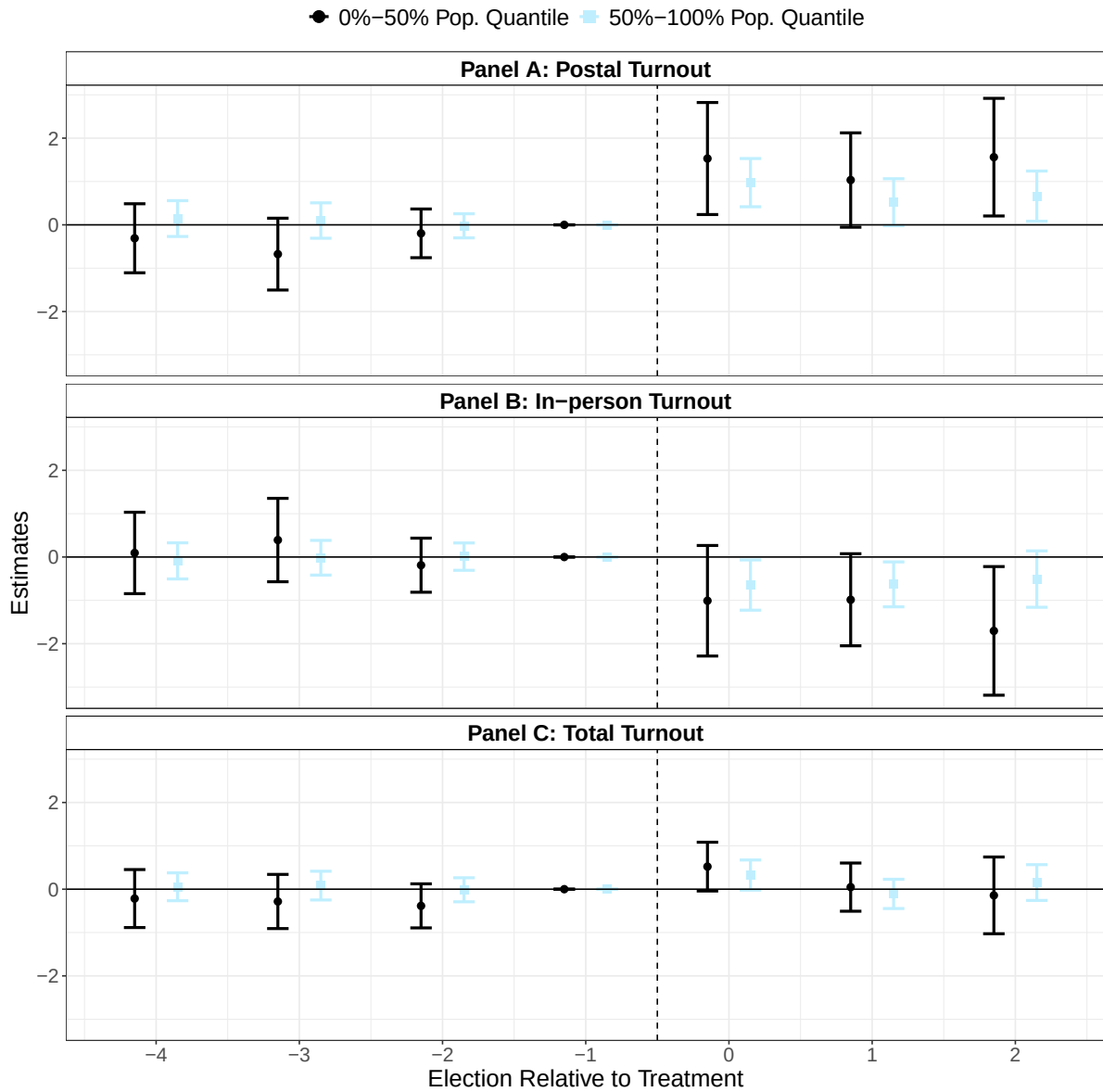
Notes: The figure presents triple difference estimates (μ^τ) based on Equation 3 for postal, in-person, and total vote shares (0-100)—each as a share of eligible voters—of the party *Christlich Soziale Union* (CSU, Panel A), and of left- and other right-wing parties (Panel B and C). Left-wing parties include *Die Linke* (The Left), the *Sozialdemokratische Partei Deutschlands* (SPD), and *Bündnis 90/Die Grünen* (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 F10.

Figure E5: Party Heterogeneity by Past Turnout (Cast Votes)



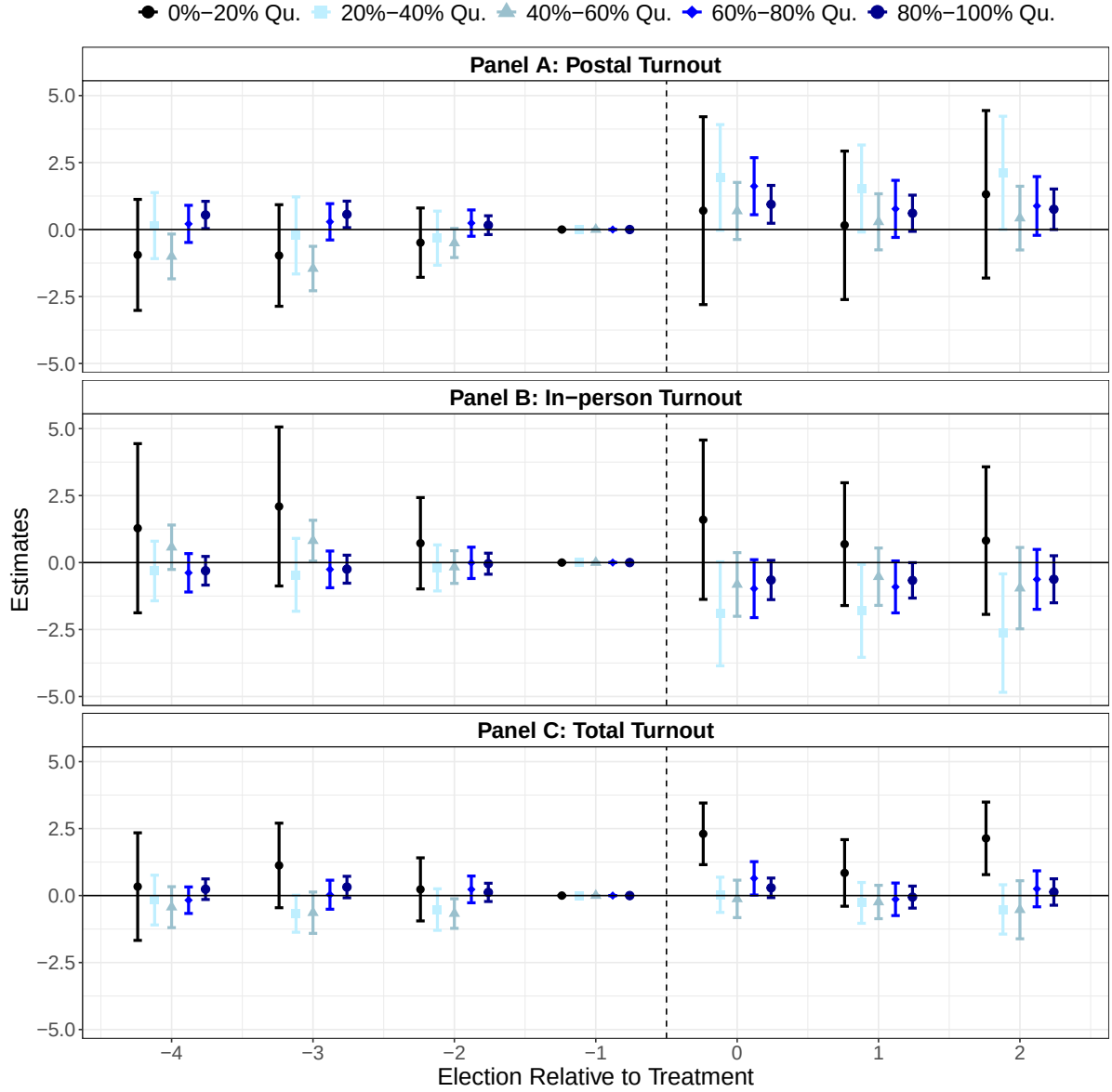
Notes: The figure presents triple difference estimates (μ^τ) based on Equation 3 for postal, in-person, and total vote shares (0-100)—each as a share of of votes cast—of the party *Christlich Soziale Union* (CSU, Panel A), and of left- and other right-wing parties (Panel B and C). Left-wing parties include *Die Linke* (The Left), the *Sozialdemokratische Partei Deutschlands* (SPD), and *Bündnis 90/Die Grünen* (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 F11.

Figure E6: Heterogeneity by Population Size: Median



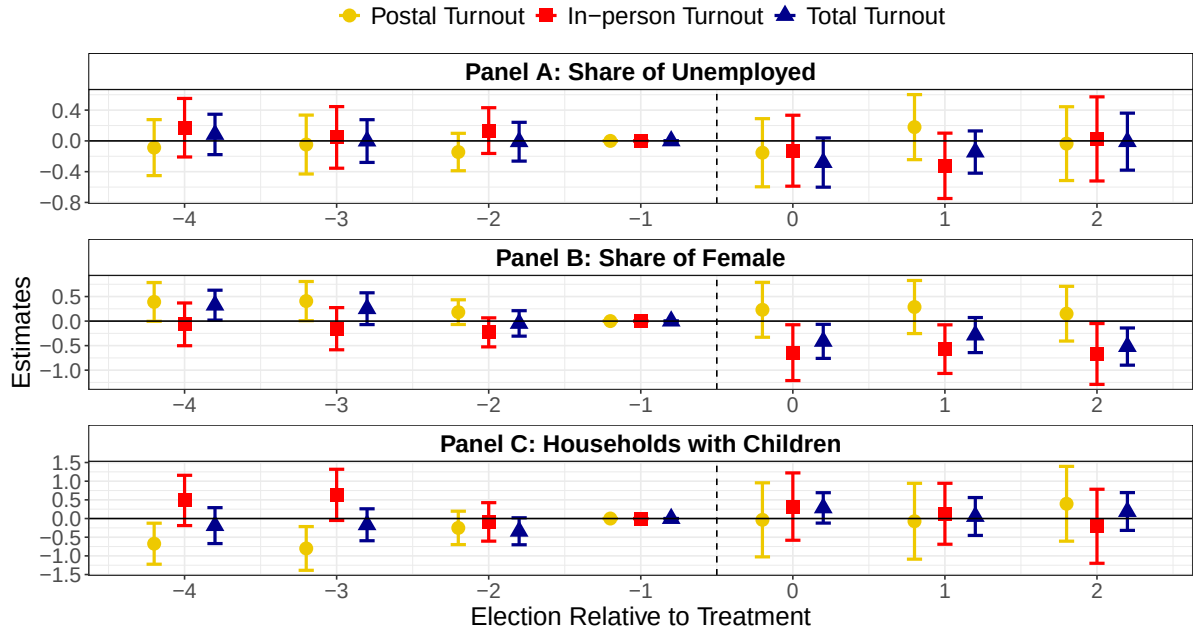
Notes: The figure presents event study results based on Equation 3 for postal, in-person, and total turnout (0-100). Marginal effects for each category, instead of coefficients of interaction terms, are shown. The reference election ($t - 1$) is the 2018 State Election and it is controlled for the population 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. 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 F12.

Figure E7: Heterogeneity by Population Size: Quintiles



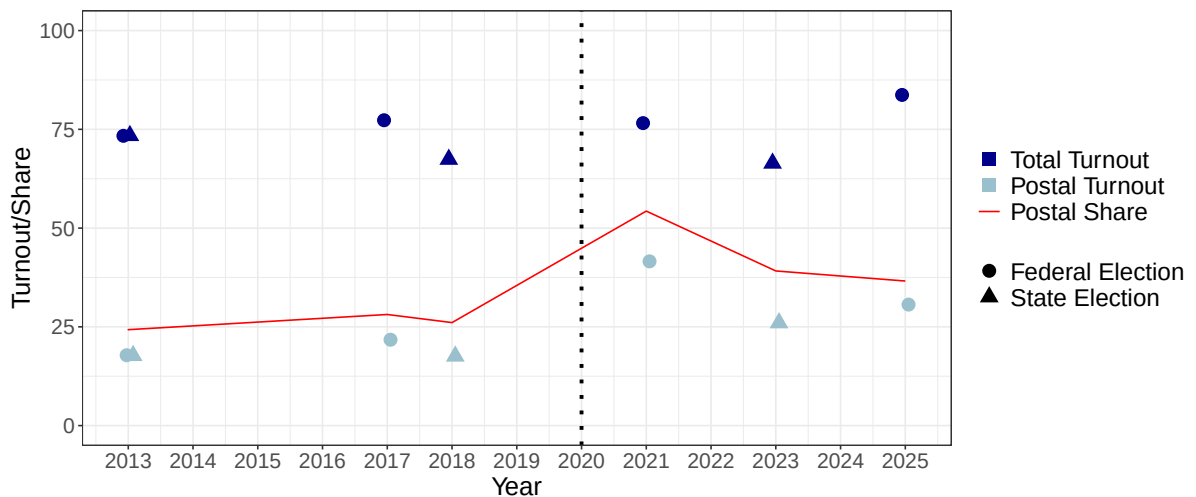
Notes: The figure presents event study results based on Equation 3 for postal, in-person, and total turnout (0-100). Marginal effects for each category, instead of coefficients of interaction terms, are shown. The reference election ($t - 1$) is the 2018 State Election and it is controlled for the population 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. 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 F14.

Figure E8: 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 unit 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 F16.

Figure E9: 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.

F Tables

Table F1: Summary Statistics

Panel A: Full Sample								
Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Median	Pctl(75)	Max
Total Turnout (%)	1,457	74.610	5.112	53.463	71.459	74.931	78.238	95.664
Postal Turnout (%)	1,457	28.317	5.677	0.000	24.578	28.213	32.235	49.671
In-person Turnout (%)	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 (%)	205	73.869	5.430	57.238	70.154	74.477	77.644	84.349
Postal Turnout (%)	205	28.999	4.474	19.002	25.853	28.521	32.236	44.481
In-person Turnout (%)	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 (%)	1,252	74.731	5.050	53.463	71.671	75.013	78.323	95.664
Postal Turnout (%)	1,252	28.205	5.844	0.000	24.332	28.114	32.235	49.671
In-person Turnout (%)	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 F2: Balance Table (Socio-Economic and Election Covariates)

	Treatment														
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Eligible Voters (log)	0.1250*** (0.0132)	0.0954*** (0.0232)	0.1168*** (0.0143)	0.1245*** (0.0133)	0.1202*** (0.0147)	0.1245*** (0.0140)	0.1230*** (0.0133)	0.1229*** (0.0137)	0.1511*** (0.0270)	0.1250*** (0.0132)	0.1267*** (0.0132)	0.1249*** (0.0132)	0.1265*** (0.0132)	0.1240*** (0.0133)	0.1234*** (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 women, share of households with children, and the shares of the population aged 0–14, 15–17, 18–19, 20–29, 30–59, 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](#). Standard errors are clustered at the municipality level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table F3: Balance Table (School/Daycare Closure Records)

	Treatment	
	(1)	(2)
Any closure record	-0.0115 (0.0564)	
Total closure records		0.0032 (0.0150)
Observations	1,457	1,457
R ²	0.13276	0.13277
County FE	✓	✓

Notes: Each column reports a cross-sectional balance regression at baseline with the treatment indicator as the dependent variable. Column (1) includes an indicator for whether any closure record is observed in the municipality; Column (2) includes the total number of closure records aggregated at the municipality level. Sources are the school/daycare closure records from [Baskaran et al. \(2023\)](#). All regressions include the log number of eligible voters and county fixed effects, analogous to [Equation 2](#). Standard errors are clustered at the municipality level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table F4: Balance Table (COVID)

	Treatment							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
COVID deaths 2021 per 1,000 population	-0.0030 (0.0655)	-0.0232 (0.0437)	-0.0024 (0.0080)	-0.0073 (0.0050)				
COVID deaths 2023 per 1,000 population					-0.0407 (0.0667)	-0.0826 (0.0527)	0.0122 (0.0238)	-0.0056 (0.0040)
Observations	1,457	1,457	1,457	1,457	1,457	1,457	1,457	1,457
R ²	0.13273	0.13281	0.13277	0.13349	0.13283	0.13339	0.13285	0.13353
Specification	30 days	90 days	Year to date	All to date	30 days	90 days	Year to date	All to date
County FE	✓	✓	✓	✓	✓	✓	✓	✓

Notes: The table reports cross-sectional balance tests with the treatment indicator as the dependent variable. Covariates are COVID deaths in the 30 or 90 days before the 2021 Bundestag election and the 2023 State Election, measured per 1,000 eligible voters at the municipality level. COVID deaths are defined using the broad definition, including deaths for which the underlying or contributing cause of death is coded as COVID-19. Each single regression includes the log number of eligible voters and county fixed effects, analogous to [Equation 2](#). Standard errors are clustered at the municipality level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table F5: 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 F6: Main Specification

	Postal Turnout (1)	In-person Turnout (2)	Total Turnout (3)
Treatment ($t - 4$)	0.0218 (0.1885)	-0.0396 (0.2040)	-0.0178 (0.1503)
Treatment ($t - 3$)	-0.1075 (0.1898)	0.0941 (0.2022)	-0.0134 (0.1498)
Treatment ($t - 2$)	-0.0658 (0.1286)	-0.0433 (0.1454)	-0.1091 (0.1231)
Treatment ($t + 0$)	1.122*** (0.2741)	-0.7456*** (0.2819)	0.3765*** (0.1436)
Treatment ($t + 1$)	0.6601*** (0.2512)	-0.7305*** (0.2436)	-0.0705 (0.1386)
Treatment ($t + 2$)	0.8988*** (0.2862)	-0.8279*** (0.3191)	0.0710 (0.1901)
Population ($t - 4$)	1.160*** (0.0974)	-0.8467*** (0.0945)	0.3133*** (0.0836)
Population ($t - 3$)	1.185*** (0.1032)	-0.3337*** (0.0933)	0.8512*** (0.0943)
Population ($t - 2$)	0.5945*** (0.0728)	0.3148*** (0.0732)	0.9093*** (0.0734)
Population ($t + 0$)	-1.304*** (0.1309)	1.476*** (0.1230)	0.1718* (0.1018)
Population ($t + 1$)	-1.751*** (0.1252)	1.314*** (0.1165)	-0.4367*** (0.1016)
Population ($t + 2$)	-1.222*** (0.1378)	1.975*** (0.1400)	0.7528*** (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 F7: 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 (0.1374)	-0.1733 (0.1549)	0.0172 (0.1991)
Treatment ($t - 3$)	-0.0169 (0.1351)	-0.2012 (0.1755)	-0.2181 (0.2012)
Treatment ($t - 2$)	-0.0486 (0.1113)	-0.2119 (0.1317)	-0.2605 (0.1759)
Treatment ($t + 0$)	0.2748* (0.1523)	0.0248 (0.1649)	0.2996* (0.1686)
Treatment ($t + 1$)	0.4362*** (0.1556)	0.1649 (0.1691)	0.6011*** (0.2138)
Treatment ($t + 2$)	0.2933* (0.1686)	-0.1809 (0.1725)	0.1124 (0.1845)
R ²	0.90855	0.94353	0.91549
Panel B: Left-wing Parties			
Treatment ($t - 4$)	0.0670 (0.0890)	0.0904 (0.1240)	0.1575 (0.1660)
Treatment ($t - 3$)	0.0212 (0.0873)	0.1178 (0.1292)	0.1390 (0.1665)
Treatment ($t - 2$)	-0.0325 (0.0837)	-0.0370 (0.1017)	-0.0695 (0.1477)
Treatment ($t + 0$)	0.7230*** (0.1346)	-0.4953*** (0.1122)	0.2277 (0.1397)
Treatment ($t + 1$)	0.2825** (0.1176)	-0.3585*** (0.0940)	-0.0759 (0.1446)
Treatment ($t + 2$)	0.3478*** (0.1091)	-0.2905*** (0.1123)	0.0573 (0.1463)
R ²	0.94508	0.94800	0.95847
Panel C: Right-wing Parties			
Treatment ($t - 4$)	-0.2953** (0.1209)	0.0735 (0.1571)	-0.2218 (0.2099)
Treatment ($t - 3$)	-0.0803 (0.1374)	0.1287 (0.1635)	0.0485 (0.2310)
Treatment ($t - 2$)	0.0124 (0.1064)	0.1967 (0.1239)	0.2091 (0.1942)
Treatment ($t + 0$)	0.0376 (0.1660)	-0.2761* (0.1486)	-0.2385 (0.2168)
Treatment ($t + 1$)	-0.2078 (0.2062)	-0.5402*** (0.1622)	-0.7480*** (0.2833)
Treatment ($t + 2$)	0.1242 (0.1631)	-0.3772** (0.1779)	-0.2530 (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 Grünen* (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 F8: 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 (0.2127)	-0.4337* (0.2596)	-0.1272 (0.2948)
Treatment ($t - 3$)	-0.0400 (0.2148)	-0.3938 (0.2874)	-0.4339 (0.3125)
Treatment ($t - 2$)	-0.0056 (0.1589)	-0.0954 (0.1865)	-0.1010 (0.2543)
Treatment ($t + 0$)	0.2838 (0.2010)	0.1378 (0.2244)	0.4216* (0.2350)
Treatment ($t + 1$)	0.6639*** (0.2098)	0.3272 (0.2399)	0.9911*** (0.3060)
Treatment ($t + 2$)	0.4143* (0.2215)	0.0090 (0.2349)	0.4233 (0.2660)
R ²	0.87166	0.95626	0.94471
Panel B: Left-wing Parties			
Treatment ($t - 4$)	0.1833 (0.1435)	0.2860 (0.2030)	0.4693* (0.2745)
Treatment ($t - 3$)	0.0760 (0.1444)	0.2798 (0.2039)	0.3558 (0.2729)
Treatment ($t - 2$)	-0.1009 (0.1135)	-0.0823 (0.1408)	-0.1831 (0.2043)
Treatment ($t + 0$)	0.7814*** (0.1698)	-0.7392*** (0.1507)	0.0422 (0.1904)
Treatment ($t + 1$)	0.3672** (0.1610)	-0.4768*** (0.1320)	-0.1096 (0.2057)
Treatment ($t + 2$)	0.3375** (0.1429)	-0.4315*** (0.1467)	-0.0939 (0.1966)
R ²	0.92946	0.94683	0.94760
Panel C: Right-wing Parties			
Treatment ($t - 4$)	-0.4651** (0.1852)	0.1231 (0.2358)	-0.3421 (0.3245)
Treatment ($t - 3$)	-0.1235 (0.2004)	0.2016 (0.2403)	0.0780 (0.3356)
Treatment ($t - 2$)	-0.0107 (0.1527)	0.2948* (0.1693)	0.2841 (0.2678)
Treatment ($t + 0$)	-0.0679 (0.2108)	-0.3958* (0.2056)	-0.4638 (0.2826)
Treatment ($t + 1$)	-0.1951 (0.2549)	-0.6865*** (0.2237)	-0.8816** (0.3571)
Treatment ($t + 2$)	0.1086 (0.2030)	-0.4380* (0.2249)	-0.3294 (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 Grünen* (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 F9: Heterogeneity by Past Turnout

	Postal Turnout (1)	In-person Turnout (2)	Total Turnout (3)
Treatment ($t - 4$)	0.0221 (0.1894)	-0.0457 (0.2049)	-0.0236 (0.1481)
Treatment ($t - 3$)	-0.1013 (0.1926)	0.0932 (0.2030)	-0.0081 (0.1425)
Treatment ($t - 2$)	-0.0474 (0.1297)	-0.0626 (0.1368)	-0.1099 (0.1025)
Treatment ($t + 0$)	1.182*** (0.2739)	-0.7729*** (0.2470)	0.4091*** (0.1169)
Treatment ($t + 1$)	0.6826*** (0.2523)	-0.7174*** (0.2230)	-0.0347 (0.1285)
Treatment ($t + 2$)	0.9625*** (0.2894)	-0.8823*** (0.2761)	0.0802 (0.1361)
Turnout $\times$ Treatment ($t - 4$)	0.0269 (0.1611)	-0.0650 (0.1670)	-0.0381 (0.1351)
Turnout $\times$ Treatment ($t - 3$)	0.0816 (0.1667)	0.0004 (0.1688)	0.0820 (0.1396)
Turnout $\times$ Treatment ($t - 2$)	0.1839 (0.1205)	-0.1426 (0.1260)	0.0413 (0.1092)
Turnout $\times$ Treatment ($t + 0$)	0.5420** (0.2414)	-0.1631 (0.1934)	0.3788** (0.1572)
Turnout $\times$ Treatment ($t + 1$)	0.2008 (0.2373)	0.1932 (0.1985)	0.3940*** (0.1518)
Turnout $\times$ Treatment ($t + 2$)	0.5995** (0.2599)	-0.4059* (0.2371)	0.1936 (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 unit standard deviation). Standard errors are clustered at the municipality level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table F10: 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 $\times$ Treatment ($t - 4$)	-0.0167 (0.1240)	0.0082 (0.1371)	-0.0085 (0.1802)
Turnout $\times$ Treatment ($t - 3$)	0.0788 (0.1227)	0.0138 (0.1553)	0.0926 (0.1831)
Turnout $\times$ Treatment ($t - 2$)	0.0264 (0.0989)	-0.0275 (0.1218)	-0.0011 (0.1676)
Turnout $\times$ Treatment ($t + 0$)	0.1107 (0.1380)	0.3305** (0.1372)	0.4412*** (0.1680)
Turnout $\times$ Treatment ($t + 1$)	0.1223 (0.1334)	0.4671*** (0.1409)	0.5894*** (0.1904)
Turnout $\times$ Treatment ($t + 2$)	0.1732 (0.1482)	0.1867 (0.1489)	0.3599** (0.1802)
R ²	0.90930	0.94744	0.91847
Panel B: Left-wing Parties			
Turnout $\times$ Treatment ($t - 4$)	0.1396* (0.0755)	0.0091 (0.1084)	0.1487 (0.1472)
Turnout $\times$ Treatment ($t - 3$)	0.0518 (0.0797)	-0.0520 (0.1161)	-0.0002 (0.1552)
Turnout $\times$ Treatment ($t - 2$)	0.1222 (0.0750)	-0.0951 (0.0841)	0.0271 (0.1316)
Turnout $\times$ Treatment ($t + 0$)	0.2611** (0.1199)	-0.2805*** (0.0900)	-0.0194 (0.1350)
Turnout $\times$ Treatment ($t + 1$)	0.2218** (0.1077)	-0.0808 (0.0821)	0.1410 (0.1406)
Turnout $\times$ Treatment ($t + 2$)	0.1847** (0.0935)	-0.2725*** (0.0979)	-0.0878 (0.1372)
R ²	0.94679	0.94900	0.95990
Panel C: Right-wing Parties			
Turnout $\times$ Treatment ($t - 4$)	-0.1965* (0.1069)	0.0223 (0.1420)	-0.1742 (0.2000)
Turnout $\times$ Treatment ($t - 3$)	-0.2253* (0.1271)	0.0071 (0.1527)	-0.2181 (0.2265)
Turnout $\times$ Treatment ($t - 2$)	0.0035 (0.0985)	0.0382 (0.1081)	0.0418 (0.1780)
Turnout $\times$ Treatment ($t + 0$)	-0.0647 (0.1466)	-0.1093 (0.1244)	-0.1740 (0.1930)
Turnout $\times$ Treatment ($t + 1$)	-0.2592 (0.1947)	-0.1266 (0.1370)	-0.3858 (0.2415)
Turnout $\times$ Treatment ($t + 2$)	0.0037 (0.1470)	-0.1889 (0.1359)	-0.1851 (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 Grünen* (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 unit standard deviation). Standard errors are clustered at the municipality level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table F11: 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 $\times$ Treatment ($t - 4$)	-0.0602 (0.1944)	-0.0335 (0.2306)	-0.0938 (0.2731)
Turnout $\times$ Treatment ($t - 3$)	0.1738 (0.1939)	-0.0296 (0.2586)	0.1442 (0.2874)
Turnout $\times$ Treatment ($t - 2$)	-0.0007 (0.1353)	-0.0437 (0.1679)	-0.0443 (0.2321)
Turnout $\times$ Treatment ($t + 0$)	0.0731 (0.1847)	0.3442* (0.1938)	0.4173* (0.2241)
Turnout $\times$ Treatment ($t + 1$)	0.0721 (0.1785)	0.4945** (0.2019)	0.5666** (0.2652)
Turnout $\times$ Treatment ($t + 2$)	0.1885 (0.1932)	0.2259 (0.2037)	0.4144* (0.2372)
R ²	0.87323	0.95659	0.94524
Panel B: Left-wing Parties			
Turnout $\times$ Treatment ($t - 4$)	0.2511** (0.1191)	0.0238 (0.1795)	0.2750 (0.2402)
Turnout $\times$ Treatment ($t - 3$)	0.1594 (0.1300)	-0.0062 (0.1821)	0.1532 (0.2518)
Turnout $\times$ Treatment ($t - 2$)	0.1901* (0.1073)	-0.1187 (0.1199)	0.0714 (0.1936)
Turnout $\times$ Treatment ($t + 0$)	0.2587* (0.1526)	-0.3459*** (0.1297)	-0.0872 (0.1877)
Turnout $\times$ Treatment ($t + 1$)	0.2767* (0.1439)	-0.0872 (0.1150)	0.1895 (0.1893)
Turnout $\times$ Treatment ($t + 2$)	0.2286* (0.1240)	-0.3267** (0.1335)	-0.0981 (0.1876)
R ²	0.93159	0.94953	0.95175
Panel C: Right-wing Parties			
Turnout $\times$ Treatment ($t - 4$)	-0.2416 (0.1576)	0.0238 (0.1795)	-0.2416 (0.1576)
Turnout $\times$ Treatment ($t - 3$)	-0.3105* (0.1867)	-0.0062 (0.1821)	-0.3105* (0.1867)
Turnout $\times$ Treatment ($t - 2$)	-0.0270 (0.1394)	-0.1187 (0.1199)	-0.0270 (0.1394)
Turnout $\times$ Treatment ($t + 0$)	-0.1551 (0.1853)	-0.3459*** (0.1297)	-0.1551 (0.1853)
Turnout $\times$ Treatment ($t + 1$)	-0.4634* (0.2468)	-0.0872 (0.1150)	-0.4634* (0.2468)
Turnout $\times$ Treatment ($t + 2$)	-0.0386 (0.1824)	-0.3267** (0.1335)	-0.0386 (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 Grünen* (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 unit standard deviation). Standard errors are clustered at the municipality level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table F12: 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 (0.4059)	0.0938 (0.4789)	-0.2161 (0.3408)
Treatment ($t - 3$)	-0.6754 (0.4225)	0.3917 (0.4905)	-0.2837 (0.3185)
Treatment ($t - 2$)	-0.1964 (0.2864)	-0.1885 (0.3180)	-0.3849 (0.2591)
Treatment ($t + 0$)	1.530** (0.6588)	-1.009 (0.6500)	0.5207* (0.2869)
Treatment ($t + 1$)	1.034* (0.5548)	-0.9862* (0.5414)	0.0475 (0.2833)
Treatment ($t + 2$)	1.561** (0.6917)	-1.704** (0.7562)	-0.1427 (0.4514)
R ²	0.96767	0.94899	0.95918
Panel B: 50%-100% Population Quantile			
Treatment ($t - 4$)	0.1446 (0.2105)	-0.0890 (0.2124)	0.0556 (0.1640)
Treatment ($t - 3$)	0.0993 (0.2081)	-0.0166 (0.2042)	0.0827 (0.1691)
Treatment ($t - 2$)	-0.0223 (0.1417)	0.0086 (0.1609)	-0.0137 (0.1412)
Treatment ($t + 0$)	0.9736*** (0.2836)	-0.6472** (0.2953)	0.3263* (0.1772)
Treatment ($t + 1$)	0.5220* (0.2762)	-0.6309** (0.2641)	-0.1089 (0.1721)
Treatment ($t + 2$)	0.6624** (0.2951)	-0.5093 (0.3315)	0.1531 (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 F13: 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 (0.7072)	0.5974 (0.9099)	0.4194 (0.6356)
Treatment ($t - 3$)	-0.4204 (0.7253)	0.8593 (0.9491)	0.4389 (0.4949)
Treatment ($t - 2$)	0.0143 (0.4989)	-0.0527 (0.5361)	-0.0384 (0.4057)
Treatment ($t + 0$)	1.088 (1.190)	0.1546 (1.112)	1.242** (0.4938)
Treatment ($t + 1$)	0.9775 (0.9634)	-0.5314 (0.9462)	0.4460 (0.4689)
Treatment ($t + 2$)	1.387 (1.164)	-0.4707 (1.166)	0.9159 (0.5850)
R ²	0.96766	0.94897	0.95917
Panel B: 33%-67% Population Quantile			
Treatment ($t - 4$)	-0.8323** (0.3350)	0.3034 (0.3276)	-0.5289* (0.2889)
Treatment ($t - 3$)	-1.143*** (0.3409)	0.5876* (0.3171)	-0.5554* (0.3074)
Treatment ($t - 2$)	-0.5837** (0.2390)	0.1969 (0.2713)	-0.3868 (0.2608)
Treatment ($t + 0$)	1.245** (0.4969)	-1.251** (0.5266)	-0.0067 (0.2994)
Treatment ($t + 1$)	0.5946 (0.4690)	-0.9210** (0.4501)	-0.3264 (0.2594)
Treatment ($t + 2$)	0.8603* (0.5193)	-1.365** (0.6056)	-0.5051 (0.4117)
R ²	0.96771	0.94905	0.95915
Panel C: 67%-100% Population Quantile			
Treatment ($t - 4$)	0.5083** (0.2260)	-0.3203 (0.2357)	0.1880 (0.1638)
Treatment ($t - 3$)	0.5136** (0.2227)	-0.3122 (0.2248)	0.2014 (0.1691)
Treatment ($t - 2$)	0.2151 (0.1485)	-0.1477 (0.1688)	0.0674 (0.1353)
Treatment ($t + 0$)	1.025*** (0.3038)	-0.6305** (0.3058)	0.3948** (0.1656)
Treatment ($t + 1$)	0.5723* (0.2963)	-0.6573** (0.2835)	-0.0849 (0.1867)
Treatment ($t + 2$)	0.7325** (0.3308)	-0.5346 (0.3648)	0.1978 (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 F14: Heterogeneity by Population Size: Quintiles

	Postal Turnout (1)	In-person Turnout (2)	Total Turnout (3)
Panel A: 0%-20% Population Quintile			
Treatment ($t - 4$)	-0.9463 (1.056)	1.281 (1.610)	0.3350 (1.023)
Treatment ($t - 3$)	-0.9686 (0.9662)	2.093 (1.513)	1.125 (0.8044)
Treatment ($t - 2$)	-0.4894 (0.6593)	0.7206 (0.8692)	0.2313 (0.6004)
Treatment ($t + 0$)	0.7046 (1.787)	1.600 (1.516)	2.304*** (0.5863)
Treatment ($t + 1$)	0.1575 (1.413)	0.6873 (1.168)	0.8448 (0.6337)
Treatment ($t + 2$)	1.316 (1.593)	0.8194 (1.404)	2.135*** (0.6902)
R ²	0.96762	0.94898	0.95919
Panel B: 20%-40% Population Quintile			
Treatment ($t - 4$)	0.1471 (0.6283)	-0.3146 (0.5673)	-0.1674 (0.4733)
Treatment ($t - 3$)	-0.2203 (0.7331)	-0.4582 (0.6932)	-0.6785* (0.3520)
Treatment ($t - 2$)	-0.3223 (0.5147)	-0.2000 (0.4382)	-0.5224 (0.3952)
Treatment ($t + 0$)	1.943* (1.006)	-1.914* (0.9895)	0.0294 (0.3350)
Treatment ($t + 1$)	1.532* (0.8279)	-1.804** (0.8840)	-0.2722 (0.3883)
Treatment ($t + 2$)	2.116** (1.076)	-2.633** (1.127)	-0.5173 (0.4696)
R ²	0.96767	0.94905	0.95913
Panel C: 40%-60% Population Quintile			
Treatment ($t - 4$)	-1.004** (0.4258)	0.5720 (0.4225)	-0.4319 (0.3893)
Treatment ($t - 3$)	-1.456*** (0.4226)	0.8167** (0.3884)	-0.6389 (0.3936)
Treatment ($t - 2$)	-0.5018* (0.2771)	-0.1689 (0.3102)	-0.6707** (0.2813)
Treatment ($t + 0$)	0.6933 (0.5425)	-0.8172 (0.6056)	-0.1239 (0.3563)
Treatment ($t + 1$)	0.2881 (0.5334)	-0.5281 (0.5457)	-0.2400 (0.3179)
Treatment ($t + 2$)	0.4267 (0.6063)	-0.9561 (0.7744)	-0.5294 (0.5525)
R ²	0.96767	0.94899	0.95914
Panel D: 60%-80% Population Quintile			
Treatment ($t - 4$)	0.2106 (0.3532)	-0.3826 (0.3655)	-0.1719 (0.2524)
Treatment ($t - 3$)	0.2879 (0.3452)	-0.2556 (0.3495)	0.0323 (0.2761)
Treatment ($t - 2$)	0.2405 (0.2505)	-0.0102 (0.2980)	0.2302 (0.2545)
Treatment ($t + 0$)	1.617*** (0.5428)	-0.9739* (0.5512)	0.6434** (0.3173)
Treatment ($t + 1$)	0.7703 (0.5430)	-0.9091* (0.4945)	-0.1388 (0.3097)
Treatment ($t + 2$)	0.8799 (0.5586)	-0.6276 (0.5698)	0.2523 (0.3425)
R ²	0.96768	0.94902	0.95916
Panel E: 80%-100% Population Quintile			
Treatment ($t - 4$)	0.5435** (0.2597)	-0.3059 (0.2728)	0.2377 (0.1956)
Treatment ($t - 3$)	0.5650** (0.2524)	-0.2469 (0.2662)	0.3182 (0.2045)
Treatment ($t - 2$)	0.1626 (0.1791)	-0.0427 (0.2000)	0.1200 (0.1738)
Treatment ($t + 0$)	0.9403*** (0.3600)	-0.6511* (0.3736)	0.2892 (0.1861)
Treatment ($t + 1$)	0.6081* (0.3444)	-0.6649** (0.3366)	-0.0569 (0.2095)
Treatment ($t + 2$)	0.7567** (0.3855)	-0.6238 (0.4477)	0.1329 (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 F15: Heterogeneity by Age

	Postal Turnout (1)	In-person Turnout (2)	Total Turnout (3)
Panel A: Share of 15 to 17 year old			
$Z \times \text{Treatment } (t - 4)$	0.1657 (0.1961)	-0.2538 (0.2042)	-0.0881 (0.1676)
$Z \times \text{Treatment } (t - 3)$	0.1519 (0.1942)	-0.2347 (0.2039)	-0.0828 (0.1604)
$Z \times \text{Treatment } (t - 2)$	-0.0047 (0.1305)	-0.1934 (0.1574)	-0.1981 (0.1277)
$Z \times \text{Treatment } (t + 0)$	-0.0667 (0.2751)	0.0563 (0.2741)	-0.0104 (0.1435)
$Z \times \text{Treatment } (t + 1)$	-0.0266 (0.2788)	0.1986 (0.2569)	0.1720 (0.1490)
$Z \times \text{Treatment } (t + 2)$	0.0224 (0.3056)	-0.1252 (0.3187)	-0.1028 (0.1901)
R^2	0.96780	0.94911	0.95917
Panel B: Share of 18 to 19 year old			
$Z \times \text{Treatment } (t - 4)$	-0.4392** (0.1979)	0.2120 (0.2287)	-0.2273 (0.1884)
$Z \times \text{Treatment } (t - 3)$	-0.4139* (0.2115)	0.3832* (0.2295)	-0.0306 (0.1753)
$Z \times \text{Treatment } (t - 2)$	0.0289 (0.1474)	-0.0843 (0.1653)	-0.0554 (0.1521)
$Z \times \text{Treatment } (t + 0)$	-0.3415 (0.3140)	0.4814 (0.3149)	0.1399 (0.1825)
$Z \times \text{Treatment } (t + 1)$	-0.5808** (0.2875)	0.5838** (0.2688)	0.0030 (0.1780)
$Z \times \text{Treatment } (t + 2)$	-0.5286 (0.3448)	0.5885* (0.3550)	0.0599 (0.2136)
R^2	0.96767	0.94903	0.95919
Panel C: Share of 20 to 29 year old			
$Z \times \text{Treatment } (t - 4)$	0.1505 (0.1827)	-0.1413 (0.1914)	0.0093 (0.1581)
$Z \times \text{Treatment } (t - 3)$	0.0948 (0.1892)	-0.1179 (0.1956)	-0.0231 (0.1595)
$Z \times \text{Treatment } (t - 2)$	0.0036 (0.1289)	0.1287 (0.1460)	0.1323 (0.1241)
$Z \times \text{Treatment } (t + 0)$	-0.6467** (0.3092)	0.9184*** (0.2972)	0.2716* (0.1563)
$Z \times \text{Treatment } (t + 1)$	-0.5274* (0.2882)	0.6849** (0.2705)	0.1575 (0.1590)
$Z \times \text{Treatment } (t + 2)$	-0.5769* (0.3131)	1.047*** (0.3284)	0.4701** (0.2144)
R^2	0.96773	0.94917	0.95935
Panel D: Share of 30 to 59 year old			
$Z \times \text{Treatment } (t - 4)$	-0.0050 (0.1664)	-0.0566 (0.2031)	-0.0616 (0.1470)
$Z \times \text{Treatment } (t - 3)$	-0.1383 (0.1696)	-0.0936 (0.2030)	-0.2320 (0.1496)
$Z \times \text{Treatment } (t - 2)$	0.1308 (0.1420)	-0.2032 (0.1427)	-0.0724 (0.1233)
$Z \times \text{Treatment } (t + 0)$	0.1587 (0.2469)	0.0715 (0.2732)	0.2302 (0.1407)
$Z \times \text{Treatment } (t + 1)$	0.0445 (0.2214)	0.1198 (0.2360)	0.1643 (0.1203)
$Z \times \text{Treatment } (t + 2)$	0.1221 (0.2617)	-0.0166 (0.3198)	0.1055 (0.1711)
R^2	0.96773	0.94899	0.95942
Panel E: Share of over 60 year old			
$Z \times \text{Treatment } (t - 4)$	-0.3130* (0.1749)	0.3925** (0.1931)	0.0795 (0.1475)
$Z \times \text{Treatment } (t - 3)$	-0.2180 (0.1792)	0.3381* (0.2046)	0.1200 (0.1516)
$Z \times \text{Treatment } (t - 2)$	-0.2740** (0.1346)	0.3012** (0.1453)	0.0272 (0.1167)
$Z \times \text{Treatment } (t + 0)$	0.2200 (0.2536)	-0.4333 (0.2800)	-0.2133 (0.1589)
$Z \times \text{Treatment } (t + 1)$	0.4672* (0.2459)	-0.5412** (0.2479)	-0.0741 (0.1386)
$Z \times \text{Treatment } (t + 2)$	0.1938 (0.2800)	-0.3321 (0.3435)	-0.1383 (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 unit standard deviation). Standard errors are clustered at the municipality level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table F16: Heterogeneity by Socio-Economic Characteristics

	Postal Turnout (1)	In-person Turnout (2)	Total Turnout (3)
Panel A: Unemployment			
$Z \times \text{Treatment } (t - 4)$	-0.0874 (0.1855)	0.1714 (0.1939)	0.0840 (0.1338)
$Z \times \text{Treatment } (t - 3)$	-0.0473 (0.1951)	0.0452 (0.2041)	-0.0021 (0.1416)
$Z \times \text{Treatment } (t - 2)$	-0.1447 (0.1239)	0.1340 (0.1516)	-0.0106 (0.1285)
$Z \times \text{Treatment } (t + 0)$	-0.1538 (0.2257)	-0.1276 (0.2350)	-0.2814* (0.1632)
$Z \times \text{Treatment } (t + 1)$	0.1790 (0.2156)	-0.3240 (0.2165)	-0.1450 (0.1400)
$Z \times \text{Treatment } (t + 2)$	-0.0356 (0.2445)	0.0258 (0.2787)	-0.0098 (0.1889)
R^2	0.96868	0.95201	0.95977
Panel B: Share Female			
$Z \times \text{Treatment } (t - 4)$	0.3913* (0.2013)	-0.0664 (0.2221)	0.3249** (0.1553)
$Z \times \text{Treatment } (t - 3)$	0.4076** (0.2034)	-0.1549 (0.2191)	0.2527 (0.1652)
$Z \times \text{Treatment } (t - 2)$	0.1824 (0.1282)	-0.2295 (0.1508)	-0.0472 (0.1322)
$Z \times \text{Treatment } (t + 0)$	0.2301 (0.2853)	-0.6431** (0.2900)	-0.4130** (0.1769)
$Z \times \text{Treatment } (t + 1)$	0.2867 (0.2757)	-0.5715** (0.2524)	-0.2848 (0.1820)
$Z \times \text{Treatment } (t + 2)$	0.1509 (0.2842)	-0.6701** (0.3162)	-0.5192*** (0.1932)
R^2	0.96777	0.94944	0.95931
Panel C: Households with Children			
$Z \times \text{Treatment } (t - 4)$	-0.6739** (0.2799)	0.4847 (0.3432)	-0.1892 (0.2451)
$Z \times \text{Treatment } (t - 3)$	-0.8007*** (0.2985)	0.6339* (0.3494)	-0.1668 (0.2180)
$Z \times \text{Treatment } (t - 2)$	-0.2507 (0.2279)	-0.0903 (0.2626)	-0.3410* (0.1841)
$Z \times \text{Treatment } (t + 0)$	-0.0351 (0.5055)	0.3201 (0.4595)	0.2850 (0.2073)
$Z \times \text{Treatment } (t + 1)$	-0.0726 (0.5177)	0.1276 (0.4160)	0.0549 (0.2586)
$Z \times \text{Treatment } (t + 2)$	0.3952 (0.5104)	-0.2072 (0.5054)	0.1879 (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 unit standard deviation). Standard errors are clustered at the municipality level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table F17: Placebos

	(1)	(2)	(3)	(4)
Panel A: Postal Turnout				
Treatment ($t - 4$)	0.0218 (0.1885)		-0.3531 (0.6326)	-0.0383 (0.3035)
Treatment ($t - 3$)	-0.1075 (0.1898)		-0.3512 (0.6363)	-0.0953 (0.2912)
Treatment ($t - 2$)	-0.0658 (0.1286)	-0.0094 (0.1353)	0.0088 (0.3066)	-0.2497 (0.2285)
Treatment ($t + 0$)	1.122*** (0.2741)	0.2025 (0.2934)	-0.2267 (0.9565)	0.3650 (0.3463)
Treatment ($t + 1$)	0.6601*** (0.2512)	-0.0972 (0.3259)	-0.3613 (0.5619)	-0.1458 (0.3791)
Treatment ($t + 2$)	0.8988*** (0.2862)	-0.2339 (0.4836)	-0.4560 (0.7441)	-0.2728 (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 (0.2040)		-0.8388 (0.7495)	0.0402 (0.2831)
Treatment ($t - 3$)	0.0941 (0.2022)		-0.8290 (0.7507)	0.1590 (0.2541)
Treatment ($t - 2$)	-0.0433 (0.1454)	-0.0534 (0.1726)	-0.6765 (0.8040)	0.3019 (0.1984)
Treatment ($t + 0$)	-0.7456*** (0.2819)	-0.1125 (0.2772)	-0.4943 (1.077)	-0.1235 (0.3240)
Treatment ($t + 1$)	-0.7305*** (0.2436)	0.0770 (0.3436)	0.3289 (0.5616)	0.3850 (0.3399)
Treatment ($t + 2$)	-0.8279*** (0.3191)	0.1695 (0.4327)	-0.3511 (0.8685)	0.0342 (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 (0.1503)		-1.192** (0.5544)	0.0019 (0.2212)
Treatment ($t - 3$)	-0.0134 (0.1498)		-1.180** (0.5480)	0.0637 (0.1985)
Treatment ($t - 2$)	-0.1091 (0.1231)	-0.0628 (0.1705)	-0.6677 (0.7653)	0.0522 (0.2012)
Treatment ($t + 0$)	0.3765*** (0.1436)	0.0900 (0.2475)	-0.7210 (0.6455)	0.2414 (0.2602)
Treatment ($t + 1$)	-0.0705 (0.1386)	-0.0202 (0.2693)	-0.0324 (0.6838)	0.2391 (0.2627)
Treatment ($t + 2$)	0.0710 (0.1901)	-0.0644 (0.3294)	-0.8070 (1.056)	-0.2386 (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) reports the main results, shown in Figure 3. Column (2) reports 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 F18: Main Results with Randomization Inference

	Postal Turnout (1)	In-person Turnout (2)	Total Turnout (3)
Treatment ($t - 4$)	0.0218 (0.015) [0.913]	-0.0396 (0.018) [0.859]	-0.0178 (0.015) [0.911]
Treatment ($t - 3$)	-0.1075 (0.025) [0.654]	0.0941 (0.025) [0.666]	-0.0134 (0.012) [0.948]
Treatment ($t - 2$)	-0.0658 (0.024) [0.677]	-0.0433 (0.022) [0.774]	-0.1091 (0.026) [0.413]
Treatment ($t + 0$)	1.122*** (<0.001) [<0.001]	-0.7456*** (0.003) [0.004]	0.3765** (0.010) [0.040]
Treatment ($t + 1$)	0.6601** (0.005) [0.011]	-0.7305*** (0.004) [0.006]	-0.0705 (0.024) [0.688]
Treatment ($t + 2$)	0.8988*** (0.002) [0.001]	-0.8279*** (0.003) [0.003]	0.0710 (0.022) [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 randomization inference with 1,000 permutations following [Heß \(2017\)](#), are reported in brackets.

Table F19: Additional Controls

	(1)	(2)
Panel A: Postal Turnout		
Treatment ($t - 4$)	-0.0185 (0.1893)	0.0129 (0.1905)
Treatment ($t - 3$)	-0.1477 (0.1881)	-0.1130 (0.1891)
Treatment ($t - 2$)	-0.0440 (0.1285)	-0.0993 (0.1276)
Treatment ($t + 0$)	1.168*** (0.2743)	1.215*** (0.2646)
Treatment ($t + 1$)	0.6952*** (0.2470)	0.7438*** (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 (0.2063)	-0.0307 (0.2028)
Treatment ($t - 3$)	0.1077 (0.2025)	0.0913 (0.2014)
Treatment ($t - 2$)	-0.0476 (0.1475)	-0.0741 (0.1445)
Treatment ($t + 0$)	-0.7731*** (0.2820)	-0.9031*** (0.2691)
Treatment ($t + 1$)	-0.7213*** (0.2424)	-0.8293*** (0.2342)
Treatment ($t + 2$)		-0.9585*** (0.2939)
R ²	0.95949	0.95372
Panel C: Total Turnout		
Treatment ($t - 4$)	-0.0445 (0.1516)	-0.0178 (0.1504)
Treatment ($t - 3$)	-0.0400 (0.1479)	-0.0216 (0.1488)
Treatment ($t - 2$)	-0.0916 (0.1241)	-0.1734 (0.1197)
Treatment ($t + 0$)	0.3954*** (0.1445)	0.3114** (0.1411)
Treatment ($t + 1$)	-0.0261 (0.1369)	-0.0855 (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 women, 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, except for 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 F20: Time-varying COVID Mortality Controls

	(1)	(2)	(3)	(4)	(5)
Panel A: Postal Turnout					
Treatment ($t - 4$)	0.02176 (0.1885)	0.0218 (0.1885)	0.0218 (0.1885)	0.0218 (0.1885)	0.0218 (0.1885)
Treatment ($t - 3$)	-0.10750 (0.1898)	-0.1075 (0.1898)	-0.1075 (0.1898)	-0.1075 (0.1898)	-0.1075 (0.1898)
Treatment ($t - 2$)	-0.06579 (0.1286)	-0.0658 (0.1287)	-0.0658 (0.1287)	-0.0658 (0.1287)	-0.0658 (0.1287)
Treatment ($t + 0$)	1.12200*** (0.2741)	1.122*** (0.2742)	1.121*** (0.2743)	1.116*** (0.2729)	1.085*** (0.2736)
Treatment ($t + 1$)	0.66010*** (0.2512)	0.6586** (0.2512)	0.6546** (0.2514)	0.6612** (0.2510)	0.6328* (0.2508)
Treatment ($t + 2$)	0.89880*** (0.2862)	0.8988** (0.2863)	0.8862** (0.2863)	0.8988** (0.2863)	0.8661** (0.2858)
R ²	0.96761	0.96761	0.96762	0.96767	0.96780
Panel B: In-person Turnout					
Treatment ($t - 4$)	-0.03959 (0.2040)	-0.0396 (0.2040)	-0.0396 (0.2040)	-0.0396 (0.2040)	-0.0396 (0.2040)
Treatment ($t - 3$)	0.09409 (0.2022)	0.0941 (0.2022)	0.0941 (0.2022)	0.0941 (0.2022)	0.0941 (0.2022)
Treatment ($t - 2$)	-0.04334 (0.1454)	-0.0433 (0.1454)	-0.0433 (0.1454)	-0.0433 (0.1454)	-0.0433 (0.1454)
Treatment ($t + 0$)	-0.74560*** (0.2819)	-0.7454** (0.2824)	-0.7425** (0.2823)	-0.7404** (0.2808)	-0.7114* (0.2802)
Treatment ($t + 1$)	-0.73050*** (0.2436)	-0.7265** (0.2435)	-0.721** (0.2435)	-0.7342** (0.2429)	-0.7092** (0.2431)
Treatment ($t + 2$)	-0.82790*** (0.3191)	-0.8279** (0.3191)	-0.8107* (0.3191)	-0.8279** (0.3191)	-0.8037* (0.3185)
R ²	0.94892	0.94895	0.94898	0.94903	0.94916
Panel C: Total Turnout					
Treatment ($t - 4$)	-0.01783 (0.1503)	-0.0178 (0.1503)	-0.0178 (0.1503)	-0.0178 (0.1503)	-0.0178 (0.1503)
Treatment ($t - 3$)	-0.01336 (0.1498)	-0.0134 (0.1498)	-0.0134 (0.1498)	-0.0134 (0.1498)	-0.0134 (0.1498)
Treatment ($t - 2$)	-0.10910 (0.1231)	-0.1091 (0.1231)	-0.1091 (0.1232)	-0.1091 (0.1231)	-0.1091 (0.1232)
Treatment ($t + 0$)	0.37650*** (0.1436)	0.3766** (0.1439)	0.3784** (0.1439)	0.376** (0.1437)	0.3739** (0.1440)
Treatment ($t + 1$)	-0.07047 (0.1386)	-0.0679 (0.1388)	-0.0664 (0.1387)	-0.0731 (0.1387)	-0.0764 (0.1388)
Treatment ($t + 2$)	0.07096 (0.1901)	0.071 (0.1901)	0.0756 (0.1902)	0.071 (0.1901)	0.0624 (0.1906)
Specification	Baseline	COVID 30d	COVID 90d	COVID ytd	COVID atd
R ²	0.95911	0.95913	0.95912	0.95912	0.95912
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 additional time-varying COVID mortality controls. Column (1) controls for COVID deaths in the 30 days before each election; Column (2) controls for COVID deaths in the 90 days before each election; Column (3) controls for COVID deaths within the same year up to each election date; and Column (4) controls for COVID deaths since the start of the pandemic up to each election date. COVID deaths are defined using the ICD COVID code and measured per 1,000 eligible voters at the municipality level. The reference election ($t - 1$) is the 2018 State Election. All regressions include the log number of eligible voters and county fixed effects, analogous to Equation 2. Standard errors are clustered at the municipality level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table F21: Time-invariant COVID Mortality Controls

	(1)	(2)	(3)	(4)	(5)
Panel A: Postal Turnout					
Treatment ($t - 4$)	0.02176 (0.1885)	0.0217 (0.1885)	0.0228 (0.1884)	0.0217 (0.1885)	0.03 (0.1888)
Treatment ($t - 3$)	-0.10750 (0.1898)	-0.1075 (0.1898)	-0.1052 (0.1899)	-0.1083 (0.1898)	-0.0999 (0.1899)
Treatment ($t - 2$)	-0.06579 (0.1286)	-0.0656 (0.1289)	-0.0643 (0.1288)	-0.0665 (0.1286)	-0.0666 (0.1287)
Treatment ($t + 0$)	1.12200*** (0.2741)	1.122*** (0.2745)	1.121*** (0.2744)	1.114*** (0.2728)	1.086*** (0.2737)
Treatment ($t + 1$)	0.66010*** (0.2512)	0.6594** (0.2519)	0.6558** (0.2517)	0.6534** (0.2508)	0.6343* (0.2508)
Treatment ($t + 2$)	0.89880*** (0.2862)	0.8983** (0.2873)	0.896** (0.2869)	0.8927** (0.2854)	0.8711** (0.2859)
R ²	0.96761	0.96767	0.96764	0.96779	0.96784
Panel B: In-person Turnout					
Treatment ($t - 4$)	-0.03959 (0.2040)	-0.0396 (0.2040)	-0.0415 (0.2040)	-0.0386 (0.2038)	-0.0339 (0.2041)
Treatment ($t - 3$)	0.09409 (0.2022)	0.0942 (0.2023)	0.0916 (0.2021)	0.0954 (0.2020)	0.0971 (0.2024)
Treatment ($t - 2$)	-0.04334 (0.1454)	-0.0435 (0.1455)	-0.0442 (0.1454)	-0.0429 (0.1454)	-0.0421 (0.1457)
Treatment ($t + 0$)	-0.74560*** (0.2819)	-0.7452** (0.2829)	-0.7415** (0.2825)	-0.7383** (0.2807)	-0.7079* (0.2804)
Treatment ($t + 1$)	-0.73050*** (0.2436)	-0.73** (0.2450)	-0.7248** (0.2447)	-0.7257** (0.2433)	-0.7092** (0.2429)
Treatment ($t + 2$)	-0.82790*** (0.3191)	-0.8272* (0.3208)	-0.8221* (0.3202)	-0.8225** (0.3184)	-0.8058* (0.3182)
R ²	0.94892	0.94903	0.94907	0.94912	0.94915
Panel C: Total Turnout					
Treatment ($t - 4$)	-0.01783 (0.1503)	-0.0179 (0.1503)	-0.0187 (0.1503)	-0.0168 (0.1500)	-0.0038 (0.1496)
Treatment ($t - 3$)	-0.01336 (0.1498)	-0.0132 (0.1500)	-0.0136 (0.1498)	-0.0129 (0.1498)	-0.0028 (0.1493)
Treatment ($t - 2$)	-0.10910 (0.1231)	-0.1091 (0.1232)	-0.1085 (0.1232)	-0.1095 (0.1232)	-0.1087 (0.1232)
Treatment ($t + 0$)	0.37650*** (0.1436)	0.3767** (0.1440)	0.379** (0.1441)	0.3757** (0.1437)	0.3782** (0.1438)
Treatment ($t + 1$)	-0.07047 (0.1386)	-0.0705 (0.1385)	-0.069 (0.1389)	-0.0723 (0.1386)	-0.0749 (0.1391)
Treatment ($t + 2$)	0.07096 (0.1901)	0.0711 (0.1903)	0.0739 (0.1903)	0.0701 (0.1901)	0.0653 (0.1907)
Specification	Baseline	COVID 30d	COVID 90d	COVID ytd	COVID atd
R ²	0.95911	0.95912	0.95914	0.95914	0.95920
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 additional time-invariant COVID mortality controls. Column (1) controls for COVID deaths in the 30 days before the 2021 federal election; Column (2) controls for COVID deaths in the 90 days before the 2021 federal election; Column (3) controls for COVID deaths within 2021 up to the 2021 federal election; and Column (4) controls for COVID deaths since the start of the pandemic up to the 2021 federal election. COVID deaths are defined using the ICD COVID code and measured per 1,000 eligible voters at the municipality level. The reference election ($t - 1$) is the 2018 State Election. All regressions include the log number of eligible voters and county fixed effects, analogous to Equation 2. Standard errors are clustered at the municipality level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table F22: COVID-related School Closure Controls

	(1)	(2)
Panel A: Postal Turnout		
Treatment ($t - 4$)	0.0217 (0.1886)	0.0212 (0.1885)
Treatment ($t - 3$)	-0.1077 (0.1899)	-0.1078 (0.1898)
Treatment ($t - 2$)	-0.0654 (0.1287)	-0.0662 (0.1287)
Treatment ($t + 0$)	1.122*** (0.2741)	1.122*** (0.2743)
Treatment ($t + 1$)	0.6601*** (0.2513)	0.6604*** (0.2514)
Treatment ($t + 2$)	0.8994*** (0.2863)	0.8987*** (0.2864)
R ²	0.96761	0.96761
Panel B: In-person Turnout		
Treatment ($t - 4$)	-0.0393 (0.2040)	-0.0389 (0.2041)
Treatment ($t - 3$)	0.0944 (0.2022)	0.0940 (0.2022)
Treatment ($t - 2$)	-0.0438 (0.1455)	-0.0430 (0.1454)
Treatment ($t + 0$)	-0.7460*** (0.2820)	-0.7454*** (0.2821)
Treatment ($t + 1$)	-0.7310*** (0.2437)	-0.7301*** (0.2438)
Treatment ($t + 2$)	-0.8283*** (0.3192)	-0.8270*** (0.3193)
R ²	0.94892	0.94892
Panel C: Total Turnout		
Treatment ($t - 4$)	-0.0176 (0.1503)	-0.0177 (0.1503)
Treatment ($t - 3$)	-0.0133 (0.1499)	-0.0139 (0.1498)
Treatment ($t - 2$)	-0.1092 (0.1232)	-0.1092 (0.1232)
Treatment ($t + 0$)	0.3763*** (0.1437)	0.3763*** (0.1437)
Treatment ($t + 1$)	-0.0708 (0.1387)	-0.0697 (0.1386)
Treatment ($t + 2$)	0.0712 (0.1902)	0.0717 (0.1901)
Controls	Any closure	Total closures
R ²	0.95911	0.95912
Observations	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 additional time-invariant school and daycare closure controls. Column (1) controls for whether any closure record is observed in the municipality; Column (2) controls for the total number of closure records aggregated at the municipality level. Sources are the school/daycare closure records from [Baskaran et al. \(2023\)](#). The reference election ($t - 1$) is the 2018 State Election. All regressions include the log number of eligible voters and county fixed effects, analogous to [Equation 2](#). Standard errors are clustered at the municipality level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table F23: Population Controls

	(1)	(2)	(3)	(4)
Panel A: Postal Turnout				
Treatment ($t - 4$)	0.0218 (0.1885)	0.0014 (0.1878)	0.0168 (0.1886)	0.0835 (0.1912)
Treatment ($t - 3$)	-0.1075 (0.1898)	-0.1278 (0.1892)	-0.1145 (0.1901)	-0.0197 (0.1936)
Treatment ($t - 2$)	-0.0658 (0.1286)	-0.0735 (0.1289)	-0.0708 (0.1287)	-0.0014 (0.1292)
Treatment ($t + 0$)	1.122*** (0.2741)	1.153*** (0.2740)	1.117*** (0.2743)	1.063*** (0.2718)
Treatment ($t + 1$)	0.6601*** (0.2512)	0.7025*** (0.2503)	0.6545*** (0.2518)	0.5202** (0.2524)
Treatment ($t + 2$)	0.8988*** (0.2862)	0.9318*** (0.2861)	0.8933*** (0.2865)	0.8219*** (0.2840)
R ²	0.96761	0.96781	0.96760	0.96725
Panel B: In-person Turnout				
Treatment ($t - 4$)	-0.0396 (0.2040)	-0.0266 (0.2039)	-0.0012 (0.2033)	-0.1046 (0.2056)
Treatment ($t - 3$)	0.0941 (0.2022)	0.0991 (0.2023)	0.1339 (0.2015)	0.0761 (0.2019)
Treatment ($t - 2$)	-0.0433 (0.1454)	-0.0548 (0.1455)	-0.0081 (0.1455)	-0.0060 (0.1439)
Treatment ($t + 0$)	-0.7456*** (0.2819)	-0.7770*** (0.2816)	-0.7052** (0.2822)	-0.6613** (0.2818)
Treatment ($t + 1$)	-0.7305*** (0.2436)	-0.7584*** (0.2430)	-0.6875*** (0.2443)	-0.6301*** (0.2440)
Treatment ($t + 2$)	-0.8279*** (0.3191)	-0.8752*** (0.3178)	-0.7825** (0.3201)	-0.6879** (0.3195)
R ²	0.94892	0.94916	0.94883	0.94854
Panel C: Total Turnout				
Treatment ($t - 4$)	-0.0178 (0.1503)	-0.0252 (0.1504)	0.0156 (0.1510)	-0.0212 (0.1488)
Treatment ($t - 3$)	-0.0134 (0.1498)	-0.0287 (0.1498)	0.0193 (0.1505)	0.0565 (0.1498)
Treatment ($t - 2$)	-0.1091 (0.1231)	-0.1283 (0.1231)	-0.0789 (0.1239)	-0.0074 (0.1265)
Treatment ($t + 0$)	0.3765*** (0.1436)	0.3760*** (0.1439)	0.4121*** (0.1437)	0.4018*** (0.1445)
Treatment ($t + 1$)	-0.0705 (0.1386)	-0.0559 (0.1387)	-0.0329 (0.1388)	-0.1099 (0.1398)
Treatment ($t + 2$)	0.0710 (0.1901)	0.0565 (0.1900)	0.1108 (0.1896)	0.1340 (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) reports 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 F24: Robustness Matching

	(1)	(2)	(3)	(4)
Panel A: Postal Turnout				
Treatment ($t - 4$)	-0.1472 (0.1907)	-0.1178 (0.2196)	0.0059 (0.3034)	0.0311 (0.1724)
Treatment ($t - 3$)	-0.2958 (0.1924)	-0.1854 (0.2231)	-0.1066 (0.3017)	-0.0821 (0.1728)
Treatment ($t - 2$)	-0.1706 (0.1296)	-0.0584 (0.1516)	-0.0292 (0.2254)	-0.0496 (0.1202)
Treatment ($t + 0$)	1.034*** (0.2815)	1.251*** (0.3194)	1.280*** (0.3807)	0.9789*** (0.2592)
Treatment ($t + 1$)	0.6034** (0.2540)	0.8596*** (0.2966)	0.7299* (0.3964)	0.5335** (0.2356)
Treatment ($t + 2$)	0.8363*** (0.2874)	1.085*** (0.3320)	0.8485** (0.4288)	0.7461*** (0.2674)
R ²	0.97044	0.97620	0.97615	0.97421
Panel B: In-person Turnout				
Treatment ($t - 4$)	0.0831 (0.2102)	-0.0232 (0.2364)	-0.1360 (0.3021)	-0.0208 (0.1849)
Treatment ($t - 3$)	0.2328 (0.2053)	-0.0347 (0.2402)	-0.0309 (0.3005)	0.1136 (0.1840)
Treatment ($t - 2$)	0.0418 (0.1449)	-0.0960 (0.1693)	-0.2061 (0.2549)	-0.0105 (0.1370)
Treatment ($t + 0$)	-0.6408** (0.2863)	-0.8051** (0.3246)	-0.9130** (0.3846)	-0.6167** (0.2690)
Treatment ($t + 1$)	-0.6205** (0.2501)	-0.7117** (0.2831)	-0.7537** (0.3640)	-0.5613** (0.2344)
Treatment ($t + 2$)	-0.7140** (0.3205)	-0.9765*** (0.3713)	-0.8648* (0.4517)	-0.6533** (0.3033)
R ²	0.95349	0.96252	0.96405	0.95797
Panel C: Total Turnout				
Treatment ($t - 4$)	-0.0641 (0.1500)	-0.1410 (0.1813)	-0.1300 (0.2627)	0.0102 (0.1377)
Treatment ($t - 3$)	-0.0630 (0.1449)	-0.2201 (0.1769)	-0.1375 (0.2430)	0.0316 (0.1412)
Treatment ($t - 2$)	-0.1289 (0.1185)	-0.1544 (0.1531)	-0.2353 (0.1975)	-0.0602 (0.1212)
Treatment ($t + 0$)	0.3934*** (0.1394)	0.4457*** (0.1663)	0.3666* (0.2177)	0.3622*** (0.1328)
Treatment ($t + 1$)	-0.0171 (0.1409)	0.1479 (0.1833)	-0.0238 (0.2525)	-0.0278 (0.1356)
Treatment ($t + 2$)	0.1223 (0.1881)	0.1088 (0.2374)	-0.0163 (0.3039)	0.0928 (0.1837)
R ²	0.96671	0.97765	0.97811	0.97572
Procedure SE cluster	Local Municipality	Propensity Municipality	Mahalanobis TW Match & Cntrl. Gr.×Elctn.	Entropy Bal. 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 D7). 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); these standard errors are reported in parentheses. For the other approaches, standard errors are clustered at the municipality level and reported in parentheses.

Table F25: Number of Runoffs

	(1)	(2)	(3)
Panel A: Postal Turnout			
Treatment ($t - 4$)	-0.1965 (0.3088)	-0.1649 (0.2535)	-0.1719 (0.2399)
Treatment ($t - 3$)	-0.2269 (0.3142)	-0.1662 (0.2535)	-0.2069 (0.2406)
Treatment ($t - 2$)	-0.1015 (0.2059)	0.0331 (0.1676)	-0.0089 (0.1606)
Treatment ($t + 0$)	1.014** (0.4513)	1.153*** (0.3628)	1.226*** (0.3485)
Treatment ($t + 1$)	1.074*** (0.3909)	0.8079** (0.3268)	0.8718*** (0.3172)
Treatment ($t + 2$)	1.595*** (0.4541)	1.223*** (0.3662)	1.277*** (0.3540)
R ²	0.97430	0.97397	0.97383
Panel B: In-person Turnout			
Treatment ($t - 4$)	0.1348 (0.3490)	0.1803 (0.2745)	0.1975 (0.2599)
Treatment ($t - 3$)	0.2368 (0.3466)	0.3409 (0.2713)	0.3564 (0.2576)
Treatment ($t - 2$)	0.0581 (0.2488)	-0.0134 (0.2013)	0.0181 (0.1907)
Treatment ($t + 0$)	-0.5802 (0.4471)	-0.6369* (0.3653)	-0.6762* (0.3533)
Treatment ($t + 1$)	-0.8794** (0.3862)	-0.7429** (0.3206)	-0.7502** (0.3100)
Treatment ($t + 2$)	-1.207** (0.4862)	-0.8653** (0.3996)	-0.9296** (0.3873)
Panel C: Total Turnout			
Treatment ($t - 4$)	-0.0617 (0.2523)	0.0154 (0.2044)	0.0256 (0.1914)
Treatment ($t - 3$)	0.0099 (0.2596)	0.1748 (0.2037)	0.1495 (0.1937)
Treatment ($t - 2$)	-0.0434 (0.2125)	0.0197 (0.1574)	0.0093 (0.1489)
Treatment ($t + 0$)	0.4341* (0.2323)	0.5161*** (0.1888)	0.5500*** (0.1771)
Treatment ($t + 1$)	0.1943 (0.2149)	0.0650 (0.1831)	0.1216 (0.1749)
Treatment ($t + 2$)	0.3885 (0.2932)	0.3581 (0.2358)	0.3473 (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 F26: Off-Cycle Control Group

	(1)	(2)
Panel A: Postal Turnout		
Treatment ($t - 4$)	-0.5693* (0.3315)	-0.6547 (0.7892)
Treatment ($t - 3$)	-0.7018** (0.3285)	-0.7604 (0.8210)
Treatment ($t - 2$)	-0.2438 (0.2491)	-0.0268 (0.7135)
Treatment ($t + 0$)	1.404*** (0.4482)	1.924 (1.503)
Treatment ($t + 1$)	1.057** (0.4503)	2.302* (1.251)
Treatment ($t + 2$)	1.296*** (0.4940)	3.087** (1.421)
R ²	0.97921	0.98004
Panel B: In-person Turnout		
Treatment ($t - 4$)	0.6356* (0.3584)	0.3198 (0.8573)
Treatment ($t - 3$)	0.6686* (0.3521)	0.4668 (0.9989)
Treatment ($t - 2$)	-0.0210 (0.3015)	0.0556 (0.8251)
Treatment ($t + 0$)	-0.9990** (0.4672)	-1.430 (1.576)
Treatment ($t + 1$)	-1.299*** (0.4431)	-2.716* (1.432)
Treatment ($t + 2$)	-1.435*** (0.5220)	-3.210** (1.497)
R ²	0.96476	0.96649
Panel C: Total Turnout		
Treatment ($t - 4$)	0.0663 (0.2768)	-0.3349 (0.5241)
Treatment ($t - 3$)	-0.0333 (0.3168)	-0.2936 (0.7734)
Treatment ($t - 2$)	-0.2648 (0.2880)	0.0288 (0.7231)
Treatment ($t + 0$)	0.4050 (0.2832)	0.4943 (0.8569)
Treatment ($t + 1$)	-0.2416 (0.3059)	-0.4141 (0.6627)
Treatment ($t + 2$)	-0.1382 (0.3438)	-0.1239 (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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table F27: Doubly Robust DiD Approach

	Postal Turnout (1)	In-person Turnout (2)	Total Turnout (3)
Treatment ($t - 4$)	0.0311 (0.1757)	-0.0208 (0.187)	0.0102 (0.1398)
Treatment ($t - 3$)	-0.0821 (0.1771)	0.1136 (0.1878)	0.0315 (0.1442)
Treatment ($t - 2$)	-0.0496 (0.1233)	-0.0105 (0.1384)	-0.0602 (0.1231)
Treatment ($t + 0$)	0.9789*** (0.2526)	-0.6167** (0.2742)	0.3622*** (0.135)
Treatment ($t + 1$)	0.5335** (0.2397)	-0.5613** (0.2392)	-0.0278 (0.137)
Treatment ($t + 2$)	0.7461** (0.2733)	-0.6533** (0.3101)	0.0928 (0.1843)
Procedure	DR-IPW	DR-IPW	DR-IPW
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-time ATTs for postal, in-person, and total turnout (0-100) estimated using the doubly robust difference-in-differences estimator of [Sant'Anna and Zhao \(2020\)](#). Each point estimate is obtained from a separate two-period comparison between the reference election ($t - 1$), the 2018 state election, and the election at event time $t + k$. All specifications condition on time-invariant covariates, including county indicators and baseline population held constant at 2018. Standard errors are clustered at the municipality level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.