No Surprises, Please: Voting Costs and Electoral Turnout

Jean-Victor Alipour[a,*], Valentin Lindlacher[b]

[a] alipour@ifo.de. *LMU Munich and ifo Institute for Economic Research*

[b] valentin.lindlacher@tu-dresden.de. *TUD Dresden University of Technology, ifo Institute for Economic Research, and CESifo*

Accepted at *Journal of Political Economy Microeconomics*, 2024

Abstract

Can well-intentioned policies create barriers to voting? Election administrators in Munich (Germany) recruit new polling places and control precinct sizes to improve voting accessibility, creating variation in the assignment of citizens to polling locations. Event study estimates suggest that polling place reassignments cause a persistent shift from in-person to mail-in voting and a transitory drop in total turnout of 0.4 percentage points (0.6 percent). The results are consistent with inattention to reassignments, causing some voters to miss requesting mail-in ballots and temporarily abstain from voting. Reassignments depress turnout more in elderly-heavy precincts and when distance to the polling location increases.

Keywords: Voter Turnout; Election Administration; Inattention; Polling Places; Event Study

JEL Codes: D72; D73; D83; R41

*Corresponding author. Poschingerstr. 5, 81679 Munich, Germany. For suggestions that have improved this article, we thank the editor Anna Dreber Almenberg and two anonymous reviewers. We greatly benefitted from comments and discussions with Enrico Cantoni, Sascha O. Becker, Clémence Tricaud, Claudia Steinwender, Oliver Falck, Niklas Potrafke, Anna Kerkhof, Thomas Fackler, Jan Schymik, Jerome Schäfer, Marcel Garz, Jon Fiva, Vassilis Sarantides, Ramon Paul Degennaro, Christoph Vanberg, Sebastian Schirner, Felix Rösel, Oliver Herrmann, Shane Mahen, Anna Hasselqvist, and participants at the Econometric Society European Winter Meeting, the Royal Economic Society Annual Conference, the Annual Meeting of the Public Choice Society, and seminars at the University of Cambridge and the University of Munich. We are thankful to Ingrid Kreuzmair, Janette Lorch, and Heiko Flehmig for sharing data and information about the administration of elections in Munich. We also thank Emil Phillip for excellent research assistance. Jean-Victor Alipour gratefully acknowledges funding by the Society for the Promotion of Economic Research (ifo Freundesgesellschaft e.V.).

1. Introduction

Organizing elections that foster trust in the electoral process and encourage voter participation is a key challenge for modern democracies. In recent years, a number of controversies have brought the importance of electoral administration into the public spotlight. Leading up to the 2020 presidential Election, reforms at the US Postal Service led former President Obama to accuse then-President Trump of attempting to "actively kneecap" the Postal Service to sway voter turnout in his favor. In Germany, the 2021 Berlin Marathon impeded the accessibility of polling places to the extent that the Constitutional Court decided that the entire State Election must be repeated.[1] But while large-scale controversies quickly become the subject of public scrutiny, supposedly benign or well-intentioned policies can pose an overlooked barrier to democratic participation.

This article studies the consequences of a seemingly innocuous practice for voter participation: the relocation of polling places. We analyze a natural experiment in Munich (Germany), where election administrators aim to "facilitate [voting] as much as possible" (Federal Election Code, Section 12:2). Upholding this objective involves recruiting new polling places with barrier-free access and controlling precinct sizes to prevent congestion at polling locations. A by-product of these policies is that some eligible citizens are assigned to vote at a *different* polling location than before. Observable voting costs are only marginally affected by this practice: 90 percent of reassignments between 2013 and 2020 changed citizens' walking distance to their polling location by less than one kilometer.

Given the insignificance of any single vote for the election outcome, classical voting theory suggests that even small shocks to voting costs may heavily impact turnout (Downs, 1957); either positively (e.g., due to shorter travel distance or better accessibility of the building), or negatively (e.g., due to unfamiliarity with the new polling place or longer travels). More recent voting literature contrasts this view by highlighting the significance of expressive reasons for voting, such as a sense of civic duty, self-expression, ethics, or social pressure (Ali and Lin, 2013; Pons and Tricaud, 2018; Funk, 2010; Dellavigna et al., 2017). Given the importance of these motives, small voting costs are typically considered negligible for voter turnout. And thus, moving polling places may produce null effects. We contribute to this debate by estimating the causal impact of polling place reassignments on the evolution of electoral turnout and the mode of voting.

We find a persistent shift from in-person to mail-in voting and a transitory decline in total turnout after a polling place relocation. The turnout recovery is consistent with inattention to reassignment, causing some voters to postpone the switch to mail-in voting to the subsequent election and temporarily abstain. Because reassignments are not geographically concentrated

[1] Reportedly, the Berlin Marathon was only one of several complications, including a reduced number of voting booths at polling locations, wrong ballot papers, and irregular opening hours of polling places, leading to "chaotic conditions" and "completely overloaded" polling places according to the Berlin Constitutional Court.

and voters are not heavily segregated in Munich, no party significantly gains or loses from this practice; although relocations could be pivotal in close single-member constituency elections, where residents directly elect their representative.

Understanding the determinants of voter turnout has engaged a vast literature, which has increasingly focused on the role of electoral institutions.[2] Given the importance of voting *in person* in most democracies, provisions governing voting at the polling place are surprisingly understudied. While observational research suggests that polling place accessibility (e.g., in terms of proximity) can be relevant for turnout, few studies establish causality, and nearly all examine settings in the US. Notably, Cantoni (2020) uses a regression discontinuity design at precinct boundaries in Minnesota and Massachusetts and finds that turnout is 2.5–7 percentage points lower per kilometer from the polling place.[3] Using the same identification strategy, Bagwe et al. (2022) document heterogeneous turnout effects, with smaller estimates in Pennsylvania (0.6–0.8 percentage points lower turnout per kilometer) and zero effects in Georgia. The authors attribute much of the heterogeneity to the convenience of using absentee voting. Tomkins et al. (2023) extend the regression discontinuity framework to ten US States. The results largely reveal no turnout differences across precinct borders as individuals living further from their polling location use more early voting options.

Moving a polling location is an active intervention and thus requires particular scrutiny, especially where election laws and administration are politically charged. In the US, the closing of polling sites frequently raises concerns over partisan efforts to reduce voting access for certain groups, particularly racial minorities (Amos et al., 2017; Curiel and Clark, 2021; Chen et al., 2022). Partisan motives and other unobserved determinants of polling place relocations pose a key challenge to establishing causal effects. Indeed, Brady and McNulty (2011) show that the precinct consolidations preceding the 2003 LA Gubernatorial Recall Election caused non-random polling place reassignments. Using matching techniques to compare voters whose polling location moved *further away* with those without a change, they document a 1.8 percentage point turnout decline among reassigned individuals. Further results suggest that most of the effect comes from the move itself rather than the increased distance. Tomkins et al. (2023) compare turnout across precinct boundaries in six US states and find that early voting fully offsets lower turnout at the polls among registrants whose polling location moved. However, the authors note that causality is difficult to establish due to correlation of relocations with

[2] For instance, studies have evaluated the role of personal characteristics (e.g., education, religiosity, overconfidence) (Milligan et al., 2004; Gerber et al., 2016; Ortoleva and Snowberg, 2015) or contextual factors (Cantoni and Pons, 2022), and specifically electoral institutions including ID laws (Cantoni and Pons, 2021), registration procedures (Braconnier et al., 2017), voting technology (Fujiwara, 2015), or compulsory voting regimes (Bechtel et al., 2018; Hoffman et al., 2017).

[3] Cantoni's results are consistent with observational research (Haspel and Knotts, 2005; Fauvelle-Aymar and François, 2018; Gibson et al., 2013; Bhatti, 2012; McNulty et al., 2009; Dyck and Gimpel, 2005; Gimpel and Schuknecht, 2003). However, these studies do not account for potential endogeneity, leaving room for biased estimates due to unobserved confounders or selection problems. Other studies have investigated the turnout effects of polling place opening hours (Potrafke and Roesel, 2020; Garmann, 2017).

underlying population characteristics (p. 178). Clinton et al. (2021) provide correlational evidence that moving polling places in North Carolina produced null effects across two presidential elections, as affected voters switched to early voting. Using the same data with a longer time frame, Yoder (2019) estimates a 1–2 percentage point turnout decline.

In sum, the literature to date has focused on turnout responses to changing polling locations in the US, but estimates vary greatly, and no consensus has been reached. Moreover, evidence exists only on *instantaneous* turnout effects, leaving unanswered the question of longer-term consequences of this practice. For instance, it is unclear whether turnout effects are persistent or revert (e.g., if voters familiarize themselves with their new polling place). Evolving turnout differences could also reflect gradual adjustments in voting behavior or a new voting habit in response to a reassignment shock (Fujiwara et al., 2016). Thus, a comprehensive empirical framework requires a dynamic perspective.

We depart from the existing literature in four important ways. First, our empirical framework significantly improves on the identification of turnout effects of reassignments. We study a panel covering the eight elections held between 2013 and 2020 and demonstrate that polling place reassignments occur "as good as randomly". Specifically, we show that *i)* current turnout (by mail, in-person, and overall) is unrelated to reassignments in future elections conditional on election and precinct fixed effects (parallel pretrends), *ii)* the timing of reassignments is uncorrelated with changes in observable precinct characteristics, and *iii)* reassignments do not systematically skew toward increasing or decreasing the distance to the polling location. A second key novelty is the evaluation of effect *persistence* by analyzing turnout up to three elections after a reassignment. Third, the panel structure also allows us to shed light on a much-debated determinant of voting: habit formation. Habitual voting implies that the act of voting itself increases its consumption value and thereby the likelihood of voting in the future (Fujiwara et al., 2016). While scholars have long been aware that turnout differences tend to be persistent (e.g., Plutzer, 2002; Green and Shachar, 2000; Brody and Sniderman, 1977), causal evidence for habit formation has proved inconclusive.[4] Fourth, this is the first study to estimate the causal impact of reassignments and distance to the polling location outside the US and in the context of a multi-party system with proportional representation. This is partly motivated by the mixed findings on how turnout is affected by distance and relocations, along with recent evidence highlighting the importance of place-specific factors for turnout (Cantoni and Pons, 2022; Chyn and Haggag, 2023). We use aggregate party votes to estimate

[4] Meredith (2009) demonstrates that voters who had just turned eighteen at the time of the 2000 US General Election are also more likely to cast their ballot in the subsequent election than their peers who fell short of the age threshold. Gerber et al. (2003) provide evidence suggesting that get-out-the-vote campaigns increase turnout in subsequent elections. Fujiwara et al. (2016) propose election-day rainfall as an exogenous and transitory shock to voting costs and find that the decrease in turnout induced by rainfall also reduces turnout in subsequent US presidential elections. By contrast, compulsory voting in Switzerland and Austria showed no persistent effects on turnout after its abolition (Bechtel et al., 2018; Gaebler et al., 2020). Similarly, Potrafke and Roesel (2020) find that longer opening hours of polling places increase contemporaneous turnout but do not affect turnout in subsequent elections.

the partisan consequences of moving polling locations; an aspect lacking in the existing literature. Election administration in Germany is not politically charged and reassignments are uncontroversial. Moreover, the country counts among the few democracies to offer universal access to mail-in voting. Thus, our setting is well suited to test the importance of convenient alternatives to voting at the polling place.[5]

To fix ideas, we present a simple rational choice model of voting that combines three key ingredients. First, polling place reassignments alter the cost of voting in person by changing the distance to the polling location; second, reassignments always generate a disutility from engaging with an unfamiliar environment, independent of distance. Third, we allow for *inattention* to reassignments as citizens in Munich, unlike in the US, are not explicitly informed of *changes* to their polling location. This raises the possibility that a fraction of voters is surprised by a reassignment or does not notice the change at all. The model delivers three key predictions. First, reassignments generate asymmetric turnout effects by distance: Increasing distance always reduces turnout at the polling place by making it less attractive relative to mail-in voting and abstention; however, decreasing distance does not raise polling place turnout, unless it is enough to compensate for the reassignment disutility. Second, inattention amplifies the shift toward abstention when reassignments make poll voting more costly. This is due to inattentive poll voters who are surprised by reassignments after the deadline for requesting mail-in ballots. Some inattentive voters (who would have switched to mail-in voting) end up abstaining from turning out, leading to higher turnout losses relative to a scenario without inattention. Third, inattention attenuates turnout gains when reassignments reduce travel distance. Intuitively, inattention creates inertia among abstainers who do not notice reassignments at all.

Our empirical results suggest sizable and persistent effects of polling place relocations. We use an event study design that focuses on turnout dynamics around the time that a precinct is assigned to a different polling place. On average, reassignments cause a *persistent* substitution between the modes of voting: Turnout at the polling place falls by 1.0 percentage points immediately after the change, mirrored by an increase in mail-in turnout. Remarkably, the substitution is only *partial* in the first post-reassignment election, causing total turnout to fall temporarily by 0.4 percentage points. Given the policy's good intentions and the minor changes in proximity to the polling place, a declining turnout is notable. The magnitude of the drop is comparable to reducing the number of early (in-person) voting days in the US by two (Kaplan and Yuan, 2020), and would be enough to offset the positive turnout effect of an additional newspaper during the turn of the twentieth century in the US (Gentzkow et al., 2011). The drop also contrasts the lack of participation effects estimated by Clinton et al. (2021) and Tomkins et al. (2023).

[5]Only 6 percent of countries globally and 29 percent of OECD countries (including Germany, parts of the US, Canada, and the UK) offer access to mail-in voting for all eligible voters (International Institute for Democracy and Electoral Assistance (IDEA)).

Next, we examine a key dimension of reassignment heterogeneity: changes in proximity to the polling location. We estimate differential treatment effects for reassignments that increased versus decreased distance to the polling place. In line with our model, we find strikingly asymmetric patterns. When distance increases, the shift towards mail-in voting and the temporary drop in total turnout are amplified. By contrast, distance reductions generate no statistically significant turnout effects, on average. Our model suggests that reassignment disutility may offset lower voting costs from a closer polling location. We estimate that distance needs to be halved, on average, to compensate for the shock of the move itself and prevent a drop in total turnout. We only find weak evidence of increases in overall participation in extreme cases of distance declines. This result is consistent with inattentive abstainers, who remain abstainers even when their polling location moves very close. However, we cannot rule out alternative explanations for the lack of positive participation effects. Overall, changes in distance account for less than 40 percent of the turnout effects, highlighting the relocation itself as a barrier to voting overlooked by election administrators.

We explore the mechanism explaining the drop and subsequent recovery of voter participation. Results show that the recovery is entirely explained by an increase in mail-in rather than polling place turnout. This is at odds with the hypothesis that abstainers return to vote in person after familiarizing themselves with their new polling place. Instead, the pattern is consistent with inattention to reassignments. Inattentive poll voters are surprised by reassignments after the deadline for requesting mail-in ballots. Consequently, some inattentive voters abstain in the first post-reassignment election and only turn to mail-in voting in the subsequent election. This channel highlights the importance of offering access to mail-in voting to prevent a persistent decline in participation. The result speaks to previous research suggesting that the availability of convenience voting systems can increase participation rates (Thompson et al., 2020; Barber and Holbein, 2020; Kaplan and Yuan, 2020; Hodler et al., 2015; Gerber et al., 2013). Moreover, the fact that turnout losses are recovered is incompatible with the hypothesis that voting (or abstaining) is habit-forming. Instead, the dynamics are consistent with rational behavior in response to a positive shock to voting costs that is temporarily amplified by inattention. The mechanism implies that increasing the salience of reassignments ahead of election day to remedy inattention (e.g., by explicitly notifying affected citizens) could alleviate detrimental turnout effects.

Our baseline estimates obscure a great amount of heterogeneity. In particular, we find that turnout effects vary significantly by the age composition of the local electorate. We estimate a triple difference model that traces the differential turnout trend among treated precincts with a higher share of elderly eligible voters before and after the reassignment. A primary reason for polling place turnover during our observation period is the city council's resolution to recruit new barrier-free venues to improve access for elderly voters and citizens with physical impairments. However, our estimates suggest that total turnout drops more in elderly-heavy precincts after reassignment and *does not* fully recover in subsequent elections. Using a similar estimation strategy, we find that the shift from in-person to mail-in voting is significantly weaker in precincts with a higher fraction of Germans with migration background; yet, the change in total turnout is not statistically different. We find no evidence that reassignments affect turnout differently in less affluent precincts (measured by the average quoted rent) or in precincts with a higher share of households with children, who may also benefit from barrier-free access.

The presence of heterogeneous treatment effects may undermine the representativeness of the electoral outcome. Our results suggest it does not. Turnout effects are similar across the six largest parties, and party vote shares do not change significantly after a reassignment. This finding is likely explained by the lack of heavy spatial segregation along party lines in Munich, ensuring that polling place relocations are not concentrated among a particular party's supporters. However, small changes in voter participation could matter in close constituency elections during state and federal elections, where voters directly elect their representative on a plurality rule.

The next section describes the institutional setting. Section 3 describes how we build our estimation panel and outlines our empirical strategy. We present our main results in Section 4. Section 5 analyzes heterogeneous effects across precinct characteristics and explores partisan consequences of reassignments. Section 6 concludes.

2. Institutional Background: Elections and Polling Place Reassignments

2.1. Elections in Munich

Our panel covers the eight elections held in Munich between 2013 and 2020. These include elections to four legislative bodies reflecting the federal system in Germany: the *Bundestag* (federal parliament), which constitutes the main body of the central government, the Bavarian *Landtag* (state parliament), the *Stadtrat* (Munich city council), which governs the city alongside the mayor, and the European Parliament, which effectively exercises some of the power of the federal government since Germany is a member of the European Union. All elections follow the principles of proportional representation but differ in electoral rules. In Appendix A, we describe key features of the different electoral processes. We also compare Munich's population to the German average and the 20 largest cities in the country in Appendix Table E.1. While voter turnout is close to the national average, Munich stands out regarding income and the share of high-skilled residents. This makes the city broadly representative of the urban, more affluent population.

Eligible voters are automatically registered on the electoral roll. Voting is not compulsory and mail-in voting is open to all without excuse or separate photo identification.[6] Registered citizens receive an election notification via mail no later than 21 days before the election. The letter includes the election date, the location and opening hours of the polling place, and whether

[6]Voting by mail was introduced in West Germany in 1957, and since 2008, it can be requested without providing a reason. Mail-in ballots can be submitted to any mailbox without postage.

it offers barrier-free access. There is no explicit information about *changes* to the polling location in the letter nor a separate notification. This contrasts with the US, where such changes typically trigger the requirement to notify affected voters (Cantoni, 2020; Clinton et al., 2021; Tomkins et al., 2023). Eligible voters may vote in person at their assigned polling place on election day. In this case, they must present their election notification and a photo ID at the voting station. Those who wish to vote by mail must request a "polling card" (*Wahlschein*) by returning a form included in the election notification at least two days before the election.[7]

Figure 1 illustrates the election timeline in our panel. Two elections were held in 2013 and 2014 (but not on the same day), and one was held every year between 2017 and 2020. The vertical bars illustrate the number of eligible voters (left axis). The triangles and the solid line trace the evolution of total turnout and the share of votes cast at the polling place, respectively (right axis). The number of eligible voters is distinctively higher in municipal elections, in which EU foreigners living in Munich are also entitled to vote.[8] Total turnout increases over time when comparing the same election type; the share of votes cast in person typically lies between 50 and 60 percent and declines slightly over time.[9]

[Figure 1 about here.]

2.2. Polling Place Reassignments

Elections are organized by the Munich Election Office (*Wahlamt*). The employees are nonpartisan civil servants and have no direct incentives to manipulate the electoral process. In every election, the electorate is geographically partitioned into more than 600 voting precincts based on eligible citizens' home addresses.[10] Precincts constitute the smallest administrative unit and serve to enable a manageable election process. We use official electoral rolls to georeference polling locations and residential addresses.[11] Figure 2 depicts a typical electoral map. Black boundaries delineate the 618 precincts; blue lines delineate the 25 city districts. Each precinct is assigned one polling place, marked by black stars, and it is not uncommon that one venue accommodates the polling places of several neighboring precincts located in the same district. Gray lines indicate the assignment of home addresses to polling places.

[Figure 2 about here.]

[7] In principle, the polling card also entitles one to vote at another polling place in the city; however, typically, more than 98 percent of ballots cast using polling cards are mail-in votes. And more than 90 percent of voters requesting a polling card actually cast a vote.

[8] For instance, in the 2020 Municipal Election, 17.5 percent of eligible voters were foreign EU citizens. Foreign EU citizens who wish to vote in European elections in Munich instead of their country of origin must lodge a registration request.

[9] With more than half of all votes cast by mail, the 2020 Municipal Election held during the Covid-19 pandemic marks an exception.

[10] By law, citizens must notify the city's registration office (*Meldeamt*) within two weeks of moving into a new residence. This also applies to moves within a municipality.

[11] We identify 156,261 residential addresses from the 2018 electoral roll. We successfully geolocate 153,938 (98.5 percent) and are able to match 150,779 to a unique precinct in every election (97.9 percent of all geolocated addresses). We use geocoding services provided by OpenCage (Zeigermann, 2018), which uses *OpenStreetMaps (OSM)* to generate the addresses' coordinates.

Recruitment of Polling Locations. One source of variation in polling place assignments is turnover in the venues used to host them. Polling places are typically located in public properties, usually schools (71 percent of all venues), but also Church-affiliated facilities (11 percent), and retirement homes (5 percent).[12] In each election year, district inspectors (*Bezirksinspektoren*) are charged with recruiting potential locations and verifying they meet the required standards. While recruitment usually focuses on venues that were used in the past, new polling place requirements, competing events on election day, building closures, or ongoing construction work may leave some locations unavailable. There is no documentation of why venues become inactive or new venues are recruited. Correspondence with the election office suggests two main drivers of turnover in polling locations during our observation period. First, following a resolution of the city council (*Stadtrat*), the election office prioritized recruiting locations with barrier-free access for elderly people and voters with physical impairments after 2014.[13] Second, the extensive renovations under Munich's school construction program (*Schulbauoffensive*), which began in 2016 with over 3.8 billion Euros allocated to refurbishing educational facilities, led to prolonged closures of school buildings. Our review of public documents on the investment plans revealed that in 61 percent of the cases in which schools no longer hosted polling places, the election date collided with a construction period. We observe 293 distinct venues used in at least one election between 2013 and 2020. The number of active venues is typically around 200 in any given election. Appendix Figure D.2 illustrates their activity status over time.

Precinct Reconfigurations. The second source of polling place reassignments is reconfigurations of precinct boundaries and the allocation of existing polling places. The law requires that precincts be drawn so that "participation in the election is facilitated as much as possible" (Federal Election Code, Section 12:2). Besides monitoring proximity to polling locations and recruiting barrier-free venues, the election office's main objective is to minimize congestion risk. In practice, this involves controlling precinct sizes and adjusting the number of polling places hosted by the same venue in case it serves multiple precincts.[14] Consequently, precincts may be merged, split, or entirely assigned to another (existing) polling place.

We illustrate two instances of polling place reassignments between 2014 and 2017 in Figure 3. The black borders delineate precincts as of 2017. The blue-shaded areas highlight precincts as of 2014. The dark (light) gray lines connect eligible voters' addresses to their assigned polling place in 2017 (2014). In Panel (a), the pub that hosted the precinct's polling place in 2014 was not recruited in 2017. Instead, the precinct was assigned another polling place (a public

[12] See Appendix Figure D.1 for an overview of venue types.

[13] Specifically, the resolution mandated that the number of barrier-free polling places be doubled between 2014 and 2017 and that a share of at least 75 percent be reached by 2020. Internal documents provided by the election office suggest that 80 percent of polling places were barrier-free by 2018.

[14] The law specifies that precincts must not be larger than 2,500 eligible voters. The election office aims at an average size of 1,500 eligible voters per precinct. See Appendix Figure D.3 for a density plot of precinct size across all elections.

school) about nine walking minutes west of the old location. In the example, the relocation led to an increase in the average distance to the polling place. Panel (b) illustrates an instance in which a precinct's boundaries were redrawn. A new precinct (black borders) was carved out from the original precinct (light blue area). Residents of the newly created precinct were consequently reassigned from the polling place at the top of the map to the location further south. Unlike in the preceding example, both polling places remained in operation in 2017.

[Figure 3 about here.]

Figure 4 documents the fraction of residential addresses reassigned to a different polling place relative to the previous election.[15] There were no reassignments in the 2013 Federal Election and the 2014 European Election, as other elections were held earlier in the same year. Before 2017, changes in precinct size were addressed by adjusting the number of onsite poll workers, thus limiting the number of reassignments caused by precinct reconfigurations. In 2017, 41 percent of home addresses were assigned to a different polling place. The main drivers were a major consolidation of precincts facilitated by the introduction of new urban planning technology and the new focus on recruiting barrier-free polling places.[16] Munich's school construction program contributed to the turnover of polling venues starting in 2017. In 2020, reassignments were mainly the result of an increased number of precincts and the recruitment of suitable venues to meet social distancing provisions during the Covid-19 pandemic. Overall, 43 percent of all addresses are never reassigned between 2013 and 2020, 26 percent are reassigned once, and 24 percent twice (Appendix Figure D.4).[17]

[Figure 4 about here.]

Figure 5 plots the distribution of walking distances between home addresses and polling places (left panel), and the distribution of distance *changes* conditional on reassignment across all elections (right panel).[18] Negative values indicate that the new polling place moved closer (relative to the previous election); positive values correspond to a relocation further away. We distinguish between changes due to recruitment of polling locations and due to precinct reconfigurations. For 90 percent of residential addresses, proximity to the polling place is *below* 1.4 kilometers, equivalent to an 18-minute walk (median: 0.74 km, mean: 0.82 km). The distribution of distance changes is closely centered around zero (median: +0.04 km, mean: +0.05 km) and approximately symmetric (skewness: 0.08), indicating that polling places are not systematically located closer or further away after reassignment. Splitting by

[15] Reassignments in the 2013 State Election are determined relative to polling place assignments in the 2009 Federal Election.

[16] The re-division of the city caused a significant reduction in the variance of precinct sizes (Appendix Figure D.3)

[17] When an address is reassigned more than once, the median period between the first and second reassignment is three elections.

[18] We use the osrmtime package (Huber and Rust, 2016), which makes use of *Open Source Routing Machine (OSRM)* and *OpenStreetMaps (OSM)*, to calculate walking distances, defined as the shortest walking distance between two points using the public road network.

reason of reassignment leaves the moments of the two distributions nearly unchanged. 90 percent of reassignments change the walking distance by *less* than 0.87 km (or 10 walking minutes). Hence, the practice generates only marginal shocks to observable voting costs.

[Figure 5 about here.]

3. Empirical Strategy

3.1. A Precinct-Level Panel

We use official election results and electoral rolls provided by the Munich Election Office in the empirical analysis. One limitation is that the finest resolution available for turnout data is at the precinct level. Thus, we aggregate reassignments and distance from the polling location from the address level to precinct delineations. To obtain a constant unit of observation, we impose *time-invariant* precinct borders corresponding to the 2018 configuration for aggregation. Because voter turnout is recorded at election-specific precinct configurations, we also harmonize turnout to the 2018 delineations. The number of precincts in 2018 was small compared to other years. In this way, harmonization mostly involves aggregating turnout to larger units. For the remaining cases, we use conversion weights based on the number of eligible voters, provided by the election office. This leaves us with a panel of 618 precincts with consistent boundaries, which we observe over eight elections between 2013 and 2020. To ensure that treatment effects do not merely pick up measurement error created by harmonization, we perform robustness tests using precincts where polling place changes did not coincide with boundary changes (Appendix C). We also obtain data on time-varying structural indicators, including information on the local population (size, age, marital status, migrant status, citizenship, duration of residence, households with children, migration across and within precinct boundaries) and average housing cost.[19] Another limitation is the missing information on the number of residents per address. This can create potential distortions of the average distance to the polling place or the share of reassigned individuals if housing types within precincts vary a lot. We argue that concerns over such measurement error are limited. First, we focus on the model case, in which an *entire* precinct is treated in the causal analysis. Second, since precincts are relatively small (1,500 eligible voters per precinct, on average), the risk of within-precinct heterogeneity in housing types affecting our measure is limited. Additionally, our preferred specification includes election×district fixed effects. This ensures that we compare turnout dynamics among close precincts, limiting the risk of between-precinct heterogeneity distorting our estimates. We turn to the details of the empirical strategy in Section 3.3. Appendix Table E.2 reports summary statistics of our precinct-level variables.

[19]Information on local rents is from the RWI Institute for Economic Research. All other data are provided by the Munich Statistical Office and are available for download from the city's election review website (*Wahlatlas*). Migration data were specially prepared and are available upon request.

3.2. Conceptual Framework: Voting Costs, Inattention, and Turnout

To fix ideas, we develop a simple rational choice model of voting. We summarize the key intuitions here and relegate the details to Appendix B. The model combines three main ingredients: First, reassigning individuals to a different polling place raises or reduces the cost of voting in person by moving the polling location closer or further away. Second, reassignments always generate a disutility from engaging with an unfamiliar environment, independent of distance.[20] Third, we allow for *inattention* to reassignments. Unlike in the US, citizens in Munich are not explicitly informed of *changes* to their polling location. To notice a reassignment, they must review their polling place's address stated in the election notification, mailed a few weeks before the election.[21]

Thus, reassignments may go unnoticed by some. We model inattention by imposing that a fraction of poll voters are surprised by reassignments after the deadline to request mail-in ballots. Since the election notification must be presented to officials onsite, inattentive poll voters realize that the polling location moved only shortly before going to vote. We also allow for inattentive abstainers, who do not notice reassignments at all. Finally, we assume no inattention among mail-in voters because mail-in ballots must be requested by returning a form enclosed in the election notification.

The model delivers three key predictions. First, reassignments generate asymmetric turnout effects by distance: Increasing distance always reduces turnout at the polling place by making it less attractive relative to mail-in voting and abstention; however, decreasing distance does not raise polling place turnout, unless it is enough to compensate for the reassignment disutility. Second, inattention amplifies the shift toward abstention when reassignments make voting in person more costly. This is due to inattentive poll voters, who are surprised by reassignments. Some inattentive voters, who would have switched to mail-in voting, end up abstaining, leading to higher turnout losses relative to a scenario without inattention. Third, inattention attenuates turnout gains when reassignments reduce distance. Intuitively, inattention creates inertia among abstainers who remain unaware of reassignments.

3.3. Main Specification: An Event Study Design

We use an event study framework to trace out changes in voting behavior around polling place relocations. In the baseline, we define the event as the *first* election in which the *entire* electorate in a precinct is assigned to a different polling place. Reassignment of the entire precinct is the modal case, with 41 percent of all instances where at least some home addresses are reassigned.[22] 280 of the 618 precincts are classified as treated using this definition. We also trim

[20] In the model proposed by Brady and McNulty (2011), the reassignment disutility would capture what the authors label "search costs", i.e., a positive shock to the cost of voting in person, independent distance changes. They do not formally distinguish between search costs and distance costs; thus, our model extends their conceptual framework.

[21] Individuals are rational, conditional on inattention; however, the attention choice itself may or may not be optimal (Maćkowiak et al., 2023).

[22] We report density plots of the share of reassigned home addresses in Appendix Figure D.5.

precinct time series after a second reassignment to capture the effects of a single reassignment rather than multiple changes.[23] We test the sensitivity to alternative assumptions, including different binary and continuous treatment definitions, in Appendix C.

Let $E_p \in \{1, 2, ..., 8\}$ denote the election in which precinct p is fully reassigned for the first time (the event), and $\tau \equiv t - E_p$ denote time relative to the event. Then, our preferred specification is given by:

$$Y_{pt} = \sum_{k \neq -1} \mu^k \mathbb{1}(\tau = k) + \mathbf{X}'_{pt}\phi + \delta_p + \delta_{d(p)t} + \varepsilon_{pt}, \tag{1}$$

where an outcome Y_{pt} (e.g., turnout at the polling place, via mail, and overall) in precinct p and election t is regressed on election indicators relative to the event and a series of control variables and fixed effects. Specifically, we include precinct effects δ_p, which absorb any time-invariant factors that influence the outcome, and election fixed effects $\delta_{d(p)t}$, which we allow to be district-specific. Election fixed effects account for common shocks, such as differences in voting propensity across elections or the weather on election day. Interacting election with district fixed effects accounts for different electoral conditions across districts: Unlike precincts, districts are directly contested in some elections. At the state and federal level, the 25 districts are combined into single-member constituencies, where residents directly elect their representatives. In municipal elections, citizens also elect their local district committee. Thus, there may be systematic differences in voting incentives across districts if, for example, close races are anticipated in some constituencies (Bursztyn et al., 2023). Moreover, polling place recruitment is done by district inspectors. Thus, election×district fixed effects can also account for systematic differences in recruitment practices by comparing outcomes only within district-election cells. The vector **X** comprises a set of time-varying covariates: precinct size (log of the number of residents and the share of residents eligible to vote), the age structure of the electorate (share of eligible voters aged 18–24, 25–34, 35–44, and 45–59), the share of EU foreigners in the electorate, the average duration of residence (in years), the share of households with children, the average quoted rent per square meter, and the shares of native German residents, non-native German residents, single residents, and married residents, respectively. We discuss sensitivity to excluding covariates and replacing them with time-invariant controls interacted with election fixed effects in Appendix C.2.

The error ε_{pt} captures unobserved precinct×election shocks to the outcome that are assumed to be uncorrelated with the regressors of interest. Then, the event-time coefficients $\hat{\mu}^k$ trace the differential time path of the outcome in treated relative to untreated precincts before and after the reassignment. Specifically, estimates $\hat{\mu}^{k,\tau \geq 0}$ deliver the average effect of reassignment on treated units in election $\tau = k$ after the event.

The two identifying assumptions for a causal interpretation are *i)* that polling place reassignments and changes in distance are unrelated to other determinants of voting behavior (that are

[23] Out of 280 treated precincts, 150 (54 percent) are treated exactly once (Appendix Figure D.6).

not accounted for by fixed effects), and *ii)* parallel trends. For instance, a violation occurs if the expectation of turnout changes influences polling place reassignments; e.g., if the election office consolidated precincts expecting high mail-in turnout to save costs of operating polling places.[24]

Although these assumptions cannot be tested directly, we present several indirect tests: *i)* a balancing exercise that examines the co-occurrence of changes in precinct characteristics and reassignments, *ii)* an analysis of pretrends, and *iii)* bounds on treatment effects under the assumption of parallel trend violations, proposed by Rambachan and Roth (2023). Rambachan and Roth formalize the idea that pre-treatment deviations from parallel trends inform unobserved violations after treatment adoption. They provide different approaches to formalize this intuition, taking into account the statistical uncertainty in estimating event study coefficients.

A few final estimation details. First, we determine polling place changes between two *consecutive* elections throughout the paper. This implicitly assumes that treatment effects are mainly driven by regular voters, who participate in all types of elections. Our panel length somewhat limits exploring different treatment definitions across election types. Thus, we aim to absorb the influence of varying electoral conditions on voting propensity via election×district fixed effects and leave heterogeneity across election types to future research. Second, because votes by mail are recorded only at the district level, we are confined to relying on *requested* polling cards as a proxy for mail-in votes. About 90 percent of requested cards are returned as ballots, and more than 98 percent of these ballots are mail-in votes. Third, since not all event-time indicators are identified, we choose the election before the reassignment $\tau = -1$ as our reference period and normalize μ^1 to zero. We then estimate the whole range of event-time indicators and report the coefficients for the four elections before and three elections after reassignment. Fourth, we cluster standard errors at the precinct level to account for the correlation of model errors over time. We test the sensitivity to alternative assumptions about the variance-covariance matrix in Appendix C. Fourth, specifications are weighted by precinct size (number of eligible voters) to improve precision and the representativeness of the effect estimates. Finally, we estimate Equation 1 using OLS to produce our baseline results. As recently shown, OLS two-way fixed effect (TWFE) estimates may yield biased results with staggered treatment and heterogeneous effects across treatment cohorts.[25] The reason is that the TWFE estimator uses "forbidden" comparisons between already-treated precincts and newly-treated precincts, thereby violating the parallel trend assumption in the presence of effect heterogeneity. The treatment timing in our setting is illustrated in Appendix Figure D.6. Of 618 precincts, 338 are never treated, and most of the treated precincts had their polling

[24]The election office asserts that past and expected turnout is not considered when redrawing precinct boundaries.

[25]See e.g., Athey and Imbens (2022); de Chaisemartin and D'Haultfœuille (2020); Borusyak et al. (2023); Goodman-Bacon (2021); Sun and Abraham (2021).

location changed in the 2017 Federal Election (62 percent).[26] To account for the staggered treatment timing, we also estimate the event study using the estimators proposed by Borusyak et al. (2023), Callaway and Sant'Anna (2021), Sun and Abraham (2021), and de Chaisemartin and D'Haultfœuille (2020). A discussion of the different estimators and their underlying assumptions is beyond the scope of this paper. For recent reviews, see Roth et al. (2023) and de Chaisemartin and D'Haultfœuille (2023).

3.4. Reassignment Timing

Under our identifying assumptions, the timing of reassignments is uncorrelated with other determinants of turnout. One approach to assess this assumption's plausibility is to examine whether precinct characteristics are balanced conditional on election and precinct fixed effects. Since the fixed effects account for time-invariant factors, the residual correlation reflects the association between treatment timing and *changes* in precinct characteristics. We present the results in Figure 6.

Panel A shows estimates and 95 percent confidence bands from univariate OLS regressions of a dummy identifying reassignments that changed the polling location of the entire precinct on precinct characteristics, conditional on election and precinct fixed effects. Each estimate comes from a separate regression. Independent variables are standardized to have mean zero and unitary standard deviation. The estimates are near zero and insignificant, suggesting that treatment timing is uncorrelated to observable changes in precinct characteristics. The dependent variable in Panel B is the log average walking distance. Out of 20 estimates, only one is statistically significant at the 5 percent level. Still, F-tests cannot reject the null that the coefficients are jointly zero in any panel. Coefficients and test statistics are reported in Appendix Table E.3, which also includes results for reassignments due to polling location recruitment and precinct reconfigurations, separately. Again, we find no evidence that changes in observable precinct characteristics co-occur with polling place relocations.

[Figure 6 about here.]

4. Main Results

4.1. Average Effects on Turnout and the Mode of Voting

We start by pooling all polling place reassignments to estimate the average treatment effects on treated precincts. Figure 7 plots event-time estimates based on Equation 1 using different outcomes in Panels A–D. Panel A illustrates the average treatment intensity by using the share of reassigned addresses and the *change* in proximity to the polling location (relative to the preceding election) as dependent variables, respectively. Since reassignments at intensities below 100 percent are allowed to occur before and after the event, the coefficients in $\tau \neq 0$ are

[26] 14 percent (13 percent) of precincts have their polling place moved in the 2020 Municipal Election (2018 State Election), and the remainder is treated in other elections. Appendix Figure D.7 maps the spatial distribution of polling place relocations.

not precisely equal to zero, and the coefficient in $\tau = 0$ is less than one (left axis). Importantly, the design captures a sharp reassignment shock relative to the baseline. The coefficients on the change in distance (right axis) suggest that reassignments increase the distance to the polling location by 0.12 kilometers, on average. This represents a moderately larger increase compared to the overall distribution of proximity changes caused by reassignments presented in Figure 5.

[Figure 7 about here.]

Coefficients preceding the reassignment are close to zero and insignificant in Panels B–D. The absence of pretrends supports the validity of the identifying assumption; trends in outcomes across comparison groups evolve in parallel except through the treatment. By contrast, we observe a sharp and persistent increase of 0.94 percentage points in the share of votes cast by mail immediately after reassignment (Panel B). The results in Panel C show that this jump can only be partly explained by substitution between modes of voting: In-person turnout falls by 1.0 percentage points immediately after reassignment (equivalent to 3.0 percent at the mean), while mail-in turnout increases by only 0.61 percentage points (2.1 percent). Thus, the initial shift to mail-in voting is not large enough to offset votes lost at the polls, generating a decline in total turnout of 0.39 percentage points (0.6 percent) in Panel D. This result is consistent with a positive shock to the cost of voting in person on average, making mail-in voting relatively more attractive and inducing some voters to abstain from turning out.

The shift from poll voting to mail-in voting is persistent, suggesting a lasting shock to voting costs. By contrast, the decline in total turnout completely recovers in the subsequent election. A possible explanation is that the initial shock to voting costs wanes over time. For example, abstainers may familiarize themselves with their new polling place and return to vote there after one election. An alternative explanation is inattention to reassignments. Inattentive poll voters are surprised by the reassignment after the deadline for requesting a mail-in ballot. Among those who would have switched to mail-in voting, some abstain and only turn to mail-in voting in the subsequent election, thus producing a transitory drop in turnout. In Section 4.3, we make the case that the transitory decline is consistent with inattention and incompatible with the waning cost hypothesis. The argument is that the recovery is entirely driven by an increase in mail-in turnout, not in-person turnout.

Albeit transitory, the turnout decline caused by changing polling places is sizable. The average decline is roughly equivalent to reducing the number of early (in-person) voting days in the US by two (Kaplan and Yuan, 2020). Moving a polling place would also be enough to offset the positive turnout effect of an additional newspaper around the turn of the twentieth century in the US (Gentzkow et al., 2011). The drop contrasts the absence of *instantaneous* participation effects of moving a polling place estimated by Clinton et al. (2021) and Tomkins et al. (2023) and appears in line with turnout decline found by Brady and McNulty (2011). However, the inattention channel is likely prevented in the US by informing affected voters of changes to their polling location. Thus, the findings are not necessarily contradictory; especially, since

Brady and McNulty (2011) study a setting in which distance increased for nearly all voters whose polling place changed. We explore the role of distance changes in the next section.

A key insight of Figure 7 is that the estimates *do not* substantiate the hypothesis that (non)voting is habit forming. If abstaining was habit-forming by increasing its consumption value, the initial decline in turnout would carry over to subsequent elections. This would be true even if voting costs were entirely restored to pre-treatment levels. The full turnout recovery is inconsistent with this implication. This result contrasts with Fujiwara et al. (2016), who find that a decline in past turnout due to rainfall on election day also reduces current turnout, and are in line with Bechtel et al. (2018), who show that compulsory voting in Switzerland did not instill a voting habit by generating persistently higher turnout after its abolition. Importantly, we cannot rule out that habit formation contributes to the observed persistence in the *mode* of voting. A sharp test would require a scenario where shocks to voting costs are truly transitory. Then, any persistence must be driven by habit. If one is willing to assume that a second reassignment back to the original polling location does not create new costs, such cases could be exploited to test the hypothesis. However, the assumption is hard to defend, and temporary relocations are too rare in our data to deliver credible estimates.

Point estimates based on Equation 1 appear in Table C.1 of Appendix C. The section also presents several robustness checks: We show that the estimates are robust *i)* to replacing election×district fixed effects with election fixed effects, *ii)* to using unweighted regressions, *iii)* to not trimming the time series once a second reassignment occurs, *iv)* to limiting the sample to treated precincts with exactly one (full) reassignment and control precincts with zero (partial) reassignments, *v)* to using a balanced panel around the 2017 Federal Election and the 2018 State Election, where most reassignments occurred, and *vi)* to removing treated precincts where the reassignment coincided with a change to precinct boundaries. We demonstrate that TWFE-OLS estimates are very similar to those produced by DiD estimators that account for potential effect heterogeneity across treatment cohorts (Figure C.1). We show robustness to alternative assumptions about the variance-covariance matrix in Table C.2 and test if the different reasons for reassignments (polling location recruitment versus precinct reconfiguration) carry different turnout effects. We find they do not (Figure C.2). Our treatment estimates also hold when allowing for linear violations of the parallel trend assumption using the methodology by Rambachan and Roth (2023). Finally, we show robustness to different matching procedures and to excluding time-varying covariates.

4.2. The Role of Distance to the Polling Location

The baseline estimates are informative about the effects of an average reassignment. However, they mask a key dimension of reassignment heterogeneity: the change in distance to the polling location. As noted, 90 percent of reassignments change the walking distance by *less* than one kilometer. In this section, we analyze whether even such small changes to voting costs matter and can help explain the underlying changes in voting behavior. To this end, we estimate two modified versions of Equation 1.

Effect Heterogeneity by Change in Distance. First, we allow for different treatment effects between reassignments that increased versus decreased distance to the polling place. One limitation is that we can relate outcomes only to *average* distance changes at the precinct level. Aggregate changes obscure heterogeneity among individuals within a precinct, e.g., when some individuals are closer and some further away from the new polling location. We address this below by limiting the analysis to cases where within-precinct heterogeneity is as small as possible.

Formally, let D_p^+ be an indicator equal to 1 for precincts where reassignment caused an *increase* in average distance to the polling location. D_p^- denotes the analogous indicator for cases in which distance *decreased*. Then, the modified event study specification takes the following form:

$$Y_{pt} = D_p^+ \times \sum_{k\neq -1} \beta^k \mathbb{1}(\tau = k) + D_p^- \times \sum_{k\neq -1} \alpha^k \mathbb{1}(\tau = k) + \mathbf{X}'_{pt}\phi + \delta_p + \delta_{d(p)t} + \varepsilon_{pt}, \tag{2}$$

where the coefficients $\hat{\beta}^k$ and $\hat{\alpha}^k$ trace the differential time path of turnout separately for the two groups defined by D_p^+ and D_p^-. Note that since we do not condition on distance in Equation 1, the baseline estimates $\hat{\mu}^k$ correspond to a weighted average of $\hat{\beta}^k$ and $\hat{\alpha}^k$. Again, the specification includes election×district fixed effects, precinct fixed effects, and time-varying controls.

The results are presented in Figure 8. Panel A shows that distance increases by 330 meters (or about 4 walking minutes), on average, when the new polling location is moved further away. When the new polling place is moved closer, the reduction is about 240 meters (3 walking minutes). Consistent with our model, turnout effects are strikingly asymmetric: Reassignments that increase distance cause a sharp and persistent decline in polling place turnout (Panel B). The estimate on the immediate effect is -1.89 (p <0.001), which is equivalent to a 5.6 percent decline at the mean and nearly double the average effect. By contrast, when reassignments reduce the distance to the polling place, in-person turnout tends to rise slightly, albeit not statistically significant. Panels C and D show a similar picture. The impact on mail-in turnout is negative but insignificant when the new polling location is closer and strongly positive when relocated further away. Overall, participation declines only in precincts in which distance increases. The drop amounts to 0.63 percentage points, which is 62 percent greater than the average effect. Our model proposes that reassignments always cause a disutility from engaging with an unfamiliar environment. The results suggest that the reassignment disutility and distance decline offset each other when the polling location moves closer, on average. Consequently, we find minimal substitution between the modes of voting. By contrast, the reassignment disutility is compounded by additional travel costs when the new polling location is further away. This generates a significant shift from in-person to mail-in voting and a sizable drop in overall participation. The point estimates and results based on a balanced panel and other robustness checks are reported in Appendix Table C.3 and further discussed in Appendix C.

[Figure 8 about here.]

We also estimate a version of Equation 2 in which we allow treatment effects to vary by *three* reassignment types in Figure 9: those that produce a "large" distance decrease, "little" distance change, and a "large" distance increase. While the shift from polling place to mail-in voting is visibly attenuated when distance barely changes, the decline in total remains comparable to reassignments that strongly increased distance (Panel D). This pattern bolsters the case that the reassignment disutility alone burdens voters beyond proximity changes. By contrast, when the new polling place is relocated significantly closer to voters, substitution is *reversed*; mail-in turnout declines (albeit not statistically significant), mirrored by a significant and permanent increase in polling place turnout. Total participation increases slightly; however, the estimate is not statistically significant.[27]

We also estimate treatment effects on a sample that minimizes cases with ambiguous distance changes within precincts. Specifically, we restrict the treatment group to three cases: precincts where the reassignment consistently increased (respectively decreased) the distance to the polling place for at least 90 percent of home addresses, and to precincts where the polling place moved only "a little", i.e., less than 800 meters from the old location.[28] The estimates in Appendix Figure D.9 confirm the previous conclusions: We find an amplified shift from in-person to mail-in voting when distance increases for nearly all citizens. The pattern is reversed when distance decreases. Here, the estimates show an uptick in total turnout coming from increased participation at the polling place. This suggests that former abstainers can be incentivized to turn out, highlighting the sensitivity of voting behavior to seemingly minor reductions in voting costs. When distance changes only slightly for all citizens, we again find a transitory drop in total turnout, consistent with a disutility from reassignments. We also estimate treatment effects for four (equal-size) groups determined by the share of addresses that experienced a distance increase in Appendix Figure D.10. When within-precinct heterogeneity is high, i.e., distance changes in opposite directions, the average distance change is closer to zero and the substitution between mail-in and in-person voting is attenuated in the aggregate. However, we still find a distinct (transitory) drop in total turnout, supporting the importance of reassignment disutility.

[Figure 9 about here.]

[27] In Appendix Figure D.8, we estimate treatment effects by four reassignment types; those that produced a small distance reduction, a large reduction, a small increase, and a large increase. The results paint a similar picture: Large distance reductions generate substitution away from mail-in toward poll voting; yet, the effect on total turnout is insignificant. Small distance reductions are insufficient to compensate for reassignment disutility, resulting in a decline in polling place turnout. Finally, relocations further away always cause a shift away from in-person to mail-in voting and abstention.

[28] 800 meters corresponds to the median change in distance between new and old polling locations.

Decomposition Exercise: Distance and Reassignment Disutility. In our second exercise, we introduce the log average walking distance to the polling location as a covariate in our dynamic event study specification (Equation 1):

$$Y_{pt} = \sum_{k\neq -1} \mu^k \mathbb{1}(\tau = k) + \lambda \log\_\text{distance}_{pt} + \mathbf{X}'_{pt}\phi + \delta_p + \delta_{d(p)t} + \varepsilon_{pt}. \tag{3}$$

Since the specification includes precinct fixed effects, the parameter λ is identified from variation in distance *within* precincts, which is driven by polling place reassignments only. Thus, we can interpret the residual turnout effects, captured in μ^k, as the impact of reassignments *per se*. In our model, reassignments always cause a disutility, raising the costs of voting at the polling place.[29] The relative reduction of μ^k caused by including the distance control gives us a sense of the importance of distance changes versus the reassignment disutility for turnout effects.

The results are presented in Table 1. The outcomes are turnout at the polling place (Columns 1 and 2), turnout by mail (Columns 3 and 4), and total turnout (Columns 5 and 6). Odd columns use election×district fixed effects; even columns use election fixed effects. Absorbing the distance effect attenuates the event-time estimates relative to the baseline results (Columns 2 and 4, Table C.1). However, the estimates remain mostly statistically significant, consistent with the notion that reassignments induce a disutility beyond the change in travel distance. The estimate on log distance is negative and statistically significant in Columns (1) and (2): Polling place turnout falls by 0.34 percentage points when distance increases by 10 percent. Increasing distance has the opposite effect on mail-in turnout (Columns 3 and 4); however, the effect size does not completely offset the negative impact on in-person turnout: Increasing distance by 10 percent reduces total turnout by 0.08 percentage points (Columns 5 and 6). Thus, to offset the turnout drop at the polling place due to reassignment disutility, the polling location would have to move 17–19 percent or about 130 meters closer to voters, on average.[30] To avoid a decline in total participation, the polling location would have to move 37–53 percent, or about 260–380 meters closer to voters. Interestingly, the immediate impact on mail-in turnout becomes insignificant, but it more than doubles and turns significant in the subsequent election. Again, this pattern is consistent with inattentive voters delaying the switch to mail-in voting because they missed the opportunity to request a mail-in ballot. We corroborate the inattention hypothesis in the next section.

The change in point estimates $\hat{\mu}^k$ suggests that relocations *per se* are the primary driver of changes in voting behavior: Distance explains 34–38 percent of the treatment effects on in-person turnout (across the three post-reassignment elections), and 18–25 percent of the drop

[29] Reassignment disutility is conceptually similar to what Brady and McNulty (2011) label "search costs", i.e., the costs that arise on top of increased travel distance due to the time of looking up and going to the new polling location.

[30] Note that the compensating effect comes partly from fewer voters switching away from in-person voting and more mail-in voters turning to vote at the polling place.

in total turnout in the first election after reassignment. The insight is important. Election officials monitor the proximity of polling locations; yet, the voting barrier created by merely changing these places has so far been overlooked. Causal estimates of the turnout effects of distance to the polling location use cross-sectional variation near precinct borders in a regression discontinuity design (Cantoni, 2020; Bagwe et al., 2022; Tomkins et al., 2023). Based on the negative impact of greater distance, it is tempting to prescribe a policy of simply moving polling places closer to voters to boost turnout. Our results highlight that this may deliver unexpected outcomes as distance reductions come at the cost of changing familiar polling locations.

In Appendix C.5, we perform an alternative decomposition approach using a triple difference framework. Instead of including distance as a covariate, we estimate the differential turnout trend among treated units induced by moving the polling place by an additional kilometer. We also allow the impact of distance to differ depending on whether the new polling place is closer or further away. The results corroborate our findings quantitatively. Moreover, we reject the hypothesis that distance generates different turnout effects (in absolute terms) in cases that increase versus decreased distance.

[Table 1 about here.]

4.3. Mechanism: What Drives the Recovery in Total Turnout?

Perhaps intriguingly, the decline in total turnout is recovered after one election, even when reassignments strongly increase the distance to the polling place. This pattern could be explained by inattention to reassignments. As formalized in our model (see Appendix B), inattention implies that some voters delay switching to mail-in voting by one election and instead temporarily abstain from turning out. The reason is that they are surprised by the reassignment *after* the deadline for requesting mail-in ballots has passed. However, an alternative explanation could be the waning of the initial shock to voting costs. Waning costs imply that voters temporarily abstain from turning out and return to voting in person, for instance, because they familiarized themselves with their new polling place. Thus, while inattention implies that the recovery in the subsequent election is driven by an increase in *mail-in* voting, waning costs imply that the recovery is driven by an increase in turnout *at the polling place*.

A visual inspection of the baseline estimates in Figure 7, Panel C lends some support for the inattention hypothesis as the effect size estimates on mail-in turnout further increase between the first and the second post-reassignment election. This pattern is even more pronounced for estimates on reassignments that caused an increase in distance to the polling location (Panels B and C, Figure 8). Polling place turnout, on the other hand, tends to *decline* between the first and second post-event election, inconsistent with the waning-costs hypothesis.

Formally, we test whether the event-time indicators in the first and second election after reassignment differ; and whether the sign of the difference implies an increase in mail-in or in-person turnout, respectively. We use estimates restricted to cases that generated a *greater* distance to the polling location (i.e., $\hat{\beta}^1 - \hat{\beta}^0$ from Equation 2) to rule out ambiguity due to cases where voting costs declined. Indeed, we find that the difference for mail-in turnout is positive and statistically significant (0.56, $p < 0.01$). The difference for in-person turnout is negative, albeit not statistically significant (-0.07, $p > 0.1$). Another approach is to test the difference of the event-time coefficients holding distance to the polling location constant as proposed in the previous section (Equation 3). In this specification, event-time dummies capture turnout effects that are driven by the reassignment disutility. Again, the test suggests that mail-in turnout further *increases* in the second election after reassignment (0.41, $p < 0.05$), while polling place turnout, if anything, marginally decreases (-0.04, $p > 0.1$). Hence, the results strongly support the hypothesis that the recovery in total turnout is driven by inattentive voters switching from nonvoting to mail-in voting, and are inconsistent with the waning-cost hypothesis.

We replicate the tests using the novel DiD estimators that account for heterogeneous treatment effects and staggered treatment timing. The event study results are plotted in Appendix Figure D.11 for specifications using a restricted sample excluding reassignments that caused a distance *decrease*, and in Appendix Figure D.12 for specifications controlling for the log walking distance. Appendix Table E.5 reports the difference between the first and second post-reassignment coefficients. The results support the conclusion that the transitory decline in turnout is driven by inattention to reassignments: All estimators show that mail-in turnout further increases in the second post-event election; the difference is statistically significant in seven out of ten cases. Instead, there is no evidence that in-person turnout drives the recovery. Most estimated differences are negative, and none are statistically significant.

Our model also considers inattention to reassignments among abstainers (e.g., because they do not open the election notification). Here, inattention attenuates turnout gains when reassignments reduce the distance to the polling location. Intuitively, some individuals would have turned out at their new (closer) polling location if informed but, instead, remain abstainers. Empirically identifying this type of inattention is challenging because we cannot determine when the decreased travel distance is sufficient to make poll voting attractive to abstainers.

5. Effect Heterogeneity and Partisan Consequences of Reassignments

Our estimates deliver average turnout effects among treated precincts. Yet, the effects may mask heterogeneity across different voter groups. Uncovering heterogeneity is important. First, policymakers may be concerned about reassignments imposing a disproportional burden on minorities, the elderly, or disadvantaged people. In fact, one motive for recruiting *new* polling places was to assure barrier-free access for voters with physical impairments. Second, if some groups are more likely to be discouraged from turning out, reassignments could compromise the representativeness of the electoral outcome. Thus, we devote this section to analyzing heterogeneity, starting with differences across demographic groups followed by partisan consequences of reassignments.

5.1. Heterogeneity across Precinct Characteristics

Who responds to reassignment shocks? To explore heterogeneity across voter groups, we extend the standard DiD (Equation 1) to include a set of interaction terms between event-time indicators and a variable Z_p along which we allow for heterogeneity. Z_p is measured at the precinct level and chosen to be time-invariant. Then, the modified specification corresponds to a triple-difference estimator that introduces a comparison among treated groups to the base comparison of treated and untreated units:

$$Y_{pt} = \sum_{k \neq -1} \gamma^k [Z_p \times \mathbb{1}(\tau = k)] + \sum_{k \neq -1} \theta^k \mathbb{1}(\tau = k) + \mathbf{X}'_{pt}\eta + \pi_p + \pi_{d(p)t} + \epsilon_{pt}, \tag{4}$$

where θ^k capture the base effect of the standard double difference, $\mathbf{X}$ is a vector of time-varying covariates, and π_p and $\pi_{d(p)t}$ denote precinct and election×district fixed effects, respectively. For intuition, suppose that Z_p is a dummy identifying precincts with an above-average share of elderly eligible voters. Then, the estimates $\hat{\gamma}^k$ trace the differential turnout trend in "old" relative to "young" *treated* precincts before and after the polling place relocation. Note that all first and second-order interaction terms required for identification of the triple-difference estimator are included in the specification or absorbed by the fixed effects.

In practice, we estimate Equation 4 separately for different Z_p's. Characteristics are standardized (i.e., unitary standard deviation and mean zero) and measured in 2013 (the first year in our panel). Hence, the triple-difference estimates reflect the differential turnout trend among treated units when Z_p is increased by one standard deviation. Note that since Z_p is standardized, the base effects θ^k correspond to the average treatment effects on the treated (ATT) shown in Figure 7. We thus restrict the output to the triple-difference estimates.

The results appear in Figure 10. In each panel, the outcomes in the left plot are turnout at the polling place and via mail. The effect on total turnout is reported in the right plot. The main conclusions are that precincts with a higher share of elderly eligible voters show a greater decline in polling place turnout and a weaker shift to mail-in voting when reassigned. This results in a stronger drop in total turnout (Panel A). The effects on total turnout are statistically significant and *persistently* negative, suggesting that participation rates among elderly voters are permanently depressed. Indeed, an F-test that the *overall* effect on total turnout is equal to zero in the two subsequent elections ($H_0 : \hat{\gamma}^1 + \hat{\theta}^1 = \hat{\theta}^2 + \hat{\gamma}^2 = 0$) is rejected at the 5 percent level (F=3.58, p=0.03). Given that recruiting barrier-free venues to improve access for voters is a main driver of polling place changes, this result is important. It suggests that the burden of reassignments outweighs the potential benefits of better access to the buildings. In Panel B, the positive (negative) estimates on in-person (mail-in) turnout suggest that the effects are attenuated in precincts with a higher share of younger eligible voters. This is unsurprising, given that a greater share of first-time voters implies more individuals who do not experience reassignments. We find no measurable treatment effect differences for precincts with a higher fraction of households with children, who may also benefit from barrier-free access (Panel C).

Effects are also not different in precincts where housing is more expensive (Panel D). Panel E shows that the substitution between modes of voting is weaker in precincts with a higher fraction of Germans with a migrant background; yet, total turnout appears not statistically different. This finding might reflect that migrants are unfamiliar with mail-in voting from their country of origin or the process of requesting a mail-in ballot (e.g., due to language barriers).[31] These results contrast with Cantoni (2020), who finds that a greater distance to the polling location reduces turnout stronger in areas with higher minority and low-income presence.

[Figure 10 about here.]

Two remarks are in place. First, since inference is not based on (quasi-)random variation, the heterogeneity analysis provides only suggestive evidence of the mechanisms driving differential turnout trends. For instance, other characteristics correlated with Z_p (e.g., unobserved aspects of voters' socioeconomic status) could be the actual *cause* of heterogeneity. We report summary statistics and correlations among the heterogeneity dimensions in Table E.7, showing that they are indeed interrelated. Second, we did not account for the change in distance to the polling location generated by reassignments. To rule out that differential trends are merely the result of correlation between Z_p and proximity to the polling place, we re-estimate all specifications conditional on the log walking distance. Appendix Figure D.13 shows that the conclusions still hold.

5.2. Partisan Consequences of Reassignments

Next, we examine the partisan consequences of reassignments. One limitation is that we observe precinct-level party outcomes only for votes cast *in-person*. Party votes from mail-in ballots are recorded at the *district level*. As there are only 25 districts, estimates based on district-level observations are likely underpowered. Consequently, we first analyze party results using our precinct panel. The results help us understand whether reassignments disproportionately dissuade specific party supporters from turning out at the polling place. Next, we verify if the conclusions hold in the district-level panel using party outcomes from mail-in ballots.

We estimate Equation 1 for two outcomes: party turnout, defined as the number of party votes relative to the number of eligible voters, and party vote share, defined as the number of party votes relative to the number of total votes. For expositional convenience, we group the outcomes of the six largest parties that were on the ballot in every election during our observation period into a "left-wing" and a "right-wing" cluster according to the parties' platforms.[32]

The results presented in Figure 11 suggest that in-person turnout declines slightly more for right-wing parties after reassignment (left plot, Panel A); however, the effects are not statisti-

[31] For instance, election notifications, which include information on requesting polling cards to vote by mail, are only sent out in German.

[32] We use the left-right categorization suggested by ParlGov (parlgov.org) to group parties. Left-wing parties include SPD, *Grüne*, and *Die Linke*; right-wing parties include CSU, *Freie Wähler*, and FDP.

cally different from each other in any period (right plot, Panel A). Panel B presents the results for party vote shares, which is the relevant metric for the composition of parliament. None of the event-time indicators are statistically significant from zero (left plot, Panel B) nor statistically different from each other in any period (right plot, Panel B). Thus, assuming that voters who switch to voting by mail do not simultaneously change their party preference because of reassignment, the results suggest negligible partisan consequences. We present the results for all parties individually in Appendix Figure D.14. Again, the estimates do not suggest that any party particularly gains or loses from reassignments. We also find null effects when estimating a modified event study specification using a district-level panel and party outcomes from mail-in votes, corroborating the results (Appendix Figure D.15).

The null effects on the electoral outcomes are reassuring from an administrator's perspective. Polling place relocations are not notably concentrated geographically (Appendix Figure D.7). In addition, the absence of significant spatial segregation along party lines in Munich ensures that polling place relocations are not particularly targeted at a particular party's supporters. However, small changes in voter participation could matter in close constituency elections during state and federal elections, where voters directly elect their representative on a plurality rule. The vulnerability to adverse effects is also markedly higher for democracies with two-party systems and strong partisan segregation. Thus, our results should not imply that electoral consequences of polling place relocations are universally benign.

[Figure 11 about here.]

6. Conclusion

Voting is the backbone of democracy. Yet, the likelihood of a pivotal vote is negligible, raising the possibility that seemingly innocuous changes to voting costs affect electoral turnout. Election officials in Munich recruit new polling places to improve their accessibility and control precinct sizes to prevent congestion, producing plausibly exogenous variation in the assignment of polling places. We study the turnout effects of relocating polling places using an event study design. Results suggest that polling place reassignments induce a persistent substitution away from in-person voting toward mail-in voting and a transitory decline in total turnout by 0.4 percentage points (0.6 percent). The effects are amplified when the polling place is moved further away and insignificant, on average, when reassignments reduce the distance to the polling location. Our findings suggest that changes in turnout are mostly driven to the relocation itself rather than changes in proximity to the polling place, similar to the findings by Brady and McNulty (2011). This result cautions about targeting distance to the polling place as the sole accessibility factor (Cantoni, 2020), as distance reductions come at the cost of relocation.

We identify inattention to reassignments as the likely channel behind the drop and subsequent recovery in total turnout. Inattentive voters are surprised by reassignments after the deadline for requesting mail-in ballots. Among those who would have switched to voting by mail, some abstain and turn to mail-in voting only in the subsequent election. Thus, increasing the salience of polling place relocation could effectively prevent turnout losses by mitigating inattention. Our results complement correlational evidence suggesting no participation declines after relocations in the US, where voters are typically informed of changes to their polling location ahead of election day (Clinton et al., 2021; Tomkins et al., 2023). Moreover, the full turnout recovery is incompatible with the hypothesis that voting is habit-forming.

Heterogeneity analyses suggest that reassignments cause a stronger and persistent turnout decline in precincts with more elderly eligible voters. The result is intriguing, given that recruiting barrier-free locations was a primary reason for polling place reassignments. Thus, our findings highlight that a well-intentioned policy can have unintended consequences when small changes in voting costs are overlooked. We find no evidence that moving polling locations adversely affected the electoral outcome by altering party shares. However, democracies characterized by spatial voter segregation along party lines and two-party systems may be more vulnerable to partisan consequences, justifying particular scrutiny of this practice.

Finally, our results highlight the role of mail-in voting in compensating for turnout losses at the polls and preventing a persistent drop in participation. Other convenience voting arrangements, such as in-person early voting at pre-defined locations, could similarly cushion the negative consequences of moving polling places. For instance, voters may opt to vote at a familiar or nearby early voting site after noticing that their election-day polling location has moved.[33] But access to alternative voting options is not common: Mail-in voting is available to all eligible voters only in 6 percent of countries globally (15 percent of EU member states and 29 percent of OECD countries). Early voting is offered by 8 percent globally, and 11 percent offer at least one alternative to election-day poll voting (International Institute for Democracy and Electoral Assistance (IDEA)). Thus, in contexts where the substitution between modes of voting is limited, negative turnout effects of moving polling locations are likely larger and more persistent, underscoring the importance of monitoring this practice outside of Germany.

[33] Additionally, early voting may mitigate losses coming from inattention by providing an alternative to voters who notice the reassignment too late to request a mail-in ballot but before election day. Early voting itself can also boost participation (Kaplan and Yuan, 2020).

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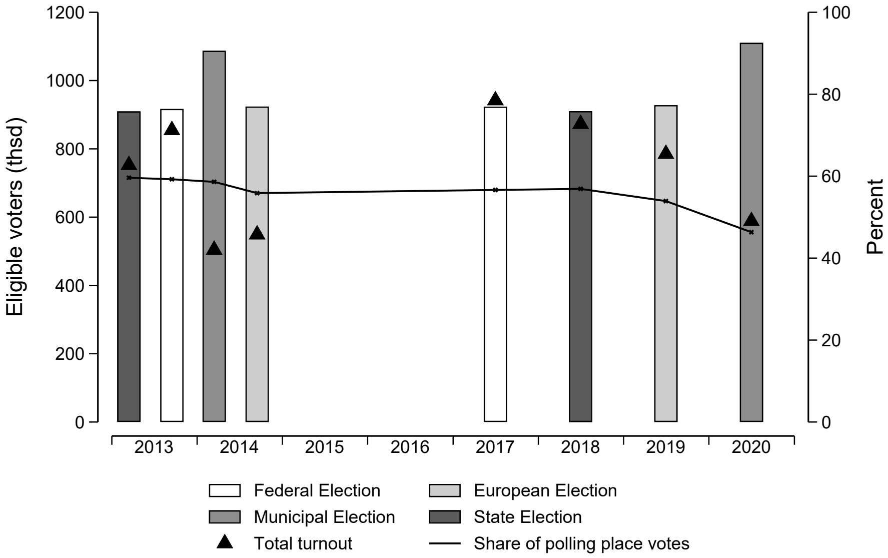
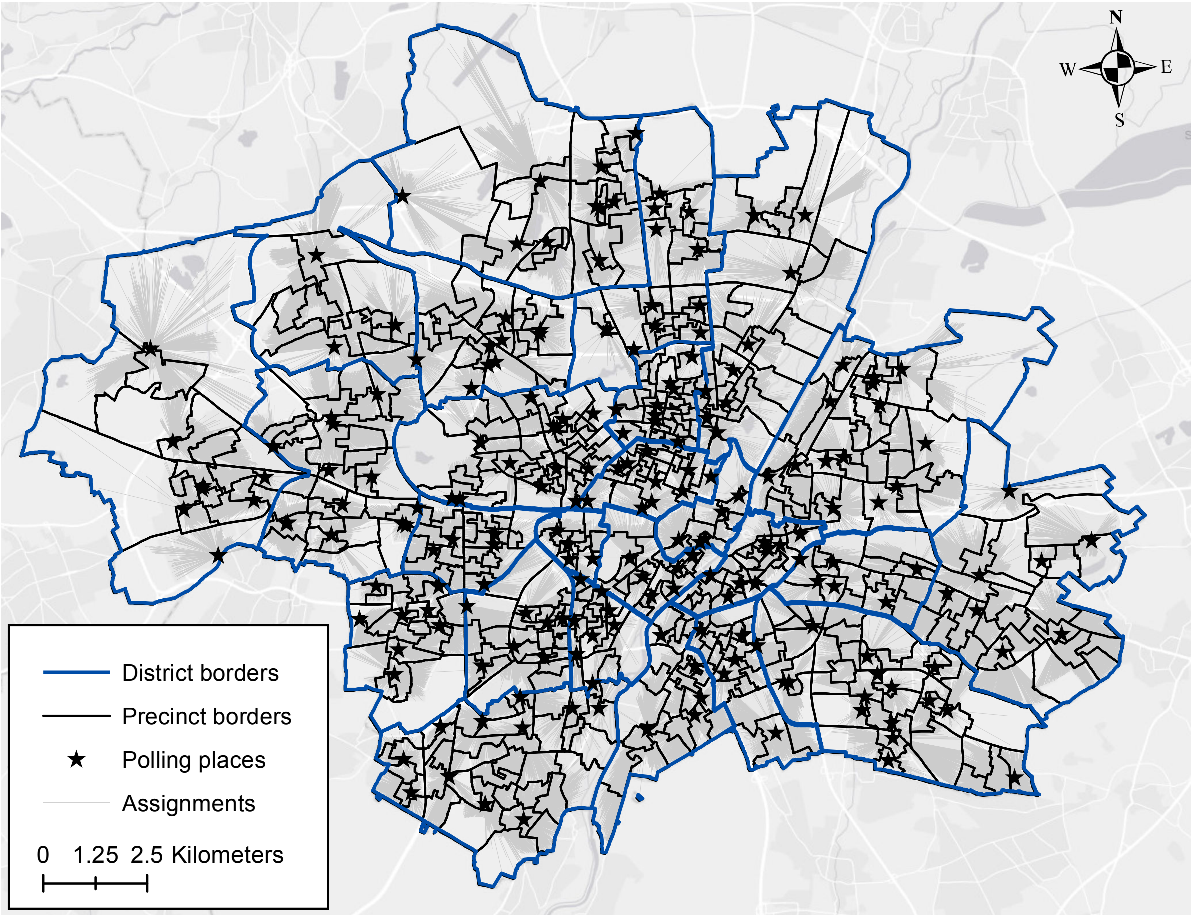
Figure 1: Timeline and Turnout of Elections Held between 2013 and 2020



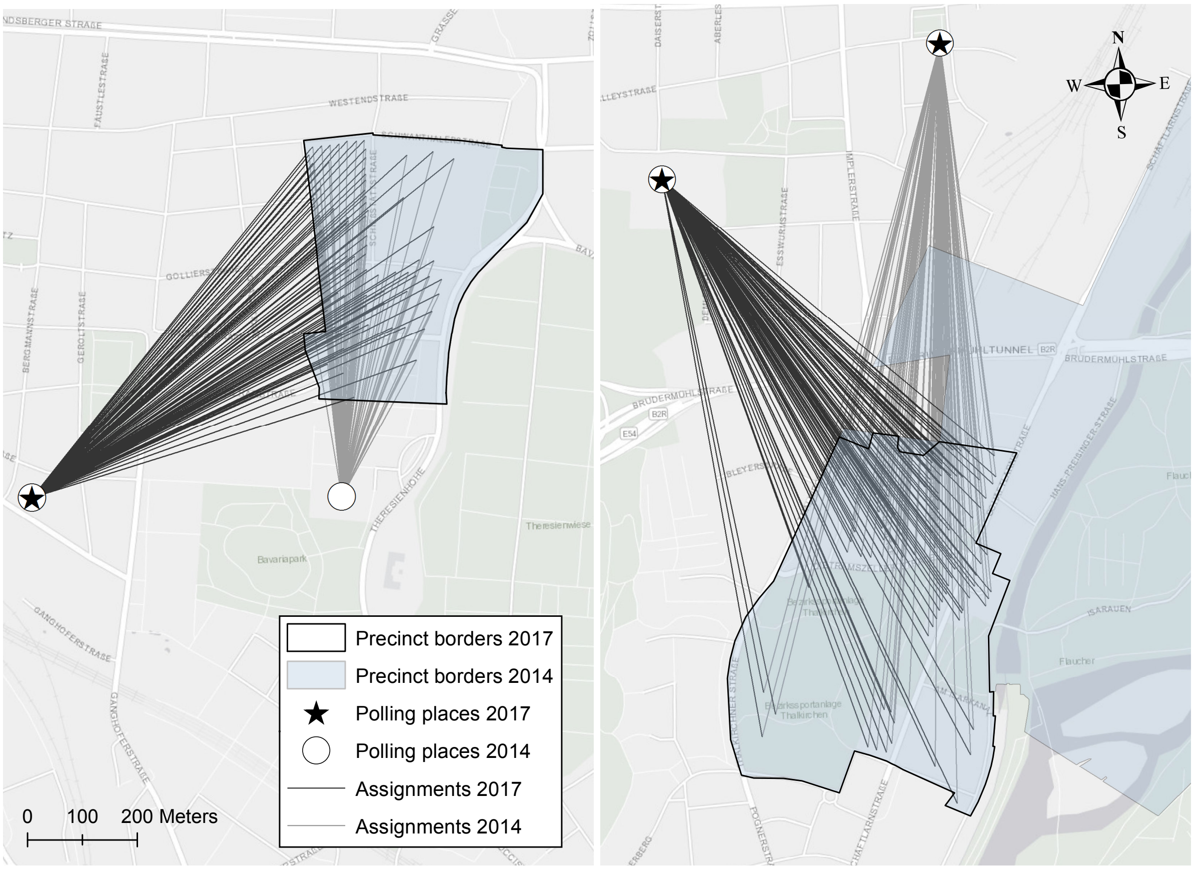
Notes: The figure presents the number of eligible voters (vertical bars), total turnout, defined as the percentage of eligible voters who cast a vote (triangles), and the percentage of polling place votes (solid line) for the eight elections included in our sample. The shading of the bars reflects the different election types. Between 2013 and 2020, two state elections, two federal elections, two European elections, and two municipal elections were held in Munich. The data are from the Munich Election Office (*Wahlamt*).

Figure 2: Electoral Map of Munich for the 2018 State Election



Notes: The map shows the delineations of the 25 city districts (blue lines) and the 618 voting precincts (black lines) in Munich for the 2018 State Election. Black stars mark the locations of polling places. Gray lines connect the addresses of eligible voters to their assigned polling place. The data are from the official electoral rolls provided by the Munich Election Office (*Wahlamt*).

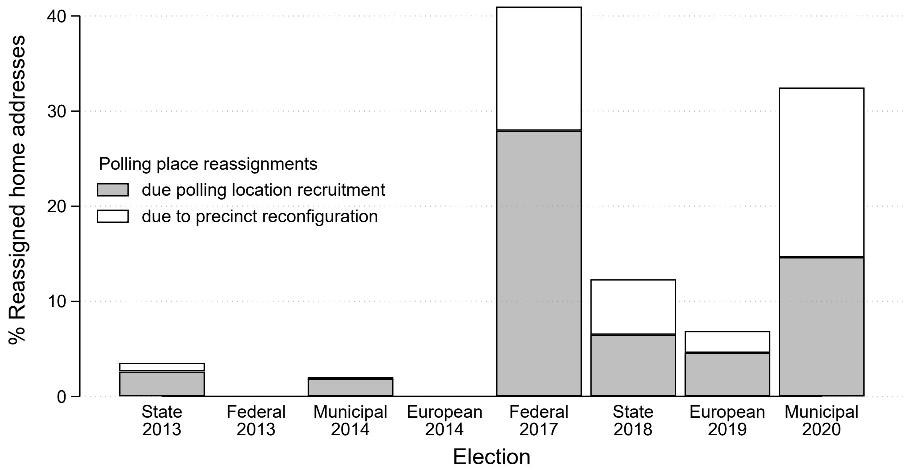
Figure 3: Illustration of Polling Place Reassignments



(a) Polling place recruitment **(b)** Precinct reconfiguration

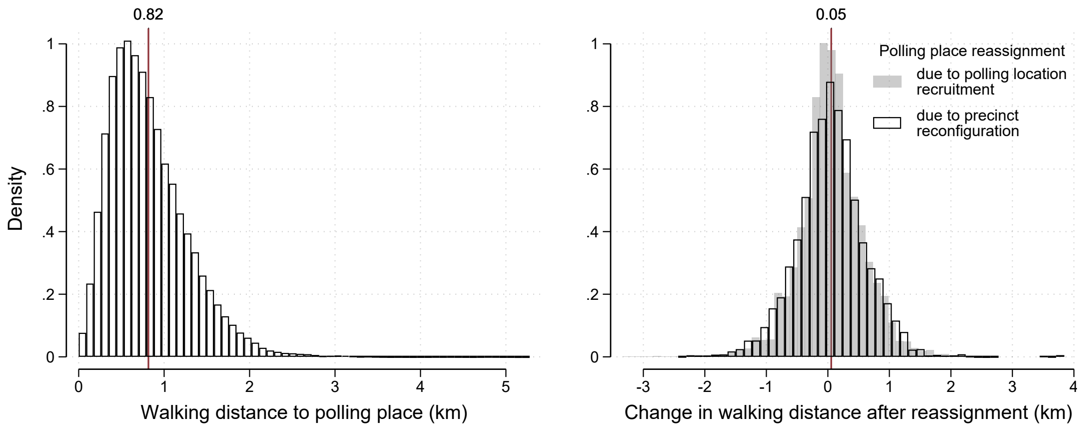
Notes: The figure illustrates two instances of polling place reassignments between the 2014 European Election and the 2017 Federal Election. Dark (light) gray lines connect the residential addresses of eligible voters to their 2017 (2014) polling location. In Panel (a), the precinct was reassigned to a different polling place (black star) as the old polling location became inactive (white circle). Panel (b) illustrates a precinct reconfiguration. Black borders delineate a newly created precinct that was spun off from a larger precinct. Citizens living within the black borders were thus reassigned from the polling place in the north to the location in the northwest of the map. Both locations were active in 2014 and 2017.

Figure 4: Share of Addresses Assigned to Different Polling Place Relative to Previous Election



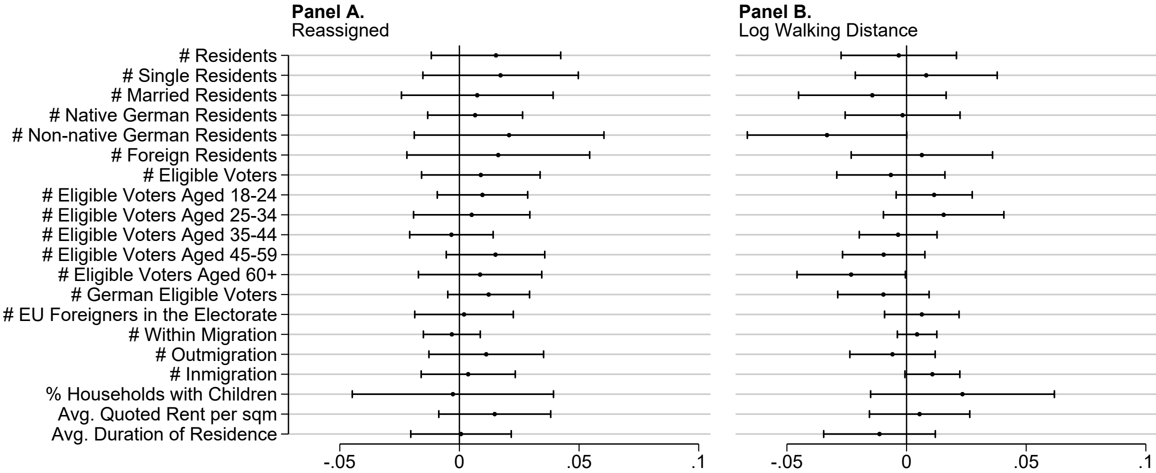
Notes: The figure plots the share of reassigned residential addresses relative to the previous election. The election preceding the 2013 State Election is the 2009 Federal Election (not shown). Reassignment can be due to the reconfiguration of precincts or due to the recruitment of a different polling venue.

Figure 5: Density of Walking Distance and Change in Proximity to the Polling Place



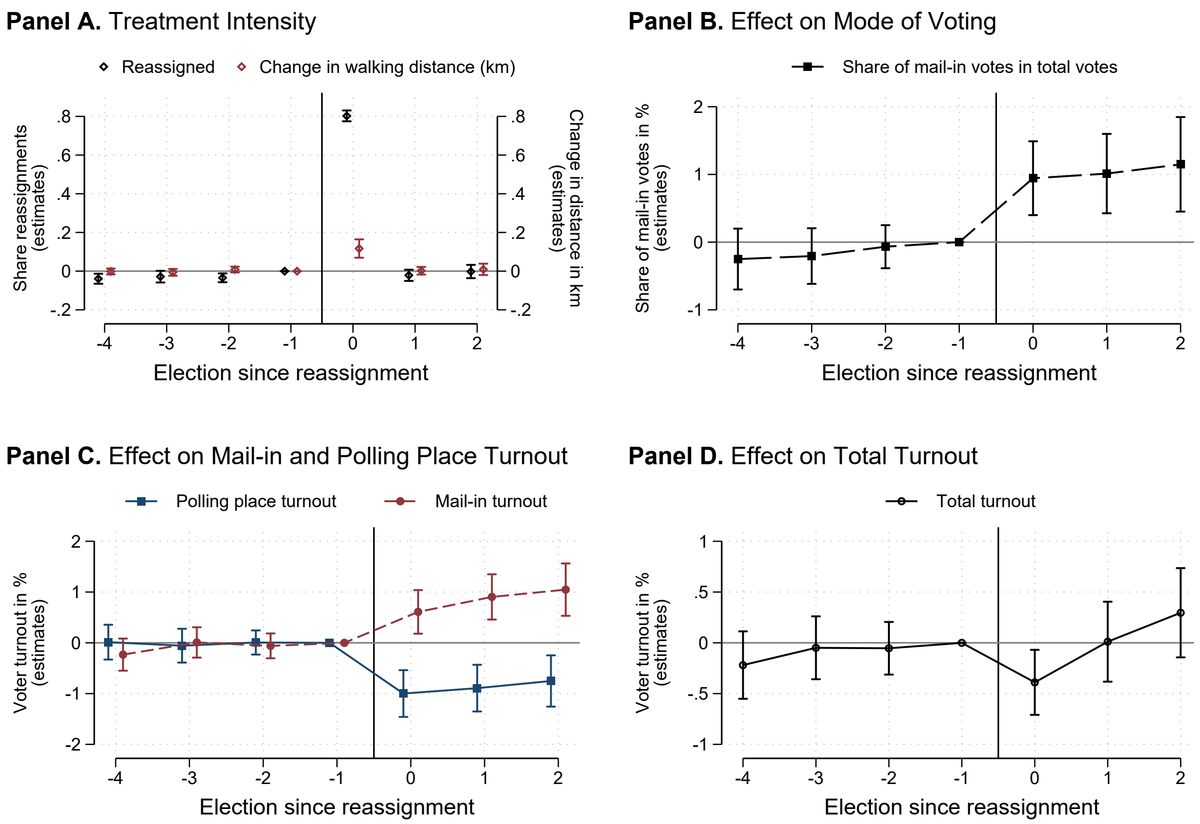
Notes: The figures present density plots of the walking distance between residential addresses of eligible voters and their assigned polling places (left plot, $N = 1,206,232$) and the *change* in distance conditional on assignment to a different polling place relative to the previous election (right plot, $N = 147,874$). The sample covers the eight elections held between 2013 and 2020. Vertical lines highlight the mean of the distribution.

Figure 6: Reassignment Timing and Changes in Precinct Characteristics



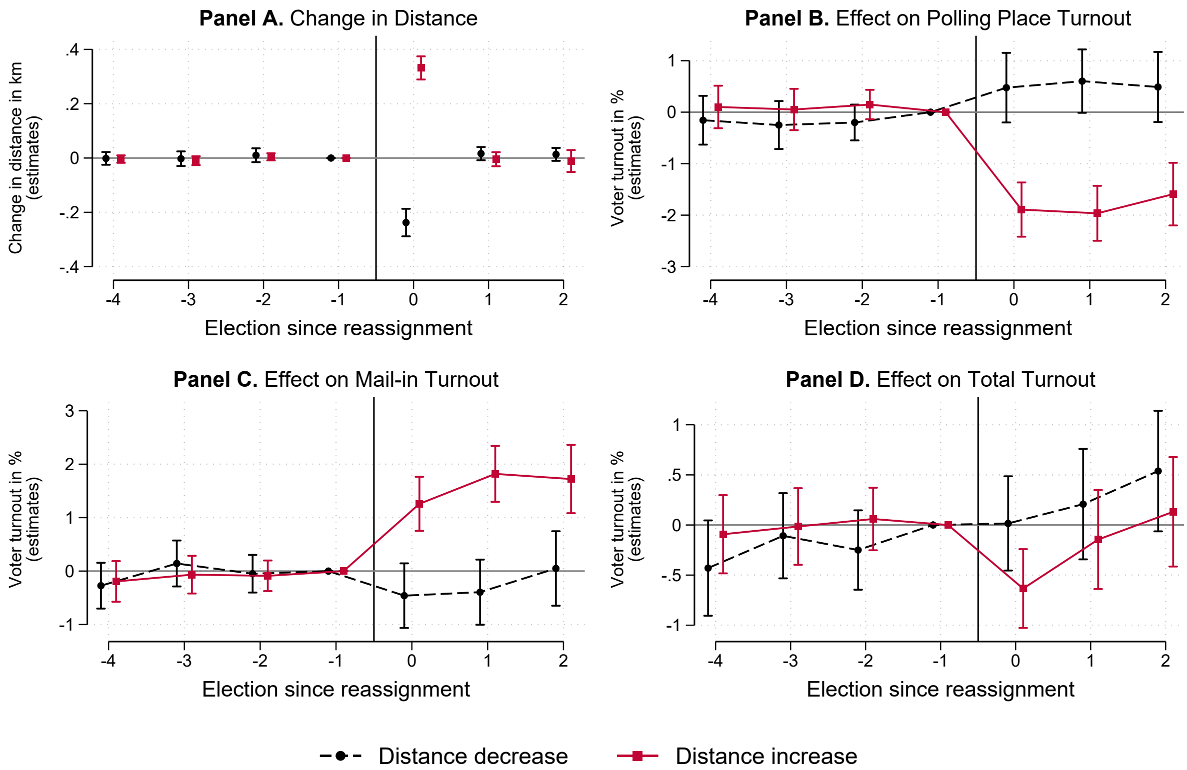
Notes: Panels A and B report OLS estimates from separate univariate regression on standardized (mean zero, unitary standard deviation) precinct characteristics conditional on election and precinct fixed effects. The dependent variables are an indicator identifying full reassignments to a different polling place (Panel A) and the log average walking distance to the polling location (Panel B). Migration variables refer to the number of moves within and across precinct boundaries, respectively. Confidence intervals are drawn at the 95 percent level using standard errors clustered at the precinct level. F-tests cannot reject the null that coefficients are jointly equal to zero in any panel. The coefficients and test statistics are reported in Appendix Table E.3. Estimation results with non-standardized precinct characteristics are shown in Appendix Table E.4. Information on local rents is from the RWI Institute for Economic Research. All other precinct characteristics are from the Munich Statistical Office.

Figure 7: The Effect of Reassignments on Turnout and the Mode of Voting



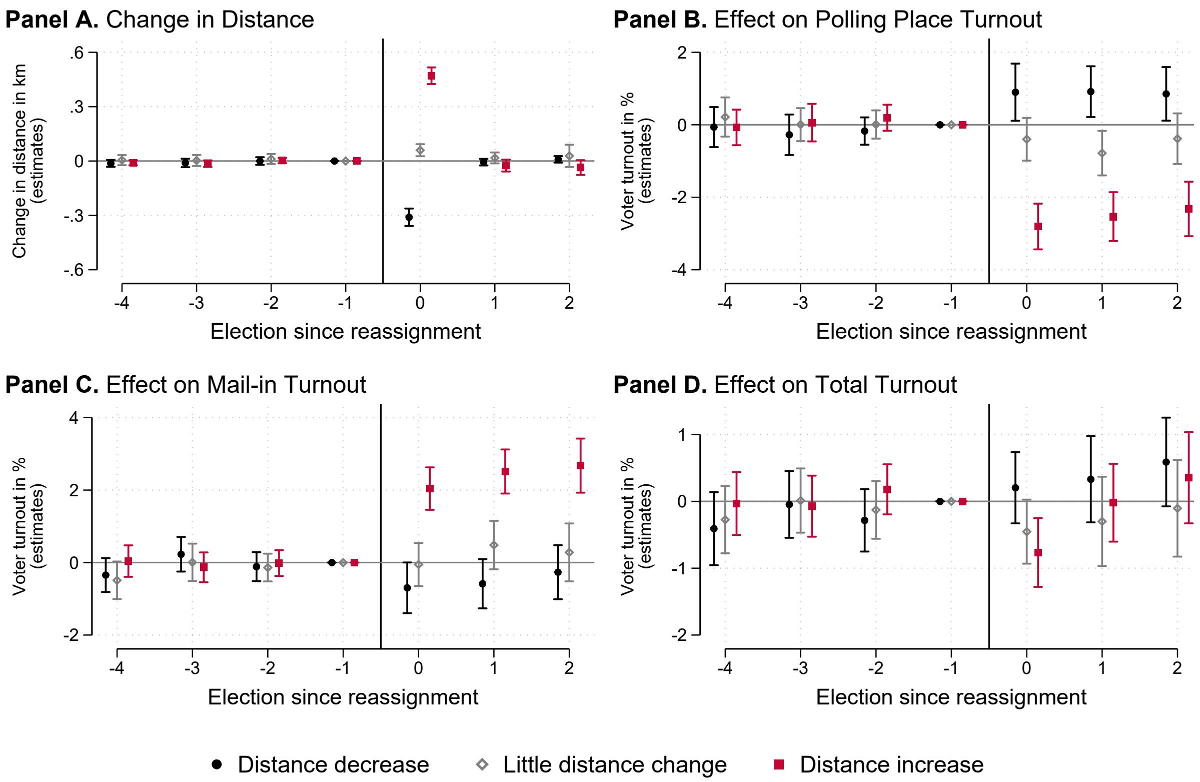
Notes: The figure presents event study results based on Equation 1. The event is defined as the first time in which the entire precinct is reassigned to a different polling place. All specifications include time-varying covariates listed in Section 3.3. Regressions are weighted by the number of eligible voters. Confidence intervals are drawn at the 95 percent level using standard errors clustered at the precinct level. The point estimates and standard errors underlying the results in Panels C and D appear in Column (2) of Appendix Table C.1.

Figure 8: Effect Heterogeneity by Change in Proximity to the Polling Location



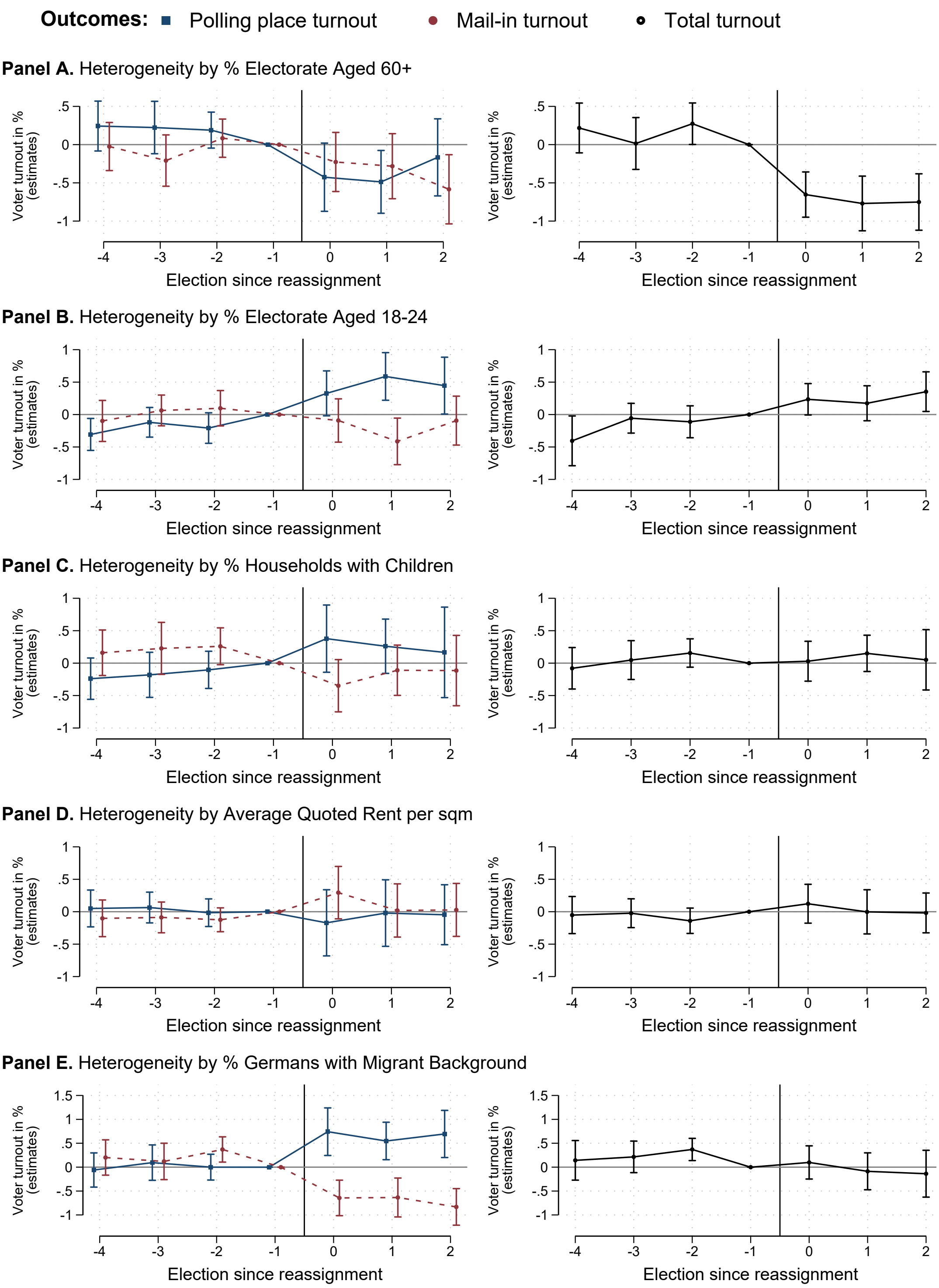
Notes: The figure presents event study results based on Equation 2. Each panel report estimates on interaction terms between event-time indicators and a dummy identifying reassignments that generated an average increase (black coefficients) and decrease (red coefficients) to the polling location, respectively. The event is defined as the first time in which the entire precinct is reassigned to a different polling place. Regressions are weighted by the number of eligible voters. Confidence intervals are drawn at the 95 percent level using standard errors clustered at the precinct level. Point estimates and standard errors are reported in Appendix Table C.3.

Figure 9: Effect Heterogeneity by Change in Proximity to the Polling Location



Notes: The figure presents event study results based on a version of Equation 2 in which event-time dummies are interacted separately with three mutually exclusive indicators for average distance increase, little average distance change, and average distance decrease due to reassignment. The event is defined as the first time in which the entire precinct is reassigned to a different polling place. Regressions are weighted by the number of eligible voters. Confidence intervals are drawn at the 95 percent level using standard errors clustered at the precinct level. Point estimates and standard errors are reported in Appendix Table C.4.

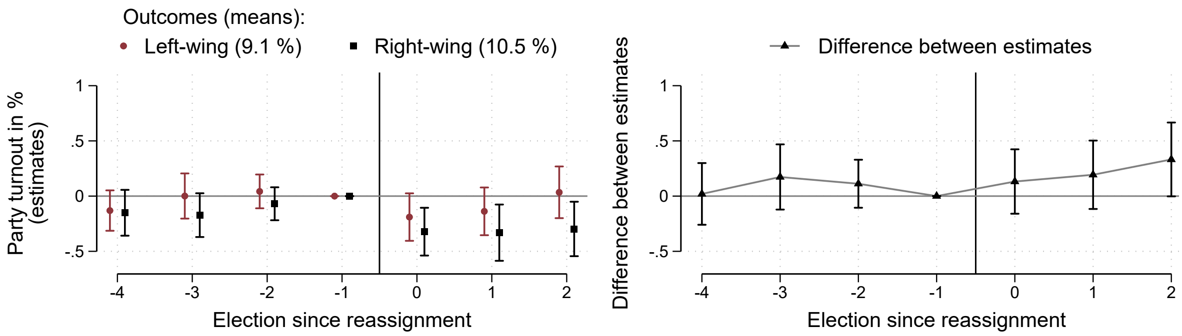
Figure 10: Effect Heterogeneity by Precinct Characteristics (Triple Difference Estimates)



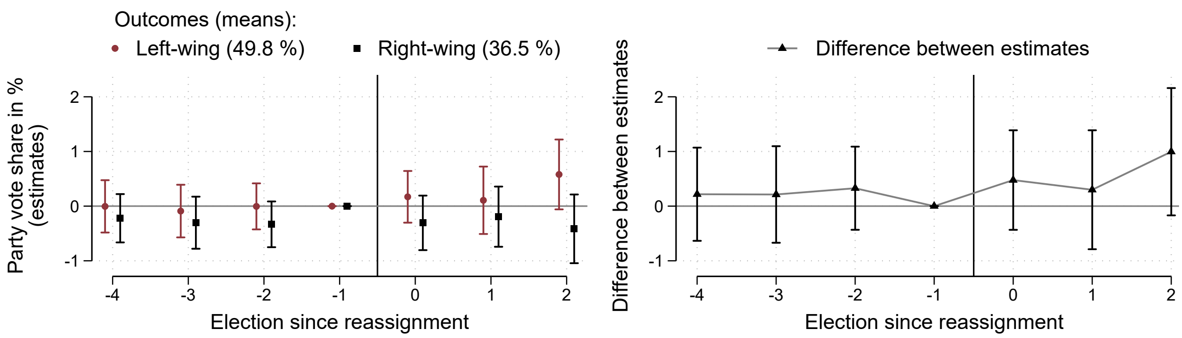
Notes: The figure presents event study results based on the triple difference estimator introduced in Equation 4. Each panel uses a different heterogeneity dimension Z_p and plots the triple-difference coefficients $\hat{\gamma}^k$ for the three outcomes: polling place turnout, mail-in turnout, and total turnout. The event is defined as the first time in which the entire precinct is reassigned to a different polling place. Regressions are weighted by the number of eligible voters. Confidence intervals are drawn at the 95 percent level using standard errors clustered at the precinct level. Point estimates and standard errors are reported in Appendix Table E.6.

Figure 11: Effects of Reassignments on Party Outcomes at the Polling Place

Panel A. Effect on Party Turnout



Panel B. Effect on Party Vote Shares



Notes: The figure presents event study results based on Equation 1. The outcomes are party turnout (Panel A) and party vote shares (Panel B) at the polling place. Party turnout is defined as the number of votes relative to the number of eligible voters for left-wing and right-wing parties, respectively. Party vote share is defined as the number of votes relative to total votes for left-wing and right-wing parties, respectively. Left-wing parties include SPD, *Grüne*, and *Die Linke*; right-wing parties include CSU, *Freie Wähler*, and FDP. The right plot in each panel presents estimates and confidence bands for the difference between event-time indicators in each period. The event is defined as the first time in which the entire precinct is reassigned to a different polling place. Regressions are weighted by the number of eligible voters. Confidence intervals are drawn at the 95 percent level using standard errors clustered at the precinct level.

Table 1: Event Study Estimates Conditional on Log Walking Distance

	Turnout at the Polling Place		Turnout by Mail		Total Turnout	
	(1)	(2)	(3)	(4)	(5)	(6)
Log walking distance	-3.39***	-3.43***	2.61***	2.62***	-0.78***	-0.81***
	(0.25)	(0.26)	(0.25)	(0.26)	(0.22)	(0.24)
Reassignment $(t-4)$	0.01	-0.15	-0.23	-0.08	-0.22	-0.23
	(0.17)	(0.19)	(0.16)	(0.17)	(0.17)	(0.17)
Reassignment $(t-3)$	-0.10	-0.10	0.04	-0.07	-0.06	-0.17
	(0.17)	(0.20)	(0.15)	(0.20)	(0.16)	(0.17)
Reassignment $(t-2)$	0.02	0.16	-0.07	-0.17	-0.05	-0.01
	(0.12)	(0.14)	(0.12)	(0.14)	(0.13)	(0.15)
Reassignment $(t+0)$	-0.58**	-0.66**	0.29	0.23	-0.29	-0.43**
	(0.21)	(0.22)	(0.20)	(0.23)	(0.16)	(0.17)
Reassignment $(t+1)$	-0.63**	-0.64**	0.70***	0.69**	0.07	0.05
	(0.20)	(0.23)	(0.20)	(0.22)	(0.20)	(0.20)
Reassignment $(t+2)$	-0.43	-0.44	0.80***	0.77**	0.37	0.33
	(0.23)	(0.24)	(0.24)	(0.26)	(0.23)	(0.24)
R^2	0.98	0.97	0.96	0.95	0.99	0.99
Fraction of treatment effect explained by distance	0.38	0.34	0.33	0.33	0.25	0.18
Observations	4,666	4,666	4,666	4,666	4,666	4,666
Precinct FE	×	×	×	×	×	×
Election-District FE	×		×		×	
Election FE		×		×		×

Notes: The table presents event study results based on Equation 3. The dependent variables are voter turnout (0–100) at the polling place (Columns 1 and 2), by mail (Columns 2 and 4), and overall (Columns 5 and 6). Odd columns use election×district fixed effects, even columns use election fixed effects. The fraction of the effect explained by distance corresponds to the relative decrease of the point estimates $\hat{\mu}^k$ when controlling for log distance. We report the average over the three post-reassignment elections for in-person and mail-in turnout and in the first-reassignment election for total turnout. The event is defined as the first time in which the entire precinct is reassigned to a different polling place. All specifications include time-varying covariates listed in Section 3.3. Regressions are weighted by the number of eligible voters. Standard errors are clustered at the precinct level and reported in parentheses. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$.

Online Appendix

No Surprises, Please: Voting Costs and Electoral Turnout
by *Jean-Victor Alipour* and *Valentin Lindlacher*

Appendix A. Elections in Munich

Federal Elections. The German *Bundestag* is elected by German citizens aged eighteen and older for a four-year term. Elections are based on a mixed-member proportional representation system, in which half of the members of parliament are elected directly in 299 constituencies (*Wahlkreise*), four of which are located in Munich. Constituencies are composed of adjacent city districts and have constant delineations throughout our observation period. The other half of the parliament is elected via (closed) party lists in the sixteen states. Accordingly, voters cast one vote for their local representative, who is elected by a plurality rule, and a second vote for a party list, which is drawn up by the respective party caucus. Each constituency is represented by one seat in the *Bundestag*. The remaining seats are allocated to achieve proportionality based on the second vote.

Bavarian State Elections. The Bavarian *Landtag* is elected for a five-year term on the basis of mixed-member proportional representation. German citizens aged eighteen and older with residence in Bavaria elect the representatives of their constituencies (*Stimmkreise*) and vote for an (open) party list. In contrast to the federal parliament, the allocation of seats in the state parliament takes into account the parties' aggregate first (constituency) votes as well as their second (party-list) votes. The number of single-member constituencies in Munich increased from eight to nine in 2018 due to stronger population growth in Munich compared to the rest of the state.

Munich City Council Elections. Municipal elections in Munich comprise three distinct elections, which are held on the same day every six years: the election of the local district committees (*Bezirksausschuss)*, charged with representing the interests of citizens living in 25 city districts in Munich, the mayor's race, which is decided based on an absolute majority rule in a direct election, and the election of the city council (*Stadtrat*), which consists of 80 members elected based on (open) party lists and the mayor as the chairperson. In addition to German citizens with residence in Munich, EU foreigners are also eligible to vote in municipal elections. The minimum voting age is eighteen.

European Elections. The European Parliament is elected for a five-year term based on proportional representation. In Germany, each voter casts a single vote for a (closed) list of candidates nominated by a party. All Germans aged eighteen and older are eligible to vote in European elections. It is also possible for non-German EU citizens living in Munich to vote in the city, but they have to lodge a request to be registered on the electoral roll before each election.

Appendix B. Conceptual Framework: Voting Costs, Inattention, and Turnout

To inform the empirical exercise, we present a simple rational choice model of voting drawing on the "calculus of voting" framework (Riker and Ordeshook, 1968). The unit of observation in our causal analysis is the precinct. Thus, our thought experiment considers a precinct that is struck by a polling place reassignment. The counterfactual is a twin precinct without any change. The goal is to convey key intuitions about *i*) the mechanisms through which polling place reassignments alter the costs of voting, *ii*) how the shock to voting costs affects precinct-level turnout at the polling place, via mail, and overall, and *iii*) how turnout effects change when a fraction of the population is inattentive to reassignments.

Setup. Suppose a precinct populated by a unit mass of eligible voters, indexed $i \in \mathcal{I} = [0,1]$, and two periods in which an election is held $t \in \mathcal{T} = \{0,1\}$. In each period, individuals can vote in person at their assigned polling place, vote by mail, or abstain from voting. There are benefits to voting $B \geq 0$, which are assumed to be constant across time and individuals.[34] The benefits and costs of abstaining are zero. Voting by mail generates costs $c_i^m > 0$, which are constant over time. We assume that there are two types of individuals in the population; a fraction $\alpha \in (0,1)$ of type L with low costs of mail-in voting, $c^{mL} \leq B$, and a fraction $(1-\alpha)$ of type H with high costs of mail-in voting, $c^{mH} > B$. Thus, the net utility of voting by mail for type H individuals is negative, and these citizens will never vote by mail. Whether an individual is of type L or H is exogenous and independent of other parameters.

Now, suppose that the entire electorate is reassigned to a different polling place between periods 0 and 1. Voting benefits and the costs of voting by mail are unaffected; however, reassignments change the costs of voting at the polling place, $c_{i,t}^p$, which are a function of travel distance to the polling place, $dist_{i,t} \geq 0$, and a constant $q_t \geq 0$:

$$c_{i,t}^p = \gamma dist_{i,t} + q_t, \tag{B.1}$$

where $\gamma > 0$ is a preference parameter, constant across time and individuals, and q_t is a reassignment disutility from engaging with an unfamiliar environment, arising if and only if the polling location changes. Thus, $q_0 = 0$ in period 0 and $q_1 > 0$ in period 1. To simplify the discussion, we assume the reassignment disutility to be constant across individuals.[35] Without loss of generality, we assume that individuals are ordered on the interval $\mathcal{I} = [0,1]$ such that the travel distance is continuous and strictly increasing in i. Formally, $\sigma : \mathcal{I} \times \mathcal{T} \to \mathbb{R}^+$ and we let $dist_{i,t} = \sigma(i,t) \equiv k_t i$, with $k_0 = 1$. Thus, the ranking is described by a linear function with the slope parameter $k_t > 0$. Reassignments alter the distance proportionally for all individuals via

[34] Voting benefits can reflect the expected utility if the preferred party wins a greater number of seats and any direct utility from the act of voting itself (i.e., expressive motives).

[35] In the model proposed by Brady and McNulty (2011), q_t would capture what the authors label "search costs", i.e., a positive shock to the cost of voting in person, independent distance changes. Brady and McNulty (2011) do not formally distinguish between search costs and distance costs; thus, our model extends their conceptual framework.

a change of the slope k_t. For instance, $k_1 = 1.2$ corresponds to a 20 percent distance increase to the polling location for all constituents in the precinct. This assumption is an evident abstraction from reality, where moving a polling place may change distances in opposite directions for individuals within a precinct. In such a case, predictions for aggregate turnout will be ambiguous as compensating forces drive average voting costs up and down. We will address this issue empirically by focusing a subset of the analysis on cases with minimal within-precinct heterogeneity and impose no such heterogeneity for now.

Turnout in Period 0. Individuals chose the option that confers the highest net utility. Figure B.1a draws the net utilities of voting by mail for types H and L ($U^{mH} \equiv B - c^{mH}$ and $U^{mL} \equiv B - c^{mL}$, respectively) and the net utility of voting in person ($U^p_{i,0} \equiv B - c^p_{i,0}$). Since distance is strictly increasing in i, $U^p_{i,0}$ is downward sloping. Imposing parameter restrictions such that the sets of polling place voters, mail-in voters, and abstainers are nonempty, there exist two thresholds $z^0, u^0 \in [0,1]$ such that $U^p_{i,0} = U^{mL}$ if $i = z^0$ and $U^p_{i,0} = 0$ if $i = u^0$.

Denote $\mathcal{P}^0 \subset \mathcal{I}$ the set of individuals voting in person in period 0. $\mathcal{P}^0$ includes all individuals for whom the net utility of voting in person is greater than zero and exceeds the net utility of voting by mail: $\mathcal{P}^0 = \left\{ i \in [0,1] : U^p_{i,0} \geq U^m_i \text{ and } U^p_{i,0} \geq 0 \right\}$. Thus, turnout at the polling place corresponds to the mass of $\mathcal{P}^0$, which we denote $m(\mathcal{P}^0)$:

$$\text{Polling place turnout: } m(\mathcal{P}^0) = z^0 + (1-\alpha)(u^0 - z^0) \in (0,1) \tag{B.2}$$

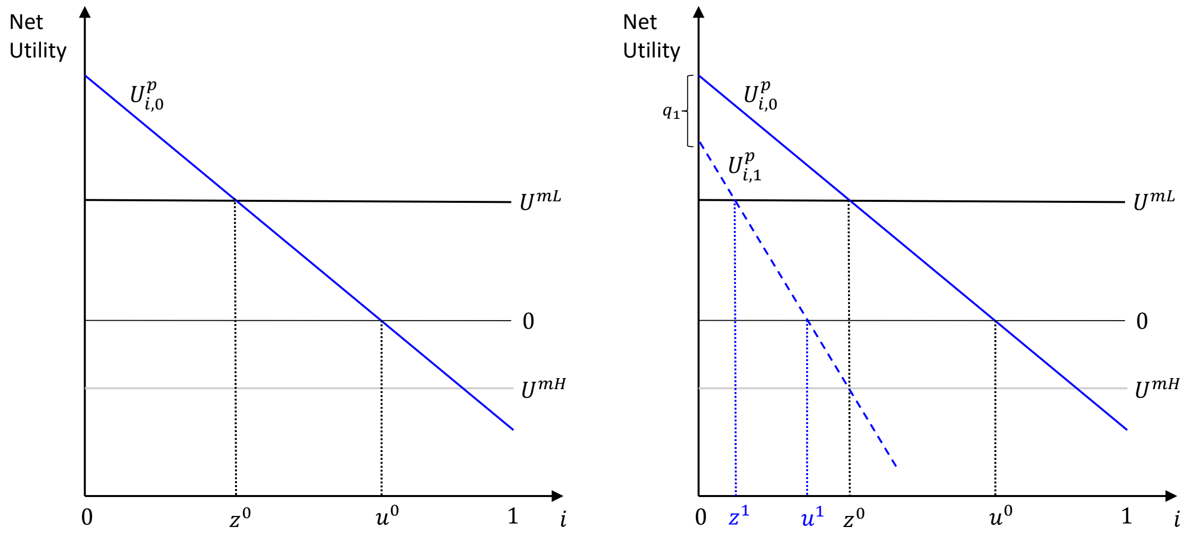
Intuitively, all individuals $i \in [0, z^0]$ with a net utility of voting in person $U^p_{i,0} \geq U^{mL} > 0$, plus a share $(1-\alpha)$ of individuals of type H on the interval $[z^0, u^0]$, who have high costs of voting by mail, turn out at the polling place. Similarly, the set of mail-in voters, $\mathcal{M}^0$, corresponds to individuals with low costs of mail-in voting and a net utility exceeding the utility of voting at the polling place: $\mathcal{M}^0 = \left\{ i \in [0,1] : U^m_i = U^{mL} \text{ and } U^{mL} > U^p_{i,0} \right\}$. Thus, turnout by mail and total turnout are given by:

$$\text{Mail-in turnout: } m(\mathcal{M}^0) = \alpha(1 - z^0) \in (0,1) \tag{B.3}$$

$$\text{Total turnout: } m(\mathcal{T}^0) = m(\mathcal{P}^0) + m(\mathcal{M}^0) = u^0 + \alpha(1-u^0) \in (0,1) \tag{B.4}$$

Change in Turnout in Period 1. Figure B.1b illustrates the impact of a reassignment that *increased* the distance to the polling place. The utility function of in-person voting in period 1, $U^p_{i,1}$, shifted downwards because of the reassignment disutility q_1 and is steeper due to the proportional distance increase. Imposing that reassignments never create empty sets of mail-in voters, in-person voters, or abstainers, we obtain new cutoffs, $z^1, u^1 \in [0,1]$ such that $U^p_{i,1} = U^{mL}$ if $i = z^1$ and $U^p_{i,1} = 0$ if $i = u^1$. These cutoffs determine turnout in period 1 equiva-

Figure B.1: Net Utility of Voting in Period 0 and Period 1



(a) Period 0: No reassignment

(b) Period 1: Reassignment with ↑ distance

Notes: The figure illustrates the utility functions of voting by mail and at the polling place. The net utility of abstaining is zero. Individuals are ranked by distance from their polling location on the interval [0,1]. Panel (a) shows the utility function of polling place voting before the polling place reassignment, $U^p_{i,0}$. Panel (b) draws the utility function of polling place voting after the entire population is reassigned to a different polling location that proportionally increased travel distance, $U^p_{i,1}$.

lently to period 0. Then, we can express turnout in period 1 relative to period 0 as a function of relative change in distance k_1 due to reassignment:

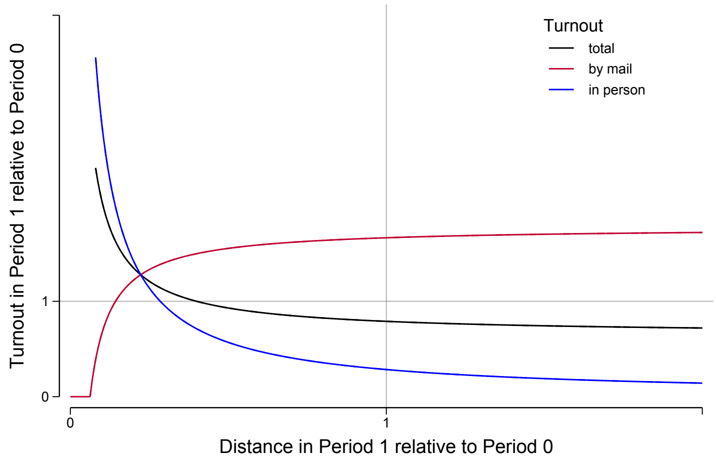
$$\hat{\mathbf{P}}(k_1) \equiv \frac{m(\mathcal{P}^1)}{m(\mathcal{P}^0)} = \frac{z^1 + (1-\alpha)(u^1 - z^1)}{z^0 + (1-\alpha)(u^0 - z^0)} \tag{B.5}$$

$$\hat{\mathbf{M}}(k_1) \equiv \frac{m(\mathcal{M}^1)}{m(\mathcal{M}^0)} = \frac{\alpha(1 - z^1)}{\alpha(1 - z^0)} \tag{B.6}$$

$$\hat{\mathbf{T}}(k_1) \equiv \frac{m(\mathcal{T}^1)}{m(\mathcal{T}^0)} = \frac{u^1 + \alpha(1 - u^1)}{u^0 + \alpha(1 - u^0)}, \tag{B.7}$$

where all cutoffs $z^0, z^1, u^0, u^1 \in [0,1]$ are determined by exogenous parameters. Figure B.2 illustrates how turnout changes in response to a relative change in distance. Right of the vertical unity line, distance increased due to reassignment. The greater the increase, the lower polling place turnout in period 1 relative to period 0 as more individuals are discouraged from turning out in person. Larger increases in distance cause more people to switch to mail-in voting, increasing turnout by mail relative to period 0 (red line). At the intersection with the vertical unity line, i.e., when distance is held constant, polling place turnout is lower and mail-in turnout greater than in period 1 due to the reassignment disutility q_1. For a reassignment to increase in-person turnout, distance must decline enough to compensate for the reassignment disutility. Similarly, total turnout falls in period 1 unless the reassignment reduces the distance to the polling location sufficiently to incentivize abstainers to start voting at the polling place.

Figure B.2: Turnout Effects of Polling Place Reassignments



Notes: The figure illustrates turnout at the polling place (blue line), via mail (red), and overall (black) in period 1 relative to period 0 as a function of the relative change in distance to the polling location after a reassignment.

Inattention to Reassignments. To notice a reassignment, citizens need to review the address of the polling place stated in the election notification, which is mailed a few weeks before election day. Unlike in the US, citizens in Munich are not separately informed of changes to precinct boundaries or their previous polling location. Thus, *inattentive* voters may be surprised by a reassignment or not notice at all that their polling place has moved. Individuals are rational, conditional on inattention; the attention choice itself may or may not be optimal (Maćkowiak et al., 2023). Conceptually, we introduce inattention as follows:

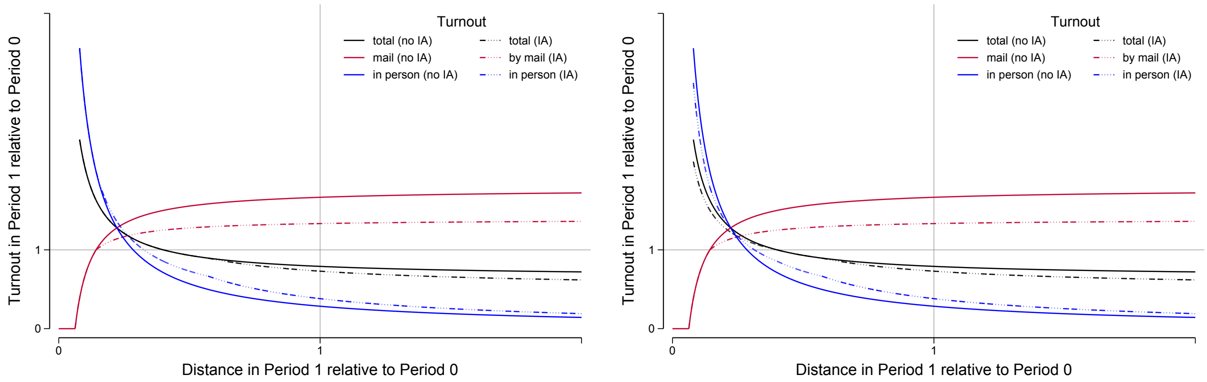
i) a fraction $\theta \in [0,1)$ of polling place voters, $i \in \mathcal{P}^0$, are surprised by reassignments *after* the deadline for requesting a mail-in ballot has passed. Citizens who choose to vote in person need to present the election notification to poll workers at the polling place. Thus, inattentive individuals may open the notification only shortly before going to vote and only notice then that it has been moved. In period 1, these citizens can only choose to vote at the *new* polling location or switch to abstention.

ii) a fraction $\pi \in [0,1)$ of abstainers, $i \in \mathcal{A}^0$, do not notice the reassignment at all and remain abstainers in period 1.

iii) mail-in voters, $i \in \mathcal{M}^0$, are never inattentive. Since mail-in ballots must be requested by opening the election notification and returning a form, we assume that mail-in voters always notice a reassignment.

Figure B.3 illustrates how turnout changes after a reassignment when there is no inattention (solid lines) and when a fraction of the electorate is inattentive to reassignments (dashed lines). In Figure B.3a only a fraction of in-person voters is inattentive, $\theta \in (0,1)$ and $\pi = 0$. In this case, inattention changes the turnout effects when a reassignment makes in-person voting unattractive to polling place voters (by not sufficiently reducing or increasing travel distance). Inattentive voters who would otherwise have switched to mail-in voting are left with choosing

Figure B.3: Turnout Effects of Reassignments with Inattentive Voters



(a) Fraction of in-person voters is inattentive, $\theta \in (0,1), \pi = 0$

(b) Fractions of in-person voters & abstainers are inattentive, $\pi, \theta \in (0,1)$

Notes: The figure illustrates the turnout at the polling place (blue line), via mail (red), and overall (black) in period 1 relative to period 0 as a function of the relative change in distance to the polling location after a reassignment. Dashed lines draw the relationship between turnout change and distance change when a fraction of the electorate is *inattentive* to reassignments. In Panel (a), only a fraction of in-person voters, $i \in \mathcal{P}^0$ is inattentive. In Panel (b), an additional fraction of abstainers, $i \in \mathcal{A}^0$, is inattentive.

between turning out at the new polling location or switching to abstention. Thus, inattention *attenuates* the shift from in-person toward mail-in voting and *amplifies* the shift toward abstention. The decline in total turnout relative to a situation without inattention becomes stronger with increasing distance.

Figure B.3b illustrates a scenario in which fractions of in-person voters and abstainers are inattentive, $\pi, \theta \in (0,1)$. This alters turnout effects relative to a situation without inattention only in cases in which reassignments *reduce* distance enough to make in-person voting attractive for previous abstainers. When a fraction of abstainers is inattentive, increases in polling place turnout and overall participation are attenuated.

To summarize, the model delivers the following key predictions:

- **Asymmetric effects by distance:** an *increase* in travel distance always makes voting at the polling place less attractive, prompting a shift away from in-person voting toward mail-in voting and abstention. By contrast, a *decrease* in travel distance makes polling place voting only more attractive if the reduction is enough to compensate for the reassignment disutility.

- **Attenuated turnout gains under inattention:** Inattention weakens the increase in total turnout when distance declines. The effect comes from inattentive abstainers who remain abstainers even when the new polling place is conveniently located nearby.

- **Amplified turnout losses under inattention:** Inattention amplifies the shift from in-person voting to abstention when in-person voting becomes unattractive (due to an increase in travel distance and/or the reassignment disutility). The effect comes from inattentive voters who would have switched to mail-in voting but missed the deadline for requesting a mail-in ballot.

Appendix C. Robustness and Additional Results

Appendix C.1. Baseline Results and Sensitivity

Pooled Reassignments. Table C.1 reports results based on Equation 1 and some variants to test the sensitivity of the estimates. Column (1) reports *unweighted* estimates. Column (2) presents the results of our preferred specification (plotted in Figure 7), which is weighted by the number of eligible voters. In Column (3), we estimate the event study using the full sample instead of trimming the time series once a second reassignment occurs. Results remain very similar to the estimates in Column (2). Column (4) replaces election×district fixed effects with election indicators. Again, the results show little sensitivity to the alternative specification; importantly, pre-event coefficients remain statistically insignificant, bolstering the parallel trend assumption. In Column (5), we re-estimate our preferred specification on a "clean" sample, which limits the treatment group to precincts with exactly one (full) reassignment and the control group to precincts with zero (partial) reassignments. Despite the reduction in sample size by more than half, the treatment effects hold and are somewhat larger due to the elimination of partial reassignments in the control group. In Column (6), we use a balanced sample of precincts that we observe for at least four pre-reassignment and three post-reassignment elections. In addition, the treatment group excludes precincts with more than one reassignment. In this sample, 83 percent of treated precincts had their polling place moved in the 2017 Federal Election; 17 percent in the 2018 State Election. The estimates are largely stable; only the (temporary) drop in total turnout turns insignificant. This could also result from the size reduction of the treatment group (by a factor of nearly three), causing some loss in precision. Indeed, we show that the drop and subsequent recovery in total turnout remain sizable and significant on a balanced panel when considering reassignments that *increase* the average distance to the polling location below (see Table C.3).[36] In Column (7), we exclude treated precincts where the reassignment coincided with precinct boundary changes. The results remain largely unchanged despite shrinking the treatment group, suggesting that the results are not merely an artifact of using aggregate turnout measures across changing precinct boundaries.

Staggered Treatment Adoption. In Figure C.1, we re-estimate the model of Column (4), Table C.1, where election fixed effects replace election×district fixed effects, using several novel estimators that account for effect heterogeneity in the context of staggered treatment timing (Borusyak et al., 2023; Callaway and Sant'Anna, 2021; Sun and Abraham, 2021; de Chaisemartin and D'Haultfœuille, 2020). The estimates are similar to the TWFE-OLS estimates, suggesting that treatment effect heterogeneity across treatment cohorts does no compromise our estimates of interest.

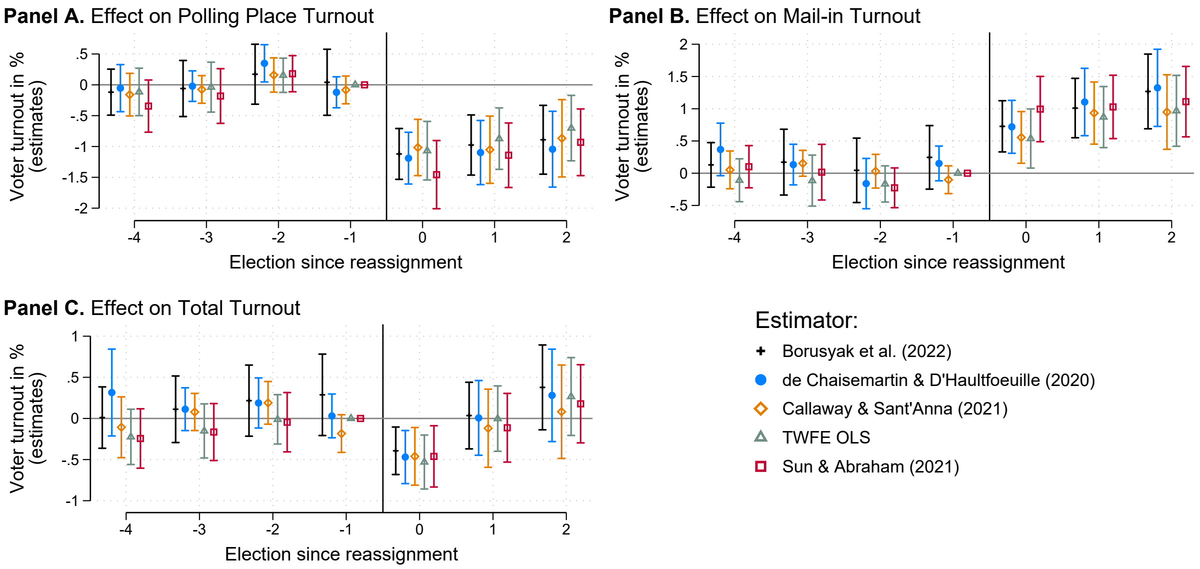
[36] In Appendix Table E.8 (for pooled reassignments) and Table E.9 (for effects by distance increase/decrease), we extended the analysis to explore the sensitivity of the balanced results to five different event windows: balancing on (i) $t \in [-4, 0]$, (ii) $t \in [-2, +1]$, (iii) $t \in [-4, +1]$, (iv) $t \in [-2, +2]$, and (v) $t \in [-4, +2]$. The balanced sample estimates are very close to the unbalanced estimates.

Table C.1: Baseline Event Study Results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Effect on Turnout at the Polling Place [Mean outcome=33.7]							
Reassignment ($t-4$)	0.03	0.01	0.00	-0.12	0.02	0.04	0.50
	(0.17)	(0.17)	(0.18)	(0.20)	(0.24)	(0.25)	(0.50)
Reassignment ($t-3$)	-0.07	-0.06	-0.06	-0.04	-0.20	0.02	0.63
	(0.17)	(0.17)	(0.17)	(0.21)	(0.25)	(0.26)	(0.52)
Reassignment ($t-2$)	0.01	0.01	0.01	0.15	0.00	0.19	0.48
	(0.12)	(0.12)	(0.12)	(0.14)	(0.20)	(0.21)	(0.42)
Reassignment ($t+0$)	-1.02***	-1.00***	-1.02***	-1.07***	-1.31***	-1.57***	-1.37**
	(0.23)	(0.23)	(0.23)	(0.24)	(0.33)	(0.37)	(0.42)
Reassignment ($t+1$)	-0.88***	-0.89***	-0.81***	-0.87***	-1.49***	-1.41***	-1.08*
	(0.24)	(0.23)	(0.21)	(0.25)	(0.31)	(0.34)	(0.49)
Reassignment ($t+2$)	-0.76**	-0.75**	-0.53*	-0.70**	-1.14***	-0.80*	-0.89
	(0.26)	(0.26)	(0.22)	(0.27)	(0.33)	(0.34)	(0.54)
Panel B: Effect on Turnout via Mail [Mean outcome=28.7]							
Reassignment ($t-4$)	-0.24	-0.23	-0.21	-0.11	-0.31	-0.18	-0.41
	(0.16)	(0.16)	(0.16)	(0.17)	(0.25)	(0.19)	(0.45)
Reassignment ($t-3$)	0.02	0.01	0.02	-0.11	0.08	-0.04	-0.76
	(0.15)	(0.15)	(0.15)	(0.20)	(0.22)	(0.24)	(0.55)
Reassignment ($t-2$)	-0.07	-0.06	-0.05	-0.17	-0.15	-0.23	-0.26
	(0.12)	(0.12)	(0.13)	(0.14)	(0.18)	(0.18)	(0.43)
Reassignment ($t+0$)	0.66**	0.61**	0.62**	0.54*	0.66*	1.32***	0.66
	(0.22)	(0.22)	(0.22)	(0.23)	(0.29)	(0.31)	(0.39)
Reassignment ($t+1$)	0.93***	0.90***	0.73***	0.87***	1.20***	1.24***	1.35***
	(0.23)	(0.23)	(0.21)	(0.24)	(0.30)	(0.32)	(0.39)
Reassignment ($t+2$)	1.07***	1.05***	0.71**	0.97***	1.27***	1.28***	1.20*
	(0.26)	(0.26)	(0.23)	(0.28)	(0.35)	(0.35)	(0.53)
Panel C: Effect on Total Turnout [Mean outcome=62.4]							
Reassignment ($t-4$)	-0.21	-0.22	-0.21	-0.22	-0.29	-0.15	0.09
	(0.17)	(0.17)	(0.17)	(0.17)	(0.25)	(0.25)	(0.50)
Reassignment ($t-3$)	-0.05	-0.05	-0.04	-0.15	-0.12	-0.03	-0.13
	(0.16)	(0.16)	(0.16)	(0.17)	(0.23)	(0.27)	(0.48)
Reassignment ($t-2$)	-0.06	-0.05	-0.04	-0.01	-0.14	-0.04	0.23
	(0.13)	(0.13)	(0.13)	(0.15)	(0.21)	(0.24)	(0.35)
Reassignment ($t+0$)	-0.36*	-0.39*	-0.40*	-0.53**	-0.64*	-0.25	-0.70*
	(0.17)	(0.16)	(0.16)	(0.17)	(0.26)	(0.28)	(0.30)
Reassignment ($t+1$)	0.04	0.01	-0.08	-0.00	-0.28	-0.16	0.27
	(0.20)	(0.20)	(0.19)	(0.20)	(0.29)	(0.31)	(0.39)
Reassignment ($t+2$)	0.31	0.30	0.18	0.27	0.13	0.47	0.31
	(0.23)	(0.22)	(0.20)	(0.24)	(0.30)	(0.29)	(0.44)
Observations	4,666	4,666	4,944	4,666	2,040	3,456	3,082
# Treated precincts	280	280	280	280	150	94	54
# Untreated precincts	338	338	338	338	105	338	338
Precinct FE	×	×	×	×	×	×	×
Election-District FE	×	×	×		×	×	×
Weights		×	×	×	×	×	×
Full sample			×				
Election FE				×			
Clean sample					×		
Balanced sample						×	
No boundary change							×

Notes: The table presents event study results based on Equation 1. The dependent variables are voter turnout (0–100) at the polling place (Panel A), by mail (Panel B), and overall (Panel C). Columns (2)–(8) are weighted by the number of eligible voters. Column (3) uses a "full sample", i.e., observations are not dropped after a second reassignment. Column (5) uses a "clean sample" that limits the treatment group to precincts with exactly one (full) reassignment and the control group to precincts with zero (partial) reassignments. Column (6) uses a balanced sample of precincts observed for at least four pre-reassignment and two post-reassignment elections. In Column (7), the sample excludes treated precincts where the reassignment coincided with a change to precinct boundaries. The event is defined as the first time in which the entire precinct is reassigned to a different polling place. All specifications include time-varying covariates listed in Section 3.3. Standard errors are clustered at the precinct level and reported in parentheses. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$.

Figure C.1: Sensitivity to Different Estimators



Notes: The figure presents event study results based on the specification presented in Column (4) of Table C.1 (i.e., Equation 1 using election fixed effects instead of election×district fixed effect). The model is estimated using TWFE-OLS as well as the estimators proposed by Borusyak et al. (2023), Callaway and Sant'Anna (2021), Sun and Abraham (2021), and de Chaisemartin and D'Haultfœuille (2020). The event is defined as the first time in which the entire precinct is reassigned to a different polling place. Where applicable, specifications include time-varying covariates listed in Section 3.3. Regressions are weighted by the number of eligible voters. Confidence intervals are drawn at the 95 percent level using standard errors clustered at the precinct level.

Clustering. We also show that the results are robust to alternative assumptions about the variance-covariance matrix in Table C.2. One concern is that model errors are correlated within districts. This may be the case because precinct reconfigurations of adjacent precincts are not performed across but only within districts. Moreover, it is not uncommon that polling places of several precincts (within a district) are located in the same building. In these cases, closing a venue affects several adjacent precincts simultaneously. We reproduce our preferred specification (from Column 2 of Table C.1) with standard errors clustered at the precinct level for comparison in Column (1). Column (2) shows that standard errors are only marginally larger when correcting for two-way clusters at the level of precincts (to account for error correlation over time) and at the level of districts in each election (to account for within-district-election correlation). Next, we test robustness to using wild bootstrapped clustered standard errors, as recommended by MacKinnon et al. (2023): Column (3) clusters are at the precinct level, and Column (4) clusters are at the district level. In both columns, treatment effects remain statistically different from zero. Column (5) implements wild bootstrap clustering at the district level and replaces election×district fixed effects with election fixed effects. The estimate on mail-in turnout in the first post-reassignment election turns marginally insignificant. All other treatment effects hold.

Table C.2: Robustness to Clustering at Different Levels

	(1) Cluster Precinct (Baseline)	(2) TW Cluster Precinct+ Election-District	(3) Wild Cluster Bootstrap Precinct	(4) Wild Cluster Bootstrap District	(5) Wild Cluster Bootstrap District
Panel A: Effect on Turnout at the Polling Place					
Reassignment ($t-4$)	0.01	0.01	0.01	0.01	-0.12
	(0.17)	(0.19)	[0.946]	[0.946]	[0.530]
Reassignment ($t-3$)	-0.06	-0.06	-0.06	-0.06	-0.04
	(0.17)	(0.19)	[0.749]	[0.769]	[0.850]
Reassignment ($t-2$)	0.01	0.01	0.01	0.01	0.15
	(0.12)	(0.14)	[0.958]	[0.961]	[0.348]
Reassignment ($t+0$)	-1.00***	-1.00***	-1.00	-1.00	-1.07
	(0.23)	(0.26)	[0.000]	[0.000]	[0.001]
Reassignment ($t+1$)	-0.89***	-0.89***	-0.89	-0.89	-0.87
	(0.23)	(0.26)	[0.000]	[0.002]	[0.029]
Reassignment ($t+2$)	-0.75**	-0.75**	-0.75	-0.75	-0.70
	(0.26)	(0.27)	[0.001]	[0.031]	[0.052]
Panel B: Effect on Turnout via Mail					
Reassignment ($t-4$)	-0.23	-0.23	-0.23	-0.23	-0.11
	(0.16)	(0.16)	[0.146]	[0.233]	[0.486]
Reassignment ($t-3$)	0.01	0.01	0.01	0.01	-0.11
	(0.15)	(0.16)	[0.963]	[0.963]	[0.603]
Reassignment ($t-2$)	-0.06	-0.06	-0.06	-0.06	-0.17
	(0.12)	(0.14)	[0.636]	[0.637]	[0.400]
Reassignment ($t+0$)	0.61**	0.61**	0.61	0.61	0.54
	(0.22)	(0.23)	[0.012]	[0.016]	[0.063]
Reassignment ($t+1$)	0.90***	0.90***	0.90	0.90	0.87
	(0.23)	(0.25)	[0.001]	[0.002]	[0.016]
Reassignment ($t+2$)	1.05***	1.05***	1.05	1.05	0.97
	(0.26)	(0.27)	[0.000]	[0.000]	[0.012]
Panel C: Effect on Total Turnout					
Reassignment ($t-4$)	-0.22	-0.22	-0.22	-0.22	-0.22
	(0.17)	(0.17)	[0.191]	[0.221]	[0.189]
Reassignment ($t-3$)	-0.05	-0.05	-0.05	-0.05	-0.15
	(0.16)	(0.16)	[0.743]	[0.752]	[0.375]
Reassignment ($t-2$)	-0.05	-0.05	-0.05	-0.05	-0.01
	(0.13)	(0.14)	[0.667]	[0.732]	[0.957]
Reassignment ($t+0$)	-0.39*	-0.39*	-0.39	-0.39	-0.53
	(0.16)	(0.19)	[0.025]	[0.027]	[0.003]
Reassignment ($t+1$)	0.01	0.01	0.01	0.01	-0.00
	(0.20)	(0.21)	[0.950]	[0.958]	[0.994]
Reassignment ($t+2$)	0.30	0.30	0.30	0.30	0.27
	(0.22)	(0.21)	[0.187]	[0.097]	[0.408]
Observations	4,666	4,666	4,666	4,666	4,666
Number of Clusters	618	200+618	618	25	25
Precinct FE	×	×	×	×	×
Election-District FE	×	×	×	×	
Election FE					×

Notes: The table presents robustness checks to the level of clustering standard errors based on the event study specification in Equation 1. The event is defined as the first time in which the entire precinct is reassigned to a different polling place. Column (1) replicates the baseline results with standard errors (SE) clustered at the precinct level for comparison. Column (2) uses two-way clustered SE at the level of precincts and district-elections (reported in parentheses). Column (3) uses wild cluster bootstrap (WCB) at the precinct level. Column (4) uses WCB at the district level. Column (5) uses WCB at the district level and replaces election×district fixed effects with election fixed effects. p-values from wild bootstrap clustering are reported in square brackets. We use Rademacher weights and 1000 replications. Regressions are weighted by the number of eligible voters. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$.

Effect Heterogeneity by Reassignment Reason. Next, we investigate whether the reason for reassignment matters for treatment effect. Precinct reconfigurations are less likely to lead to *entire* precincts being reassigned (see Figure D.5). To ensure enough power, we define the event as the first time that 50 percent or more of residential addresses of a precinct are reassigned. Formally, let V_p be an indicator equal to 1 for precincts where reassignment occurred because of recruitment of a new polling venue and let B_p denote an analogous indicator for cases in which reassignments are due to reconfiguration of precincts. Then, the modified event study specification takes the following form:

$$Y_{pt} = V_p \times \sum_{k \neq -1} \beta^k \mathbb{1}(\tau = k) + B_p \times \sum_{k \neq -1} \alpha^k \mathbb{1}(\tau = k) + \mathbf{X}'_{pt}\phi + \delta_p + \delta_{d(p)t} + \varepsilon_{pt}, \tag{C.1}$$

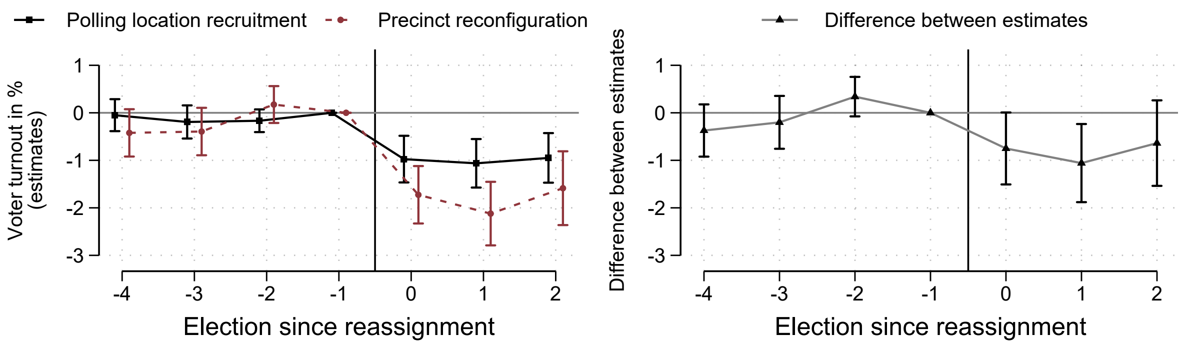
where the coefficients $\hat{\beta}^k$ and $\hat{\alpha}^k$ trace the differential time path of turnout separately for the two groups defined by V_p and B_p. As in our main specification, we include election×district fixed effects, precinct fixed effects, and time-varying controls.

The results are presented in Figure C.2. The outcome in Panel A is turnout at the polling place; Panels B and C show the results for mail-in and total turnout, respectively. The left plot in each panel reports estimated coefficients $\hat{\alpha}^k$ and $\hat{\beta}^k$ for $k \in \{-4, ..., 2\}$; the right plot reports estimates and 95 percent confidence bands of the *difference* between the pair of estimates in each period.

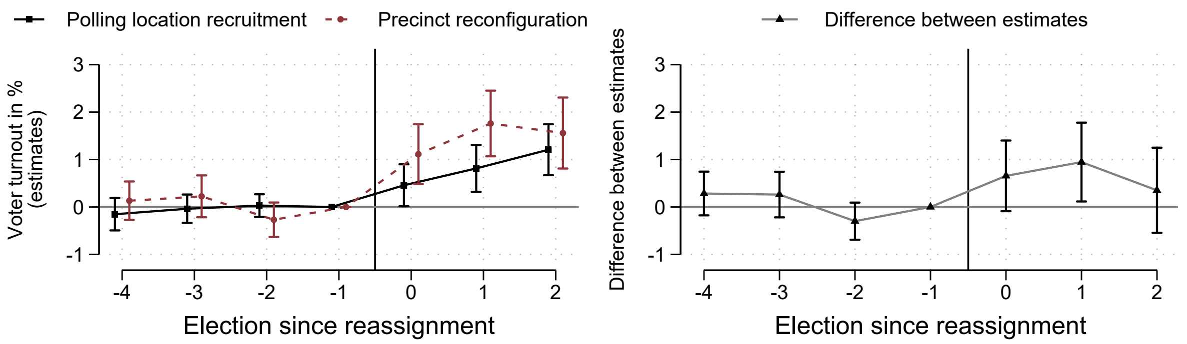
Reassuringly, pre-event estimates for both reassignment types are insignificant for all outcomes. Post-reassignment estimates follow a very similar trajectory. Treatment effects after a precinct reconfiguration seem slightly more pronounced; yet out of nine pairs of point estimates, only two are statistically different from each other. Thus, the results generally indicate that different reassignment reasons do not lead to markedly different outcomes.

Figure C.2: Effect Heterogeneity by Reassignment Reason

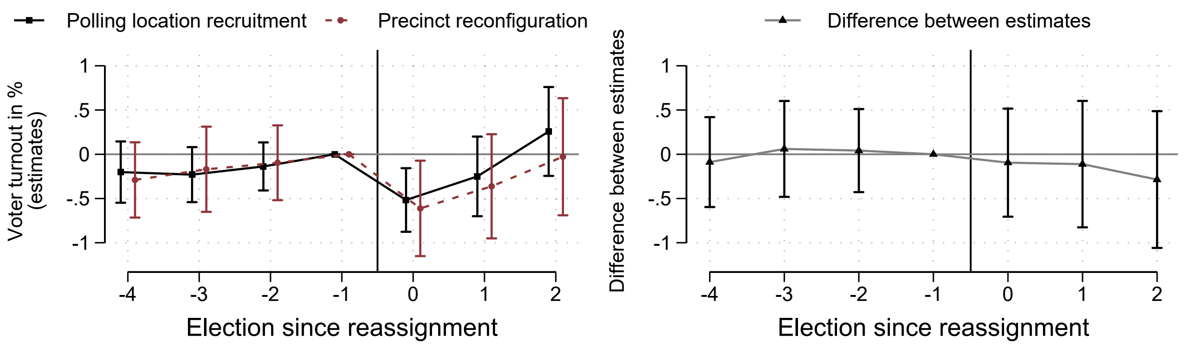
Panel A. Effect on Polling Place Turnout



Panel B. Effect on Mail-in Turnout



Panel C. Effect on Total Turnout



Notes: The figure presents event study results based on Equation C.1. The left plot in each panel report estimates on interaction terms between event-time indicators and a dummy identifying reassignments due to recruitment of a new polling place and precinct reconfiguration, respectively. The right plot in each panel presents estimates and confidence bands for the difference between estimates in each period. The event is defined as the first time in which more than 50 percent of residential addresses in a precinct is reassigned to a different polling place. Regressions are weighted by the number of eligible voters. Confidence intervals are drawn at the 95 percent level using standard errors clustered at the precinct level.

Results by Change in Proximity to the Polling Place. Table C.3 reports OLS results based on Equation 2. Columns (1), (4), and (7) report the estimates underlying the event study plots in Figure 8. Columns (2), (5), and (8) use a "clean" sample, which limits the treatment group to precincts with exactly one (full) reassignment and the control group to precincts with zero (partial) reassignments. Columns (3), (6), and (9) use a balanced sample of precincts that we observe for at least four pre-reassignment and two post-reassignment elections. In addition, the treatment group excludes precincts with more than one reassignment. Reassuringly, the results remain largely consistent across the different samples. Table C.4 reports an analogous table with event study estimates across three bins of distance changes (increase, little change, decrease). Columns (1), (4), and (7) report the estimates underlying the event study plots in Figure 9. Again, we find no concerning sensitivity of the results to using different sample restrictions.

Table C.3: Effect Heterogeneity by Change in Proximity to the Polling Place

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Polling Place Turnout			Mail-in Turnout			Total Turnout		
$\mathbb{1}$(Distance decrease)×									
Reassignment $(t-4)$	-0.16	-0.17	0.02	-0.27	-0.29	-0.12	-0.43	-0.46	-0.09
	(0.24)	(0.33)	(0.32)	(0.22)	(0.33)	(0.27)	(0.24)	(0.35)	(0.32)
Reassignment $(t-3)$	-0.25	-0.19	0.19	0.14	0.17	-0.09	-0.11	-0.02	0.09
	(0.24)	(0.32)	(0.36)	(0.22)	(0.28)	(0.32)	(0.22)	(0.29)	(0.36)
Reassignment $(t-2)$	-0.20	-0.17	0.09	-0.05	-0.07	-0.20	-0.25	-0.24	-0.11
	(0.18)	(0.26)	(0.28)	(0.18)	(0.26)	(0.27)	(0.20)	(0.31)	(0.35)
Reassignment $(t+0)$	0.48	0.13	0.41	-0.46	0.12	0.23	0.02	0.24	0.65
	(0.34)	(0.40)	(0.46)	(0.31)	(0.39)	(0.43)	(0.24)	(0.33)	(0.34)
Reassignment $(t+1)$	0.60	-0.07	0.41	-0.39	0.35	0.15	0.21	0.28	0.56
	(0.31)	(0.39)	(0.43)	(0.31)	(0.38)	(0.42)	(0.28)	(0.38)	(0.41)
Reassignment $(t+2)$	0.49	0.20	0.75	0.05	0.52	0.41	0.54	0.72	1.17**
	(0.35)	(0.40)	(0.42)	(0.36)	(0.45)	(0.48)	(0.31)	(0.37)	(0.35)
$\mathbb{1}$(Distance increase)×									
Reassignment $(t-4)$	0.10	0.13	0.05	-0.19	-0.31	-0.23	-0.09	-0.18	-0.18
	(0.21)	(0.30)	(0.31)	(0.19)	(0.30)	(0.23)	(0.20)	(0.30)	(0.31)
Reassignment $(t-3)$	0.05	-0.21	-0.09	-0.07	0.03	-0.01	-0.01	-0.19	-0.11
	(0.20)	(0.31)	(0.31)	(0.18)	(0.26)	(0.30)	(0.19)	(0.29)	(0.33)
Reassignment $(t-2)$	0.15	0.10	0.28	-0.09	-0.20	-0.27	0.06	-0.10	0.01
	(0.15)	(0.24)	(0.27)	(0.15)	(0.21)	(0.22)	(0.16)	(0.24)	(0.29)
Reassignment $(t+0)$	-1.89***	-2.24***	-2.92***	1.26***	1.02**	2.06***	-0.63**	-1.22***	-0.86*
	(0.27)	(0.38)	(0.39)	(0.26)	(0.35)	(0.36)	(0.20)	(0.30)	(0.36)
Reassignment $(t+1)$	-1.96***	-2.40***	-2.66***	1.82***	1.73***	2.00***	-0.14	-0.67	-0.67
	(0.27)	(0.37)	(0.39)	(0.27)	(0.36)	(0.38)	(0.25)	(0.35)	(0.37)
Reassignment $(t+2)$	-1.59***	-1.98***	-1.88***	1.72***	1.76***	1.87***	0.13	-0.22	-0.01
	(0.31)	(0.40)	(0.41)	(0.33)	(0.42)	(0.43)	(0.28)	(0.36)	(0.36)
R^2	0.97	0.98	0.98	0.96	0.97	0.96	0.99	0.99	0.99
Observations	4,666	2,040	3,456	4,666	2,040	3,456	4,666	2,040	3,456
Clean sample		×			×			×	
Balanced sample			×			×			×

Notes: The table presents event study results based on Equation 2. The dependent variables are voter turnout (0–100) at the polling place (Columns 1–3), by mail (Columns 4–6), and overall (Columns 7–9). The event is defined as the first time the entire precinct is reassigned to a different polling place. Estimates of Columns (1), (4), and (7) are plotted in Figure 8. Columns using a "clean sample" limit the treatment group to precincts with exactly one (full) reassignment and the control group to precincts with zero (partial) reassignments. The balanced sample restricts precincts to those observed for at least four pre-reassignment and two post-reassignment elections. Regressions are weighted by the number of eligible voters. Standard errors are clustered at the precinct level and reported in parentheses. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$.

Table C.4: Effect Heterogeneity by Change in Proximity to the Polling Place, 3 bins

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Polling Place Turnout			Mail-in Turnout			Total Turnout		
$\mathbb{1}$(Distance decrease)×									
Reassignment $(t-4)$	-0.06	-0.03	-0.05	-0.34	-0.49	-0.08	-0.41	-0.52	-0.14
	(0.28)	(0.37)	(0.36)	(0.24)	(0.32)	(0.28)	(0.28)	(0.39)	(0.35)
Reassignment $(t-3)$	-0.28	-0.22	0.09	0.23	0.25	0.10	-0.05	0.03	0.19
	(0.28)	(0.37)	(0.40)	(0.24)	(0.29)	(0.33)	(0.25)	(0.32)	(0.38)
Reassignment $(t-2)$	-0.17	-0.19	0.04	-0.11	-0.11	-0.19	-0.28	-0.30	-0.15
	(0.19)	(0.27)	(0.31)	(0.20)	(0.28)	(0.29)	(0.24)	(0.35)	(0.38)
Reassignment $(t+0)$	0.90*	0.49	0.56	-0.70	-0.23	0.03	0.20	0.25	0.59
	(0.40)	(0.45)	(0.50)	(0.36)	(0.44)	(0.47)	(0.27)	(0.36)	(0.36)
Reassignment $(t+1)$	0.92*	0.19	0.48	-0.58	0.10	0.11	0.33	0.30	0.59
	(0.36)	(0.45)	(0.46)	(0.35)	(0.43)	(0.47)	(0.33)	(0.45)	(0.45)
Reassignment $(t+2)$	0.85*	0.62	1.04*	-0.26	0.07	0.10	0.59	0.68	1.14**
	(0.38)	(0.44)	(0.44)	(0.38)	(0.48)	(0.51)	(0.34)	(0.40)	(0.37)
$\mathbb{1}$(Little distance change)×									
Reassignment $(t-4)$	0.21	0.42	0.27	-0.49	-0.94*	-0.52	-0.27	-0.52	-0.25
	(0.28)	(0.47)	(0.48)	(0.26)	(0.41)	(0.37)	(0.26)	(0.46)	(0.56)
Reassignment $(t-3)$	0.00	-0.17	0.29	0.01	-0.11	-0.14	0.01	-0.28	0.15
	(0.23)	(0.40)	(0.46)	(0.26)	(0.39)	(0.49)	(0.25)	(0.38)	(0.52)
Reassignment $(t-2)$	0.01	-0.07	0.08	-0.14	-0.04	0.08	-0.13	-0.11	0.16
	(0.20)	(0.32)	(0.37)	(0.20)	(0.32)	(0.35)	(0.22)	(0.36)	(0.50)
Reassignment $(t+0)$	-0.40	-0.82	-1.07*	-0.05	0.14	0.87	-0.45	-0.68	-0.20
	(0.30)	(0.46)	(0.53)	(0.30)	(0.45)	(0.49)	(0.24)	(0.36)	(0.45)
Reassignment $(t+1)$	-0.78*	-1.23**	-1.06*	0.48	0.80	0.74	-0.30	-0.43	-0.32
	(0.31)	(0.45)	(0.52)	(0.34)	(0.53)	(0.56)	(0.34)	(0.44)	(0.50)
Reassignment $(t+2)$	-0.38	-0.84	-0.40	0.28	0.60	0.68	-0.10	-0.24	0.28
	(0.36)	(0.44)	(0.44)	(0.41)	(0.58)	(0.56)	(0.37)	(0.45)	(0.46)
$\mathbb{1}$(Distance increase)×									
Reassignment $(t-4)$	-0.07	-0.21	-0.00	0.04	0.29	-0.09	-0.03	0.09	-0.09
	(0.25)	(0.34)	(0.36)	(0.22)	(0.35)	(0.25)	(0.24)	(0.33)	(0.30)
Reassignment $(t-3)$	0.06	-0.20	-0.20	-0.13	0.09	-0.15	-0.07	-0.11	-0.34
	(0.26)	(0.38)	(0.37)	(0.21)	(0.31)	(0.34)	(0.23)	(0.34)	(0.35)
Reassignment $(t-2)$	0.19	0.23	0.45	-0.01	-0.28	-0.51*	0.18	-0.06	-0.06
	(0.18)	(0.30)	(0.32)	(0.18)	(0.25)	(0.25)	(0.19)	(0.28)	(0.31)
Reassignment $(t+0)$	-2.81***	-3.11***	-3.80***	2.04***	1.75***	2.74***	-0.76**	-1.36***	-1.05*
	(0.32)	(0.43)	(0.44)	(0.30)	(0.38)	(0.40)	(0.26)	(0.39)	(0.46)
Reassignment $(t+1)$	-2.53***	-2.94***	-3.30***	2.51***	2.31***	2.55***	-0.02	-0.63	-0.75
	(0.34)	(0.47)	(0.47)	(0.31)	(0.42)	(0.43)	(0.30)	(0.44)	(0.45)
Reassignment $(t+2)$	-2.32***	-2.71***	-2.68***	2.68***	2.73***	2.67***	0.35	0.02	-0.01
	(0.38)	(0.51)	(0.50)	(0.38)	(0.48)	(0.49)	(0.35)	(0.46)	(0.45)
R^2	0.97	0.98	0.98	0.96	0.97	0.96	0.99	0.99	0.99
Observations	4,666	2,040	3,456	4,666	2,040	3,456	4,666	2,040	3,456
Clean sample		×			×			×	
Balanced sample			×			×			×

Notes: The table presents event study results based on a version of Equation 2 in which event-time dummies are interacted separately with three mutually exclusive indicators for distance increase, little distance change, and distance decrease due to reassignment. The dependent variables are voter turnout (0–100) at the polling place (Column 1), by mail (Column 2), and overall (Column 3). The event is defined as the first time in which the entire precinct is reassigned to a different polling place. Estimates of Columns (1), (4), and (7) are plotted in Figure 9. Columns using a "clean sample" limit the treatment group to precincts with exactly one (full) reassignment and the control group to precincts with zero (partial) reassignments. The balanced sample restricts precincts to those observed for at least four pre-reassignment and two post-reassignment elections. Regressions are weighted by the number of eligible voters. Standard errors are clustered at the precinct level and reported in parentheses. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$.

Appendix C.2. Robustness to Inclusion of Covariates

This section tests the sensitivity of the results to excluding time-varying covariates and to replacing them with a set of interaction terms between pre-treatment controls and election fixed effects. These interaction terms allow turnout effects of precinct characteristics, such as the local demographic composition, to vary across elections. This type of heterogeneity is plausible since our panel includes different types of elections, with varying conditions and voting incentives. Precinct fixed effects cannot absorb such time-varying heterogeneity. This can be problematic if it is correlated with the likelihood of treatment, as emphasized by Millimet and Bellemare (2023).

We report event study results based on Equation 1 (pooled reassignments) and Equation 2 (effect heterogeneity by distance increase versus decrease), excluding covariates, with successively added time-varying controls, and conditional on interacted covariates in Appendix Tables C.5 and C.6. Time-varying controls in the specification with "some" covariates include precinct size (log number of residents, share of residents eligible to vote), and the shares of native German residents and non-native German residents.[37] The columns including "all" time-varying controls add the age composition (share of eligible voters aged 18–24, 25–34, 35–44, and 45–59, respectively), the share of EU foreigners in the electorate, the average quoted rent per square meter, the share of households with children, the average duration of residence, and the shares of married residents and single residents, respectively; Interacted controls are measured in 2013 and thus time-invariant. They include all characteristics listed above and the average distance to the polling location and the precinct's surface area. Ten precincts treated in 2013 are excluded from this sample. The estimates are very close throughout the observation period and across specifications, alleviating concerns over bad controls and time-varying heterogeneity.

[37] The excluded category is residents without German citizenship.

Table C.5: Robustness to Inclusion of Covariates–Pooled Reassignments

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Polling Place Turnout				Mail-in Turnout				Total Turnout			
Reassignment ($t-4$)	-0.05	-0.02	0.01	0.18	-0.20	-0.17	-0.23	-0.23	-0.25	-0.18	-0.22	-0.04
	(0.19)	(0.18)	(0.17)	(0.18)	(0.18)	(0.16)	(0.16)	(0.16)	(0.20)	(0.17)	(0.17)	(0.17)
Reassignment ($t-3$)	-0.08	-0.08	-0.06	0.01	0.10	0.10	0.01	0.04	0.02	0.03	-0.05	0.06
	(0.18)	(0.17)	(0.17)	(0.17)	(0.16)	(0.16)	(0.15)	(0.14)	(0.19)	(0.16)	(0.16)	(0.15)
Reassignment ($t-2$)	-0.13	0.03	0.01	0.06	-0.18	-0.03	-0.06	0.06	-0.32	-0.00	-0.05	0.12
	(0.14)	(0.12)	(0.12)	(0.13)	(0.13)	(0.13)	(0.12)	(0.11)	(0.18)	(0.13)	(0.13)	(0.13)
Reassignment ($t+0$)	-1.12***	-1.06***	-1.00***	-1.18***	0.55*	0.62**	0.61**	0.64**	-0.57**	-0.44**	-0.39*	-0.54**
	(0.24)	(0.24)	(0.23)	(0.23)	(0.23)	(0.22)	(0.22)	(0.20)	(0.20)	(0.17)	(0.16)	(0.18)
Reassignment ($t+1$)	-0.98***	-0.97***	-0.89***	-0.91***	0.88***	0.93***	0.90***	0.89***	-0.10	-0.05	0.01	-0.02
	(0.25)	(0.24)	(0.23)	(0.25)	(0.24)	(0.23)	(0.23)	(0.22)	(0.25)	(0.21)	(0.20)	(0.24)
Reassignment ($t+2$)	-0.75**	-0.77**	-0.75**	-0.72*	0.90**	0.99***	1.05***	0.89***	0.16	0.22	0.30	0.17
	(0.28)	(0.27)	(0.26)	(0.29)	(0.29)	(0.27)	(0.26)	(0.26)	(0.30)	(0.24)	(0.22)	(0.27)
R^2	0.97	0.97	0.97	0.97	0.95	0.96	0.96	0.97	0.98	0.99	0.99	0.99
Observations	4,666	4,666	4,666	4,609	4,666	4,666	4,666	4,609	4,666	4,666	4,666	4,609
Time-varying controls	none	some	all	none	none	some	all	none	none	some	all	
Pre-treatment covariates × election FE				yes				yes				yes

Notes: The table reports event study results based on variants of Equation 1. Time-varying controls in the specification with "some" covariates include precinct size (log number of residents, share of residents eligible to vote), and the shares of native German residents and non-native German residents.. The columns including "all" time-varying controls add the age composition (share of eligible voters aged 18–24, 25–34, 35–44, and 45–59, respectively), the share of EU foreigners in the electorate, the average quoted rent per square meter, the share of households with children, the average duration of residence, and the shares of married residents and single residents, respectively. Interacted controls are measured in 2013 and include all characteristics listed above in addition to the average distance to the polling location and the precinct's surface area. Ten precincts treated in 2013 are excluded from this sample. The event is defined as the first time in which the entire precinct is reassigned to a different polling place. Regressions are weighted by the number of eligible voters. Standard errors are clustered at the precinct level and reported in parentheses. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$.

Table C.6: Robustness to Inclusion of Covariates–Effects by Distance

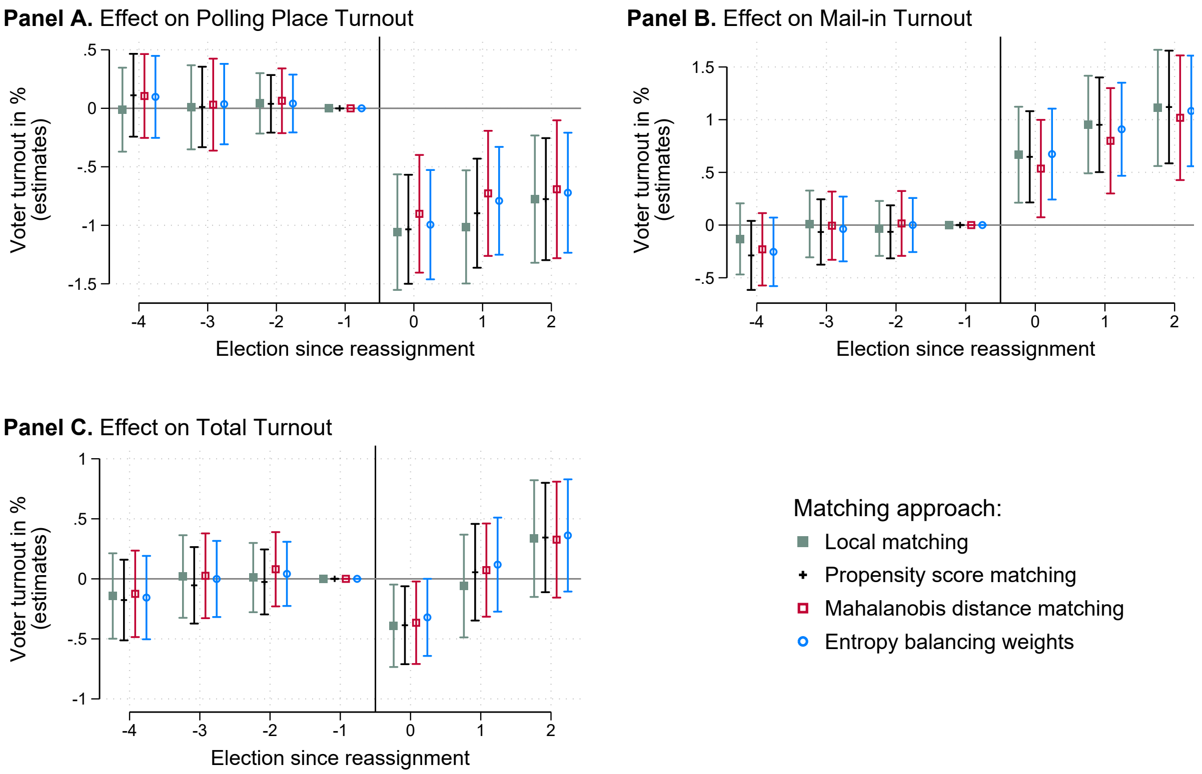
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Polling Place Turnout				Mail-in Turnout				Total Turnout			
$\mathbb{1}$(Distance decrease)×												
Reassignment $(t-4)$	-0.25	-0.19	-0.16	0.06	-0.21	-0.14	-0.27	-0.17	-0.46	-0.33	-0.43	-0.10
	(0.25)	(0.25)	(0.24)	(0.25)	(0.22)	(0.22)	(0.22)	(0.20)	(0.25)	(0.24)	(0.24)	(0.24)
Reassignment $(t-3)$	-0.37	-0.29	-0.25	-0.08	0.19	0.29	0.14	0.05	-0.18	-0.01	-0.11	-0.04
	(0.24)	(0.24)	(0.24)	(0.21)	(0.24)	(0.22)	(0.22)	(0.19)	(0.25)	(0.22)	(0.22)	(0.20)
Reassignment $(t-2)$	-0.56**	-0.20	-0.20	-0.07	-0.40*	0.02	-0.05	-0.09	-0.96***	-0.19	-0.25	-0.17
	(0.21)	(0.18)	(0.18)	(0.18)	(0.19)	(0.18)	(0.18)	(0.16)	(0.27)	(0.20)	(0.20)	(0.19)
Reassignment $(t+0)$	0.42	0.45	0.48	0.48	-0.42	-0.38	-0.46	-0.47	-0.01	0.07	0.02	0.02
	(0.36)	(0.35)	(0.34)	(0.33)	(0.33)	(0.31)	(0.31)	(0.28)	(0.32)	(0.25)	(0.24)	(0.28)
Reassignment $(t+1)$	0.65	0.62	0.60	1.02**	-0.34	-0.38	-0.39	-0.39	0.32	0.24	0.21	0.63
	(0.34)	(0.33)	(0.31)	(0.35)	(0.33)	(0.31)	(0.31)	(0.30)	(0.37)	(0.30)	(0.28)	(0.35)
Reassignment $(t+2)$	0.54	0.60	0.49	0.86*	-0.19	-0.12	0.05	-0.31	0.35	0.48	0.54	0.55
	(0.38)	(0.35)	(0.35)	(0.39)	(0.39)	(0.37)	(0.36)	(0.37)	(0.43)	(0.33)	(0.31)	(0.39)
$\mathbb{1}$(Distance increase)×												
Reassignment $(t-4)$	0.06	0.08	0.10	0.18	-0.19	-0.16	-0.19	-0.18	-0.13	-0.09	-0.09	-0.00
	(0.22)	(0.22)	(0.21)	(0.21)	(0.22)	(0.20)	(0.19)	(0.19)	(0.25)	(0.20)	(0.20)	(0.19)
Reassignment $(t-3)$	0.08	0.05	0.05	0.06	0.04	-0.00	-0.07	0.07	0.12	0.05	-0.01	0.13
	(0.22)	(0.21)	(0.20)	(0.21)	(0.19)	(0.19)	(0.18)	(0.17)	(0.22)	(0.20)	(0.19)	(0.18)
Reassignment $(t-2)$	0.15	0.17	0.15	0.15	-0.08	-0.05	-0.09	0.14	0.07	0.12	0.06	0.28
	(0.18)	(0.15)	(0.15)	(0.15)	(0.15)	(0.15)	(0.15)	(0.13)	(0.22)	(0.16)	(0.16)	(0.15)
Reassignment $(t+0)$	-2.05***	-1.97***	-1.89***	-2.11***	1.12***	1.22***	1.26***	1.24***	-0.93***	-0.75***	-0.63**	-0.87***
	(0.28)	(0.27)	(0.27)	(0.26)	(0.27)	(0.26)	(0.26)	(0.24)	(0.24)	(0.21)	(0.20)	(0.21)
Reassignment $(t+1)$	-2.15***	-2.10***	-1.96***	-2.24***	1.72***	1.79***	1.82***	1.74***	-0.43	-0.31	-0.14	-0.50
	(0.28)	(0.27)	(0.27)	(0.27)	(0.29)	(0.27)	(0.27)	(0.28)	(0.31)	(0.27)	(0.25)	(0.28)
Reassignment $(t+2)$	-1.63***	-1.65***	-1.59***	-1.75***	1.64***	1.61***	1.72***	1.69***	0.01	-0.04	0.13	-0.06
	(0.33)	(0.32)	(0.31)	(0.33)	(0.37)	(0.34)	(0.33)	(0.33)	(0.38)	(0.31)	(0.28)	(0.34)
R^2	0.97	0.97	0.97	0.97	0.95	0.96	0.96	0.97	0.98	0.99	0.99	0.99
Observations	4,666	4,666	4,666	4,609	4,666	4,666	4,666	4,609	4,666	4,666	4,666	4,609
Time-varying controls	none	some	all	none	none	some	all		none	some	all	none
Pre-treatment covariates × election FE				yes				yes				yes

Notes: The table reports point estimates and standard errors based on variants of Equation 2. Time-varying controls in the specification with "some" covariates include precinct size (log number of residents, share of residents eligible to vote), and the shares of native German residents and non-native German residents.. The columns including "all" time-varying controls add the age composition (share of eligible voters aged 18–24, 25–34, 35–44, and 45–59, respectively), the share of EU foreigners in the electorate, the average quoted rent per square meter, the share of households with children, the average duration of residence, and the shares of married residents and single residents, respectively. Interacted controls are measured in 2013 and include all characteristics listed above in addition to the average distance to the polling location and the precinct's surface area. Ten precincts treated in 2013 are excluded from this sample. The event is defined as the first time in which the entire precinct is reassigned to a different polling place. Regressions are weighted by the number of eligible voters. Standard errors are clustered at the precinct level and reported in parentheses. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$.

Appendix C.3. Robustness to Matching on Observables

This section explores the sensitivity of our findings to several matching approaches to reduce observational dissimilarities between untreated (control) and treated precincts. We re-estimate our baseline specification (Equation 1) and the heterogeneity by increased versus decreased distance (Equation 2) with each matching approach and report the results in Figures C.3 and C.4.

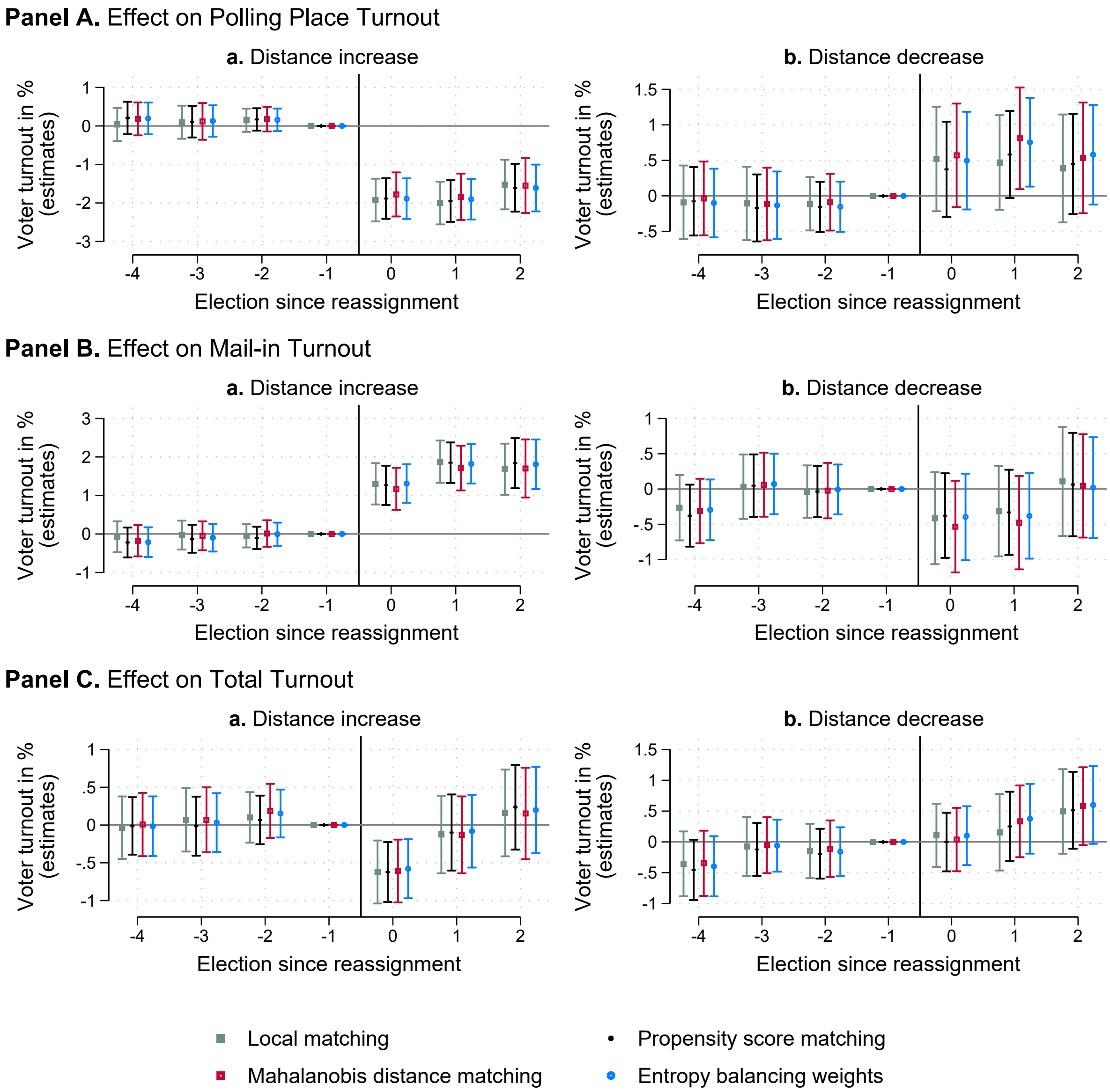
Figure C.3: Robustness to Matching on Observables–Pooled Reassignments



Notes: The figure presents event study results based on Equation 1 using different matching approaches: *i)* local matching, which restricts the sample to treated and never-treated precincts that share a border within the same district; *ii)* 1:1 nearest neighbor matching on propensity scores, which are computed from the following pre-treatment precinct characteristics: average walking distance to the polling location, log number of residents, % residents eligible to vote, % eligible voters aged 18–24, 25–34, 35–44, and 45–59, respectively, the average quoted rent per square meter, % households with children, the average duration of residence, % of native German residents, non-native German residents, married residents, and single residents, respectively, total turnout, % mail-in votes, and the party vote share for the social democrats. *iii)* 1:1 nearest neighbor matching on Mahalanobis distance, computed from the same pre-treatment characteristics; and *iv)* Entropy balancing weights from Hainmueller (2012). The event is defined as the first time in which the entire precinct is reassigned to a different polling place. Confidence intervals are drawn at the 95 percent level. Mahalanobis matching adjusts standard errors for two-way clustering at the level of the matched pair and the matched untreated precinct in each election. The other specifications use standard errors clustered at the precinct level.

Local Matching. First, we implement a local matching procedure, where we exclude treated units without adjacent control units in the same district and control units that do not share a border with at least one treated unit in the same district. The outcome of this matching procedure is illustrated in Figure C.5: Out of 618 precincts, 117 are not matched and dropped from the sample. 248 treated units are matched to 253 untreated units. The event study

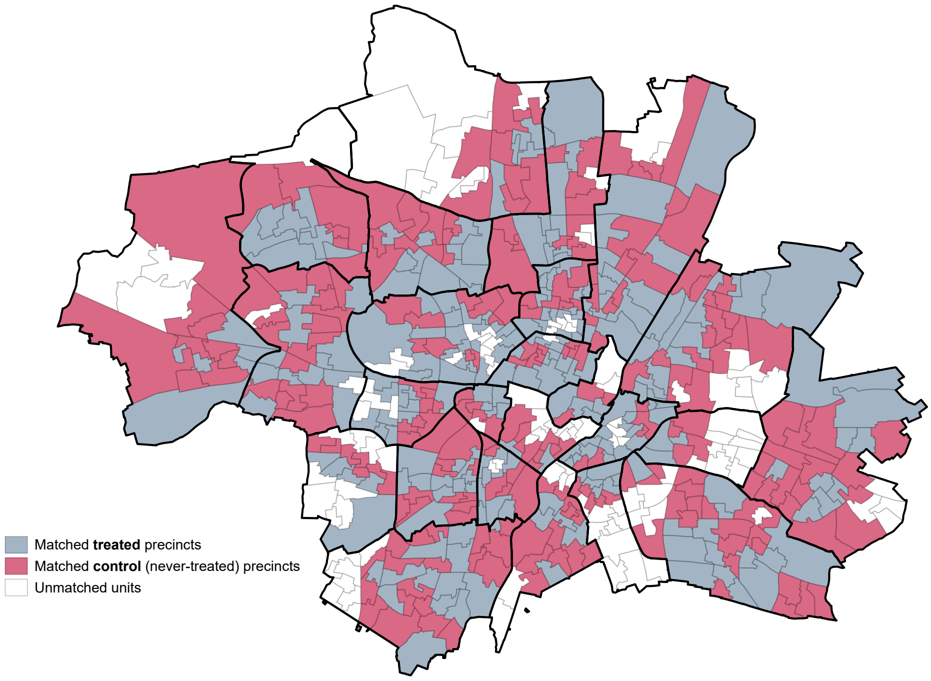
Figure C.4: Robustness to Matching on Observables–Effects by Distance Change



Notes: The figure presents event study results based on Equation 2 using different matching approaches: *i)* local matching, which restricts the sample to treated and never-treated precincts that share a border within the same district; *ii)* 1:1 nearest neighbor matching on propensity scores, which are computed from the following pre-treatment precinct characteristics: average walking distance to the polling location, log number of residents, % residents eligible to vote, % eligible voters aged 18–24, 25–34, 35–44, and 45–59, respectively, the average quoted rent per square meter, % households with children, the average duration of residence, % of native German residents, non-native German residents, married residents, and single residents, respectively, total turnout, % mail-in votes, and the party vote share for the social democrats. *iii)* 1:1 nearest neighbor matching on Mahalanobis distance, computed from the same pre-treatment characteristics; and *iv)* Entropy balancing weights from Hainmueller (2012). The event is defined as the first time in which the entire precinct is reassigned to a different polling place. Confidence intervals are drawn at the 95 percent level. Mahalanobis matching adjusts standard errors for two-way clustering at the level of the matched pair and the matched untreated precinct in each election. The other specifications use standard errors clustered at the precinct level.

results based on the locally matched sample are close to the baseline results. All treatment effects remain statistically significant.

Figure C.5: Map of Matched Treated and Control Precincts



Notes: The map illustrates the outcome of the local matching procedure. Thick black lines highlight district borders. Gray lines mark precinct borders. Matched controls include all precincts that share a border with at least one treated precinct in the same district. Matched treated units share a border with at least one control precinct in the same district. Out of 618 precincts, 117 are unmatched and dropped from the sample, 248 are matched treated, and 253 matched control units.

Propensity Score Matching. Second, we conduct a propensity score matching procedure, where the propensity of having the polling place moved is estimated as a function of the following pre-treatment socioeconomic characteristics using a probit regression: average walking distance to the polling location, precinct size (log number of residents, share of residents eligible to vote), age composition (share of eligible voters aged 18–24, 25–34, 35–44, and 45–59, respectively), the average quoted rent per square meter, the share of households with children, the average duration of residence, and the shares of native German residents, non-native German residents, married residents, and single residents, respectively. We additionally match on pre-treatment electoral outcomes: total turnout, the share of mail-in votes, and the party vote share for the social democrats (which is the incumbent party in Munich throughout the observation period). All precinct characteristics are measured in 2013 (the first election in our panel) and we exclude 10 precincts treated in the 2013 Federal Election since no pre-treatment observations exist for these units. We drop three units with estimated propensity scores outside of the common support.[38] The matched sample is then obtained using a 1:1 nearest

[38] Common support is defined as the range between the minimum of treated and the maximum of untreated units' propensity scores.

neighbor matching without replacement. This leaves us with 268 treated units matched to an equal number of control units. The estimates based on the matched sample confirm our original results and closely align with the outcome of the local matching approach.

Mahalanobis Matching. We also check robustness to matching on the Mahalanobis distance between treated and untreated units. This approach determines similarity based on the proximity of units' covariates in the vector space rather than their propensity to receive treatment. We calculate distances using the same pre-treatment covariates listed above and use a 1:1 nearest neighbor matching with replacement to determine the matched sample. 270 treated precincts are matched to 155 control units; 193 units are dropped. We follow Colmer et al. (2023) and adjust standard errors for two-way clustering to account for potential bias created by matching on covariates (Abadie and Spiess, 2022). The first cluster is at the level of the matched pair to account for error correlation across matches and time. The second cluster is at the level of the matched untreated precinct in every election to account for correlation across treated precincts that are matched to the same unit. The results show that treatment effects remain significant and close to the other matching outcomes.

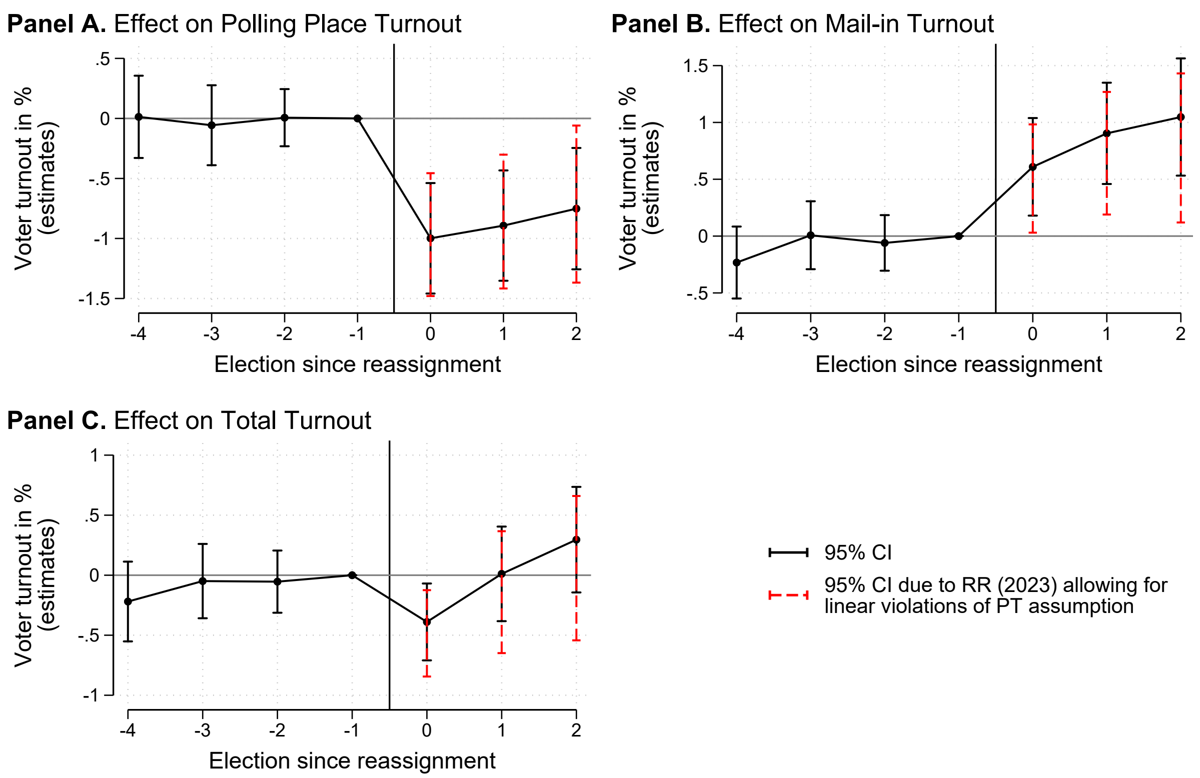
Entropy Balancing. Finally, we implement the entropy balancing approach proposed by Hainmueller (2012). One advantage of this method is that the sample is not truncated. Instead, it fits a set of weights that balance treatment and control groups on several moments of the covariate distributions. In practice, we balance the means and variances of the pre-treatment characteristics listed above and use the resulting entropy weights in the event study regressions. The results support our original findings across all specifications and outcomes.

Appendix C.4. Robustness to Parallel Trend Violations

Next, we test robustness to violations of the parallel trend assumption using the methodology proposed by Rambachan and Roth (2023, henceforth RR). RR formalize the idea that observed pre-treatment differences in trends (pretrends) can inform unobserved violations of the parallel trend assumption after treatment adoption. Imposing that post-treatment violations are not "too different" from pre-treatment deviations, RR develop methods for creating "honest" confidence intervals (CI) that are robust to parallel trend violations. Importantly, their approach also accounts for the fact that pretrends may be imprecisely estimated.

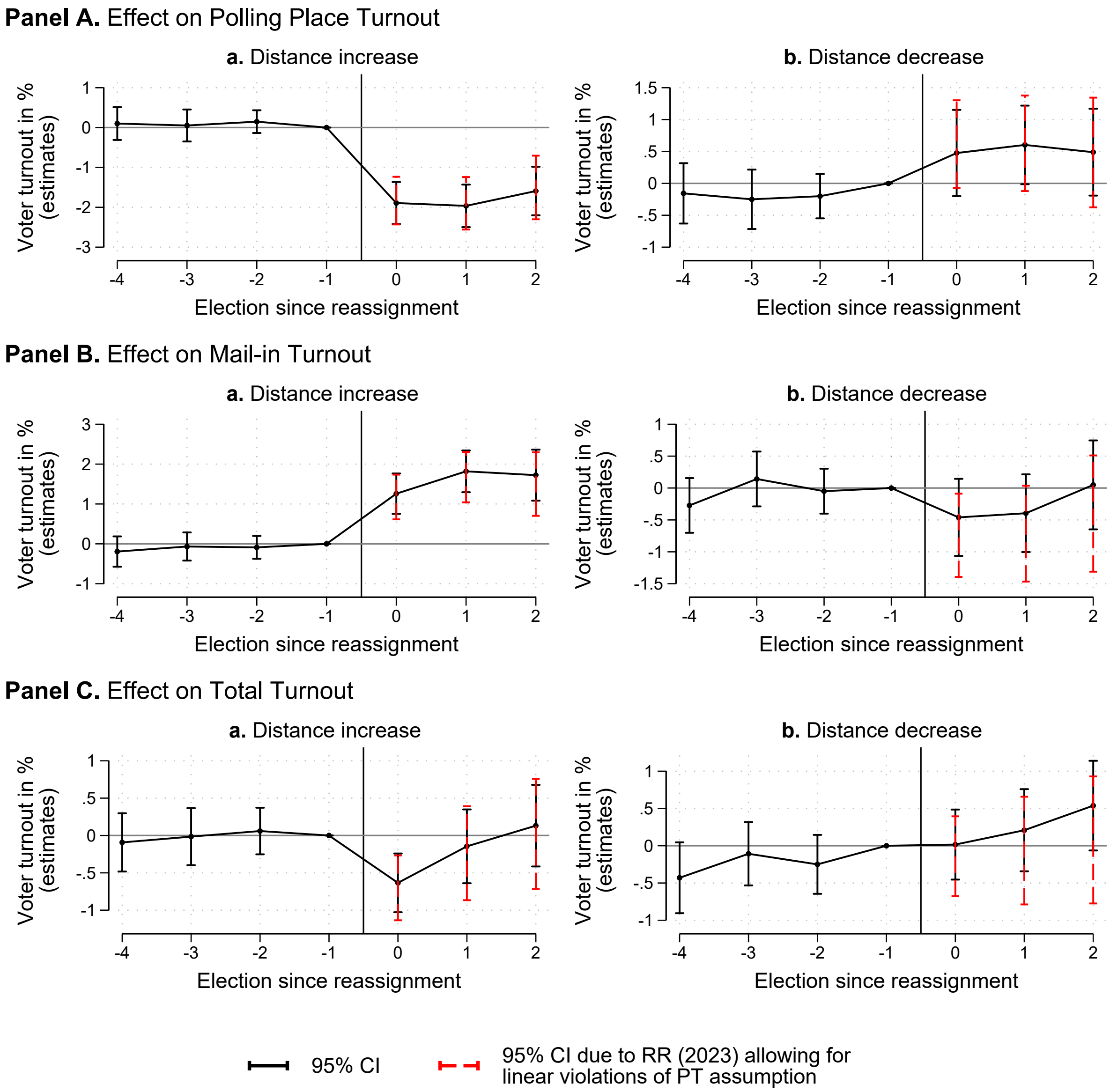
We re-estimate Equation 1 (pooled reassignments) and Equation 2 (effect heterogeneity by distance increase versus decrease) with "honest" 95% CI that allow for linear violations of the parallel trend assumption. The parameter $M \geq 0$ governs how much the counterfactual trend can deviate from linearity. We impose $M = 0$, i.e., a constant linear trend. The results in Figures C.6 and C.7 show that all treatment estimates remain statistically significant.

Figure C.6: Robustness to linear violations of parallel trend assumption (Rambachan and Roth, 2023)–Pooled Reassignments



Notes: The figure presents event study results based on Equation 1. Black confidence intervals are drawn at the 95 percent level using standard errors clustered at the precinct level. Rambachan and Roth (2023) confidence intervals that are robust to linear violations of parallel trends ($M = 0$) are drawn in red.

Figure C.7: Robustness to linear violations of parallel trend assumption (Rambachan and Roth, 2023)–Effects by Distance Change



Notes: The figure presents event study results based on Equation 2. Black confidence intervals are drawn at the 95 percent level using standard errors clustered at the precinct level. Rambachan and Roth (2023) confidence intervals that are robust to linear violations of parallel trends ($M = 0$) are drawn in red.

Appendix C.5. Robustness to Alternative Treatment Definitions

In the main analysis, we define treatment as binary and capturing reassignments of the *entire* precinct, i.e., when all home addresses are reassigned to a different polling place. We motivate this choice by the fact that the distribution of reassignments at the precinct level is highly skewed towards 100%; i.e., whenever we observe a positive share of reassigned addresses, it is likely a full reassignment (41 percent of all cases; see Figure D.5). In this section, we explore the sensitivity of our findings to alternative treatment definitions. First, we consider different binary and continuous definitions relating to the share of home addresses reassigned. We estimate variants of Equation 1 (pooled reassignments) and Equation 2 (effect heterogeneity by reassignments that increased versus decreased distance) and report the results in Figures C.8 and C.9. Next, we estimate a triple difference specification that combines treatment breadth (percent of addresses reassigned) with treatment depth (change in walking distance to the polling place) in one framework (Figure C.10). This approach also yields an alternative way to decompose the treatment effects into a portion explained by reassignments per se and by changes in distance, presented in Section 4.2. Finally, we use the DiD estimator recently proposed by de Chaisemartin et al. (2023), which accommodates staggered treatment timing, continuous treatment, and multiple treatments of the same precinct in one framework (Figure C.11). We document compositional changes to the treatment and the control group by treatment definition in Table C.7. In our baseline definition, 280 precincts are treated, 338 precincts are untreated. The sensitivity analysis confirms our initial findings both qualitatively and quantitatively, despite the changes to sample composition and the identifying variation.

Alternative Binary Treatment. First, we define a different binary treatment by lowering the treatment threshold from 100% to 50%: Hence, all precincts with at least half of their home addresses assigned to a different polling location are considered treated. This shifts 92 precincts from the control to the treatment group, increasing the size of the latter by one-third to 372 units. In cases with multiple treatments, we set the event ($\tau = 0$) to the *first* election in which the treatment condition is met and drop all precinct-election cells once a second reassignment occurs. In practice, dropping these observations leaves the estimates virtually unchanged. Compared to the baseline, the treatment effects appear slightly more pronounced but confirm the initial conclusions (Figures C.8 and C.9).

Continuous Treatment: Share of Reassigned Addresses. Second, we set the event ($\tau = 0$) to the election with the *highest* share of reassigned addresses and rescale event-time dummies by treatment intensity (i.e., the share of addresses reassigned) in $\tau = 0$. This definition classifies precincts with reassignments as low as 1% as treated, increasing the treatment group to 513 units. The median treatment intensity is 1; the 10th percentile is at 0.15. The control group comprises 105 precincts with zero reassignments throughout the observation period. As an alternative to the rescaling event-time dummies by treatment intensity using the TWFE estimator, we implement the estimator proposed by de Chaisemartin et al. (2023). Here, we assume that treatment switches on in the election with the highest share of reassignments and stays at that level afterwards. The estimator yields an "average of switchers' slopes", i.e., the average effect of switching on the treatment status scaled by the share of reassigned addresses. Similar to de Chaisemartin and D'Haultfœuille (2020), the estimator compares switchers (treated precincts) with stayers (no change in treatment status) that have the same initial treatment level. As we assume that treatment switches on once and does not change again, initial treatment levels are zero for 489 out of 513 treated precincts (95 percent). The estimator also accounts for heterogeneous treatment effects as a potential source of bias under staggered treatment timing. The estimates based on the continuous treatment definition are very close to the ones obtained using the binary treatment definitions. The standard errors of the de Chaisemartin et al. (2023) estimator are somewhat larger and the drop in total turnout is not statistically significant in the pooled regression (Figure C.8). Still, we find the drop and recovery in total turnout when conditioning on reassignments that increased distance, consistent with our original results and the inattention hypothesis.

Table C.7: Sample Composition by Treatment Definition

Treatment definition	#treated units	#untreated (control) units
binary treatment, 100% addresses reassigned	280	338
binary treatment, $\geq$ 50% addresses reassigned	372	246
continuous treatment, (% addresses reassigned or Δdistance)	513	105

Notes: The table documents compositional changes of the treatment and control group for different treatment definitions. The total number of precincts is 618.

Continuous Treatment: Triple Difference Estimator. Next, we combine the share of reassigned addresses and the change in distance in a triple difference framework. The event is the election with the highest share of reassigned addresses, and event-time dummies are rescaled by R_p, the share of addresses reassigned in $\tau = 0$. The triple difference terms correspond to interactions between event-time dummies and $\Delta dist_p$, the change in distance in kilometers in $\tau = 0$. We additionally allow the slopes of the distance effect to differ for reassignments that increased versus decreased distance by separately interacting the triple difference terms with indicator variables identifying the two different cases:

$$
\begin{aligned}
Y_{pt} = & \sum_{k \neq -1} \theta^k [R_p \times \mathbb{1}(\tau = k)] + \\
& \sum_{k \neq -1} \gamma^k [\Delta dist_p \times \mathbb{1}(\tau = k) \times \mathbb{1}(\Delta dist_p < 0)] + \\
& \sum_{k \neq -1} \rho^k [\Delta dist_p \times \mathbb{1}(\tau = k) \times \mathbb{1}(\Delta dist_p > 0)] + \\
& \mathbf{X}'_{pt} \eta + \pi_p + \pi_{d(p)t} + \epsilon_{pt},
\end{aligned}
\tag{C.2}
$$

where the estimates θ^k capture the base effects coming from the comparison of treated and control units (before and after reassignment); γ^k and ρ^k trace the turnout differences *among treated units* for a one-kilometer increase (respectively decrease) to the polling location. As usual, we control for time-varying covariates at the precinct level, precinct fixed effects, and election×district fixed effects. The results are presented in Figure C.10. The base effects in Panel A reflect the impact of reassignments, holding the distance to the polling location constant. The pattern corroborates the findings of the decomposition exercise in Section 4.2: The reassignment disutility is enough to prompt a shift towards mail-in voting and a temporary drop in total turnout. Panels B and C report the distance effects. Unsurprisingly, the patterns mirror each other: Increasing distance amplifies the substitution towards mail-in voting and the temporary drop in total turnout (Panel B). An additional kilometer reduces polling place turnout by 3.8 percentage points and total turnout by 0.9 percentage points. By contrast, moving the polling place closer to eligible voters induces an opposite shift, away from mail-in voting, and an increase in total turnout. To offset the instantaneous drop in polling place turnout, the polling location must move 140 meters closer. The decline in total turnout is offset at a 240 meters distance reduction.[39] The effect sizes are close to the ones estimated with a binary treatment definition and a different decomposition strategy (Section 4.2). Finally, we reject the hypothesis of asymmetric distance effects between cases where the polling place moves closer versus further away: The magnitude of the distance slopes in Panels B and C are neither jointly nor pairwise statistically different from each other in any post-treatment period and for any outcome.

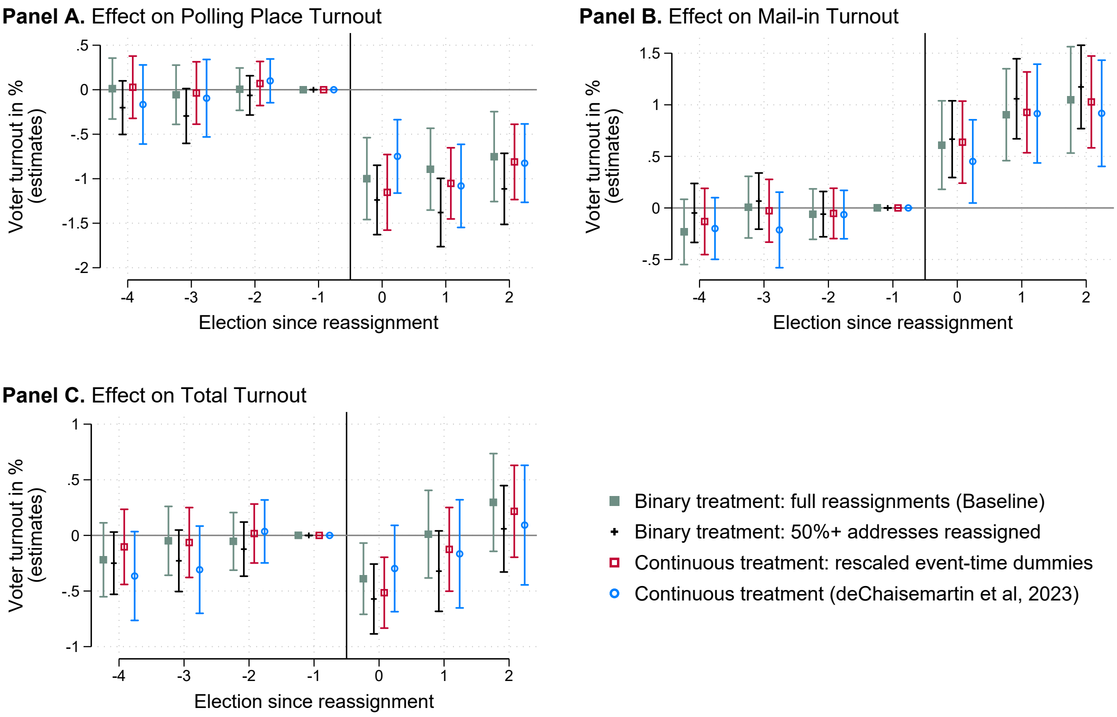
Continuous Treatment: Average Change in Distance. Finally, we consider a continuous treatment definition based solely on the change in distance to the polling location. Here, we allow for multiple treatments of the same precinct as these can be handled by the de Chaisemartin et al. (2023) estimator: For example, if a precinct has its polling place moved away from residents by 200 meters in 2014 and by an additional 100 meters in 2017, the estimator exploits both shocks by comparing these "switches" to units that experience no change ("stayers") and have the same initial treatment status. The estimator then delivers an average marginal effect on switchers.[40] We report the dynamic effects for the pooled sample and separately for distance increases ("switchers in") and distance decreases ("switchers out") in Figure C.11. The pattern closely resembles the previous findings. Turnout effects are insignificant when the polling location moves closer, as reassignment shocks and shorter travel compensate each other. As before, we find a significant shift towards mail-in voting and a drop in total turnout when the

[39] The distance reduction required to offset the drop in polling place turnout is smaller than for total turnout because of mail-in voters who switch to poll voting.

[40] de Chaisemartin et al. (2023) recommend discarding units with multiple switches that result in treatment higher than the period-one treatment at some periods and lower at other periods. This concerns precincts with multiple reassignments that move the distance in opposite directions so that the initial change is overcompensated (e.g., +200 meters in 2014 and −300m in 2017). The command did_multiplegt_dyn allows dropping these cells automatically.

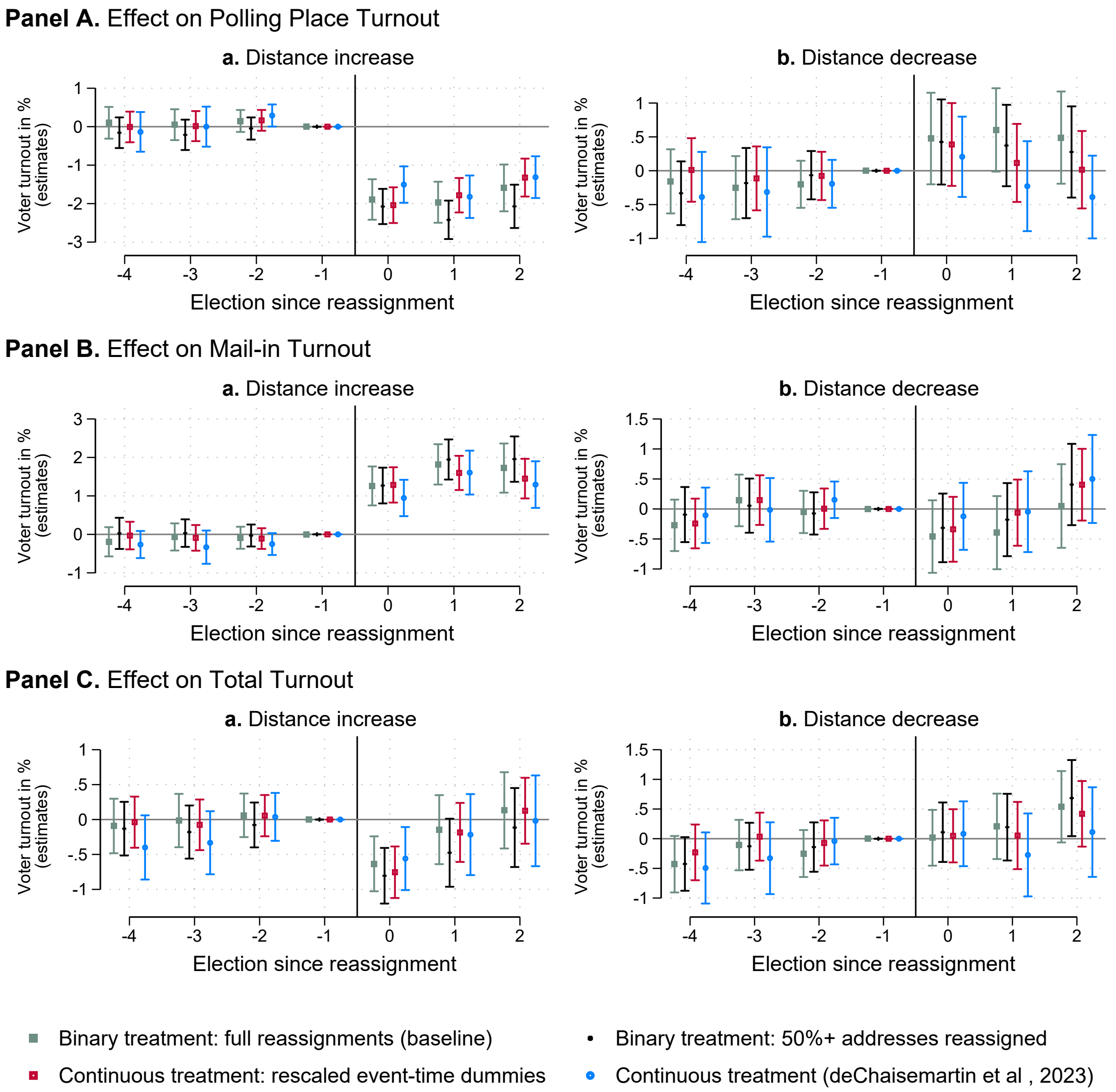
polling place is moved further away. While the turnout recovery seems somewhat slower in this specification, the previous conclusions hold.

Figure C.8: Robustness to Alternative Treatment Definitions–Pooled Reassignments



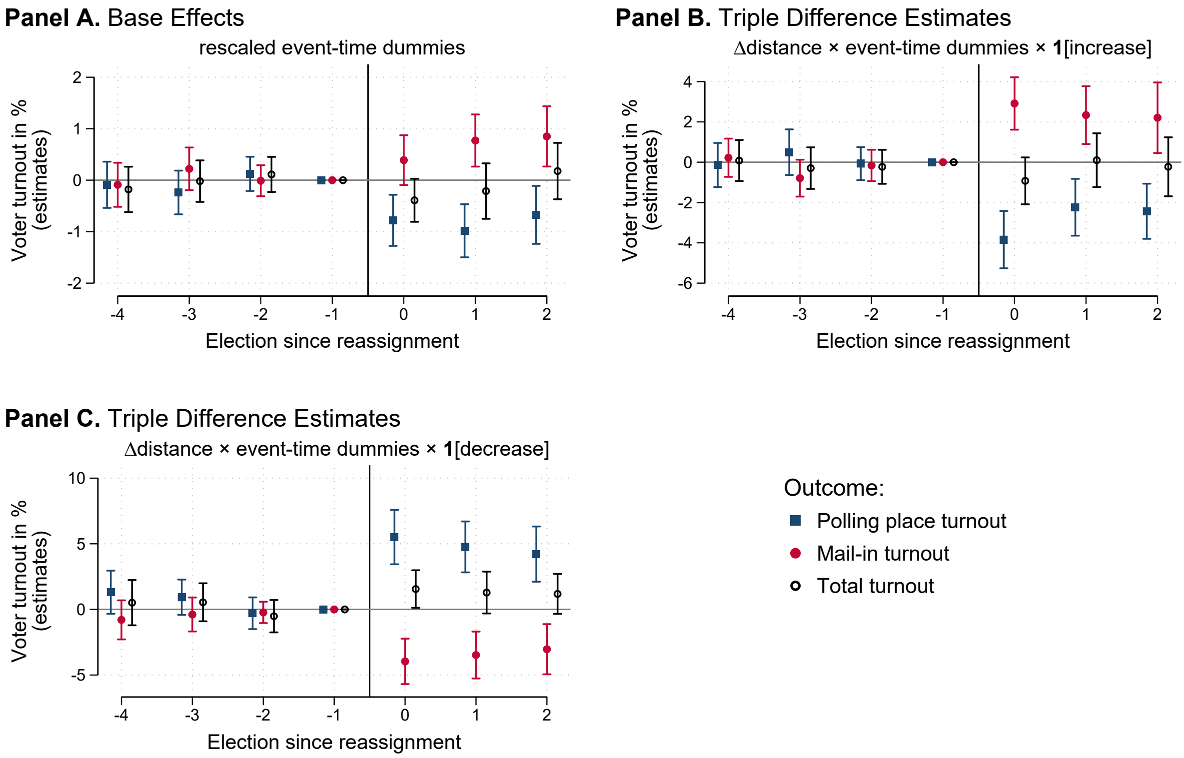
Notes: The figure presents event study results for alternative treatment definitions. The baseline estimates are based on Equation 1 using a binary treatment definition capturing full polling place reassignments. Alternative definitions include a binary treatment for reassignments that affected at least 50% of home addresses in a precinct and continuous treatment based on the share of reassigned addresses. In the latter, the event is defined as the election with the highest positive share of reassigned addresses. Regressions are weighted by the number of eligible voters. Confidence intervals are drawn at the 95 percent level using standard errors clustered at the precinct level.

Figure C.9: Robustness to Alternative Treatment Definitions–Effects by Distance Change



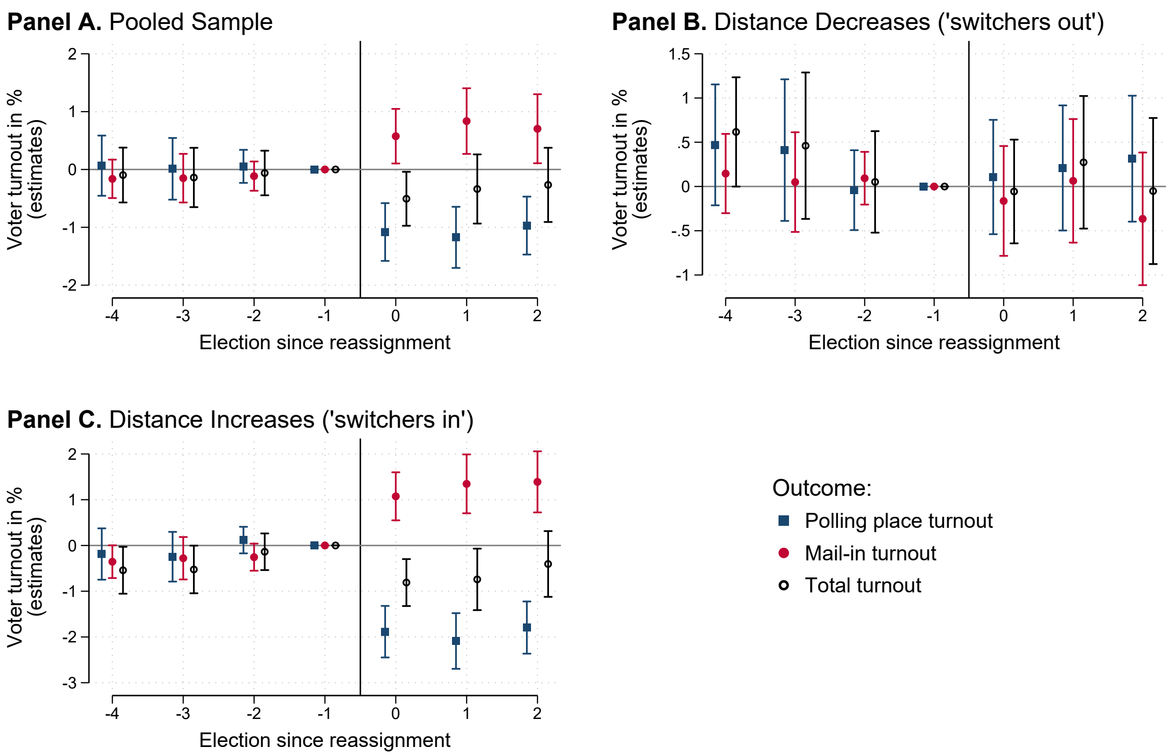
Notes: The figure presents event study results for alternative treatment definitions. The baseline estimates are based on Equation 2 using a binary treatment definition capturing full polling place reassignments. Alternative definitions include a binary treatment for reassignments that affected at least 50% of home addresses in a precinct and continuous treatment based on the share of reassigned addresses. In the latter, the event is defined as the election with the highest positive share of reassigned addresses. Regressions are weighted by the number of eligible voters. Confidence intervals are drawn at the 95 percent level using standard errors clustered at the precinct level.

Figure C.10: Triple Difference Estimates by Increase and Decrease in Distance



Notes: The figure presents event study results based on the triple difference estimator in Equation C.2. Panel A plots the base effects ($\hat{\theta}^k$). Panels B and C plot the triple difference estimates $\hat{\gamma}^k$ and $\hat{\rho}^k$, respectively. The event is defined as the election with the highest share of home addresses reassigned to a different polling place. Regressions are weighted by the number of eligible voters. Confidence intervals are drawn at the 95 percent level using standard errors clustered at the precinct level.

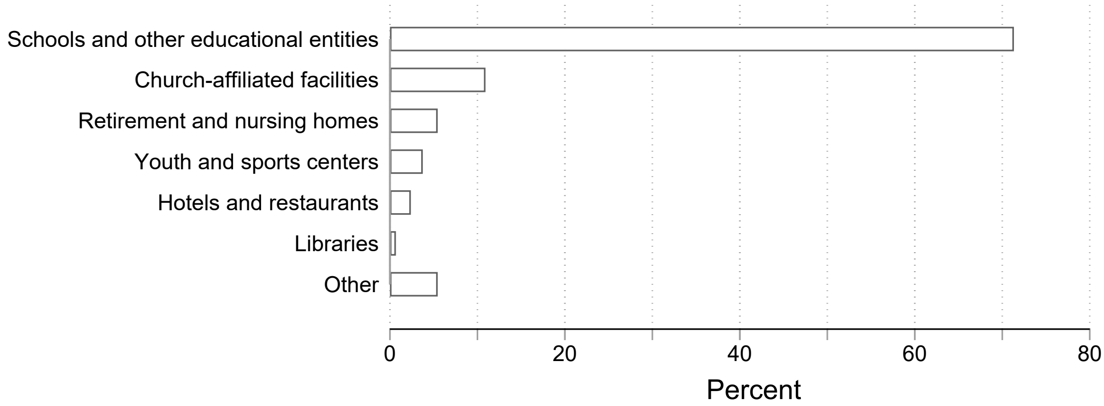
Figure C.11: Robustness to Alternative Treatment Definition: Continuous Treatment and de Chaisemartin et al. (2023) estimator



Notes: The figure presents event study results based on the de Chaisemartin et al. (2023) estimator. Treatment is continuous and defined as the average change in distance to the polling location relative to the previous election. Multiple treatments of the same units are allowed. All specifications include precinct fixed effects, election fixed effects, and time-varying covariates listed in Section 3.3. Regressions are weighted by the number of eligible voters. Confidence intervals are drawn at the 95 percent level using standard errors clustered at the precinct level.

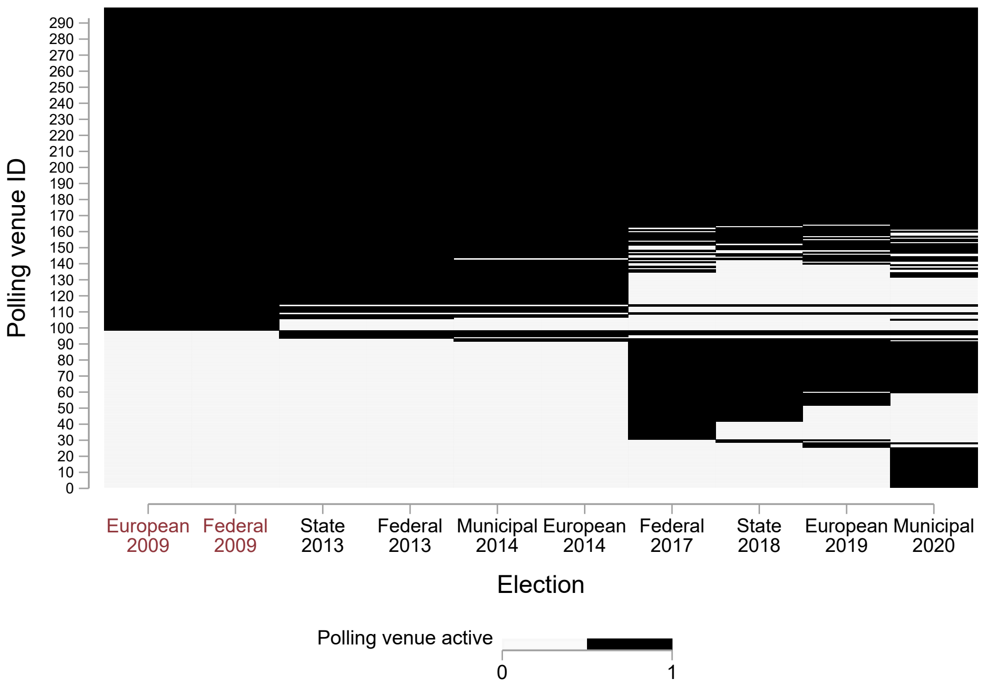
Appendix D. Figures

Figure D.1: Types of Polling Venues



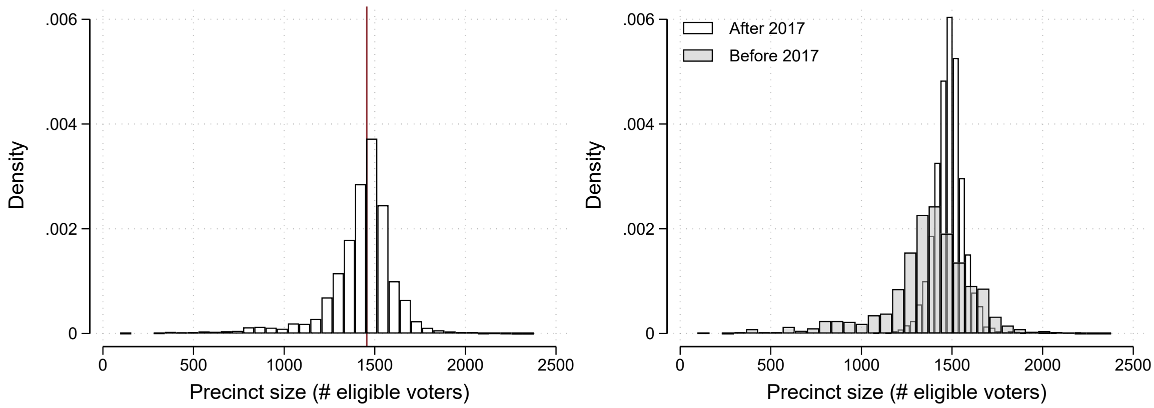
Notes: The figure shows the distribution of polling venues over different categories in the eight elections held in Munich between 2013 and 2020 (293 distinct venues in total).

Figure D.2: Activity Status of Polling Venues between 2009 and 2020



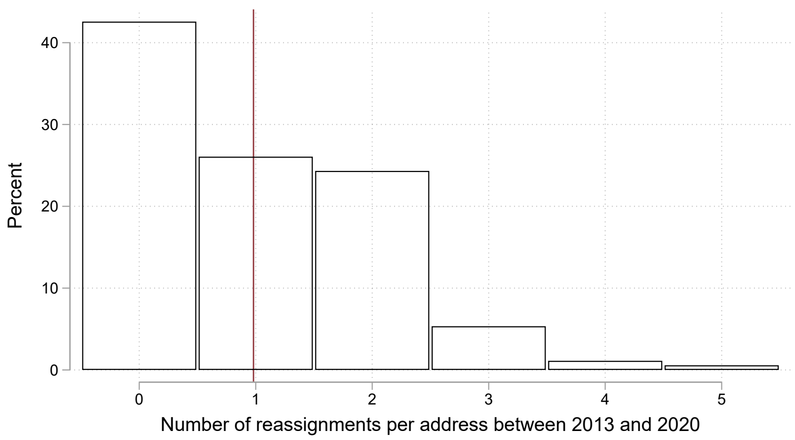
Notes: The figure illustrates the activity status of polling places in each election. We observe 293 distinct venues between 2013 and 2020. The 2009 European and Federal Elections are not part of our estimation sample (highlighted).

Figure D.3: Distribution of Precinct Size



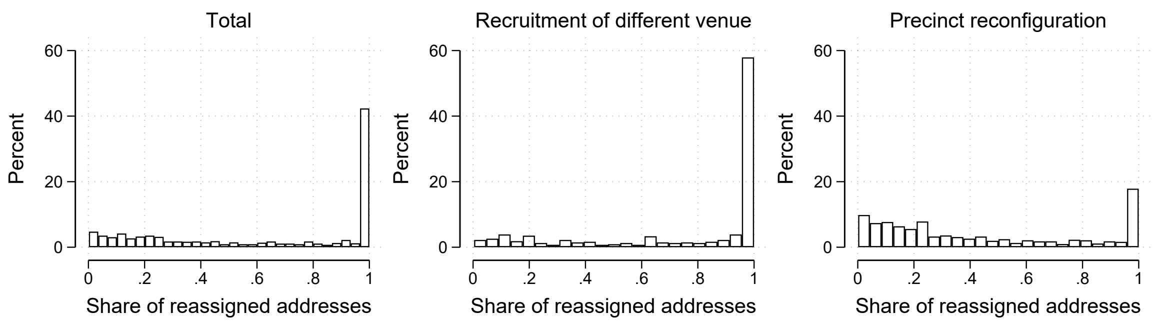
Notes: The figure plots the distribution of precinct of size (number of eligible voters) across all elections (left plot) and before and after 2017 when the election office performed a major reconfiguration of precinct boundaries (right plot). Precincts are delineated according to their election-specific boundaries (i.e., before harmonization of precinct borders). The vertical line in the left plot highlights the median of the distribution.

Figure D.4: Frequency of Polling Place Reassignments per Residential Address



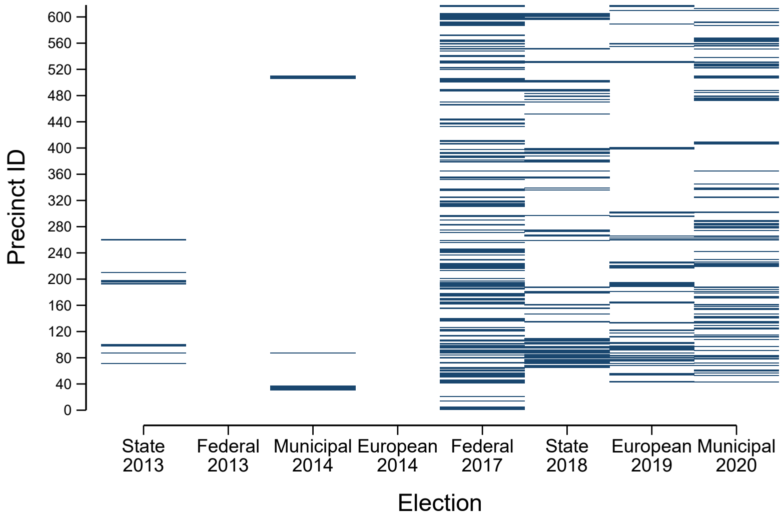
Notes: The figure plots the frequency of polling place reassignments (relative to the previous election) for residential addresses between 2013 and 2020. The vertical line highlights the mean.

Figure D.5: Reassignment Intensity at the Precinct Level



Notes: The figure shows the distribution of the share of residential addresses assigned to a different polling place relative to the preceding election at the precinct level overall (left plot) and by reason of reassignment, i.e., due to recruitment of a different polling venue (middle) or due to reconfiguration of precincts (right). Observations with zero reassignments are excluded.

Figure D.6: Timing of Polling Place Reassignments



Out of **618** precincts:

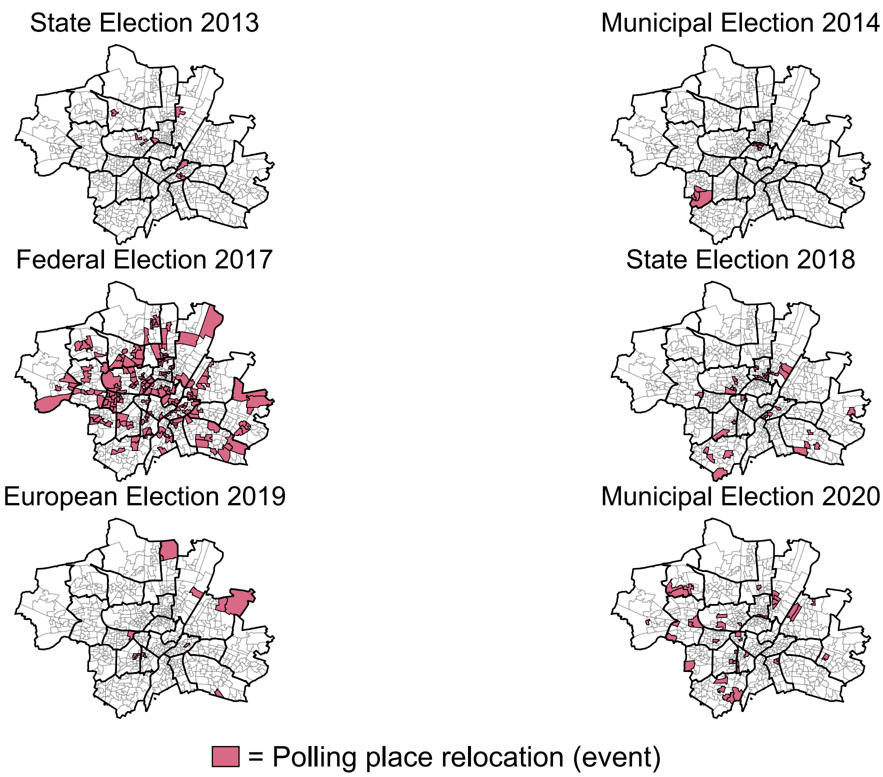
- 338 never-treated units
- 150 treated **once**
- 130 treated more than once

Timing of **first** full polling place reassignment:

- 61.8%: Federal 2017
- 14.3%: Municipal 2020
- 12.5%: State 2018
- 4.3%: European 2019
- 3.6%: State 2013
- 3.6%: Municipal 2014

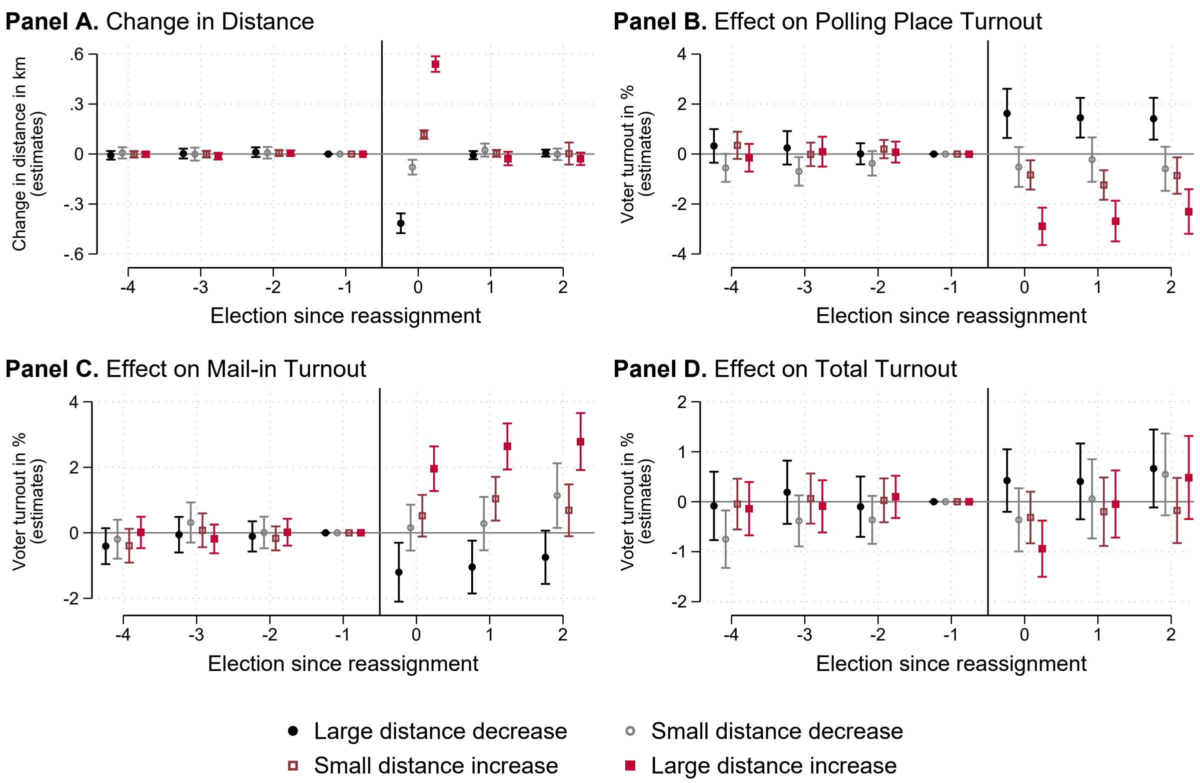
Notes: The figure illustrates the timing of polling place relocations (relative to the previous election) for the 618 precincts in our sample. Highlighted cells indicate that the entire precinct, i.e., 100% of home addresses, is assigned to a different polling place.

Figure D.7: Spatial Distribution of Polling Place Reassignments



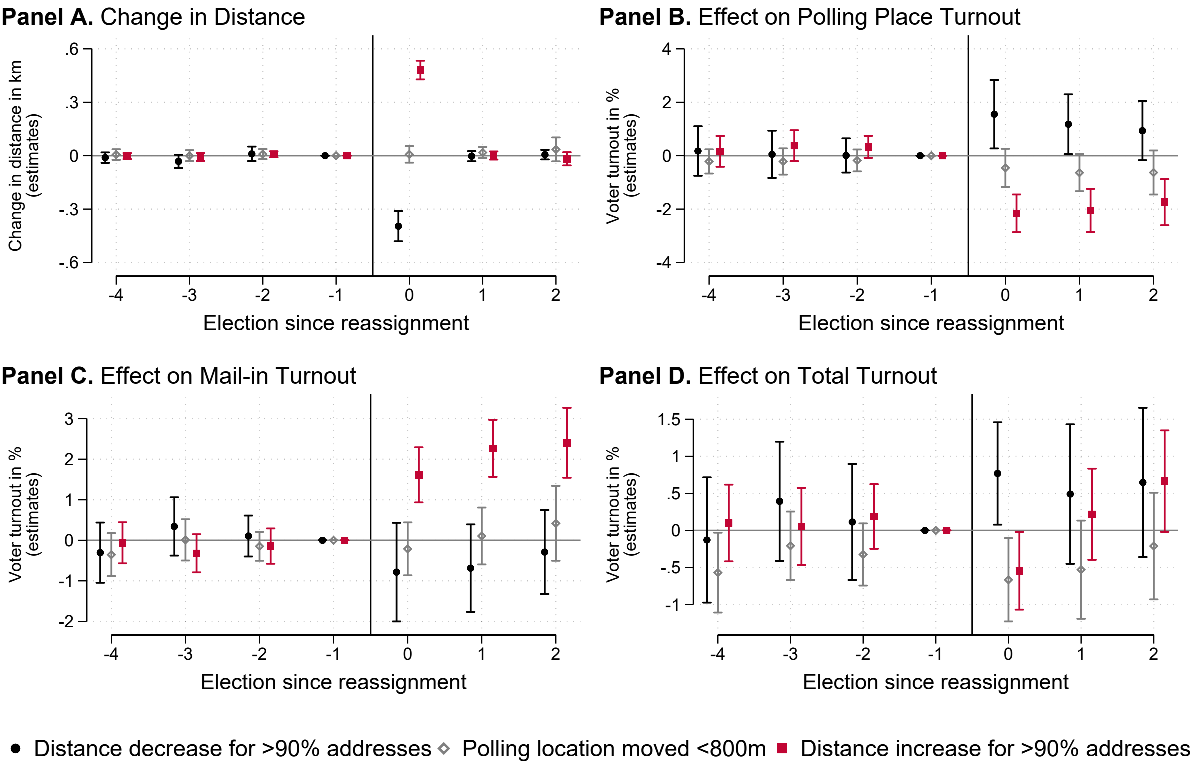
Notes: The maps illustrate the timing of polling place relocations (relative to the previous election) for the 618 precincts in our sample. Precinct boundaries are harmonized to the 2018 delineation to allow comparisons over time. Highlighted precincts indicate that the entire precinct, i.e., 100% of home addresses, is assigned to a different polling place for the first time in our panel. There were no relocations in the 2013 Federal Election and 2014 European Election.

Figure D.8: Effect Heterogeneity by Change in Proximity to the Polling Location



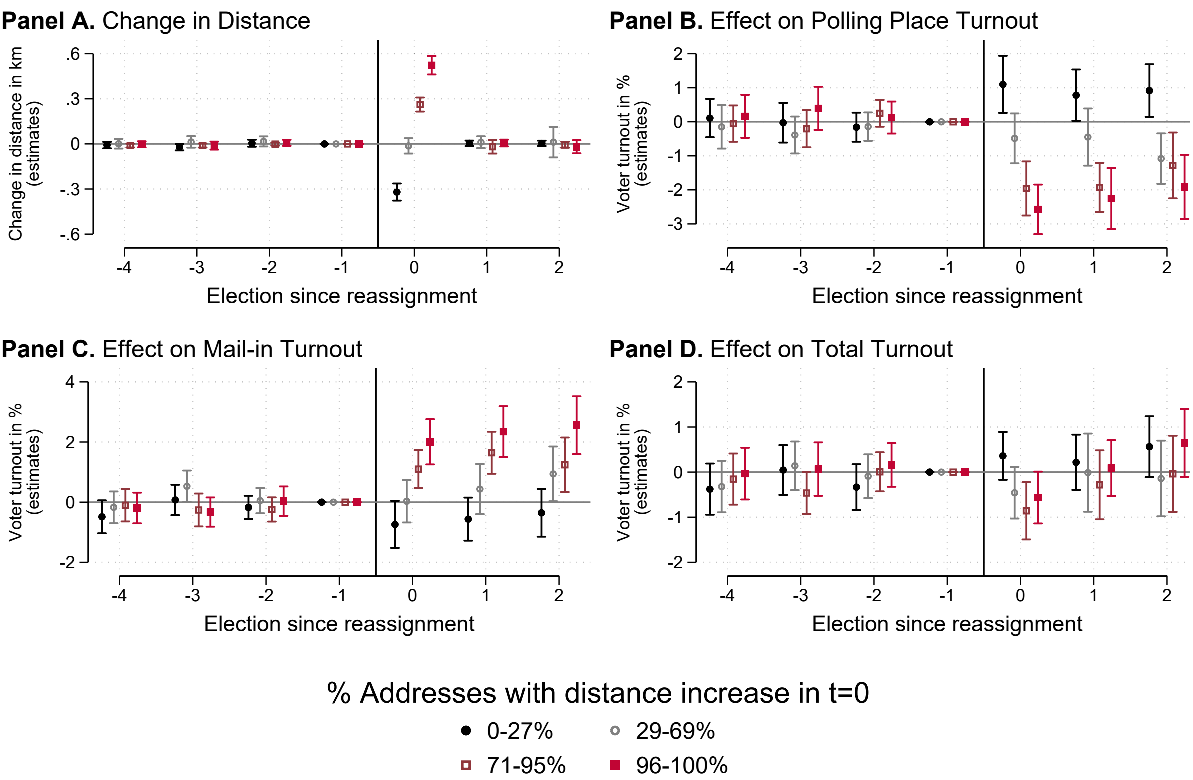
Notes: The figure presents event study results based on a version of Equation 2 in which event-time dummies are interacted separately with four mutually exclusive treatment indicators: two for distance increase and two for distance decrease due to reassignment. The event is defined as the first time in which the entire precinct is reassigned to a different polling place. Regressions are weighted by the number of eligible voters. Confidence intervals are drawn at the 95 percent level using standard errors clustered at the precinct level.

Figure D.9: Effect Heterogeneity by Change in Proximity Restricted to Cases with Consistent Distance Changes



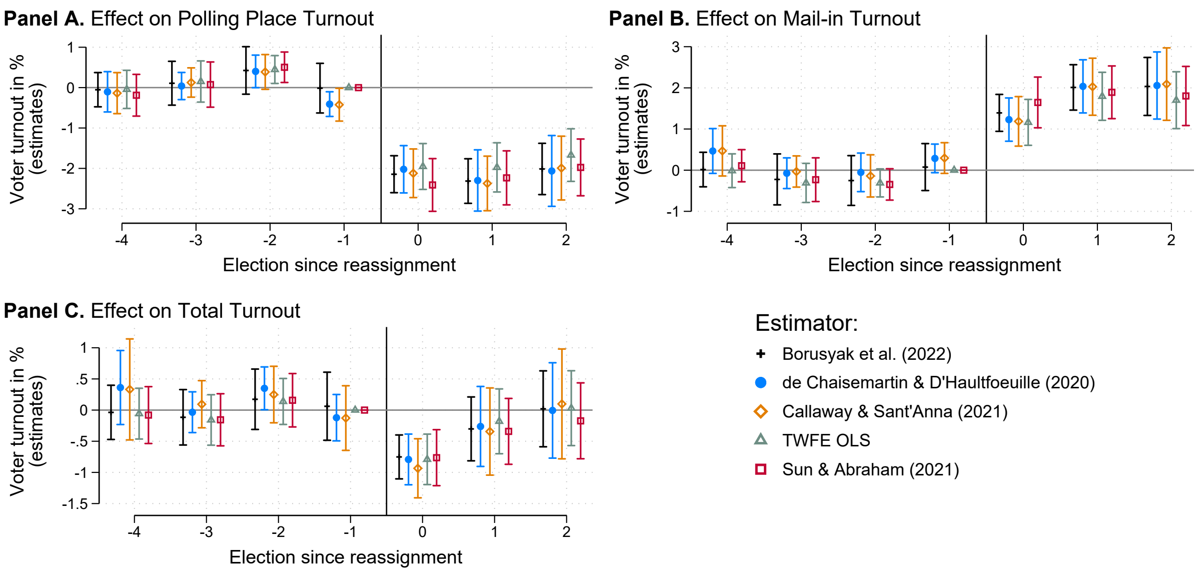
Notes: The figure presents event study results based on a version of Equation 2 in which event-time dummies are interacted separately with three mutually exclusive treatment indicators, identifying precincts where reassignments consistently increased (decreased) the distance for at least 90 percent of home addresses and where the polling place moved less than 800 meters from the old location. The event is defined as the first time in which the entire precinct is reassigned to a different polling place. Regressions are weighted by the number of eligible voters. Confidence intervals are drawn at the 95 percent level using standard errors clustered at the precinct level.

Figure D.10: Effect Heterogeneity by Share of Addresses with Distance Increase



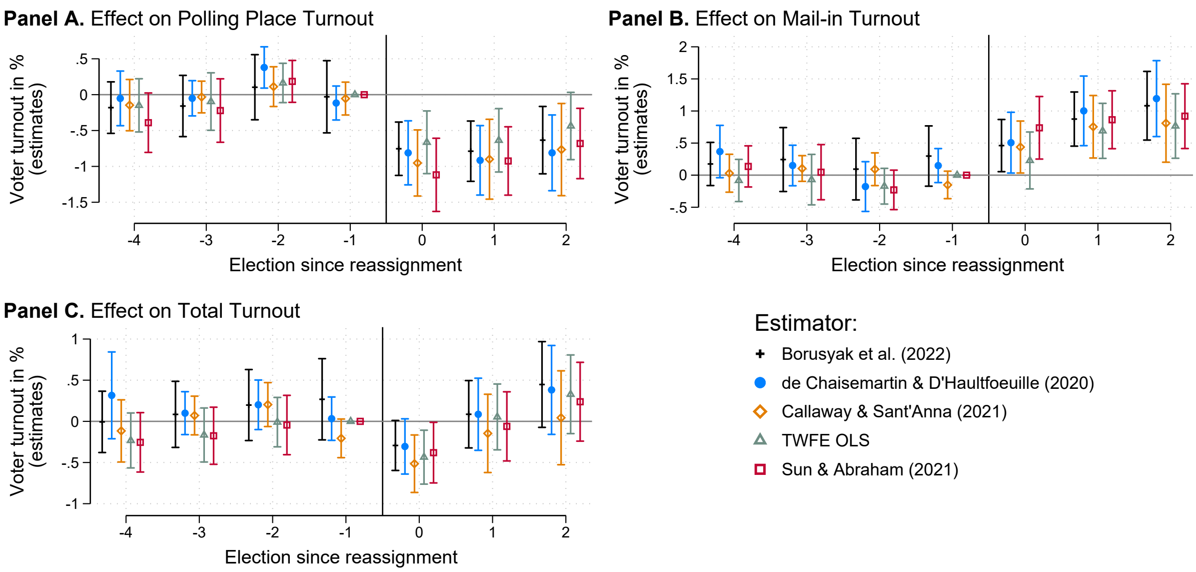
Notes: The figure presents event study results based on a version of Equation 2 in which event-time dummies are interacted separately with four mutually exclusive treatment indicators, identifying precincts belonging to each quartile of the distribution of the share of addresses that experienced a distance increase when the polling location changed. The event is defined as the first time in which the entire precinct is reassigned to a different polling place. Regressions are weighted by the number of eligible voters. Confidence intervals are drawn at the 95 percent level using standard errors clustered at the precinct level.

Figure D.11: Event Study Results Restricted to Units with Increased Distance



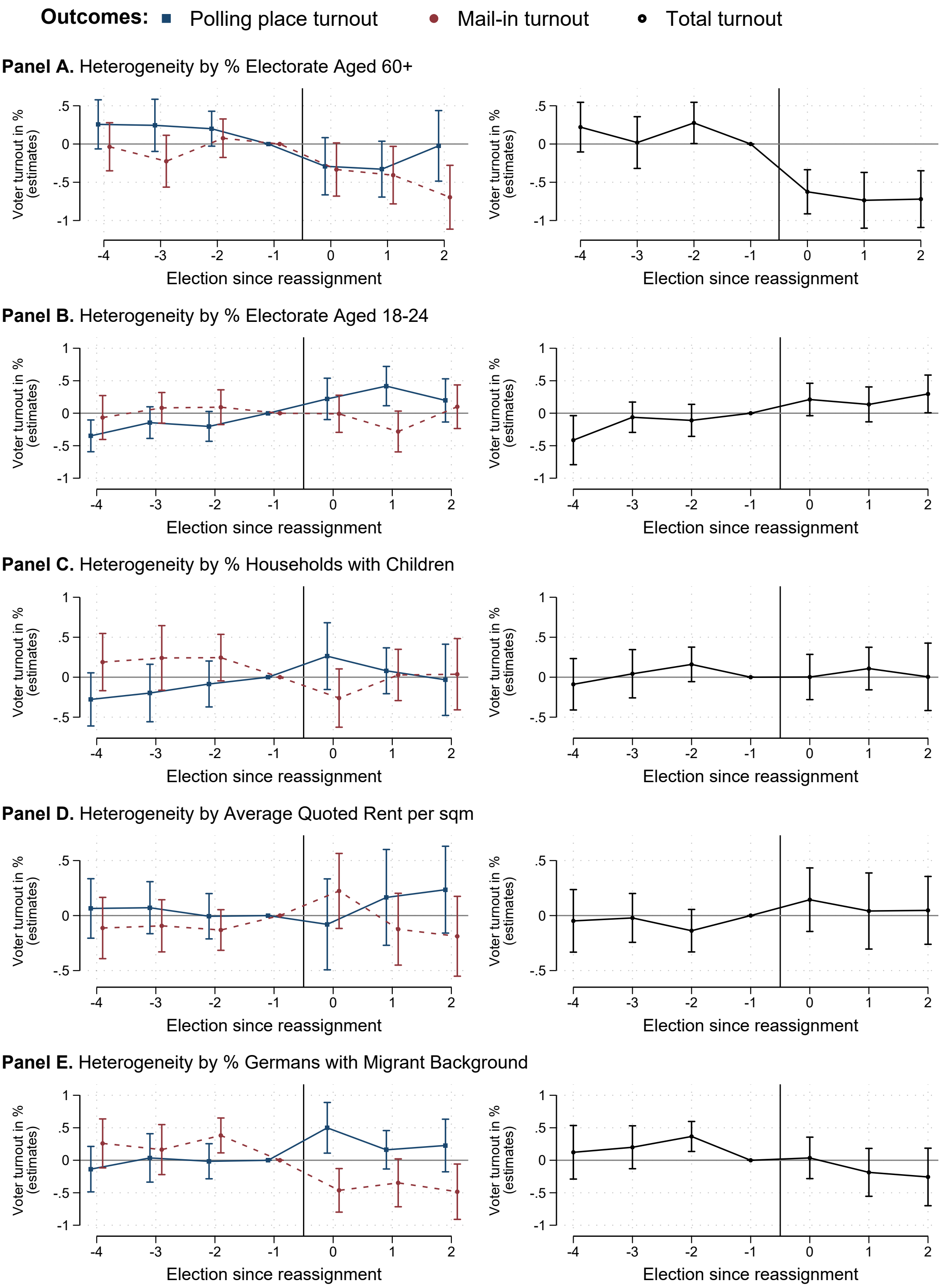
Notes: The figure presents event study results based on Equation 2 (using election fixed effects instead of election×district fixed effect). The treatment group is restricted to units where reassignments caused a distance increase to the polling place. The model is estimated using TWFE-OLS as well as the estimators proposed by Borusyak et al. (2023), Callaway and Sant'Anna (2021), Sun and Abraham (2021), and de Chaisemartin and D'Haultfœuille (2020). The event is defined as the first time in which the entire precinct is reassigned to a different polling place. Where applicable, specifications include time-varying covariates listed in Section 3.3. Regressions are weighted by the number of eligible voters. Confidence intervals are drawn at the 95 percent level using standard errors clustered at the precinct level.

Figure D.12: Event Study Results Absorbing the Distance Effect



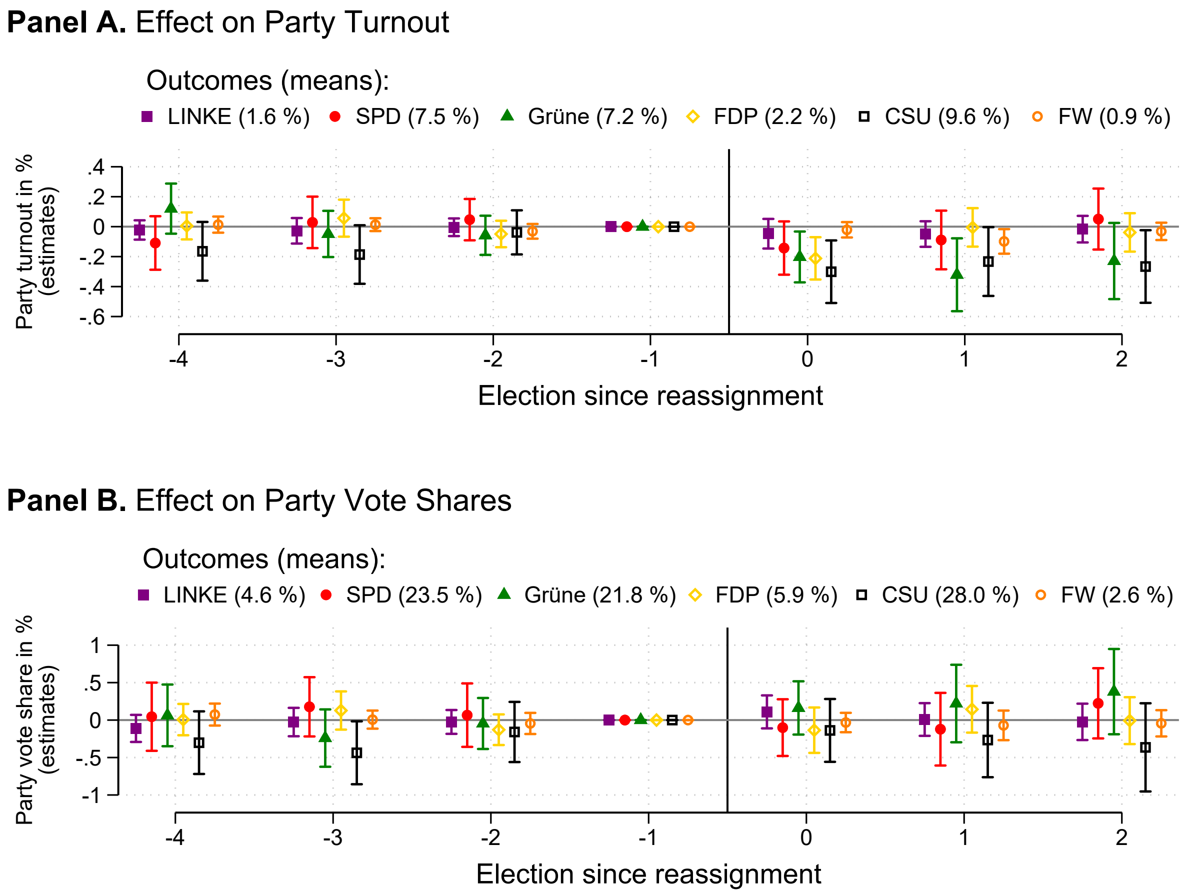
Notes: The figure presents event study results based on Equation 3 (using election fixed effects instead of election×district fixed effect). The model is estimated using TWFE-OLS as well as the estimators proposed by Borusyak et al. (2023), Callaway and Sant'Anna (2021), Sun and Abraham (2021), and de Chaisemartin and D'Haultfœuille (2020). The event is defined as the first time in which the entire precinct is reassigned to a different polling place. Where applicable, specifications include time-varying covariates listed in Section 3.3. Regressions are weighted by the number of eligible voters. Confidence intervals are drawn at the 95 percent level using standard errors clustered at the precinct level.

Figure D.13: Effect Heterogeneity by Precinct Characteristics Conditional on Distance



Notes: The figure presents event study results based on the triple difference estimator introduced in Equation 4 conditional on log walking distance. Each panel uses a different heterogeneity dimension Z_p and plots the triple-difference coefficients $\hat{\gamma}^k$ for the three outcomes: polling place turnout, mail-in turnout, and total turnout. The event is defined as the first time in which the entire precinct is reassigned to a different polling place. Regressions are weighted by the number of eligible voters. Confidence intervals are drawn at the 95 percent level using standard errors clustered at the precinct level.

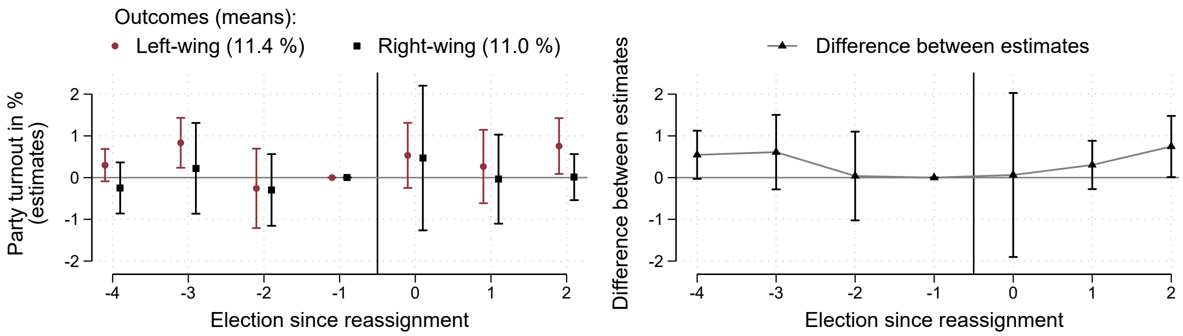
Figure D.14: Differential Effects of Reassignments on Party Outcomes



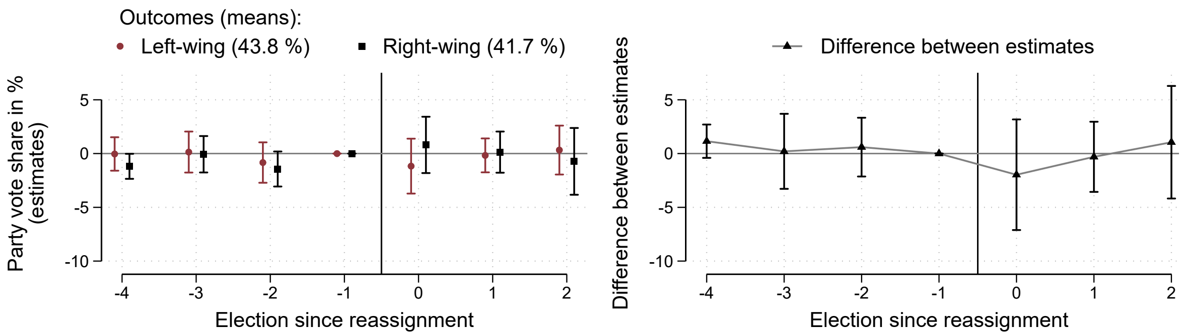
Notes: The figure presents event study results based on Equation 1. The outcomes in Panel A are party turnout defined as the number of votes relative to the number of eligible voters for the six largest parties that stood election in every election included in our panel, respectively. Dependent variables in Panel B are party vote shares, defined as the number of votes relative to total votes. Turnout and party shares capture only voting at the polling place. The event is defined as the first time in which the entire precinct is reassigned to a different polling place. Regressions are weighted by the number of eligible voters. Confidence intervals are drawn at the 95 percent level using standard errors clustered at the precinct level.

Figure D.15: Effects of Reassignments on Party Outcomes by Mail

Panel A. Effect on Party Turnout



Panel B. Effect on Party Vote Shares



Notes: The figure presents event study results at the district level. The outcomes are party turnout (Panel A) and party vote shares (Panel B) by mail. Party turnout is defined as the number of votes relative to the number of eligible voters for left-wing and right-wing parties, respectively. Party vote share is defined as the number of votes relative to total votes for left-wing and right-wing parties, respectively. The right plot in each panel presents estimates and confidence bands for the difference between event-time indicators in each period. The event is defined as the first time in which at least 70 percent of the district is reassigned to a different polling place. All specifications include district fixed effects, election fixed effects, and time-varying covariates listed in Section 3.3. Regressions are weighted by the number of eligible voters. Confidence intervals are drawn at the 95 percent level using standard errors clustered at the district level.

Appendix E. Tables

Table E.1: Representativeness of Munich

	Germany			Top 20 cities			Munich
	Min	Mean	Max	Min	Mean	Max	Value
Population (in thsd)	34.19	207.40	3669.49	315.29	843.63	3669.49	1484.23
Population density (in inhab. per sqkm)	612.00	2169.72	6439.00	2626.00	4426.80	6439.00	6439.00
% Population Aged 65+	0.15	0.22	0.32	0.15	0.19	0.22	0.17
% Working Age Population (15-65)	0.54	0.64	0.71	0.63	0.67	0.70	0.68
% College Educated	0.22	0.37	0.76	0.30	0.55	0.75	0.69
% Foreigners	0.02	0.10	0.35	0.08	0.18	0.29	0.26
% Self Employed	0.03	0.10	0.39	0.06	0.08	0.12	0.10
Median Income (in euros)	2183.00	3064.95	4635.00	2807.00	3530.25	4351.00	4169.00
Turnout, Federal Election 2017 (in %)	64.10	75.84	84.40	68.70	75.77	82.30	78.50

Notes: The table reports summary statistics on sociodemographic characteristics across all German counties, the top 20 largest cities, and Munich. Data are from Destatis and the German Federal Employment Agency, 2017–2019.

Table E.2: Summary Statistics of Precinct Characteristics

	Mean	Std. Dev.	Min	p25	Median	p75	Max
Outcome Variables							
Polling Place Turnout	34.24	9.04	9.94	26.18	35.54	41.70	55.86
Mail-in Turnout (Requested Polling Cards)	28.92	7.64	4.01	23.10	29.46	34.70	51.99
Total Turnout	63.15	14.57	15.10	51.20	65.27	75.26	91.72
Variables of Interest							
Avg. Walking Distance to the Polling Place (in km)	0.71	0.34	0.16	0.47	0.64	0.87	2.56
Share of Reassigned Residential Addresses	0.14	0.32	0.00	0.00	0.00	0.00	1.00
Share Reassigned (Precinct Reconfiguration)	0.05	0.19	0.00	0.00	0.00	0.00	1.00
Share Reassigned (Recruitment of Polling Location)	0.08	0.26	0.00	0.00	0.00	0.00	1.00
Other Precinct Characteristics							
# Residents	2428	403	758	2169	2325	2591	6272
% Residents Eligible to Vote	65.35	9.15	24.62	60.22	66.42	71.70	86.93
% Non-native German Residents	14.68	4.35	5.50	11.70	13.48	16.45	35.78
% Native German Residents	59.77	11.35	21.00	52.75	61.80	68.11	83.97
% EU Foreigners	12.90	3.97	4.00	10.13	12.38	14.99	36.05
% Non-EU Foreigners	12.66	6.18	1.91	7.97	11.49	16.06	50.82
% Single Residents	49.73	7.34	35.28	43.72	48.84	55.02	80.20
% Married Residents	37.29	6.49	15.50	32.28	37.43	42.77	51.84
% Electorate Aged 18-24	8.74	2.87	2.41	7.20	8.25	9.64	49.07
% Electorate Aged 25-34	21.15	6.57	7.40	15.73	20.83	26.01	42.30
% Electorate Aged 35-44	17.92	4.00	6.30	15.23	17.37	20.08	34.70
% Electorate Aged 45-59	24.62	3.97	4.85	21.97	24.40	27.25	45.32
% Electorate Aged 60+	27.57	8.39	2.61	21.30	27.57	33.29	63.80
% EU Foreigners in the Electorate	8.29	9.13	0.00	0.00	2.70	15.81	46.39
% Households with Children	17.53	6.08	5.31	13.35	16.69	20.43	58.75
Avg. Duration of Residence (in years)	21.69	4.45	6.80	18.53	21.72	24.51	45.11
Avg. Quoted Rent per sqm (in Euros)	17.42	4.54	6.69	13.67	16.45	20.30	43.92

Notes: The table reports summary statistics based on 4,944 observations (618 precincts with harmonized boundaries observed over eight elections between 2013 and 2020). The statistics are *not* weighted and might thus differ from values reported in the text.

Table E.3: Reassignment Timing and Changes in Precinct Characteristics

	(1) Indicator (%Reassigned =100)	(2) Indicator (%Reassigned >0)	(3) Share Reassigned	(4) Share Reassigned (Precinct Reconfig.)	(5) Share Reassigned (Recruitment)	(6) Log Avg. Walking Distance
# Residents	0.015	-0.002	0.023	0.012	0.010	-0.003
	(0.014)	(0.018)	(0.014)	(0.011)	(0.013)	(0.012)
# Single Residents	0.017	0.009	0.031	0.019	0.012	0.008
	(0.017)	(0.020)	(0.016)	(0.013)	(0.015)	(0.015)
# Married Residents	0.007	-0.019	0.015	0.001	0.014	-0.014
	(0.016)	(0.024)	(0.018)	(0.012)	(0.016)	(0.016)
# Native German Residents	0.007	-0.017	0.007	-0.005	0.012	-0.002
	(0.010)	(0.015)	(0.012)	(0.007)	(0.011)	(0.012)
# Non-native German Residents	0.021	-0.010	0.028	0.009	0.019	-0.033
	(0.020)	(0.028)	(0.021)	(0.015)	(0.018)	(0.017)
# Foreign Residents	0.016	0.016	0.028	0.026	0.002	0.006
	(0.019)	(0.020)	(0.018)	(0.016)	(0.014)	(0.015)
# Eligible Voters	0.009	-0.006	0.009	-0.008	0.017	-0.007
	(0.013)	(0.016)	(0.012)	(0.008)	(0.011)	(0.012)
# Eligible Voters Aged 18-24	0.010	0.001	0.003	0.001	0.003	0.012
	(0.010)	(0.012)	(0.010)	(0.006)	(0.009)	(0.008)
# Eligible Voters Aged 25-34	0.005	0.012	0.016	-0.007	0.023	0.016
	(0.012)	(0.015)	(0.012)	(0.007)	(0.012)	(0.013)
# Eligible Voters Aged 35-44	-0.003	-0.006	0.009	-0.002	0.011	-0.004
	(0.009)	(0.012)	(0.009)	(0.006)	(0.009)	(0.008)
# Eligible Voters Aged 45-59	0.015	-0.014	0.013	-0.002	0.015	-0.010
	(0.010)	(0.013)	(0.010)	(0.007)	(0.009)	(0.009)
# Eligible Voters Aged 60+	0.009	-0.012	-0.004	0.001	-0.004	-0.023*
	(0.013)	(0.015)	(0.013)	(0.010)	(0.011)	(0.012)
# German Eligible Voters	0.012	-0.006	0.011	-0.003	0.014	-0.010
	(0.009)	(0.012)	(0.009)	(0.005)	(0.009)	(0.010)
# EU Foreigners in the Electorate	0.002	-0.004	0.009	-0.002	0.010	0.006
	(0.010)	(0.015)	(0.010)	(0.007)	(0.010)	(0.008)
# Within Migration	-0.003	0.001	-0.004	-0.002	-0.003	0.004
	(0.006)	(0.008)	(0.007)	(0.003)	(0.006)	(0.004)
# Outmigration	0.011	0.012	0.013	0.015	-0.002	-0.006
	(0.012)	(0.012)	(0.010)	(0.011)	(0.009)	(0.009)
# Inmigration	0.004	0.004	0.002	0.011	-0.009	0.011
	(0.010)	(0.016)	(0.011)	(0.008)	(0.010)	(0.006)
% Households with Children	-0.003	-0.002	0.020	0.016	0.003	0.023
	(0.021)	(0.025)	(0.022)	(0.013)	(0.020)	(0.020)
Avg. Quoted Rent per sqm	0.015	-0.005	0.005	-0.006	0.011	0.005
	(0.012)	(0.015)	(0.012)	(0.008)	(0.011)	(0.011)
Avg. Duration of Residence	0.001	-0.006	-0.004	0.001	-0.005	-0.011
	(0.011)	(0.014)	(0.011)	(0.007)	(0.010)	(0.012)
Observations	4,944	4,944	4,944	4,944	4,944	4,944
F-test on joint insignificance $[Pr > F]$	0.54 [0.95]	0.70 [0.83]	0.59 [0.92]	0.94 [0.54]	0.55 [0.95]	1.21 [0.24]
Precinct FE	×	×	×	×	×	×
Election FE	×	×	×	×	×	×

Notes: Each cell in Columns (1)–(6) reports an OLS estimate from a separate univariate regression on precinct characteristics (in rows), conditional on election and precinct fixed effects. All precinct characteristics are standardized to have mean zero and unitary standard deviation. The dependent variables are a dummy identifying reassignments that affected 100% of home addresses in a precinct (Column 1), a dummy identifying reassignments that affected a nonzero share of addresses (Column 2), the share of addresses assigned to a different polling place (Column 3), the share of addresses reassigned due to precinct reconfiguration (Column 4), the share of addresses reassigned due to the recruitment of a different polling place (Column 5), and the log average walking distance to the polling location (Column 6), respectively. Migration variables refer to the number of moves within and across precinct boundaries, respectively. F−tests for the null that coefficients are jointly equal to zero are reported with p values in brackets. Regressions are weighted by the number of eligible voters. Standard errors are clustered at the precinct level and reported in parentheses. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$.

Table E.4: Reassignment Timing and Changes in Precinct Characteristics (Non-standardized)

	(1) Indicator (%Reassigned =100)	(2) Indicator (%Reassigned >0)	(3) Share Reassigned	(4) Share Reassigned (Precinct Reconfig.)	(5) Share Reassigned (Recruitment)	(6) Log Avg. Walking Distance
# Residents	0.038	-0.006	0.056	0.030	0.026	-0.008
	(0.034)	(0.044)	(0.035)	(0.028)	(0.031)	(0.030)
# Single Residents	0.063	0.033	0.112	0.069	0.042	0.030
	(0.060)	(0.074)	(0.060)	(0.046)	(0.056)	(0.055)
# Married Residents	0.035	-0.092	0.071	0.003	0.067	-0.068
	(0.077)	(0.112)	(0.085)	(0.058)	(0.076)	(0.074)
# Native German Residents	0.043	-0.112	0.044	-0.033	0.077	-0.011
	(0.066)	(0.096)	(0.077)	(0.044)	(0.071)	(0.080)
# Non-native German Residents	0.123	-0.059	0.169	0.053	0.116	-0.197
	(0.120)	(0.168)	(0.125)	(0.088)	(0.108)	(0.101)
# Foreign Residents	0.048	0.046	0.082	0.076	0.006	0.019
	(0.057)	(0.059)	(0.053)	(0.046)	(0.043)	(0.044)
# Eligible Voters	0.042	-0.029	0.040	-0.038	0.078	-0.031
	(0.059)	(0.074)	(0.057)	(0.040)	(0.054)	(0.054)
# Eligible Voters Aged 18-24	0.198	0.011	0.070	0.014	0.056	0.238
	(0.198)	(0.248)	(0.203)	(0.131)	(0.177)	(0.167)
# Eligible Voters Aged 25-34	0.046	0.108	0.148	-0.060	0.208	0.139
	(0.111)	(0.136)	(0.112)	(0.067)	(0.108)	(0.115)
# Eligible Voters Aged 35-44	-0.049	-0.094	0.128	-0.026	0.154	-0.051
	(0.130)	(0.171)	(0.138)	(0.086)	(0.129)	(0.121)
# Eligible Voters Aged 45-59	0.208	-0.196	0.179	-0.026	0.205	-0.132
	(0.145)	(0.176)	(0.144)	(0.102)	(0.127)	(0.121)
# Eligible Voters Aged 60+	0.063	-0.089	-0.026	0.005	-0.030	-0.170*
	(0.096)	(0.111)	(0.095)	(0.071)	(0.078)	(0.085)
# German Eligible Voters	0.087	-0.045	0.078	-0.021	0.098	-0.069
	(0.062)	(0.083)	(0.066)	(0.039)	(0.062)	(0.069)
# EU Foreigners in the Electorate	0.012	-0.024	0.054	-0.012	0.066	0.040
	(0.067)	(0.092)	(0.065)	(0.046)	(0.065)	(0.050)
# Within Migration	-0.000	0.000	-0.000	-0.000	-0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
# Outmigration	0.000	0.000	0.000	0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
# Inmigration	0.000	0.000	0.000	0.000	-0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
% Households with Children	-0.000	-0.000	0.003	0.003	0.001	0.004
	(0.004)	(0.004)	(0.004)	(0.002)	(0.003)	(0.003)
Avg. Quoted Rent per sqm (Euros)	0.003	-0.001	0.001	-0.001	0.002	0.001
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)
Avg. Duration of Residence (years)	0.000	-0.001	-0.001	0.000	-0.001	-0.003
	(0.002)	(0.003)	(0.003)	(0.002)	(0.002)	(0.003)
Observations	4,944	4,944	4,944	4,944	4,944	4,944
F-test on joint insignificance $[Pr > F]$	0.54 [0.95]	0.70 [0.83]	0.59 [0.92]	0.94 [0.54]	0.55 [0.95]	1.21 [0.24]
Precinct FE	×	×	×	×	×	×
Election FE	×	×	×	×	×	×

Notes: Each cell in Columns (1)–(6) reports an OLS estimate from a separate univariate regression on precinct characteristics (in rows), conditional on election and precinct fixed effects. The dependent variables are a dummy identifying reassignments that affected 100% of home addresses in a precinct (Column 1), a dummy identifying reassignments that affected a nonzero share of addresses (Column 2), the share of addresses assigned to a different polling place (Column 3), the share of addresses reassigned due to precinct reconfiguration (Column 4), the share of addresses reassigned due to the recruitment of a different polling place (Column 5), and the log average walking distance to the polling location (Column 6), respectively. Migration variables refer to the number of moves within and across precinct boundaries, respectively. F−tests for the null that coefficients are jointly equal to zero are reported with p values in brackets. Regressions are weighted by the number of eligible voters. Standard errors are clustered at the precinct level and reported in parentheses. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$.

Table E.5: Difference between Event-Time Indicators in Period 1 and Period 0

	(1) Mail-in Turnout	(2) Polling Place Turnout	(3) Total Turnout
Panel A: Differences based on event study estimates restricted to precincts with increased distance			
TWFE-OLS	0.63**	-0.02	0.61**
BJS (2022)	0.62**	-0.17	0.45*
dChDH (2020)	0.81**	-0.28	0.53*
SA (2021)	0.25	0.18	0.42*
CS (2021)	0.84**	-0.25	0.59*
Panel B: Differences based on event study estimates conditional on log distance			
TWFE-OLS	0.46**	0.03	0.49**
BJS (2022)	0.41*	-0.03	0.38*
dChDH (2020)	0.50*	-0.10	0.39*
SA (2021)	0.13	0.19	0.32
CS (2021)	0.32	0.05	0.37

Notes: The table reports the difference between treatment effects estimates of the second and the first post-reassignment election ($\hat{\mu}^1 - \hat{\mu}^0$) for mail-in turnout, polling place turnout, and total turnout according to the TWFE-OLS estimator and four novel estimators proposed by Borusyak et al. (2023, BJS), de Chaisemartin and D'Haultfœuille (2020, dCDH), Sun and Abraham (2021, SA), and Callaway and Sant'Anna (2021, CS), respectively. Estimates in Panel A are obtained on a sample restricted to never-treated precincts and precincts in which reassignments *increased* average distance to the polling location. Estimates in Panel B are conditional on log walking distance. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$.

Table E.6: Heterogeneity by Precinct Characteristics–Triple Difference Estimates

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Effect on Turnout at the Polling Place						
$Z_p =$	% electorate aged 60+	% electorate aged 18-24	% households with children	Average quoted rent per sqm	% non-native German residents	Average duration of residence
$Z_p \times$						
Reassignment $(t-4)$	0.24	-0.31*	-0.24	0.05	-0.06	0.29
	(0.17)	(0.13)	(0.16)	(0.14)	(0.18)	(0.16)
Reassignment $(t-3)$	0.22	-0.12	-0.18	0.06	0.09	0.23
	(0.17)	(0.12)	(0.18)	(0.12)	(0.19)	(0.16)
Reassignment $(t-2)$	0.19	-0.21	-0.10	-0.02	0.00	0.15
	(0.12)	(0.12)	(0.15)	(0.11)	(0.14)	(0.12)
Reassignment $(t+0)$	-0.43	0.33	0.38	-0.17	0.74**	-0.29
	(0.23)	(0.18)	(0.26)	(0.26)	(0.25)	(0.23)
Reassignment $(t+1)$	-0.49*	0.59**	0.26	-0.02	0.55**	-0.59**
	(0.21)	(0.19)	(0.21)	(0.26)	(0.20)	(0.20)
Reassignment $(t+2)$	-0.17	0.45*	0.17	-0.05	0.69**	-0.48*
	(0.26)	(0.22)	(0.36)	(0.24)	(0.25)	(0.23)
R^2	0.97	0.97	0.97	0.97	0.97	0.97
Panel B: Effect on Turnout via Mail						
$Z_p =$	% electorate aged 60+	% electorate aged 18-24	% households with children	Average quoted rent per sqm	% non-native German residents	Average duration of residence
$Z_p \times$						
Reassignment $(t-4)$	-0.03	-0.10	0.16	-0.10	0.20	0.08
	(0.16)	(0.16)	(0.18)	(0.14)	(0.19)	(0.15)
Reassignment $(t-3)$	-0.21	0.06	0.23	-0.09	0.12	-0.06
	(0.17)	(0.12)	(0.20)	(0.12)	(0.19)	(0.15)
Reassignment $(t-2)$	0.08	0.10	0.26	-0.12	0.37**	0.35**
	(0.13)	(0.14)	(0.14)	(0.09)	(0.13)	(0.11)
Reassignment $(t+0)$	-0.23	-0.09	-0.35	0.29	-0.64***	-0.12
	(0.20)	(0.17)	(0.21)	(0.21)	(0.19)	(0.20)
Reassignment $(t+1)$	-0.28	-0.41*	-0.11	0.02	-0.63**	0.15
	(0.22)	(0.18)	(0.20)	(0.21)	(0.21)	(0.22)
Reassignment $(t+2)$	-0.58*	-0.09	-0.11	0.03	-0.83***	-0.25
	(0.23)	(0.19)	(0.28)	(0.21)	(0.20)	(0.24)
R^2	0.96	0.96	0.96	0.96	0.96	0.96
Panel C: Effect on Total Turnout						
$Z_p =$	% electorate aged 60+	% electorate aged 18-24	% households with children	Average quoted rent per sqm	% non-native German residents	Average duration of residence
$Z_p \times$						
Reassignment $(t-4)$	0.22	-0.41*	-0.08	-0.05	0.14	0.37
	(0.17)	(0.20)	(0.16)	(0.15)	(0.21)	(0.19)
Reassignment $(t-3)$	0.01	-0.06	0.05	-0.02	0.21	0.17
	(0.17)	(0.12)	(0.15)	(0.11)	(0.17)	(0.15)
Reassignment $(t-2)$	0.27*	-0.11	0.16	-0.14	0.37**	0.50***
	(0.14)	(0.13)	(0.11)	(0.10)	(0.12)	(0.13)
Reassignment $(t+0)$	-0.65***	0.24	0.03	0.12	0.10	-0.41*
	(0.15)	(0.12)	(0.16)	(0.15)	(0.18)	(0.16)
Reassignment $(t+1)$	-0.77***	0.17	0.15	-0.00	-0.09	-0.44*
	(0.18)	(0.14)	(0.14)	(0.17)	(0.20)	(0.19)
Reassignment $(t+2)$	-0.75***	0.35*	0.05	-0.02	-0.14	-0.73***
	(0.19)	(0.16)	(0.24)	(0.16)	(0.25)	(0.21)
R^2	0.99	0.99	0.99	0.99	0.99	0.99
Observations	4,666	4,666	4,666	4,666	4,666	4,666

Notes: The table reports point estimates and standard errors underlying the plots presented in Figure 10. Results are based on the triple difference estimator presented in Equation 4. Each column in each panel represents a separate specification using a different heterogeneity dimension Z_p, which corresponds to a standardized (mean zero and unitary standard deviation) precinct characteristic measured in 2013. The dependent variables are voter turnout (0–100) at the polling place (Panel A), by mail (Panel B), and overall (Panel C). The event is defined as the first time in which the entire precinct is reassigned to a different polling place. Regressions are weighted by the number of eligible voters. Standard errors are clustered at the precinct level and reported in parentheses. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$.

Table E.7: Summary Statistics and Correlations among Precinct Characteristics, 2013

	Summary Statistics				Correlations					
	Mean	Std. Dev.	Min	Max	% Electorate Aged 60+	% Electorate Aged 18-24	% Households with Children	Avg. Quoted Rent per sqm	% Non-native German Residents	Avg. Duration of Residence
% Electorate Aged 60+	29.957	8.396	8.800	61.388	1.000					
% Electorate Aged 18-24	8.835	2.676	3.964	34.440	-0.314	1.000				
					(0.000)					
% Households with Children	17.286	6.433	5.314	58.748	0.026	0.073	1.000			
					(0.526)	(0.068)				
Avg. Quoted Rent per sqm	13.416	1.513	9.403	23.411	-0.255	-0.035	-0.339	1.000		
					(0.000)	(0.387)	(0.000)			
% Non-native German Residents	13.805	4.076	6.599	33.098	0.139	0.180	0.564	-0.285	1.000	
					(0.001)	(0.000)	(0.000)	(0.000)		
Avg. Duration of Residence	22.430	4.880	7.409	45.109	0.661	-0.172	0.096	-0.280	0.183	1.000
					(0.000)	(0.000)	(0.016)	(0.000)	(0.000)	

Notes: The table reports summary statistics and correlations among precincts characteristics used in the heterogeneity analysis in Section 5. Variables are measured in 2013, are not standardized, and not weighted. N =618 precincts. p-values are reported in parentheses.

Table E.8: Balanced Sample Results–Pooled Reassignments

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
	Polling Place Turnout						Mail-in Turnout						Total Turnout					
Reassignment ($t-4$)	0.03	0.02	0.00	-0.04	0.07	0.04	-0.21	-0.20	-0.05	-0.06	-0.17	-0.18	-0.18	-0.18	-0.05	-0.09	-0.10	-0.15
	(0.21)	(0.21)	(0.23)	(0.23)	(0.24)	(0.25)	(0.20)	(0.20)	(0.19)	(0.19)	(0.19)	(0.19)	(0.21)	(0.21)	(0.24)	(0.24)	(0.24)	(0.25)
Reassignment ($t-3$)	-0.18	-0.19	-0.09	-0.13	0.05	0.02	0.22	0.24	0.09	0.07	-0.02	-0.04	0.05	0.05	-0.00	-0.05	0.03	-0.03
	(0.21)	(0.21)	(0.24)	(0.26)	(0.25)	(0.26)	(0.18)	(0.18)	(0.23)	(0.23)	(0.24)	(0.24)	(0.19)	(0.20)	(0.24)	(0.25)	(0.26)	(0.27)
Reassignment ($t-2$)	-0.04	-0.03	0.13	0.14	0.17	0.19	-0.11	-0.12	-0.13	-0.15	-0.20	-0.23	-0.15	-0.15	0.00	-0.01	-0.03	-0.04
	(0.17)	(0.17)	(0.20)	(0.21)	(0.20)	(0.21)	(0.15)	(0.15)	(0.17)	(0.17)	(0.17)	(0.18)	(0.17)	(0.18)	(0.21)	(0.22)	(0.23)	(0.24)
Reassignment ($t+0$)	-1.14***	-1.15***	-1.53***	-1.67***	-1.42***	-1.57***	0.61*	0.58*	1.29***	1.35***	1.25***	1.32***	-0.53*	-0.56**	-0.24	-0.32	-0.17	-0.25
	(0.28)	(0.28)	(0.33)	(0.34)	(0.35)	(0.37)	(0.26)	(0.26)	(0.29)	(0.30)	(0.30)	(0.31)	(0.21)	(0.21)	(0.25)	(0.26)	(0.26)	(0.28)
Reassignment ($t+1$)	-1.32***	-1.31***	-1.35***	-1.45***	-1.29***	-1.41***	1.14***	1.09***	1.26***	1.32***	1.18***	1.24***	-0.18	-0.22	-0.09	-0.13	-0.11	-0.16
	(0.28)	(0.28)	(0.30)	(0.32)	(0.32)	(0.34)	(0.27)	(0.27)	(0.29)	(0.30)	(0.31)	(0.32)	(0.24)	(0.25)	(0.27)	(0.29)	(0.29)	(0.31)
Reassignment ($t+2$)	-0.84**	-0.86**	-0.87**	-0.86*	-0.81*	-0.80*	1.18***	1.23***	1.40***	1.38***	1.31***	1.28***	0.35	0.37	0.53*	0.51	0.49	0.47
	(0.29)	(0.30)	(0.32)	(0.33)	(0.32)	(0.34)	(0.30)	(0.31)	(0.33)	(0.34)	(0.33)	(0.35)	(0.24)	(0.24)	(0.27)	(0.28)	(0.27)	(0.29)
Observations	3,904	3,872	3,552	3,504	3,504	3,456	3,904	3,872	3,552	3,504	3,504	3,456	3,904	3,872	3,552	3,504	3,504	3,456
Unbalanced sample	×						×						×					
#treated precincts	150	146	106	100	100	94	150	146	106	100	100	94	150	146	106	100	100	94
#control precincts	338	338	338	338	338	338	338	338	338	338	338	338	338	338	338	338	338	338
Balanced sample, $\tau \in$		$[-4,0]$	$[-2,1]$	$[-4,1]$	$[-2,2]$	$[-4,2]$		$[-4,0]$	$[-2,1]$	$[-4,1]$	$[-2,2]$	$[-4,2]$		$[-4,0]$	$[-2,1]$	$[-4,1]$	$[-2,2]$	$[-4,2]$

Notes: The table reports event study results based on Equation 1. All columns exclude treated units with more than one full reassignment. The unbalanced sample includes treated units that were reassigned to a different polling location in 2013/14/17/18/19/20. The balanced sample on $\tau \in [-4,0]$ includes reassignments in 2014/17/18/19/20; balance on $\tau \in [-2,1]$ includes 2014/17/18/19; balance on $\tau \in [-2,2]$ includes 2014/17/18; balance on $\tau \in [-4,2]$ includes 2017/18. Regressions are weighted by the number of eligible voters. Standard errors are clustered at the precinct level and reported in parentheses. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$.

Table E.9: Balanced Sample Results–Effects by Distance

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
	Polling Place Turnout						Mail-in Turnout						Total Turnout					
$\mathbb{1}$(Distance decrease)×																		
Reassignment $(t-4)$	-0.18	-0.19	-0.13	-0.14	0.02	0.02	-0.24	-0.24	0.03	-0.01	-0.06	-0.12	-0.42	-0.43	-0.10	-0.15	-0.03	-0.09
	(0.30)	(0.30)	(0.31)	(0.32)	(0.31)	(0.32)	(0.28)	(0.28)	(0.25)	(0.26)	(0.26)	(0.27)	(0.32)	(0.32)	(0.29)	(0.30)	(0.31)	(0.32)
Reassignment $(t-3)$	-0.26	-0.28	0.04	0.04	0.18	0.19	0.34	0.34	0.08	0.02	-0.02	-0.09	0.08	0.07	0.12	0.06	0.16	0.09
	(0.29)	(0.29)	(0.33)	(0.35)	(0.34)	(0.36)	(0.25)	(0.25)	(0.30)	(0.31)	(0.32)	(0.32)	(0.26)	(0.26)	(0.32)	(0.34)	(0.34)	(0.36)
Reassignment $(t-2)$	-0.26	-0.26	-0.00	0.02	0.06	0.09	-0.03	-0.04	-0.08	-0.14	-0.13	-0.20	-0.29	-0.29	-0.08	-0.12	-0.07	-0.11
	(0.23)	(0.23)	(0.26)	(0.27)	(0.27)	(0.28)	(0.22)	(0.22)	(0.24)	(0.26)	(0.26)	(0.27)	(0.26)	(0.27)	(0.30)	(0.33)	(0.32)	(0.35)
Reassignment $(t+0)$	0.31	0.29	0.17	0.15	0.42	0.41	-0.06	-0.09	0.48	0.43	0.31	0.23	0.25	0.20	0.66*	0.58	0.72*	0.65
	(0.37)	(0.38)	(0.44)	(0.46)	(0.44)	(0.46)	(0.36)	(0.36)	(0.42)	(0.44)	(0.41)	(0.43)	(0.29)	(0.29)	(0.30)	(0.33)	(0.31)	(0.34)
Reassignment $(t+1)$	0.06	0.09	0.14	0.15	0.38	0.41	0.24	0.19	0.34	0.35	0.15	0.15	0.30	0.28	0.48	0.50	0.53	0.56
	(0.38)	(0.38)	(0.40)	(0.44)	(0.40)	(0.43)	(0.37)	(0.37)	(0.39)	(0.41)	(0.39)	(0.42)	(0.34)	(0.34)	(0.35)	(0.39)	(0.37)	(0.41)
Reassignment $(t+2)$	0.46	0.47	0.57	0.62	0.69	0.75	0.40	0.43	0.58	0.55	0.45	0.41	0.86**	0.90**	1.15***	1.17***	1.14***	1.17**
	(0.38)	(0.39)	(0.39)	(0.42)	(0.40)	(0.42)	(0.42)	(0.42)	(0.44)	(0.47)	(0.45)	(0.48)	(0.32)	(0.32)	(0.32)	(0.34)	(0.33)	(0.35)
$\mathbb{1}$(Distance increase)×																		
Reassignment $(t-4)$	0.16	0.14	0.06	0.01	0.10	0.05	-0.19	-0.17	-0.09	-0.08	-0.24	-0.23	-0.03	-0.02	-0.03	-0.06	-0.14	-0.18
	(0.26)	(0.26)	(0.29)	(0.29)	(0.31)	(0.31)	(0.26)	(0.26)	(0.24)	(0.24)	(0.23)	(0.23)	(0.26)	(0.26)	(0.31)	(0.32)	(0.31)	(0.31)
Reassignment $(t-3)$	-0.12	-0.15	-0.21	-0.25	-0.04	-0.09	0.15	0.17	0.11	0.12	-0.02	-0.01	0.03	0.03	-0.10	-0.13	-0.06	-0.11
	(0.27)	(0.26)	(0.31)	(0.32)	(0.30)	(0.31)	(0.22)	(0.22)	(0.29)	(0.30)	(0.30)	(0.30)	(0.25)	(0.25)	(0.30)	(0.30)	(0.32)	(0.33)
Reassignment $(t-2)$	0.11	0.11	0.23	0.24	0.27	0.28	-0.17	-0.18	-0.17	-0.17	-0.26	-0.27	-0.07	-0.07	0.06	0.07	0.01	0.01
	(0.21)	(0.21)	(0.25)	(0.26)	(0.25)	(0.27)	(0.18)	(0.18)	(0.21)	(0.22)	(0.21)	(0.22)	(0.21)	(0.21)	(0.26)	(0.27)	(0.28)	(0.29)
Reassignment $(t+0)$	-2.10***	-2.10***	-2.72***	-2.88***	-2.74***	-2.92***	1.06**	1.03**	1.85***	1.96***	1.93***	2.06***	-1.04***	-1.07***	-0.87**	-0.92**	-0.81*	-0.86*
	(0.34)	(0.34)	(0.36)	(0.37)	(0.38)	(0.39)	(0.33)	(0.33)	(0.33)	(0.34)	(0.36)	(0.36)	(0.26)	(0.26)	(0.32)	(0.33)	(0.34)	(0.36)
Reassignment $(t+1)$	-2.25***	-2.25***	-2.38***	-2.53***	-2.50***	-2.66***	1.74***	1.69***	1.90***	1.97***	1.91***	2.00***	-0.51	-0.56	-0.49	-0.56	-0.58	-0.67
	(0.33)	(0.33)	(0.36)	(0.37)	(0.38)	(0.39)	(0.33)	(0.33)	(0.35)	(0.36)	(0.38)	(0.38)	(0.31)	(0.31)	(0.34)	(0.35)	(0.37)	(0.37)
Reassignment $(t+2)$	-1.68***	-1.73***	-1.83***	-1.88***	-1.83***	-1.88***	1.70***	1.77***	1.95***	1.94***	1.88***	1.87***	0.02	0.04	0.12	0.06	0.06	-0.01
	(0.36)	(0.37)	(0.39)	(0.41)	(0.39)	(0.41)	(0.38)	(0.39)	(0.41)	(0.42)	(0.41)	(0.43)	(0.30)	(0.31)	(0.33)	(0.35)	(0.34)	(0.36)
R^2	0.98	0.98	0.98	0.98	0.98	0.98	0.96	0.96	0.96	0.96	0.96	0.96	0.99	0.99	0.99	0.99	0.99	0.99
Observations	3,904	3,872	3,552	3,504	3,504	3,456	3,904	3,872	3,552	3,504	3,504	3,456	3,904	3,872	3,552	3,504	3,504	3,456
Unbalanced sample	×						×						×					
Balanced sample $\tau \in$		$[-4,0]$	$[-2,1]$	$[-4,1]$	$[-2,2]$	$[-4,2]$		$[-4,0]$	$[-2,1]$	$[-4,1]$	$[-2,2]$	$[-4,2]$		$[-4,0]$	$[-2,1]$	$[-4,1]$	$[-2,2]$	$[-4,2]$
#treated precincts	150	146	106	100	100	94	150	146	106	100	100	94	150	146	106	100	100	94
#control precincts	338	338	338	338	338	338	338	338	338	338	338	338	338	338	338	338	338	338

Notes: The table reports point estimates and standard errors based on Equation 2. All columns include only treated units that experienced one full reassignment. The unbalanced sample includes treated units that were reassigned to a different polling location in 2013/14/17/18/19/20. The balanced sample on $\tau \in [-4,0]$ includes reassignments in 2014/17/18/19/20; balance on $\tau \in [-2,1]$ includes 2014/17/18/19; balance on $\tau \in [-2,2]$ includes 2014/17/18; balance on $\tau \in [-4,2]$ includes 2017/18. Regressions are weighted by the number of eligible voters. Standard errors are clustered at the precinct level and reported in parentheses. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$.