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. *TU Dresden, ifo Institute for Economic Research, and CESifo*

February 28, 2023 (Click here for latest version)

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 overall turnout of 0.4–0.6 percentage points (0.7–1.0%). 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 are grateful to 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 CESifo Workshop on Political Economy, the Applied Young Economist Webinar Series, the Doctoral Conference at Ruhr GSE, the Annual Meeting of the Public Choice Society, the European Public Choice Society, and seminars at the University of Cambridge and the University of Munich. We are grateful to Ingrid Kreuzmair, Janette Lorch, and Heiko Flehmig for sharing data and valuable 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 presents empirical evidence on the consequences of a seemingly innocuous practice for voter participation: the relocation of polling places. We analyze a natural experiment in Munich, the third-largest city in 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 better accessibility 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 that occurred in the eight elections between 2013 and 2020 changed citizens' walking distance to their assigned polling location by less than one kilometer. Given the insignificance of any single vote for the election outcome, classical voting theory suggests that even such 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. Thus, relocating polling places may prove irrelevant to voter

[1] Reportedly, the 2021 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, that, according to the Berlin Constitutional Court led to "chaotic conditions" and "completely overloaded" polling places. Ultimately, the Federal Parliament (*Bundestag*) will decide about a possible repeat of the Federal Election, which was held on the same day as the State Election.

turnout. 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.

Understanding the determinants of voter turnout has engaged a vast literature, which has increasingly focused on the role of electoral institutions in recent years.[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; notably, Cantoni (2020) uses a regression discontinuity design at precinct boundaries in the US, showing that differences in distance to the polling location explains differences in voter turnout.[3] *Moving* a polling place typically alters the proximity to the polling location. But the practice may also induce unobservable changes to voting costs with ambiguous consequences on voting behavior. Turnout differences could also reflect gradual adjustments of 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.

The prevalence of polling place relocations in election organization, often due to routine polling place turnover, also justifies scrutiny of this practice. In Munich, reassignments are nonpolitical and uncontroversial. But this is not always true in other democracies, especially where election laws and administration are politically charged. The closing of polling sites in the US frequently raise concerns over politically motivated efforts to reduce voting access for certain groups, particularly racial minorities (Amos et al., 2017; Curiel and Clark, 2021; **?**). Partisan motives and unobserved determinants of polling place relocations pose a key challenge to causal identification of their turnout effects. The existing literature thus offers scant evidence on the consequences of the practice. Brady and McNulty (2011) use matching techniques to account for nonrandom polling place closures in the context of the 2003 LA gubernatorial re-

[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. Using the same identification strategy, Bagwe et al. (2022) find smaller effects of distance on voter participation in Pennsylvania and Georgia. Other studies have investigated the turnout effects of polling place opening hours (Potrafke and Roesel, 2020; Garmann, 2017) .

call election.[4] Comparing voters who had their polling location moved *further away* with voters without a change, the study documents a turnout decline associated with polling place reassignments. By contrast, Clinton et al. (2021) find no measurable association between turnout changes and moving polling places between two presidential elections in North Carolina.

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 a 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 reassignment. Understanding the dynamics of voting behavior adaptations is crucial to assess the cost of the practice in terms of both participation and representativeness. 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 (see e.g., Plutzer, 2002; Green and Shachar, 2000; Brody and Sniderman, 1977), causal evidence for habit formation has proved inconclusive.[5] Fourth, this is the first study to estimate the causal impact of reassignments and distance to the polling location outside the US in the context of a multi-party system with proportional representation. We use aggregate party votes to estimate the partisan consequences of moving polling locations; an aspect lacking in the existing literature. Moreover, Germany counts among the few countries to offer universal access to

[4] Specifically, the authors match on age, past turnout, and distance to the polling place in the previous election.

[5] 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.

mail-in voting. Thus, our setting is well suited to test the importance of convenient alternatives to voting at the polling place.[6]

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 travel distance to the polling location; second, reassignments always generate a disutility from engaging with an unfamiliar environment, which is 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 introduces the possibility that a fraction of eligible voters is surprised by a reassignment or does not notice the change at all. Our 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 in-person voting more costly. This is due to inattentive polling place voters who are surprised by reassignments after the deadline for requesting mail-in ballots has passed. Some inattentive voters who would have switched to mail-in voting instead abstain from turning out, thereby increasing 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. Our estimates suggest that, on average, reassignments cause a *persistent* substitution between the modes of voting. Turnout at the polling place falls by 1.0–1.3 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 overall turnout to fall temporarily by 0.4–0.6 percentage points. Given the well-intentioned nature of the policy and the marginal changes to proximity from 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 2–3 (Kaplan and Yuan, 2020), and would

[6]Only 5 percent of countries globally and 27 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)).

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).

Next, we examine a key dimension of reassignment heterogeneity: changes in proximity to the polling location. We estimate an event study specification that allows for differential treatment effects between reassignments that increased versus decreased distance to the polling place. In line with our model, we find strikingly asymmetric effects. When reassignments increase distance, 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 the reassignment disutility may offset potential turnout gains of reducing travel time. Indeed, we find evidence of gains in polling place turnout when the polling location moves least 17 percent closer to voters. These gains come almost exclusively from former mail-in voters. We only find week evidence of increases in overall participation extreme cases of distance declines. The latter result is consistent with inattentive abstainers, who remain abstainers even when the polling location moves very close. However, we cannot rule out alternative explanation for the lack of positive participation effects. Overall, we find that the change in distance accounts for less than 60 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 found when reassignments do not decrease travel distance. Results suggest 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 temporary abstainers return to vote in person after familiarizing themselves with their new polling place. Instead, the pattern is consistent with inattention to reassignments. Inattentive polling place voters are surprised by reassignments after the deadline for requesting mail-in ballots has passed. Consequently, some inattentive voters who would have switched to mail-in voting abstain in the current election and only turn to mail-in voting in the subsequent election. Our results thus highlight the importance of offering access to mail-in voting to compensate for votes lost at the polls. This 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 results are incompatible with the hypothesis that voting is habit forming. The fact that turnout declines only temporarily after reassignments is inconsistent with the view that abstaining instilled a new habit. Moreover, the fact that turnout losses are immediately recovered

is incompatible with the hypothesis that voting (or abstaining) is habit forming. Instead, the persistent substitution of in-person for mail-in voting and the recovery in voter participation is 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 for 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, which are supposed 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 migrant citizens; yet, the change in overall turnout is not statistically different. We find no evidence that reassignments depress turnout stronger in less affluent precincts (measured by the average quoted rent) nor in precincts with a higher share of households with children.

The presence of heterogeneous treatment effects may undermine the representativeness of the electoral outcome. Our results suggest it does not. Turnout effects of reassignments are similar across parties, and party vote shares do not change significantly, on average. 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.

The next section describes the institutional setting. Section 3 outlines the conceptual framework guiding our empirical analysis. Section 4 describes how we build our estimation panel and outlines our empirical strategy. We present our main results in Section 5. Section 6 analyzes heterogeneous effects across precinct characteristics and explores potential partisan consequences of reassignments. Section 7 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 the four legislative bodies that reflect 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 with respect to the electoral rules. In Appendix C, we briefly describe the key features of the different electoral processes.

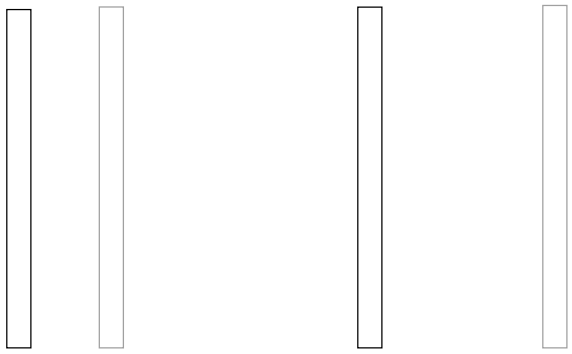
Eligible voters are automatically entered into the electoral roll. Voting is not compulsory and mail-in voting is available to all eligible voters without separate photo identification. Every person on the roll receives an election notification via mail no later than 21 days before the election. The letter includes information about the election date, the location and opening hours of the polling place, and whether it offers barrier-free access for people with physical impairments. There is no explicit information about any *changes* to the polling location—neither in the election documents nor in a separate notification. This contrasts with the US, where changes to precinct borders typically trigger the requirement to notify affected voters (Cantoni, 2020; Clinton et al., 2021). Eligible voters may cast a ballot 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. Eligible voters who instead wish to vote by mail must request a "polling card" (*Wahlschein*) by returning a form included in the election notification no later than 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

[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.

are also entitled to vote.[8] Total turnout tends to increase 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: Timeline and Turnout of Elections Held between 2013 and 2020



Notes: The figure presents the number of eligible voters (vertical bars), total turnout (triangles), and the share 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 Elections Office (*Wahlamt*).

2.2. Polling Place Reassignments

Elections are organized by the Munich Elections Office (*Wahlamt*). Employees of the Elections Office 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' registered home addresses.[10] Precincts constitute the smallest administrative unit and serve to enable a manageable election process. We use information from the official electoral rolls provided by the Munich Elections Office to georeference polling locations and resi-

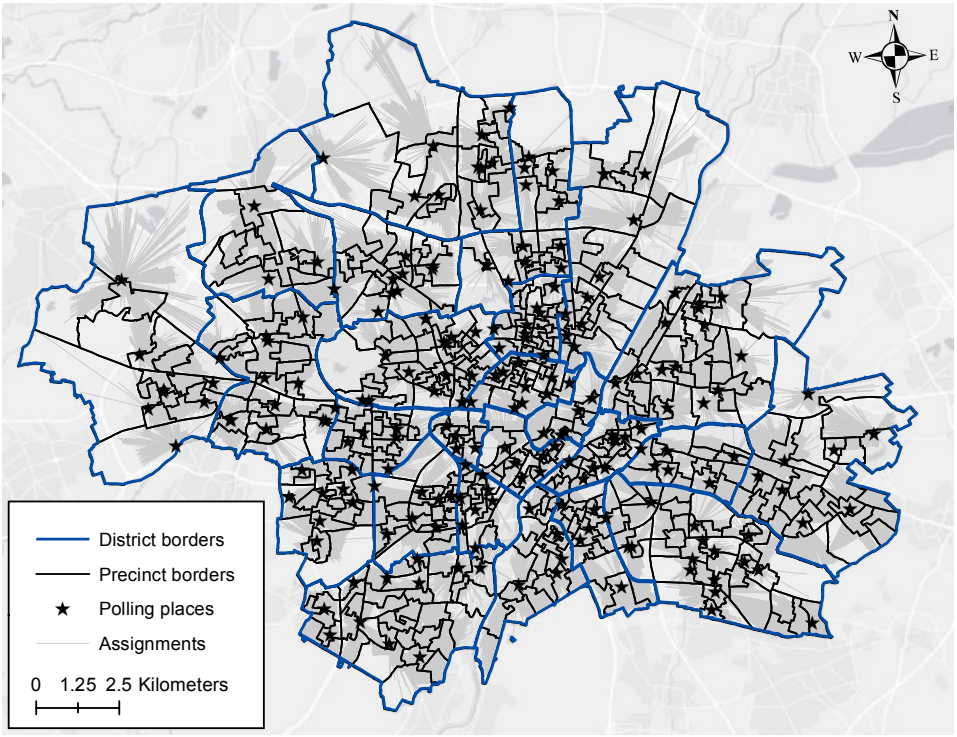
[8]For instance, in the 2020 Municipal Elections, 17.5 percent of eligible voters were foreign EU citizens. Foreign EU citizens who wish to vote in European elections in Munich instead of their country of origin must lodge a registration request.

[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]Citizens are required by law to notify the city's Registration Office (*Meldeamt*) within two weeks of moving into a new residence. This also applies to citizens who move within a municipality.

dential addresses in every election in our panel.[11] Figure 2 depicts a typical electoral map. The black boundaries delineate the 618 precincts; blue lines delineate the 25 city districts. A polling place, depicted by black stars, is assigned to each precinct, but it is not uncommon that a single venue, typically a school, accommodates the polling place of several neighboring precincts located in the same district. The gray lines indicate the assignment of home addresses to polling places.

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 Elections Office (*Wahlamt*).

Recruitment of Polling Locations. One source of variation in polling place assignments comes from turnover in the venues that are used to host polling places. These venues are typically 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,

[11] We identify and geolocate 154,156 residential addresses from the 2018 electoral roll, of which we are able to match 141,642 to a unique precinct in every election (92 percent).

[12] See Appendix Figure A.2 for an overview of venue types.

competing events on Election Day, building closures, or ongoing construction work may leave some locations unavailable. There is no documentation of the reasons why venues become inactive or new venues are recruited. Based on correspondence with the Elections Office, we identify two primary reasons for turnover in polling locations during our observation period. First, following a resolution of the city council (*Stadtrat*), the Elections Office prioritized recruiting locations with barrier-free access for elderly people and voters with physical impairments after 2014.[13] Second, Munich's school construction program (*Schulbauoffensive*), which involved investments of more than 3.8 billion Euros in refurbishing educational facilities starting in 2016, forced buildings to close down for several years. We reviewed public documents on the investment plans and found that in 70 percent of the cases in which schools were no longer used to host polling places, the election date fell within the specified construction period. Overall, we observe 293 distinct venues that were used in at least one election between 2013 and 2020. The number of operated venues is typically around 200 in any given election. Appendix Figure A.3 illustrates the activity status of polling venues over time.

Precinct Reconfigurations. The second source of variation in polling place assignments comes from 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 Elections Office's main objective is to minimize congestion risk at polling places. In practice, this involves controlling precinct sizes (to maintain an average of 1,500 eligible voters per precinct) and adjusting the number of polling places hosted by the same venue in case it serves multiple precincts.[14] As a result, precincts may be merged, split, or entirely assigned another (existing) polling place. According to the Elections Office, precinct boundaries were rarely revised before 2017 due to the cost of spatial monitoring. Instead, changes in precinct size were addressed by adjusting the number of poll workers at polling places. The introduction of a new urban planning technology in 2017 facilitated spatial monitoring and enabled more precise delineation of precincts. This resulted in a major re-division of the city and a significant reduction in the variance of precinct sizes (see Appendix Figure A.1). The number of

[13]Specifically, the resolution demanded that the number of barrier-free polling places be doubled between 2014 and 2017 and that a share of at least 75 percent should be reached by 2020. According to documents provided by the Elections Office, a share of 80 percent was achieved by 2018.

[14]The law specifies that a precinct may not accommodate more than 2,500 eligible voters in any election. See Appendix Figure A.1 for a density plot of precinct size across all elections.

precincts declined from 702 to 617 in 2017 and remained at 618 in 2018 and 2019. In 2020, the number increased again to 755 to accommodate a larger number of eligible voters during municipal elections and to account for social distancing provisions during the Covid-19 pandemic.

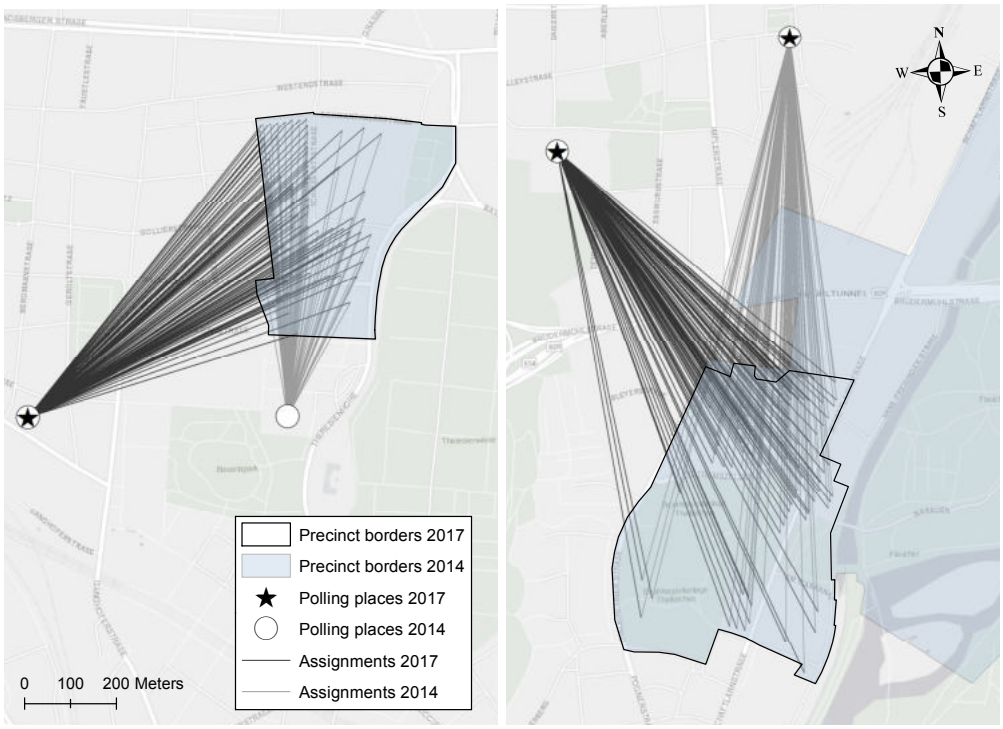
We illustrate two instances of polling place reassignments between 2014 and 2017 Figure 3. The black borders delineate precincts as of 2017. The blue-shaded areas demarcate precincts as of 2014. The dark (light) gray lines connect eligible voters' addresses to their assigned polling place in 2017 (2014). In Panel (a), the pub that served as the precinct's polling place in 2014 was not recruited in the subsequent election. Instead, the precinct was assigned to another polling place (a public 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). Voters living in 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 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, the Elections Office addressed changes in precinct size mainly by adjusting the number of poll workers at the polling locations so that reassignments due to precinct border adjustments were limited. In 2017, 41 percent of home addresses were assigned to a different polling place, mainly caused by the major consolidation of precincts (enabled by a new urban planning technology) and due to updated requirements for polling places (especially regarding barrier-free buildings). Munich's school construction program contributed to the turnover of polling venues starting in 2017. In 2020, reassignments were primarily due to the increased number of precincts and the recruitment of suitable venues to meet social distancing provisions during the Covid-19 pandemic. Overall, 42 percent of all addresses are never subject to reassignments between 2013 and 2020, 26 percent are reassigned once, and 24 percent twice (see Appendix Figure A.4).[16]

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

[16] On average, an address is reassigned once during our observation period. When an address is reassigned more than once, the median period between the first and second reassignment is three elections.

Figure 3: Illustration of Polling Place Reassignments



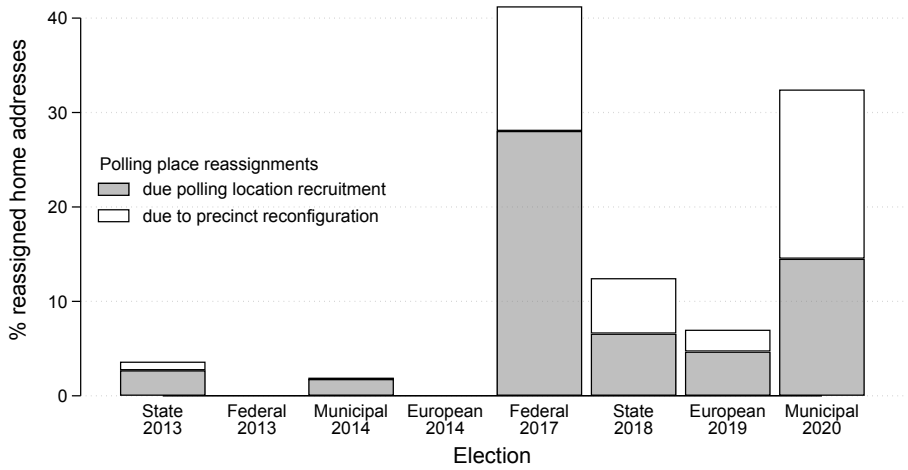
(a) Polling location recruitment **(b)** Precinct reconfiguration

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 5 plots the distribution of street (walking) distances between home addresses and polling places (left panel), and the distribution of distance *changes* conditional on reassignment across all elections (right panel).[17] 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 the reconfiguration of precincts. For 90 percent of residential addresses, the polling place is less than 1.4 kilometer away, which roughly corresponds to a 17-minute walk (median: 0.74 km, mean: 0.82 km). The overall distribution of distance changes is closely centered around zero (median: +0.04 km, mean: +0.06 km) and approximately symmetric (skewness: 0.2), indicating

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

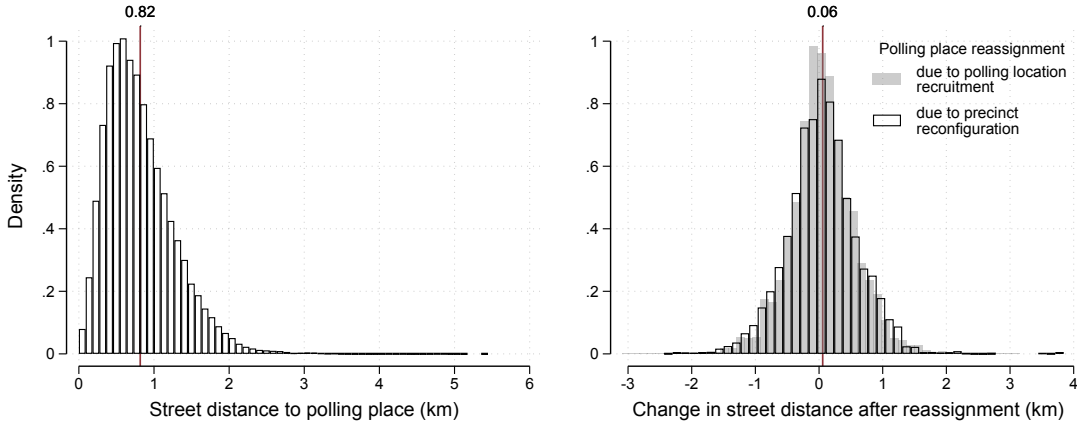
Figure 4: Share of Addresses Assigned to Different Polling Place Relative to Previous Election



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.

that polling places are not systematically located closer or further away after reassignment. Splitting by reason of reassignment leaves the moments of the two distributions nearly unchanged. The different sources of reassignments thus do not systematically produce different shocks to observable voting costs. More than 90 percent of reassignments change the walking distance by less than one kilometer, suggesting that the practice generates only marginal shocks to voting costs overall.

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



Notes: The figures present density plots for the street distance between residential addresses of eligible voters and their assigned polling places (left plot, $N = 1,133,136$) and the *change* in distance conditional on assignment to a different polling place relative to the previous election (right plot, $N = 142,062$) for the eight elections between 2013 and 2020. Vertical lines highlight the mean of the distribution.

2.3. *A Precinct-Level Panel*

We use official election results from the Munich Elections Office to estimate the impact of polling place relocations on voter turnout. One limitation for our empirical exercise is that the highest granularity of turnout 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. This way, we obtain a panel of 618 precincts with harmonized boundaries that we observe over eight elections between 2013 and 2020. We turn to the details of the empirical strategy to identify the causal effects of reassignments on turnout in Section 4.

3. 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 purpose of the model 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 may change when we allow a fraction of the population to be inattentive to reassignments.

Model 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.[18] 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 individuals of type H 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.

[18] 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).

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, $c^p_{i,t}$, which are a function of travel distance to the polling place, $dist_{i,t} \geq 0$, and a constant $q_t \geq 0$:

$$c^p_{i,t} = \gamma dist_{i,t} + q_t, \tag{1}$$

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. For simplicity, the reassignment disutility is assumed to be constant across individuals.[19] 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, $\sigma : \mathcal{I} \times \mathcal{T} \rightarrow \mathbb{R}^+$ 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 every individual via a change of the slope k_t. For instance, $k_1 = 1.2$ corresponds to a 20 percent increase in distance to the polling location for the entire electorate.

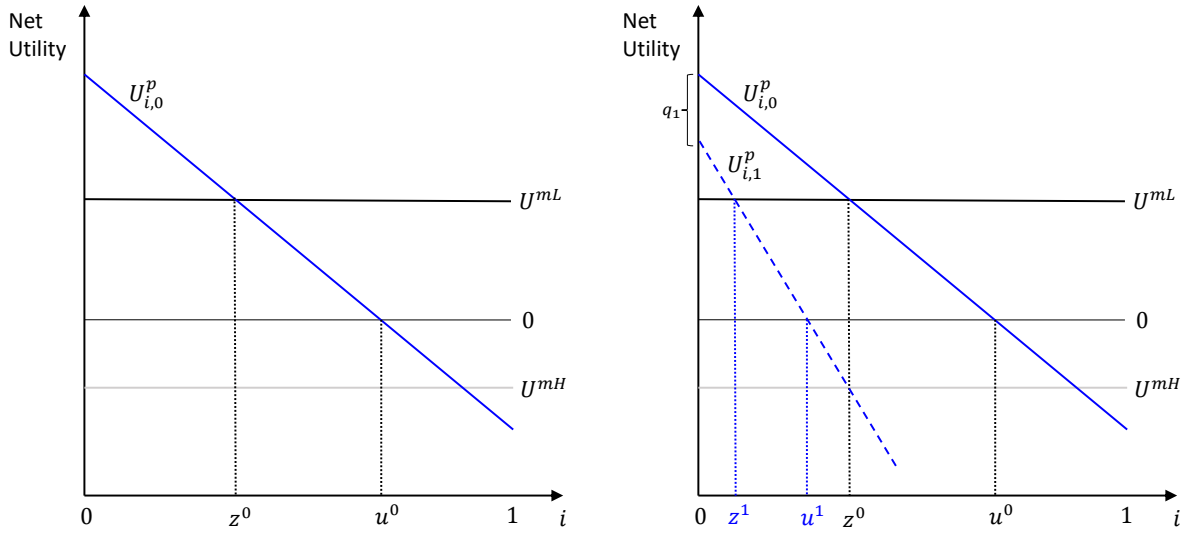
Turnout in Period 0. Individuals chose the option that confers the highest net utility. Figure 6a draws the net utilities of voting by mail for types H and L ($U^{mH} \equiv B - c^{mH}$ and $U^{mL} \equiv B - c^{mL}$, respectively) and the net utility of voting in person ($U^p_{i,0} \equiv B - c^p_{i,0}$). 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 $m(\mathcal{P}^0)$:

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

[19] In the framework 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 that is independent of the change in distance. Brady and McNulty (2011) do not formally separate between search costs and distance effects; thus, our model extends their conceptual framework.

Figure 6: Net Utility of Voting in Period 0 and Period 1



(a) Period 0: No reassignment

(b) Period 1: Reassignment

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, $U^p_{i,1}$.

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 overall turnout are given by:

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

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

Change in Turnout in Period 1. Figure 6b 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 equivalently 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:

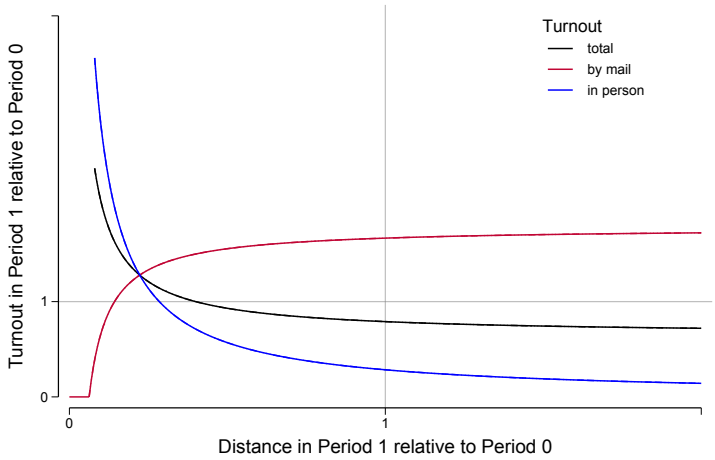
$$\hat{\mathbf{P}}(k_1) \equiv \frac{m(\mathcal{P}^1)}{m(\mathcal{P}^0)} = \frac{z^1 + (1-\alpha)(u^1 - z^1)}{z^0 + (1-\alpha)(u^0 - z^0)} \tag{5}$$

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

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

where all cutoffs $z^0, z^1, u^0, u^1 \in [0,1]$ are determined by exogenous parameters. Figure 7 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, overall 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 7: 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 reassignments or not notice at all that their polling place has moved. 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.

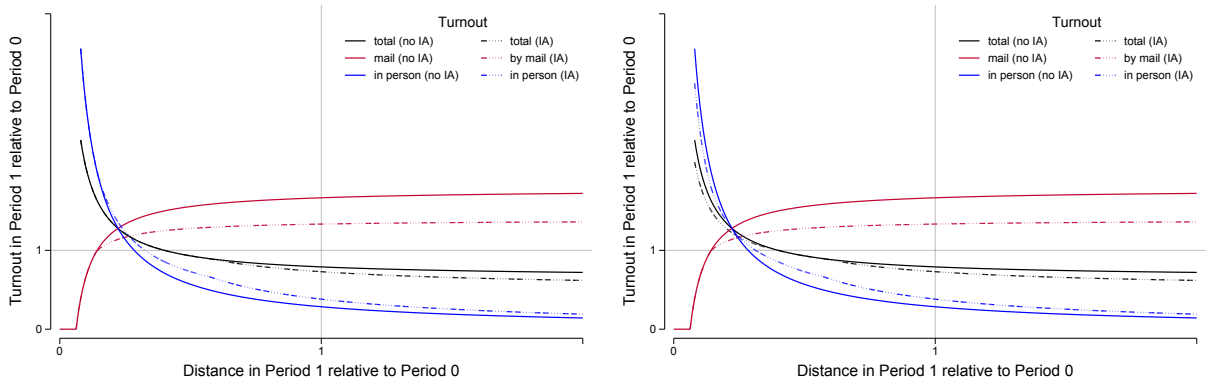
ii) a fraction $\pi \in [0,1)$ of abstainers, $i \in \mathcal{A}^0$, do not notice the reassignment at all and remain abstainers in period 1.

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 8 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 8a 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 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.

In Figure 8b 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.

Figure 8: 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.

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.

4. Empirical Strategy

4.1. 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 constitutes the modal case, both among reassignments due to recruitment of polling locations (60 percent) and due to precinct reconfigurations (16 percent); overall, we capture 40 percent of all instances in which reassignments occur using this definition (see Appendix Figure A.5). In the baseline, we also trim precinct time series from the time a second reassignment occurs to ensure that we capture the effects of a single reassignment rather than a series of changes.[20] We test the sensitivity of the results to alternative assumptions in Section 5. 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:

$$Y_{pt} = \sum_{k \neq -1} \mu^k \mathbb{1}(\tau = k) + \mathbf{X}'_{pt}\phi + \delta_p + \delta_{d(p)t} + \varepsilon_{pt}, \tag{8}$$

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 $\delta_{d(p)t}$ that 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. The motivation for interacting election fixed effects with district indicators is twofold. First, unlike precincts, districts are directly contested in some elections. In state and federal elections, the 25 districts are combined into several single-member constituencies, where residents directly elect their representatives. In Municipal Elections, citizens also elect a local district committee as the representative body. Systematic differences in voting incentives across districts may affect the validity of our estimates if, for example, close races are anticipated in certain constituencies (Bursztyn et al., 2022). Secondly, polling place recruitment is performed by local 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 $\mathbf{X}$ comprises a set of time-varying precinct character-

[20] Out of 278 treated precincts, 150 (54 percent) are treated exactly once (Appendix Figure A.6).

istics.[21] The error ε_{pt} represents 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 interpreting the effect estimates as causal are *i)* that polling place reassignments and changes in distance are not related to other determinants of voting behavior (that are not accounted for by fixed effects), and *ii)* that the expectation of changes in turnout does not drive polling place reassignments. A violation of the assumptions would occur if, for instance, the Elections Office systematically consolidated adjacent precincts that showed a stronger tendency to turn out by mail to save the costs of operating polling places. In this case, treatment effect estimates could merely reflect a pre-existing trend.[22] Although these assumptions cannot be tested directly, we present several indirect tests, including a balancing exercise, a pretend analysis, and results from alternative specifications to bolster our confidence in the causal interpretation of the findings.

A few final estimation details. First, 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. As noted above, about 90 percent of requested cards are returned as ballots, and more than 98 percent of these ballots are mail-in votes. Second, since not all event-time indicators are identified in the presence of precinct fixed effects, 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. Third, we cluster standard errors at the precinct level to account for the correlation of model errors over time. We test the sensitivity of our results to alternative assumptions about the variance-covariance matrix in Section 5. Fourth, specifications are weighted by precinct size (i.e., the number of eligible voters) to recover the conditional mean as-

[21] Specifically, controls include the 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 share of native German residents, the share of non-native German residents, the share of single residents, the share of married residents, the average duration of residence (in years), the share of households with children, and the average quoted rent per square meter. Information on local rents is from the RWI Institute for Economic Research. All other data are provided by the Munich Statistical Office.

[22] According to the Elections Office, past and expected turnout are not considered when redrawing precinct boundaries.

sociation between turnout and polling place reassignments at the individual level. Finally, we estimate Equation 8 using OLS to produce our baseline results. As pointed out by several recent contributions, OLS two-way fixed effect (TWFE) estimates may yield biased results with staggered treatment and heterogeneous effects.[23] The reason is that the TWFE estimator uses already-treated precincts as controls for newly-treated precincts, thereby violating the parallel trend assumption in the presence of treatment effect dynamics. The treatment timing in our setting is illustrated in Appendix Figure A.6. Of 618 precincts, 340 are never treated, and most of the treated precincts had their polling location changed in the 2017 Federal Election (62 percent).[24] To account for the staggered treatment timing, we also estimate the event study using the estimators proposed by Borusyak et al. (2022), 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. (2022) and de Chaisemartin and D'Haultfoeuille (2022).

4.2. Balancing Test

Under our identifying assumptions, the timing of reassignments is uncorrelated with other determinants of turnout. One approach to assess the comparability of treated and untreated precincts 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 provides information on the association between treatment timing and *changes* in precinct characteristics. We present the balancing test results in Figure 9.

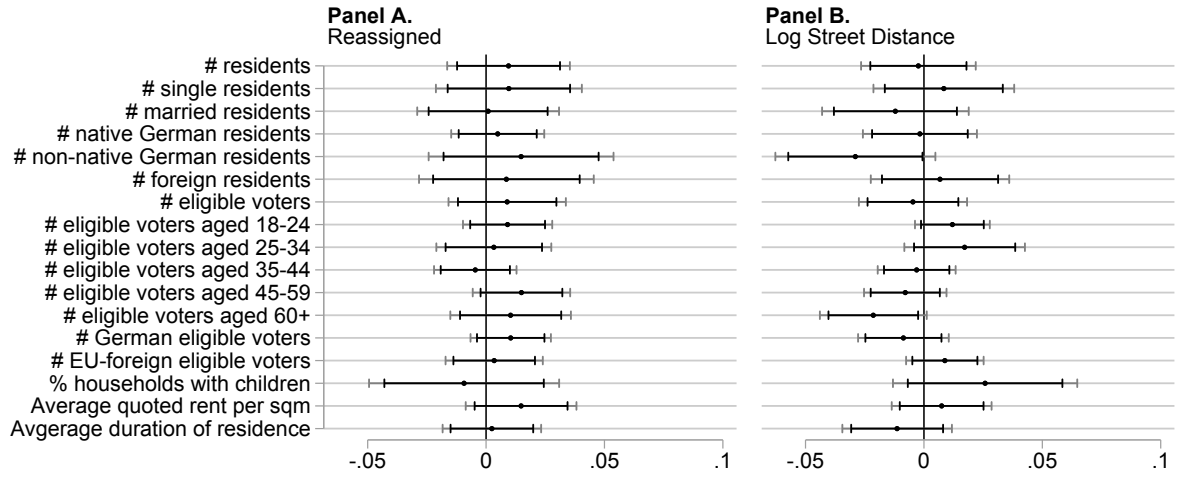
Panel A shows estimates and confidence bands from univariate OLS regressions of a dummy identifying reassignments that changed the polling location for the entire precinct on precinct characteristics, conditional on election and precinct fixed effects. Each estimate comes from a separate regression. All characteristics are standardized to have mean zero and unitary standard deviation. The estimates are close to zero and insignificant, suggesting that treatment timing is uncorrelated to observable changes in precinct characteristics. The dependent variable in Panel B is the log of average street distance. Out of seventeen estimates, only two are marginally significant at the

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

[24] 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 A.7 maps the spatial distribution of polling place relocations.

10 percent level. Still, F-tests cannot reject the null that the coefficients are jointly zero in any panel, indicating that the fixed effects perform well in eliminating any correlation between treatment and precinct characteristics. Coefficients and test statistics are reported in Appendix Table B.2. We also present correlations 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 9: Balancing Test on Precinct Characteristics



Notes: Panels A and B report OLS estimates from separate univariate regression on standardized 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 of average street distance to the polling location (Panel B). Confidence intervals are drawn at the 90 and 95 percent levels 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 B.2. Information on local rents is from the RWI Institute for Economic Research. All other precinct characteristics are obtained from the Munich Statistical Office.

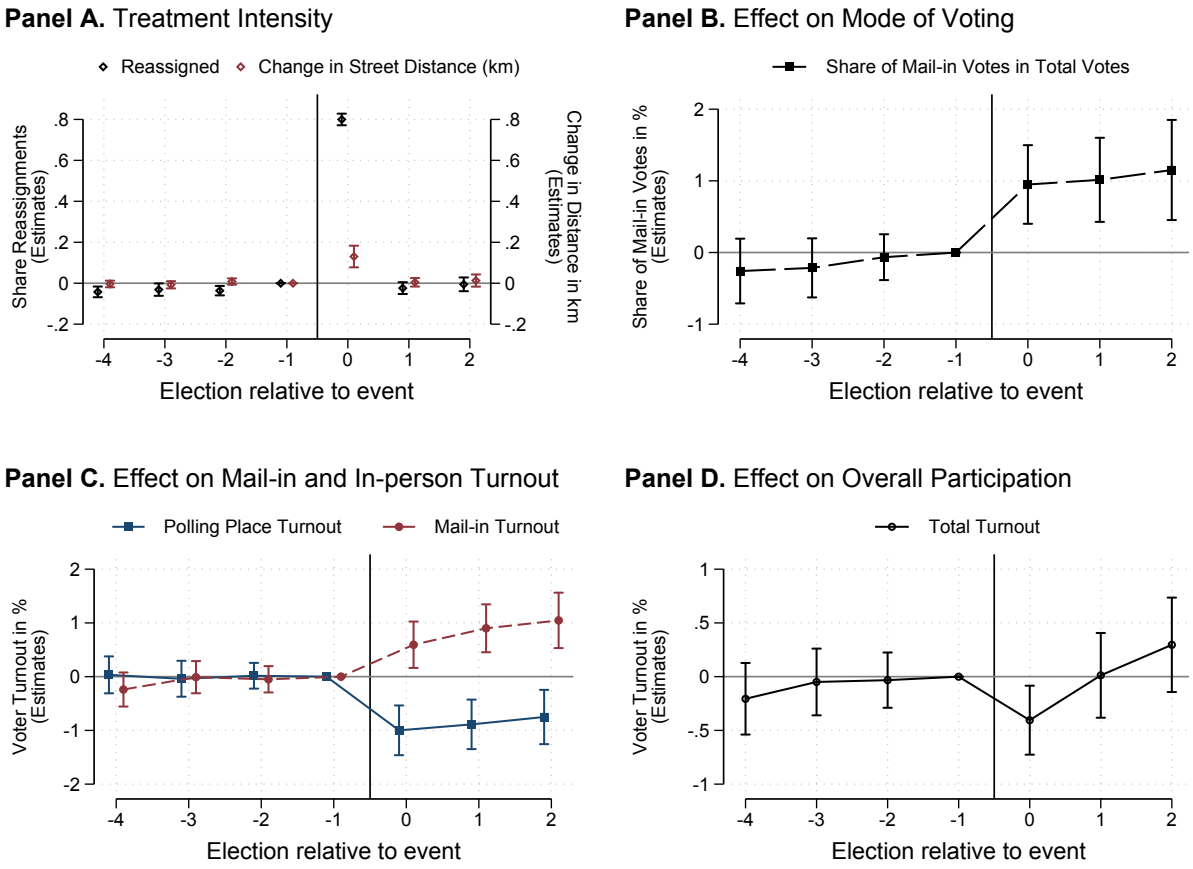
5. Main Results

5.1. Average Effects on Turnout and the Mode of Voting

We start by estimating the average effects of polling place reassignments on treated precincts. Figure 10 plots event-time estimates based on Equation 8 using different outcomes in Panels A–D. The event corresponds to the first election in which the entire precinct is assigned to a new polling place. As emphasized above, we exclude all precinct-election observations beyond any second event so that we pick up the effects of only one instance of treatment. Panel A illustrates the average treatment intensity according to this event definition 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 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, on average, the distance to the polling location increases by 0.13 kilometers due to the event. This represents a moderately larger increase compared to the overall distribution of proximity changes caused by reassignments presented in Figure 5.

Figure 10: The Effect of Reassignments on Turnout and the Mode of Voting



Notes: The figure presents event study results based on Equation 8. 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 4.1. 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 B.3.

Panels B–D plot event-time coefficients for different outcomes of voting behavior. The first notable feature is that event-time coefficients preceding the reassignment are close to zero and not statistically significant in any panel. The absence of pretrends provides important evidence in support of the identifying assumption; trends in out-

comes across comparison groups evolve in parallel except through the treatment. By contrast, we observe a sharp and persistent increase by 1 percentage point 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 percentage point immediately after reassignment (equivalent to 3 percent at the mean), while mail-in turnout increases by only 0.6 percentage points (2 percent). Thus, the shift to mail-in voting is not large enough to completely compensate for votes lost at the polls, generating a decline in total turnout by 0.4 percentage points (0.7 percent) in Panel D. This result is consistent with reassignments producing 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 estimates further show that the shift from polling place to mail-in voting persists in the two elections after the initial jump, suggesting that the shock to voting costs is lasting. Remarkably, however, the decline in total turnout completely recovers in the subsequent election and is not statistically different from zero afterward. One possible explanation for this recovery is that the reassignment shock to voting costs wanes over time. For example, temporary abstainers may familiarize themselves with their new polling place and return to vote there after one election. An alternative explanation is that the initial turnout decline is driven by inattention to reassignments. As proposed in Section 3, inattentive polling place voters are surprised by the reassignment *after* the deadline for requesting a mail-in ballot has passed. Some inattentive voters who would otherwise have switched to mail-in voting consequently abstain from voting in the first election after reassignment. But aware of the change, they switch to voting by mail in the subsequent election, recovering the drop in turnout. In Section 5.3, we make the case that the transitory decline in overall turnout is consistent with inattention and inconsistent with the waning cost hypothesis. The argument is that the recovery is demonstrably driven by an increase in mail-in turnout, not in-person turnout.

Albeit transitory, the turnout decline caused by changing a polling place is sizable. To put the estimates into perspective, the average decline in participation is roughly equivalent to reducing the number of early (in-person) voting days in the US by 2–3 (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).

A central insight of Figure 10 is that the estimates *do not* support the hypothesis that (non)voting is habit forming. If abstaining from voting was habit-forming (by increasing its consumption value), the initial decline in turnout would carry over to subsequent elections, even in a hypothetical scenario in which the costs of voting were entirely restored to pre-treatment levels. Our estimates are clearly inconsistent with this pattern. 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 increasing turnout after its abolition.

The full set of our results based on Equation 8 and some of its variants appear in Appendix Table B.3. Column (1) reports event-time estimates *excluding* time-varying controls. Column (2) presents the results of our preferred specification, which includes controls. This reduces standard errors across the board without significantly affecting point estimates. In Column (3), we estimate the event study using the full sample instead of trimming the time series once a second treatment 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, corroborating our identification strategy. In Column (5), we test the robustness of the baseline estimates to an alternative event definition. Here, the event corresponds to the first election in which at least 50 percent of a precinct is reassigned.[25] The estimated effect sizes are slightly more pronounced compared to our preferred model, but the main conclusions hold. In Appendix Figure A.8, we re-estimate the model of Column (4) using several novel estimators that account for staggered treatment timing (Borusyak et al., 2022; Callaway and Sant'Anna, 2021; Sun and Abraham, 2021; de Chaisemartin and D'Haultfœuille, 2020). The estimates remain very similar to the TWFE-OLS estimates, suggesting that heterogeneity in treatment timing does not compromise our estimates of interest.

We also show that the results are robust to alternative assumptions about the variance-covariance matrix in Appendix Table B.4. One might be concerned, for instance, that model errors are correlated within districts. This may be the case because adjustments to the boundaries of adjacent precincts are not performed across but only within districts. Moreover, it is not uncommon that polling places of several precincts (within

[25] Using this treatment definition, we capture 60 percent of all instances in which a positive share of addresses is reassigned (see Appendix Figure A.5).

a district) are located in the same building. In these cases, closing a venue might affect several adjacent precincts simultaneously. We reproduce our preferred specification (from Column 2 of Table B.3) 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 standard errors clustered at the precinct level (Column 3) and at the district level (Column 4), as recommended by MacKinnon et al. (2022). All treatment effects remain statistically different from zero. Finally, we verify that wild bootstrap clustering at the district level does not affect conclusions in the model using election fixed effects instead of election×district fixed effects. Column (5) shows that all effects remain statistically significant.

In Appendix D, we test if the different reasons for reassignments (polling location recruitment versus precinct reconfiguration) carry different turnout effects. We find they do not.

5.2. The Role of Distance to the Polling Location

The baseline turnout 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. In this section, we analyze the role of this central aspect of polling place accessibility and shed light on the mechanisms underlying the change in voting behavior documented above. To this end, we estimate two modified versions of Equation 8.

Effect Heterogeneity by Change in Distance. First, we allow for different treatment effects between reassignments that increased versus decreased distance to the polling place. Formally, let N_p^+ be an indicator equal to 1 for precincts where reassignment caused an *increase* in average distance to the polling location. N_p^- denotes the analogous indicator for cases in which distance *decreased*. Then, the modified event study specification takes the following form:

$$Y_{pt} = N_p^+ \times \sum_{k \neq -1} \beta^k \mathbb{1}(\tau = k) + N_p^- \times \sum_{k \neq -1} \alpha^k \mathbb{1}(\tau = k) + \mathbf{X}'_{pt}\phi + \delta_p + \delta_{d(p)t} + \varepsilon_{pt}, \tag{9}$$

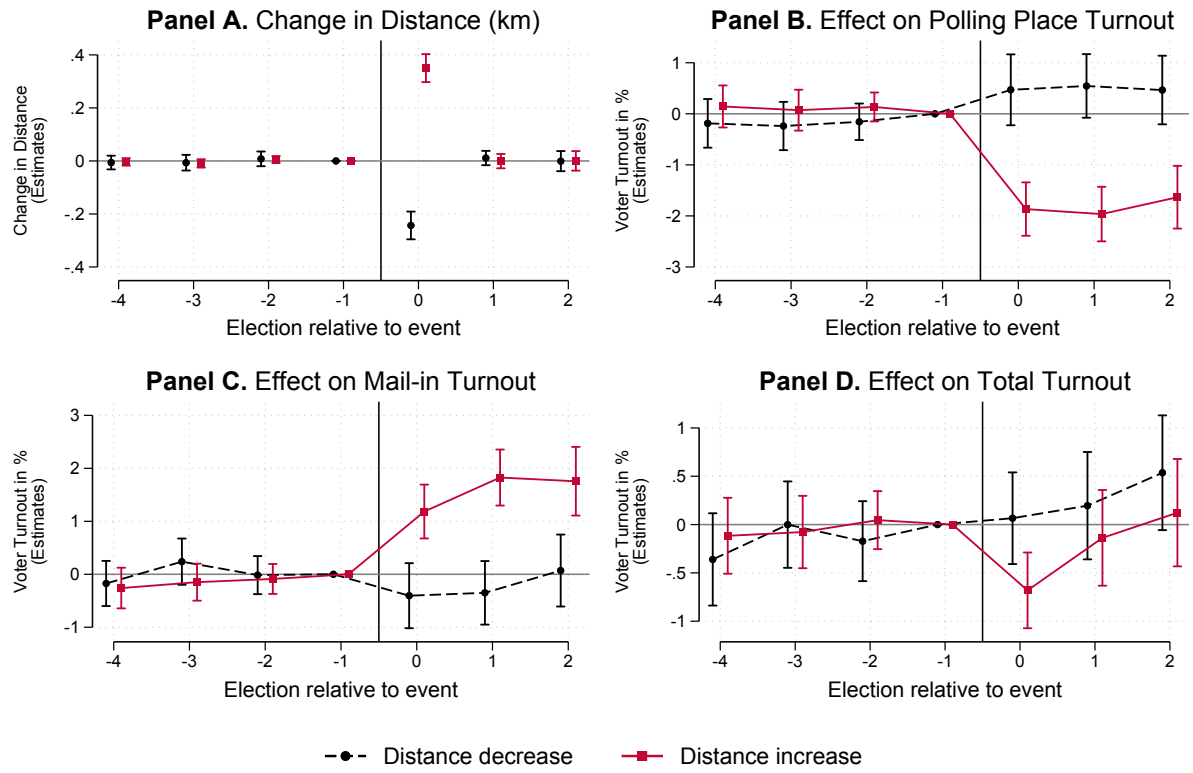
where the coefficients $\hat{\beta}^k$ and $\hat{\alpha}^k$ trace the differential time path of turnout separately for the two groups defined by N_p^+ and N_p^-. Note that since we do not condition on distance in Equation 8, the baseline estimates $\hat{\mu}^k$ correspond to a weighted average of

$\hat{\beta}^k$ and $\hat{\alpha}^k$. As before, the specification includes election×district fixed effects, a vector of precinct indicators, and time-varying controls.

The results are presented in Figure 11. Each panel report estimates and 95 percent confidence intervals 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. Panel A shows that distance increases by 350 meters, 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. Consistent with our model, turnout effects are strikingly asymmetric when comparing cases that increased versus decreased distance from the polling place. Panel B suggests that reassignments that generate a greater travel distance cause a sharp and persistent decline in polling place turnout. The estimate on the immediate effect is -1.87 (p <0.01), which is equivalent to a 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 only slightly, albeit not statistically significant. Panels C and D show a similar picture. The impact on mail-in turnout is statistically insignificant when the new polling location is closer and strongly positive when relocated further away. In total, participation declines only in precincts in which distance increases. The drop amounts to 0.68 percentage points, which is 20 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 when reassignments reduce the distance to the polling location, lower travel time and the reassignment disutility offset each other 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.

Our model implies that the distance to the polling place must decline enough to compensate for the reassignment disutility to make in-person voting relatively more attractive than mail-in voting or abstaining. To test these mechanisms, we estimate a version of Equation 9 in which we allow treatment effects to vary by *three* reassignment types in Figure 12: 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 overall participation remains comparable to cases in which distance strongly increased. This pattern bolsters the case that the reassignment disutility alone imposes a burden

Figure 11: Effect Heterogeneity by Change in Proximity to the Polling Location



Notes: The figure presents event study results based on Equation 9. 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 B.5.

on voters beyond travel time. 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. Overall participation increases slightly; however, the estimate is not statistically significant.[26]

Although we do not observe changes in voting behavior at the individual level, the pattern is consistent with our rational choice model of voting. However, one concern with

[26]In Appendix Figure A.9, 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; i.e., large distance reductions generate a sizable substitution from away from mail-in toward in-person voting; yet, we find no significant effects on total turnout. Small distance reductions are insufficient to compensate for reassignment disutility, resulting in a decline in polling place turnout. Finally, distance increases always cause a shift away from in-person towards mail-in voting and abstention.

relying on aggregate measures of distance changes is that they may mask substantial heterogeneity within precincts. For example, an increase in *average* distance to the polling location could mask that a nontrivial portion of the local electorate experienced a decrease in distance. We, therefore, estimate a specification using a sample in which we remove such ambiguous cases. Specifically, we restrict the treatment group to precincts where the reassignment consistently increased (decreased) the distance to the polling place for at least 90 percent of home addresses and to precincts where reassignments induced only "little distance change" to all citizens. In the latter group, we include only cases where the polling place moved less than 800 meters from the old location.[27] We estimate a version of Equation 9 allowing for different treatment effects in each group. The estimates in Appendix Figure A.10 show that, reassuringly, the results hold.

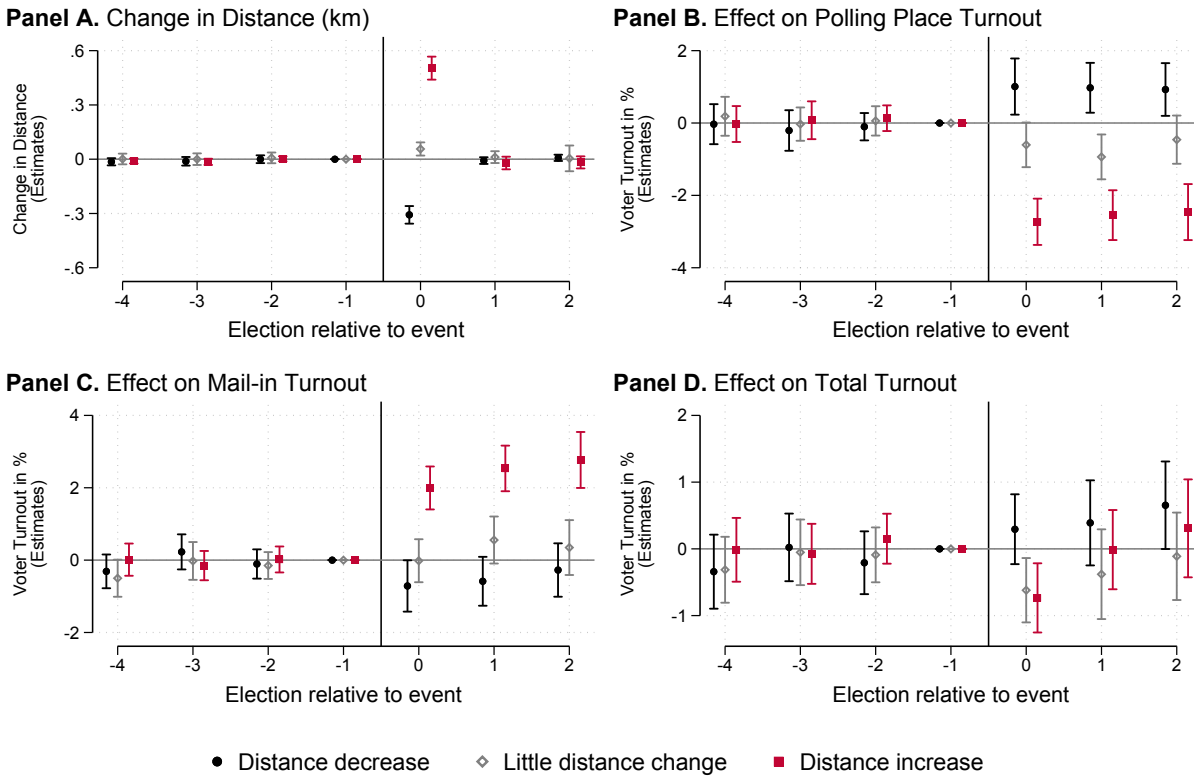
Decomposition Exercise. In our second exercise, we introduce the log of average street distance to the polling location as a covariate in Equation 8. Since the specification includes precinct fixed effects, the identifying variation comes from changes in distance *within* a precinct, which are generated by reassignments only. The results allow us to decompose the baseline effects into a portion explained by the change in distance and into a residual that reflects reassignment costs that are independent of the change in proximity. In our model, these costs correspond to the reassignment disutility (e.g., from engaging with an unfamiliar environment), which always increases the costs of voting at the polling place.[28]

The results are presented in Table 1. The outcomes are turnout at the polling place in Columns (1) and (2), turnout by mail (Columns 3 and 4), and overall 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 (Column 2 and Column 4, Table B.3). 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), suggesting that polling place turnout falls by 0.33 percentage points when distance increases by 10 percent. To compensate for votes lost at the polls due to reassignment disutility, the polling place would thus have to move 17 percent closer to voters, on average. Increasing distance has the opposite effect on mail-in turnout (Columns 3 and 4); however,

[27] 800 meters corresponds to the median change in distance between new and old polling locations.

[28] Brady and McNulty (2011) label the costs that arise on top of increased travel distance "search costs", which result from the time of looking up and going to the new polling location.

Figure 12: Effect Heterogeneity by Change in Proximity to the Polling Location



Notes: The figure presents event study results based on a version of Equation 9 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 B.6.

the effect size does not completely offset the negative impact on in-person turnout: on average, increasing distance by 10 percent results in a drop in overall participation by 0.08 percentage points (Columns 5 and 6). Interestingly, the event-time estimates on mail-in turnout turn insignificant in the first post-event election and become more than twice as large and significant in the subsequent election. Again, this pattern is consistent with inattentive voters delaying the switch from polling place to mail-in voting by one election because they missed the opportunity to request a mail-in ballot. If these voters predominantly abstain from participating before turning to mail-in voting, this would explain the temporary decline in total turnout. We test this mechanism as the driver of the turnout recovery in the next section. Comparing the point estimates with the baseline results suggest that distance accounts for 35–39 percent of the reassignment effect on in-person turnout (over the three post-event elections), and for 19–25 percent of the drop in overall turnout in the first post-reassignment elec-

tion. Thus, although distance effects are sizable and significant, only less than half of the turnout effects are attributable to changes in distance.

Table 1: Event Study Estimates Conditional on Log Street Distance

	Turnout at the Polling Place		Turnout by Mail		Total Turnout	
	(1)	(2)	(3)	(4)	(5)	(6)
Log Street Distance	-3.31***	-3.36***	2.56***	2.56***	-0.75***	-0.79***
	(0.26)	(0.26)	(0.25)	(0.26)	(0.22)	(0.23)
Reassignment ($t-4$)	0.02	-0.15	-0.23	-0.07	-0.21	-0.22
	(0.17)	(0.19)	(0.16)	(0.17)	(0.17)	(0.17)
Reassignment ($t-3$)	-0.08	-0.09	0.02	-0.07	-0.06	-0.17
	(0.17)	(0.20)	(0.15)	(0.20)	(0.16)	(0.17)
Reassignment ($t-2$)	0.03	0.16	-0.06	-0.16	-0.03	0.00
	(0.12)	(0.14)	(0.12)	(0.14)	(0.13)	(0.15)
Reassignment ($t+0$)	-0.55***	-0.65***	0.25	0.21	-0.30*	-0.44***
	(0.21)	(0.22)	(0.21)	(0.23)	(0.16)	(0.17)
Reassignment ($t+1$)	-0.62***	-0.63***	0.70***	0.69***	0.07	0.06
	(0.20)	(0.23)	(0.20)	(0.22)	(0.20)	(0.20)
Reassignment ($t+2$)	-0.44*	-0.44*	0.81***	0.78***	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 effect explained by distance	0.39	0.35	0.35	0.34	0.25	0.19
Observations	4,672	4,672	4,672	4,672	4,672	4,672
Precinct FE	×	×	×	×	×	×
Election-District FE	×		×		×	
Election FE		×		×		×

Notes: The table presents event study results based on different versions of Equation 8 in which the log of average street distance is included as a covariate. 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 average decrease of point estimates when controlling for distance compared to baseline estimates (reported in Column 2 and Column 4 of Table B.3) over the three post-event periods (for in-person and mail-in turnout) and in the first-post event period (for total turnout), respectively. 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 4.1. Regressions are weighted by the number of eligible voters. Standard errors are clustered at the precinct level and reported in parentheses. $^{***}p < 0.01$,$^{**}p < 0.05$,$^{*}p < 0.1$.

The insight that the mere relocation is the primary driver of turnout effects relative to distance changes is important. Election officials monitor the proximity to the polling locations. But that the relocation of a polling place itself may pose a barrier to voting has so far been overlooked. Existing causal estimates of distance to the polling location on turnout use cross-sectional variation near precinct borders in a regression discontinuity design (Cantoni, 2020). Based on the negative distance effects, one might be tempted to prescribe a policy of simply relocating polling places closer to voters to increase turnout. Our results highlight that such a policy may, in fact, not deliver

the expected outcome as distance reductions come at the cost of changing the polling location.

Our estimated distance effects on overall turnout—based on temporal variation—are smaller than estimated by Cantoni (2020). Cantoni's estimates imply that a 1 standard deviation greater distance (0.25 miles) reduces turnout in US elections by 1–3 percentage points. Based on the specification in Column (5) of Table 1 and replacing the log of street distance with linear distance, we estimate a decline of 0.3 percentage points (p <0.01) for every 1 standard deviation increase in distance (0.21 miles). Unlike in most US elections studied by Cantoni, mail-in voting in German elections is universally accessible. Thus, a potential reason for the discrepancy is the convenient access to mail-in voting, which we find to compensate significantly for votes lost at the polling place. Our estimates imply that a 1 standard deviation jump in distance decreases *in-person turnout* by 1.4 percentage points, which is in line with the effect range estimated by Cantoni.

5.3. Mechanism: What Drives the Recovery in Overall 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 formally introduced in Section 3, 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 10, 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 11). 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 9) to rule out ambiguity due to cases that may produce a *negative* shock to voting costs. Indeed, we find that the difference for mail-in turnout is positive and statistically significant (0.64, p <0.01). The difference for in-person turnout is negative, albeit not statistically significant (-0.10, 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 and reported in Columns (4) and (5) of Table B.3. In this specification, turnout effects are driven by the reassignment disutility. Again, the test suggests that mail-in turnout further *increases* in the second election after reassignment (0.45, p <0.01), while polling place turnout, if anything, marginally decreases (-0.07, p >0.1). Hence, the results strongly support the hypothesis that the recovery in overall turnout is driven by inattentive voters switching from nonvoting to mail-in voting, and are inconsistent with the waning-cost hypothesis.

To rule out that the results are merely an artifact of using the TWFE estimator, we replicate the tests using the novel DiD estimators that explicitly account for heterogeneity in treatment timing (Borusyak et al., 2022; Callaway and Sant'Anna, 2021; Sun and Abraham, 2021; de Chaisemartin and D'Haultfœuille, 2020). The event study results are plotted in Appendix Figure A.12 for specifications using a restricted sample excluding reassignments that caused a distance *decrease*, and in Appendix Figure A.11 for specifications controlling for the log of street distance. In addition, Appendix Table B.7 reports the difference of the event-time coefficients in the second and the first post-reassignment election for mail-in, in-person, and overall turnout according to the five estimators. The robustness check supports our conclusion that the transitory decline in voter participation is driven by inattention to reassignments. According to all estimators, mail-in turnout further increases in the second post-event election; the difference is statistically significant in almost all cases. Instead, there is no evidence that in-person turnout drives the recovery in total turnout: half of the estimated differences are negative, and none are statistically significant.

In our model, we also consider the case in which a fraction of abstainers is inattentive to reassignments (e.g., because they never open the election notification). In this scenario, inattention attenuates the increase in total turnout 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. It

is impossible to empirically identify this type of inattention since we cannot rule out that the observed reductions in travel distance are not enough to make polling place voting attractive for abstainers. However, the lack of positive turnout effects, even in cases in which reassignments significantly reduce distance, points toward inattention as a contributor to the inertia of abstainers.

6. Effect Heterogeneity and Partisan Consequences of Reassignments

The baseline event study estimates deliver average turnout effects for precincts that had their polling location moved. Yet importantly, the results may obscure heterogeneity across different voter groups. Uncovering sources of heterogeneity is central for several reasons. First, policymakers may be particularly concerned about reassignments imposing a disproportional burden on minorities, the elderly, or economically disadvantaged people. Second, if reassignments are more likely to discourage certain voter groups from turning out, the representativeness of the electoral outcome may be at risk. Thus, we devote this section to analyzing effect heterogeneity, starting with differences across demographic groups followed by partisan consequences of reassignments.

6.1. Heterogeneity across Precinct Characteristics

Who responds to reassignment shocks? To explore heterogeneity across voter groups, we estimate a version of Equation 8 by adding 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 allows for the effects of reassignments to evolve over time:

$$Y_{pt} = \sum_{k \neq -1} \gamma^k [Z_p \times \mathbb{1}(\tau = k)] + \sum_{k \neq -1} \theta^k \mathbb{1}(\tau = k) + \mathbf{X}'_{pt}\eta + \pi_p + \pi_{d(p)t} + \epsilon_{pt}, \tag{10}$$

where θ^k are the coefficients on the standard event-time dummies, $\mathbf{X}$ 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" 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.

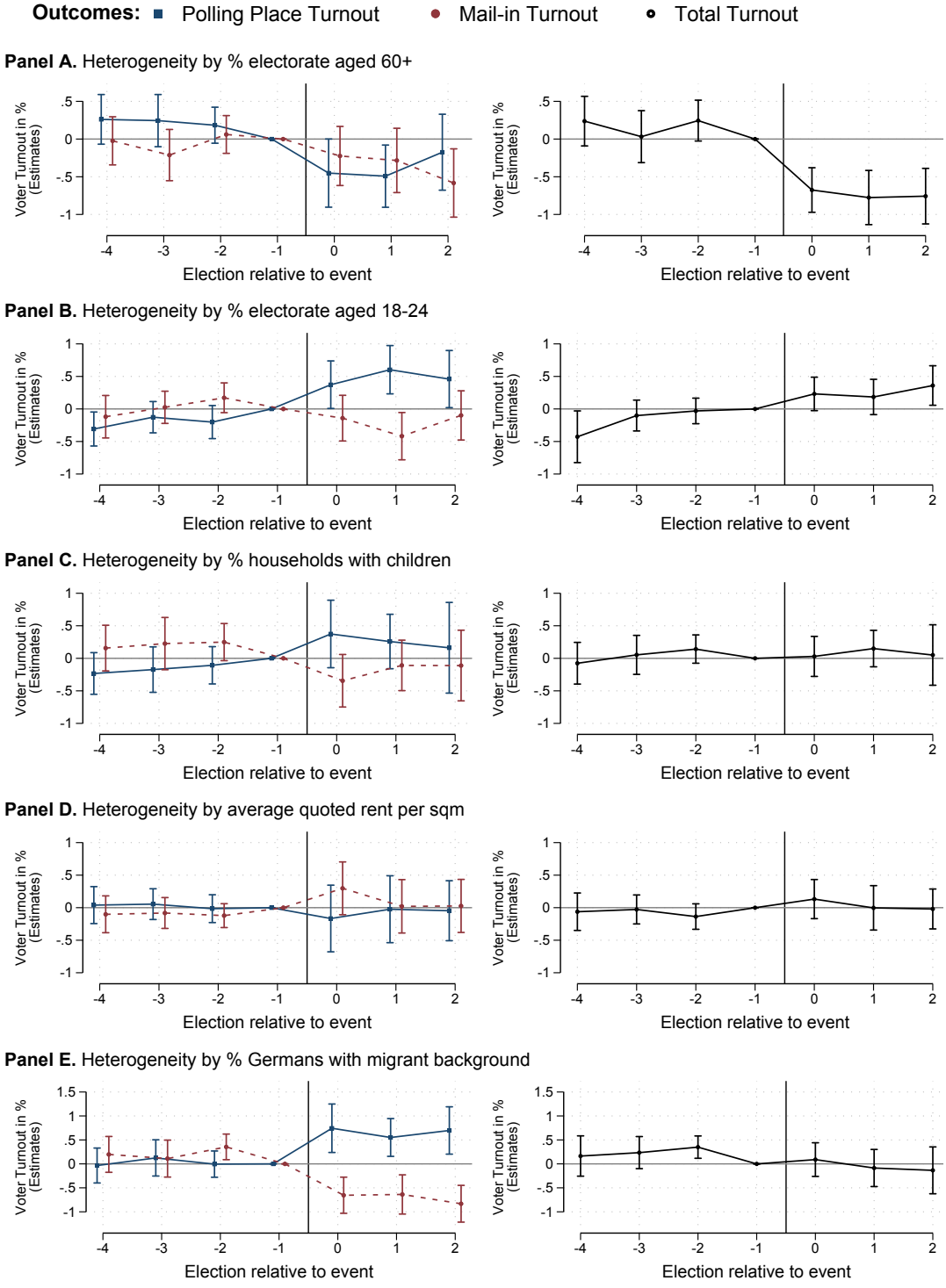
In practice, we estimate Equation 10 separately for different Z_p's, each corresponding to a standardized precinct characteristic (i.e., unitary standard deviation and mean zero) measured in 2013 (the first year in our panel). Hence, the triple-difference estimates measure the difference in turnout among treated units when Z_p is increased by one standard deviation.

The results appear in Figure 13. In each panel, the left plot shows the triple difference estimates for turnout at the polling place and via mail; the right plot shows the differential trends for overall turnout. The main conclusions from this exercise are that precincts with a higher share of elderly eligible voters show a greater decline in polling place turnout and a weaker shift toward mail-in voting when reassigned. This results in a stronger drop in overall participation (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=3.85, p=0.03). In precincts with a larger share of younger eligible voters, the impact of reassignments is visibly attenuated (Panel B): the estimated effects are negative for mail-in turnout and positive for polling place and overall turnout. This is unsurprising, given that a greater share of first-time voters implies a higher proportion of individuals who do not experience reassignments. We find no measurable differences for precincts with a higher fraction of households with children nor for precincts where housing is more expensive (Panels C and D). Panel E shows that the substitution between modes of voting is significantly weaker in precincts with a higher fraction of Germans with a migrant background; yet, overall turnout appears not statistically different. This finding might reflect that migrants are not used to mail-in voting from their country of origin or are more likely to be unfamiliar with the process of requesting a mail-in ballot (e.g., due to language barriers).[29] The findings 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.

Two remarks are in place. First, since inference is not based on (quasi-)random sources of variation, the results of the heterogeneity analysis can only be interpreted as suggestive of the mechanisms underlying differential turnout trends. For instance, other characteristics correlated with Z_p (e.g., unobserved aspects of voters' socioeconomic

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

Figure 13: Effect Heterogeneity by Precinct Characteristics



Notes: The figure presents event study results based on the triple difference estimator introduced in Equation 10. 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 overall 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 4.1. 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 B.8.

status) could constitute the actual *cause* of differential effects of reassignments. Second, we did not account for the change in distance to the polling location generated by reassignments in the regressions. To rule out the possibility 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 of street distance. Appendix Figure A.13 shows that the conclusions still hold.

6.2. Partisan Consequences of Reassignments

The presence of heterogeneous turnout effects across voter groups may threaten the representativeness of the electoral outcome. We examine this concern by estimating the partisan consequences of reassignments. One limitation is that we observe party outcomes at the precinct level only for votes cast *in-person*. Party votes from mail-in ballots are only recorded at the *district level*. As there are only 25 districts (compared to 618 precincts), estimates based on district-level observations are likely underpowered. Consequently, we first analyze party results at the polling place 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 8 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.[30]

The results presented in Figure 14 suggest that in-person turnout declines slightly more for right-wing parties after reassignment (left plot, Panel A); however, the effects are not statistically 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 determining 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 switch their party preference because of reassignment, the results suggest negligible partisan consequences. We present the results for

[30] 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.

all parties individually in Appendix Figure A.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 A.15).

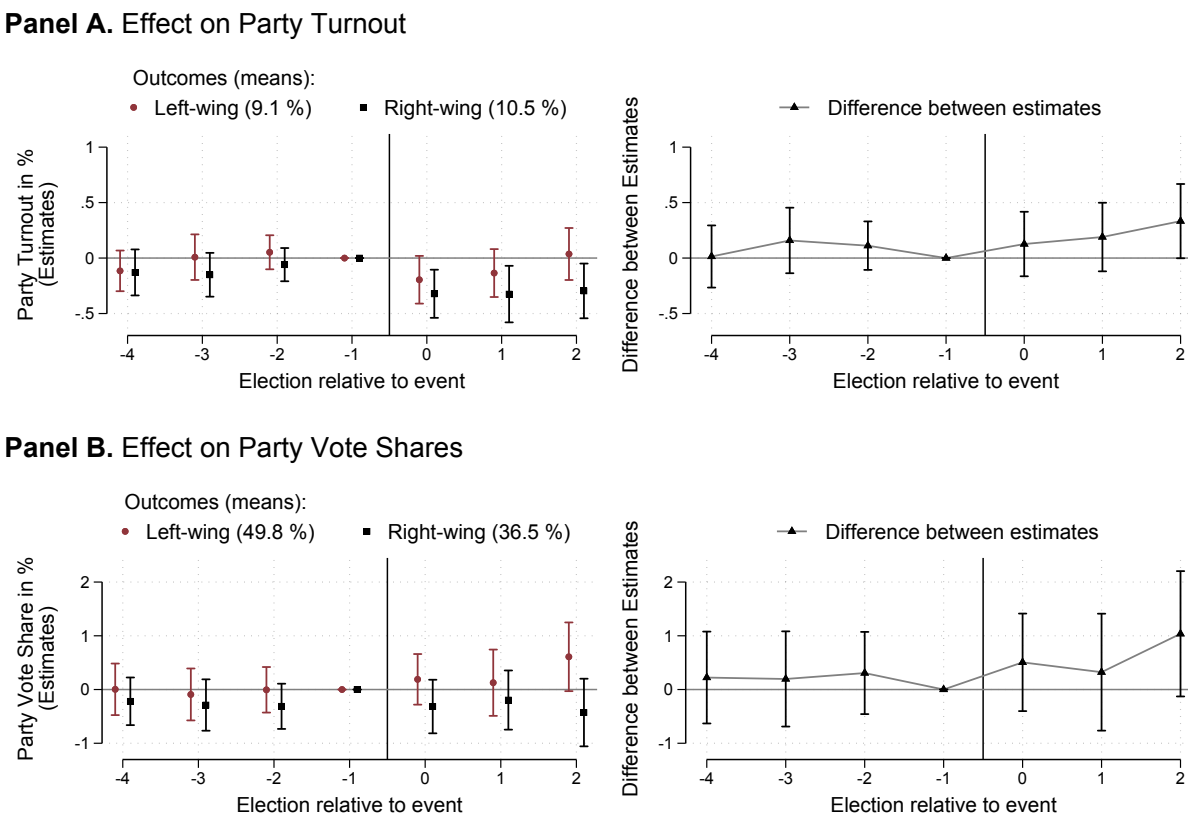
The null effects on the electoral outcomes are reassuring from an administrator's perspective. Polling place relocations are not notably concentrated geographically (Appendix Figure A.7). In addition, the absence of significant spatial segregation along party lines in Munich ensures that polling place relocations are not particularly targeted at a particular party's supporters. The vulnerability to adverse effects is 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.

7. 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 0.4–0.6 percentage points (0.7–1.0 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, for the most part, changes in turnout are attributable to the relocation itself rather than changes in proximity to the polling place. 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.

Heterogeneity analyses suggest that reassignments cause a stronger and more persistent turnout decline in precincts with a higher share of elderly eligible voters. The result is intriguing, given that recruiting new barrier-free locations was a primary motivation for reassigning polling places during our observation period. Thus, our findings highlight that a well-intentioned policy can have unintended consequences when small changes in voting costs are overlooked. We do not find 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

Figure 14: Effects of Reassignments on Party Outcomes at the Polling Place



Notes: The figure presents event study results based on Equation 8. 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. 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. All specifications include time-varying covariates listed in Section 4.1. 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.

two-party systems may be more vulnerable to partisan consequences, justifying particular scrutiny of this practice.

We find that inattention to reassignments likely explains the drop and subsequent recovery in total turnout. Inattentive citizens are surprised by reassignments after the deadline for requesting mail-in ballots has passed. Consequently, some inattentive, who would have switched to voting by mail, instead temporarily abstain and turn to mail-in voting only in the subsequent election. Increasing the salience of polling place relocation is a possible effective remedy against turnout losses by mitigating inattention.

Finally, our results highlight the role of mail-in voting in compensating for the decline in turnout at the polling place. Mail-in voting is rather uncommon by international comparison.[31] Thus, in contexts in which the substitution between modes of voting is limited, negative turnout effects of reassignments are likely larger and more persistent, underscoring the importance of monitoring this practice outside of Germany.

[31] Only 5 percent of countries globally and 27 percent of OECD countries (including Germany, parts of the US, Canada, and the UK) enable mail-in voting for all eligible voters (International Institute for Democracy and Electoral Assistance (IDEA)).

References

Ali, S. N. and Lin, C. (2013). Why People Vote: Ethical Motives and Social Incentives. *American Economic Journal: Microeconomics*, 5(2):73–98.

Amos, B., Smith, D. A., and Ste. Claire, C. (2017). Reprecincting and Voting Behavior. *Political Behavior*, 39(1):133–156.

Athey, S. and Imbens, G. W. (2022). Design-based analysis in Difference-In-Differences settings with staggered adoption. *Journal of Econometrics*, 226(1):62–79.

Bagwe, G., Margitic, J., and Stashko, A. (2022). Polling Place Location and the Costs of Voting.

Barber, M. and Holbein, J. B. (2020). The participatory and partisan impacts of mandatory vote-by-mail. *Science Advances*, 6(35):eabc7685.

Bechtel, M. M., Hangartner, D., and Schmid, L. (2018). Compulsory Voting, Habit Formation, and Political Participation. *The Review of Economics and Statistics*, 100(3):467–476.

Bhatti, Y. (2012). Distance and Voting: Evidence from Danish Municipalities: Distance and Voting. *Scandinavian Political Studies*, 35(2):141–158.

Borusyak, K., Jaravel, X., and Spiess, J. (2022). Revisiting Event Study Designs: Robust and Efficient Estimation. arXiv Preprint 2108.12419.

Braconnier, C., Dormagen, J.-Y., and Pons, V. (2017). Voter Registration Costs and Disenfranchisement: Experimental Evidence from France. *American Political Science Review*, 111(3):584–604.

Brady, H. E. and McNulty, J. E. (2011). Turning Out to Vote: The Costs of Finding and Getting to the Polling Place. *American Political Science Review*, 105(1):115–134.

Brody, R. A. and Sniderman, P. M. (1977). From Life Space to Polling Place: The Relevance of Personal Concerns for Voting Behavior. *British Journal of Political Science*, 7(3):337–360.

Bursztyn, L., Cantoni, D., Funk, P., Schönenberger, F., and Yuchtman, N. (2022). Identifying the Effect of Election Closeness on Voter Turnout: Evidence from Swiss Referenda. NBER Working Paper 23490, National Bureau of Economic Research.

Callaway, B. and Sant'Anna, P. H. (2021). Difference-in-Differences with multiple time periods. *Journal of Econometrics*, 225(2):200–230.

Cantoni, E. (2020). A Precinct Too Far: Turnout and Voting Costs. *American Economic Journal: Applied Economics*, 12(1):61–85.

Cantoni, E. and Pons, V. (2021). Strict Id Laws Don't Stop Voters: Evidence from a U.S. Nationwide Panel, 2008–2018. *The Quarterly Journal of Economics*, 136(4):2615–2660.

Cantoni, E. and Pons, V. (2022). Does Context Outweigh Individual Characteristics in Driving Voting Behavior? Evidence from Relocations within the United States. *American Economic*

Review, 112(4):1226–1272.

Clinton, J. D., Eubank, N., Fresh, A., and Shepherd, M. E. (2021). Polling place changes and political participation: Evidence from North Carolina presidential elections, 2008–2016. *Political Science Research and Methods*, 9(4):800–817.

Curiel, J. A. and Clark, J. T. (2021). Disparities in Poll Closures in the Age of COVID-19: A Case Study of Wisconsin. *Election Law Journal: Rules, Politics, and Policy*, 20(4):345–363.

de Chaisemartin, C. and D'Haultfœuille, X. (2020). Two-Way Fixed Effects Estimators with Heterogeneous Treatment Effects. *American Economic Review*, 110(9):2964–2996.

de Chaisemartin, C. and D'Haultfoeuille, X. (2022). Two-Way Fixed Effects and Differences-in-Differences with Heterogeneous Treatment Effects: A Survey. NBER Working Paper 29691, National Bureau of Economic Research.

Dellavigna, S., List, J. A., Malmendier, U., and Rao, G. (2017). Voting to Tell Others. *The Review of Economic Studies*, 84(1):143–181.

Downs, A. (1957). An Economic Theory of Political Action in a Democracy. *Journal of Political Economy*, 65(2):135–150.

Dyck, J. J. and Gimpel, J. G. (2005). Distance, Turnout, and the Convenience of Voting. *Social Science Quarterly*, 86(3):531–548.

Fauvelle-Aymar, C. and François, A. (2018). Place of registration and place of residence: The non-linear detrimental impact of transportation cost on electoral participation. *Public Choice*, 176(3):405–440.

Fujiwara, T. (2015). Voting Technology, Political Responsiveness, and Infant Health: Evidence From Brazil. *Econometrica*, 83(2):423–464.

Fujiwara, T., Meng, K., and Vogl, T. (2016). Habit Formation in Voting: Evidence from Rainy Elections. *American Economic Journal: Applied Economics*, 8(4):160–188.

Funk, P. (2010). Social Incentives and Voter Turnout: Evidence from the Swiss Mail Ballot System. *Journal of the European Economic Association*, 8(5):1077–1103.

Gaebler, S., Potrafke, N., and Roesel, F. (2020). Compulsory voting and political participation: Empirical evidence from Austria. *Regional Science and Urban Economics*, 81:103499.

Garmann, S. (2017). The effect of a reduction in the opening hours of polling stations on turnout. *Public Choice*, 171(1-2):99–117.

Gentzkow, M., Shapiro, J. M., and Sinkinson, M. (2011). The Effect of Newspaper Entry and Exit on Electoral Politics. *American Economic Review*, 101(7):2980–3018.

Gerber, A. S., Green, D. P., and Shachar, R. (2003). Voting May Be Habit-Forming: Evidence from a Randomized Field Experiment. *American Journal of Political Science*, 47(3):540–550.

Gerber, A. S., Gruber, J., and Hungerman, D. M. (2016). Does Church Attendance Cause People to Vote? Using Blue Laws' Repeal to Estimate the Effect of Religiosity on Voter Turnout. *British Journal of Political Science*, 46(3):481–500.

Gerber, A. S., Huber, G. A., and Hill, S. J. (2013). Identifying the Effect of All-Mail Elections on Turnout: Staggered Reform in the Evergreen State. *Political Science Research and Methods*, 1(1):91–116.

Gibson, J., Kim, B., Stillman, S., and Boe-Gibson, G. (2013). Time to vote? *Public Choice*, 156(3-4):517–536.

Gimpel, J. and Schuknecht, J. (2003). Political participation and the accessibility of the ballot box. *Political Geography*, 22(5):471–488.

Goodman-Bacon, A. (2021). Difference-in-differences with variation in treatment timing. *Journal of Econometrics*, 225(2):254–277.

Green, D. P. and Shachar, R. (2000). Habit Formation and Political Behaviour: Evidence of Consuetude in Voter Turnout. *British Journal of Political Science*, 30(4):561–573.

Haspel, M. and Knotts, H. G. (2005). Location, Location, Location: Precinct Placement and the Costs of Voting. *The Journal of Politics*, 67(2):560–573.

Hodler, R., Luechinger, S., and Stutzer, A. (2015). The Effects of Voting Costs on the Democratic Process and Public Finances. *American Economic Journal: Economic Policy*, 7(1):141–171.

Hoffman, M., León, G., and Lombardi, M. (2017). Compulsory voting, turnout, and government spending: Evidence from Austria. *Journal of Public Economics*, 145:103–115.

Huber, S. and Rust, C. (2016). Calculate Travel Time and Distance with Openstreetmap Data Using the Open Source Routing Machine (OSRM). *The Stata Journal*, 16(2):416–423.

Kaplan, E. and Yuan, H. (2020). Early Voting Laws, Voter Turnout, and Partisan Vote Composition: Evidence from Ohio. *American Economic Journal: Applied Economics*, 12(1):32–60.

MacKinnon, J. G., Nielsen, M. Ø., and Webb, M. D. (2022). Cluster-robust inference: A guide to empirical practice. *Journal of Econometrics*.

McNulty, J. E., Dowling, C. M., and Ariotti, M. H. (2009). Driving Saints to Sin: How Increasing the Difficulty of Voting Dissuades Even the Most Motivated Voters. *Political Analysis*, 17(4):435–455.

Meredith, M. (2009). Persistence in Political Participation. *Quarterly Journal of Political Science*, 4(3):187–209.

Milligan, K., Moretti, E., and Oreopoulos, P. (2004). Does education improve citizenship? Evidence from the United States and the United Kingdom. *Journal of Public Economics*, 88(9):1667–1695.

Ortoleva, P. and Snowberg, E. (2015). Overconfidence in Political Behavior. *American Economic Review*, 105(2):504–535.

Plutzer, E. (2002). Becoming a Habitual Voter: Inertia, Resources, and Growth in Young Adulthood. *American Political Science Review*, 96(1):41–56.

Pons, V. and Tricaud, C. (2018). Expressive Voting and Its Cost: Evidence From Runoffs With Two or Three Candidates. *Econometrica*, 86(5):1621–1649.

Potrafke, N. and Roesel, F. (2020). Opening hours of polling stations and voter turnout: Evidence from a natural experiment. *The Review of International Organizations*, 15(1):133–163.

Riker, W. H. and Ordeshook, P. C. (1968). A Theory of the Calculus of Voting. *American Political Science Review*, 62(1):25–42.

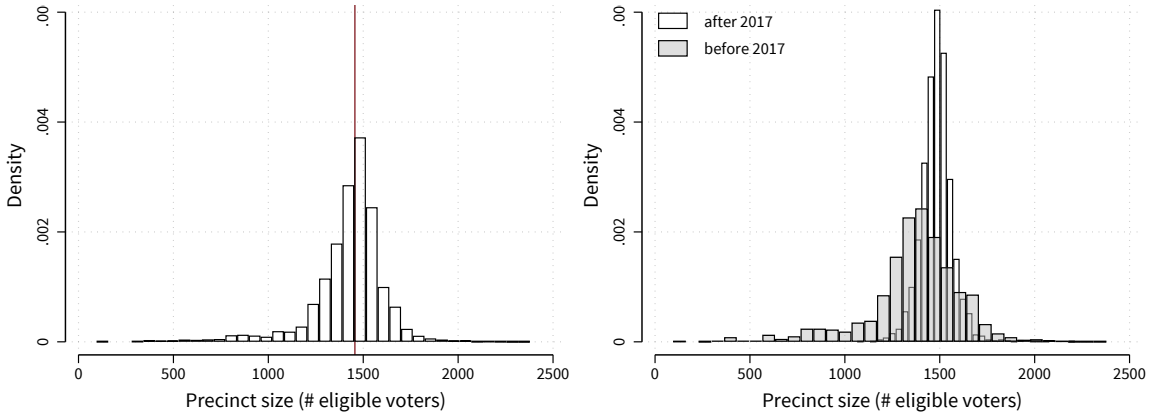
Roth, J., Sant'Anna, P. H. C., Bilinski, A., and Poe, J. (2022). What's Trending in Difference-in-Differences? A Synthesis of the Recent Econometrics Literature. arXiv Preprint arXiv:2201.01194, arXiv.

Sun, L. and Abraham, S. (2021). Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of Econometrics*, 225(2):175–199.

Thompson, D. M., Wu, J. A., Yoder, J., and Hall, A. B. (2020). Universal vote-by-mail has no impact on partisan turnout or vote share. *Proceedings of the National Academy of Sciences*, 117(25):14052–14056.

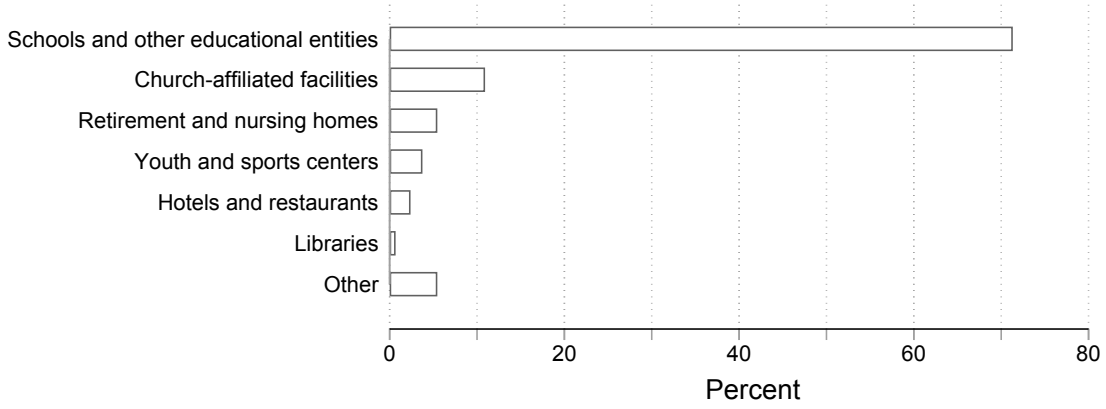
Appendix A. Figures

Figure A.1: Distribution of Precinct Size



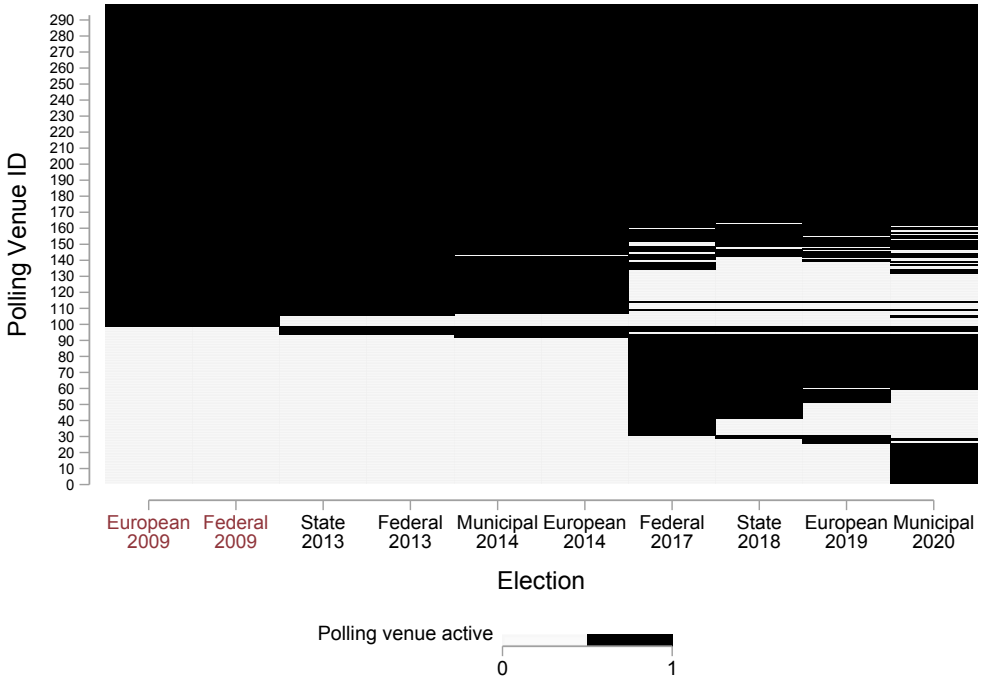
Notes: The figure plots the distribution of precinct of size (number of eligible voters) over all elections (left plot) and before and after 2017 when the Elections Office performed a major reconfiguration of precinct boundaries (right plot). Precincts are delineated according to their election-specific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

Figure A.2: Types of Polling Venues



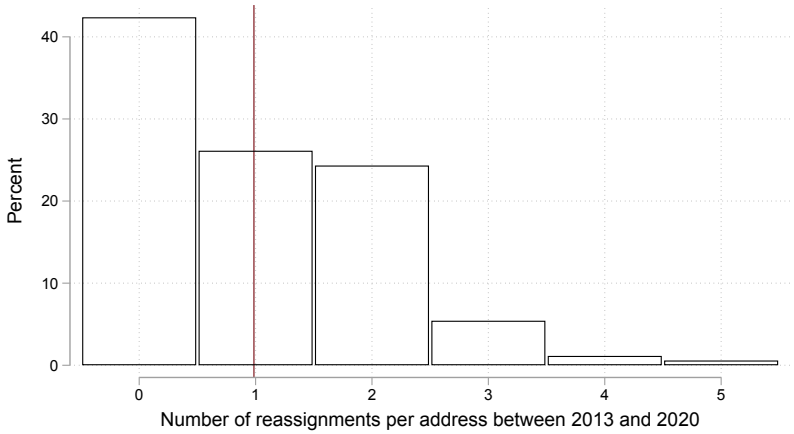
Notes: The figure shows the distribution polling venues over different categories in the eight elections held in Munich between 2013 and 2020 (293 distinct venues in total).

Figure A.3: Activity Status of Polling Venues between 2009 and 2020



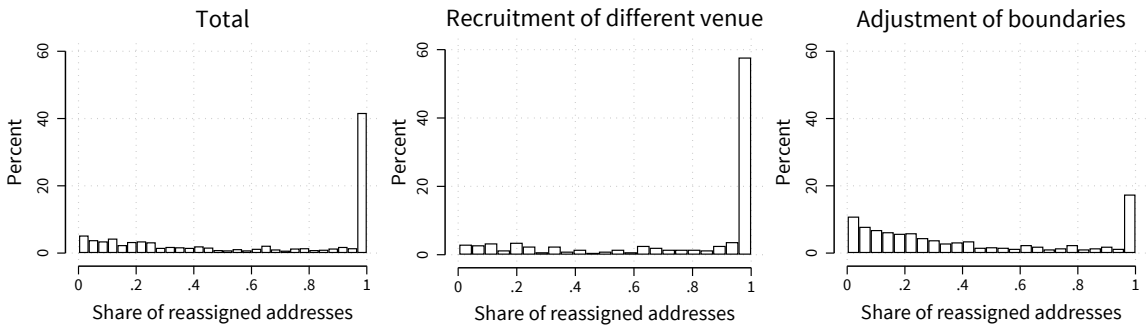
Notes: The figure illustrates the activity status of polling places in each election. We observe 293 distinct venues between 2013 and 2020. The 2009 European and Federal Elections are not part of our estimation sample (highlighted). Six venues were active only in 2009.

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



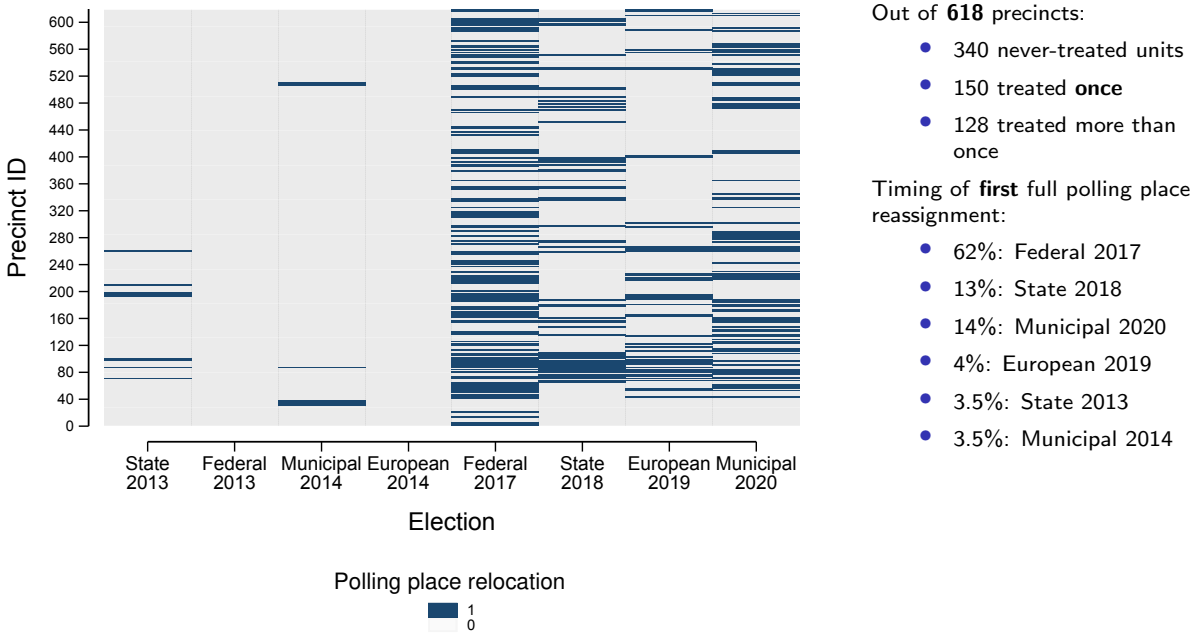
Notes: The figure plots the frequency of polling places reassignments (relative to the previous election) for residential addresses between 2013 and 2020. The vertical line highlights the mean.

Figure A.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 precinct boundaries (right). Observations with zero reassignments are excluded.

Figure A.6: Timing of Polling Place Reassignments



Out of **618** precincts:

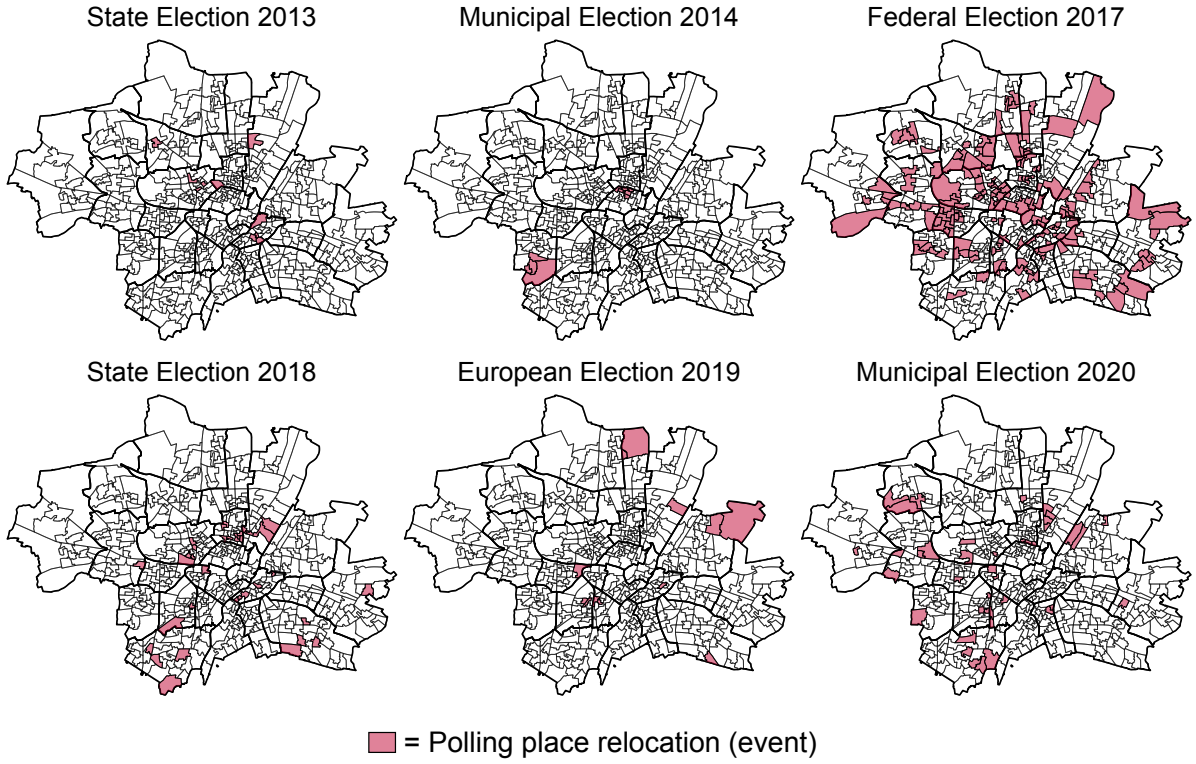
- 340 never-treated units
- 150 treated **once**
- 128 treated more than once

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

- 62%: Federal 2017
- 13%: State 2018
- 14%: Municipal 2020
- 4%: European 2019
- 3.5%: State 2013
- 3.5%: Municipal 2014

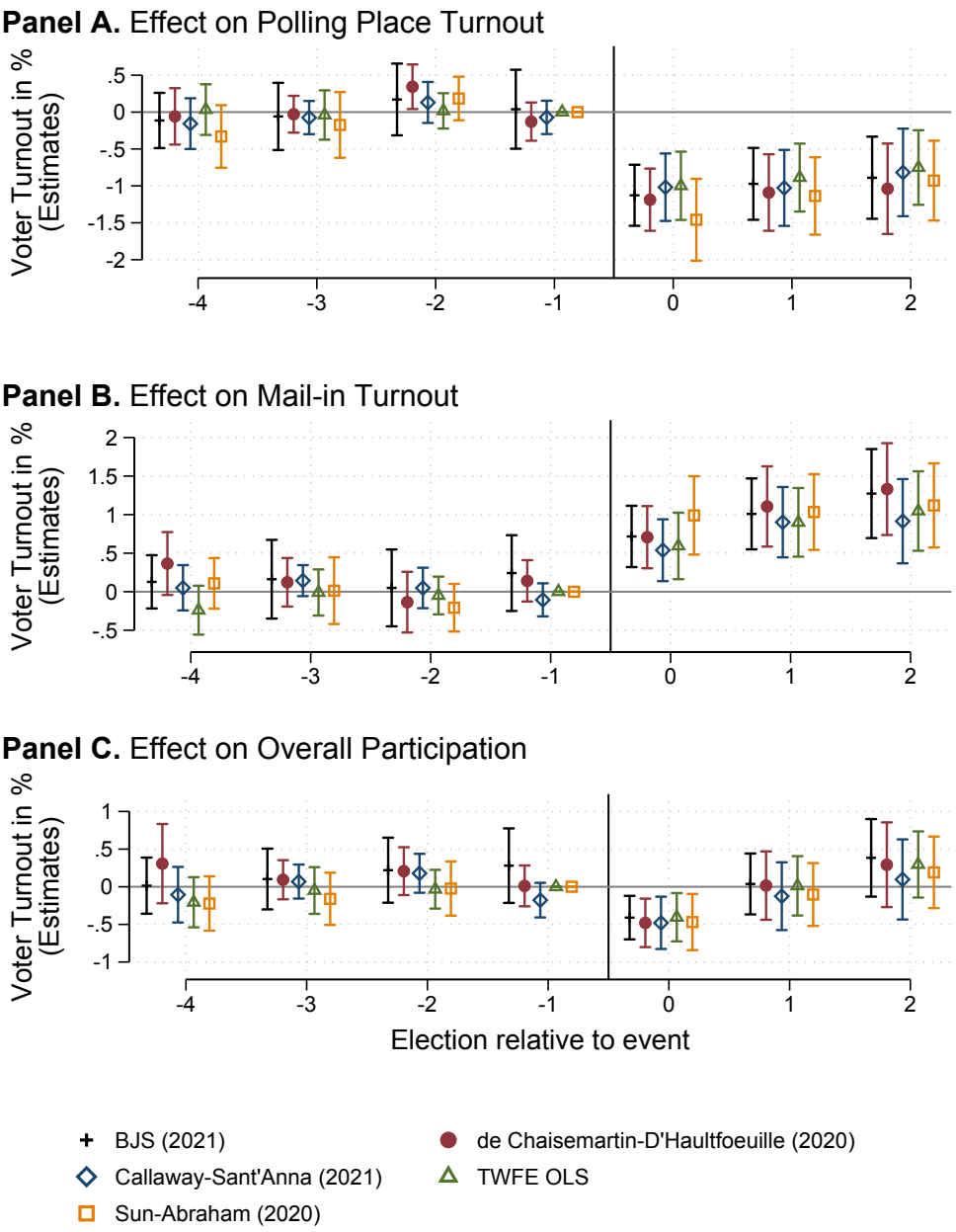
Notes: The figure illustrates the timing of polling place relocations (relative to the previous election) for the 618 precincts in our sample. Highlighted cells indicate that the entire precinct, i.e., 100% of home addresses, is assigned to a different polling place.

Figure A.7: Spatial Distribution of Polling Place Reassignments



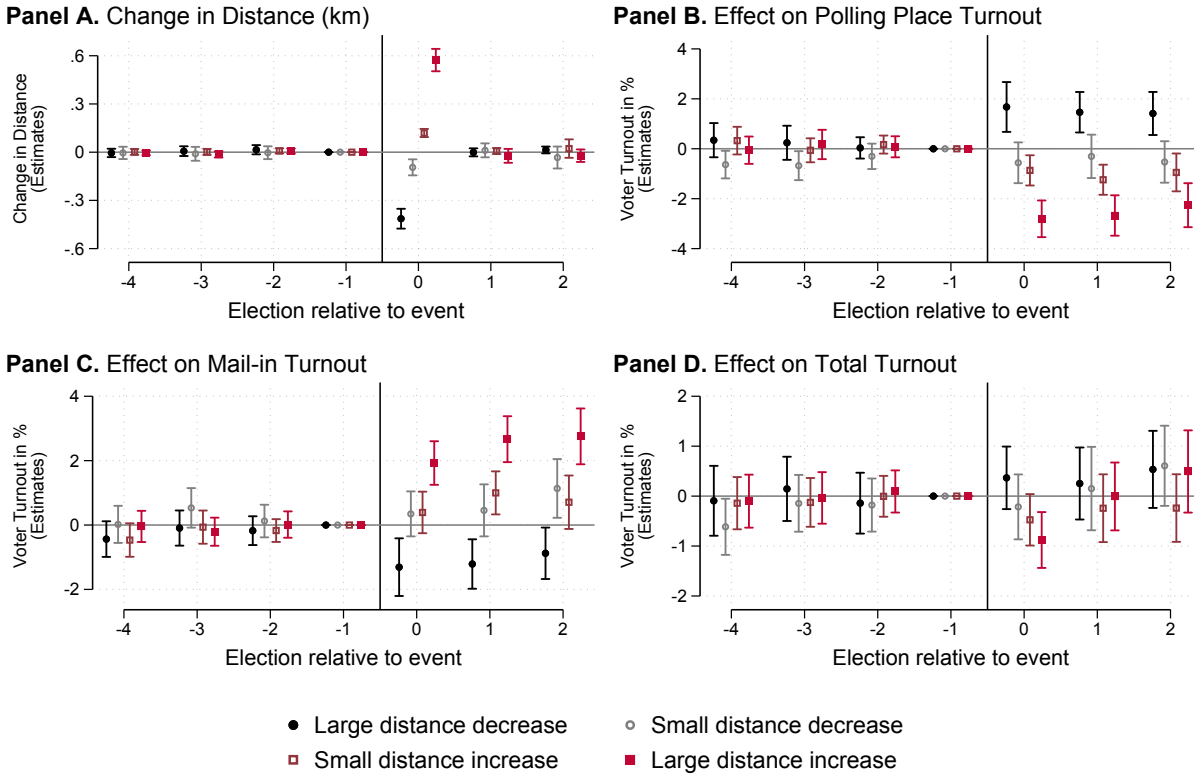
Notes: The maps illustrate the timing of polling place relocations (relative to the previous election) for the 618 precincts in our sample. Precinct boundaries are harmonized to the 2018 delineation to allow comparisons over time. Highlighted precincts indicate that the entire precinct, i.e., 100% of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

Figure A.8: Robustness of Event Study Results to Novel Estimators



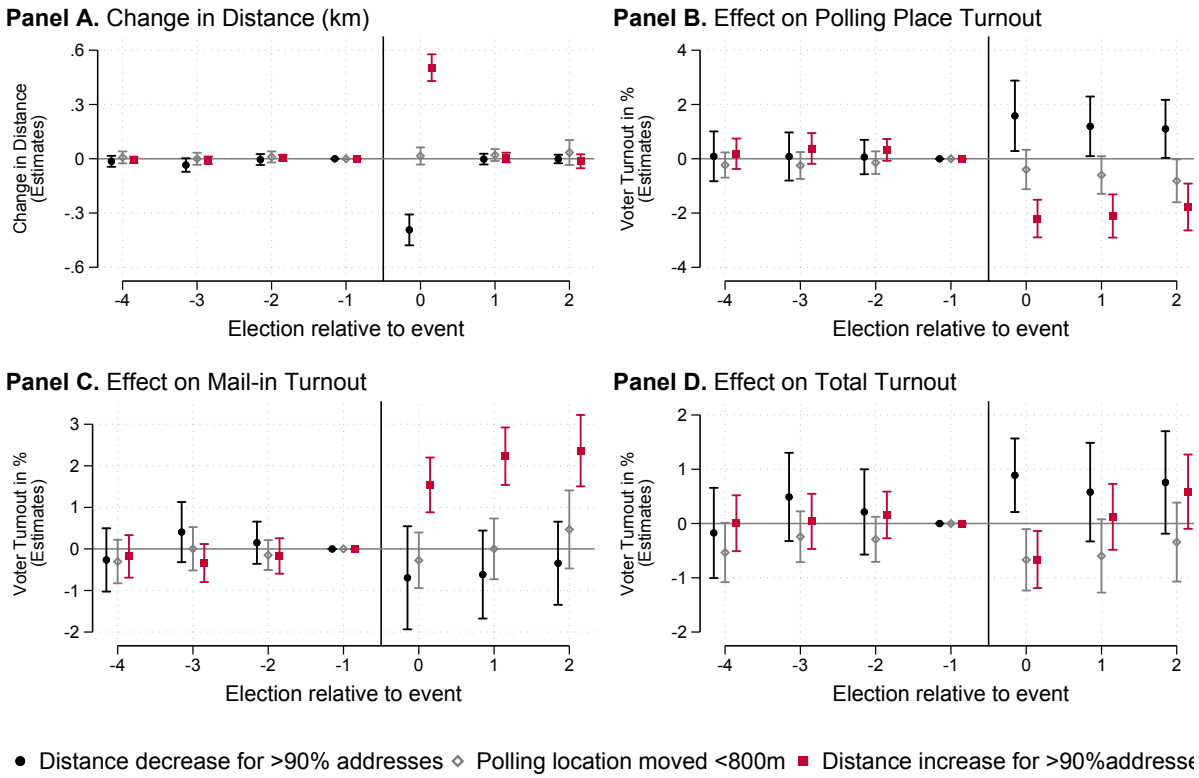
Notes: The figure presents event study results based on the specification presented in Column (4) of Table B.3 (i.e., Equation 8 using election fixed effects instead of election-district fixed effect). The model is estimated using TWFE-OLS as well as the estimators proposed by Borusyak et al. (2022), 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. All specifications include time-varying covariates listed in Section 4.1. 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 A.9: Effect Heterogeneity by Change in Proximity to the Polling Location



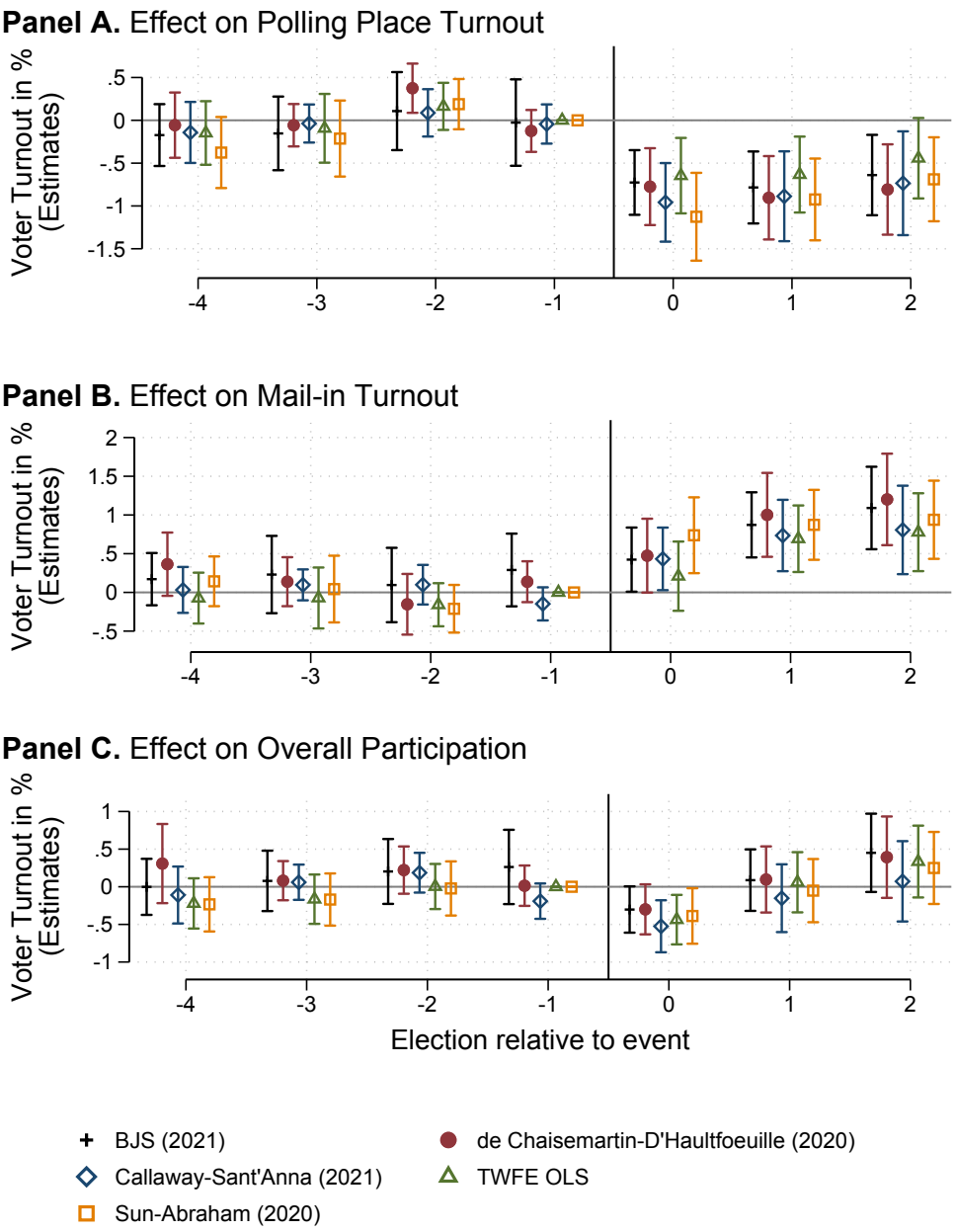
Notes: The figure presents event study results based on a version of Equation 9 in which event-time dummies are interacted separately with four mutually exclusive treatment indicators: two for distance increase and two for distance decrease due to reassignment. The event is defined as the first time 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 A.10: Effect Heterogeneity by Change in Proximity Restricted to Cases with Consistent Distance Changes



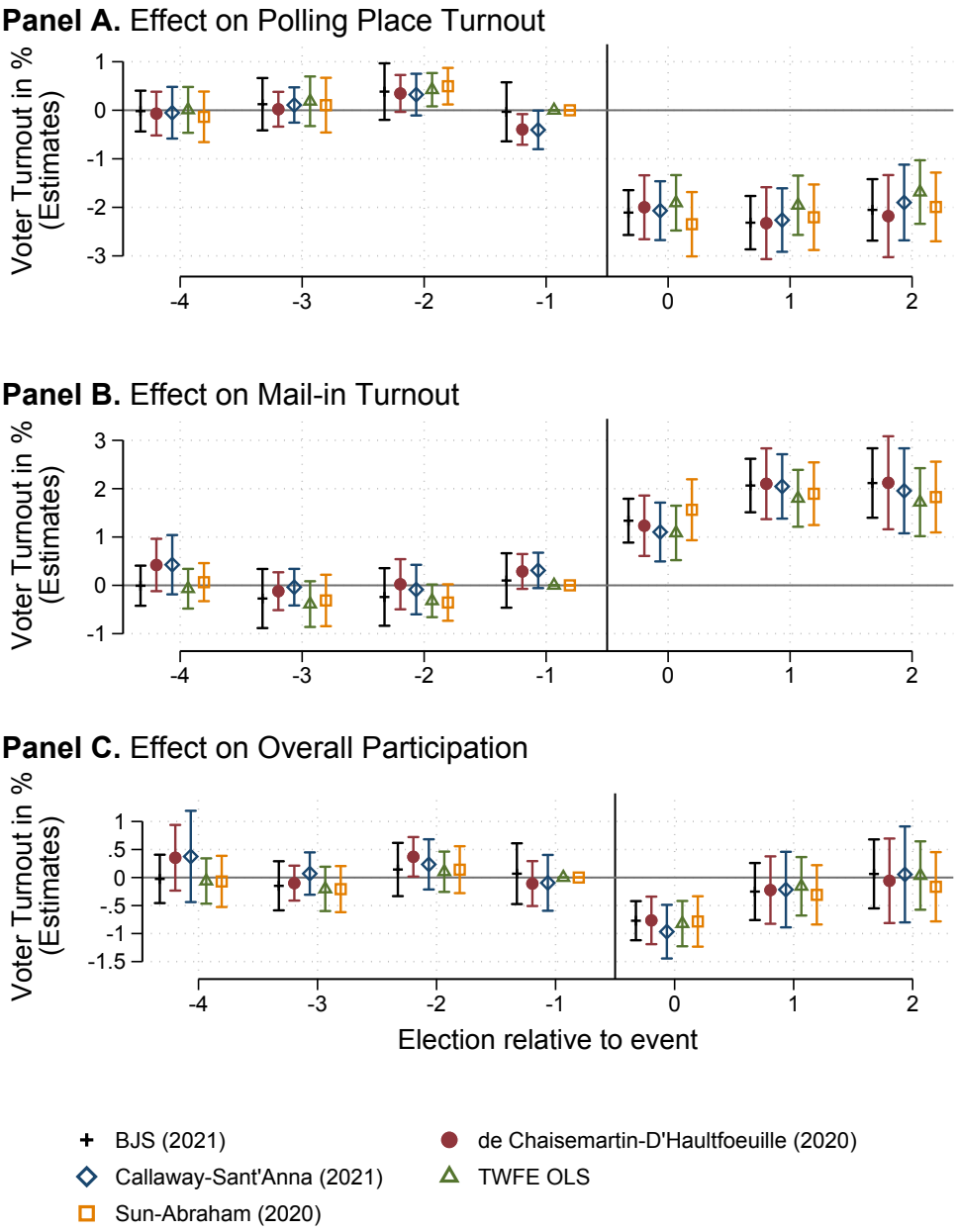
Notes: The figure presents event study results based on a version of Equation 9 in which event-time dummies are interacted separately with three mutually exclusive treatment indicators, identifying precincts where reassignments consistently increased (decreased) the distance for at least 90 percent of 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 A.11: Event Study Results Absorbing the Distance Effect



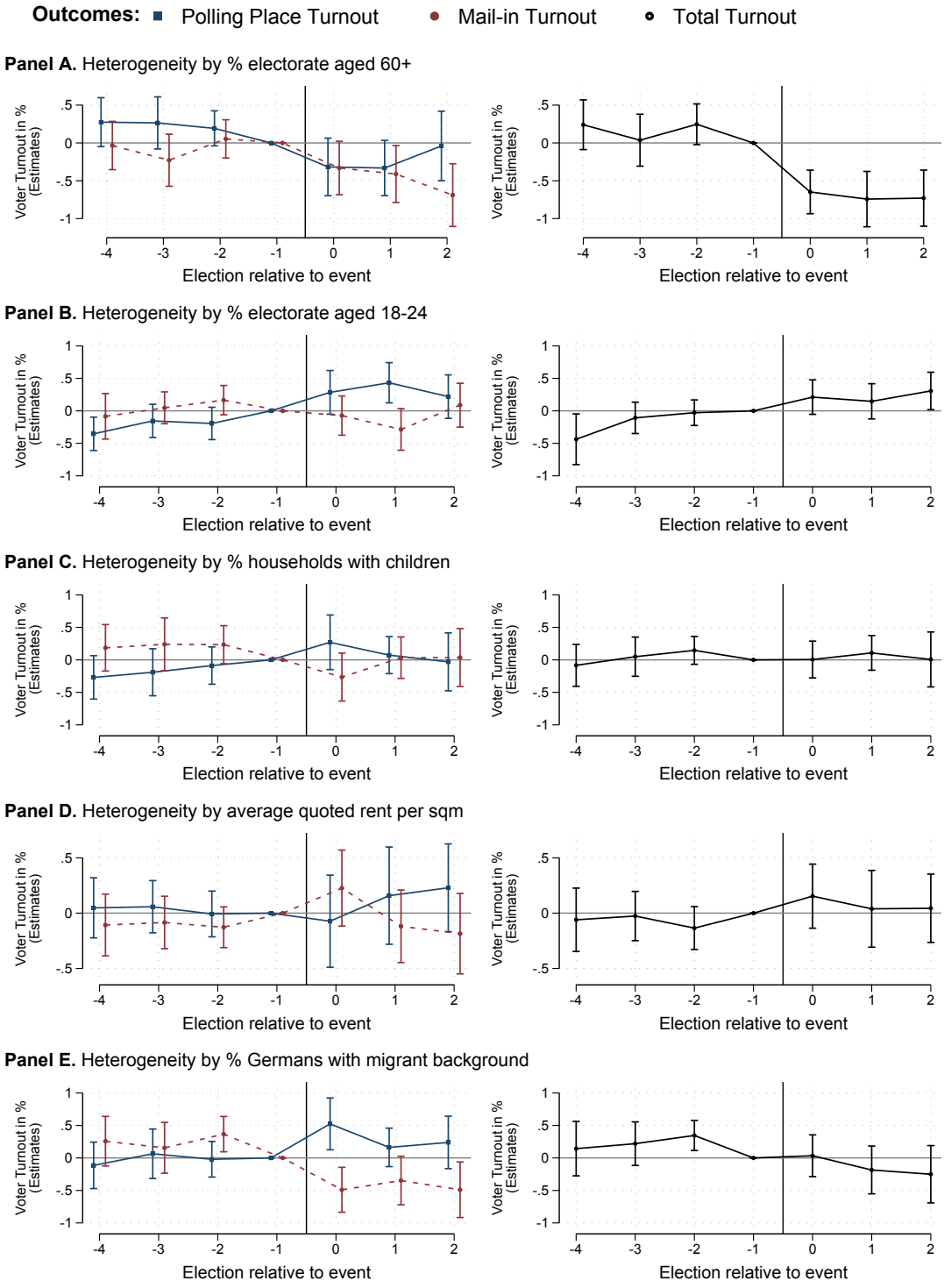
Notes: The figure presents event study results based on the specification presented in Column (4) of Table B.3 (i.e., Equation 8 using election fixed effects instead of election-district fixed effect). The model is estimated using TWFE-OLS as well as the estimators proposed by Borusyak et al. (2022), 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. All specifications include time-varying covariates listed in Section 4.1. 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 A.12: Event Study Results Restricted to Units with Increased Distance



Notes: The figure presents event study results based on the specification presented in Column (4) of Table B.3 (i.e., Equation 8 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. (2022), 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. All specifications include time-varying covariates listed in Section 4.1. 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.

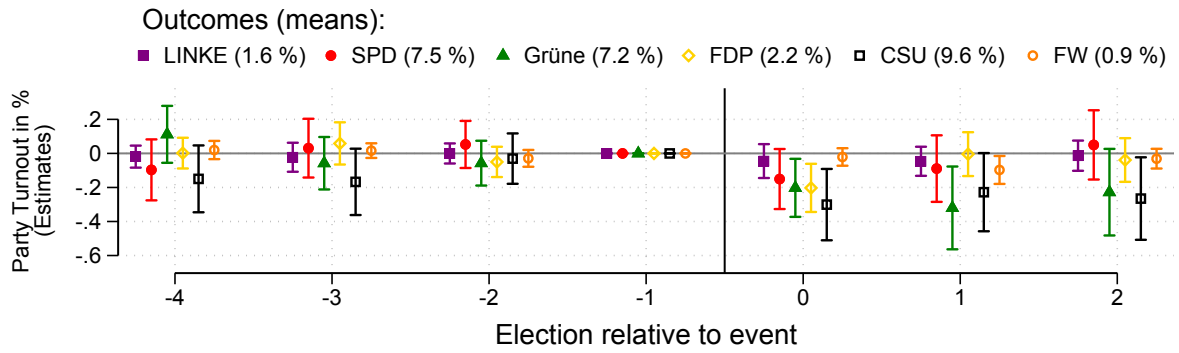
Figure A.13: Effect Heterogeneity by Precinct Characteristics Conditional on Distance



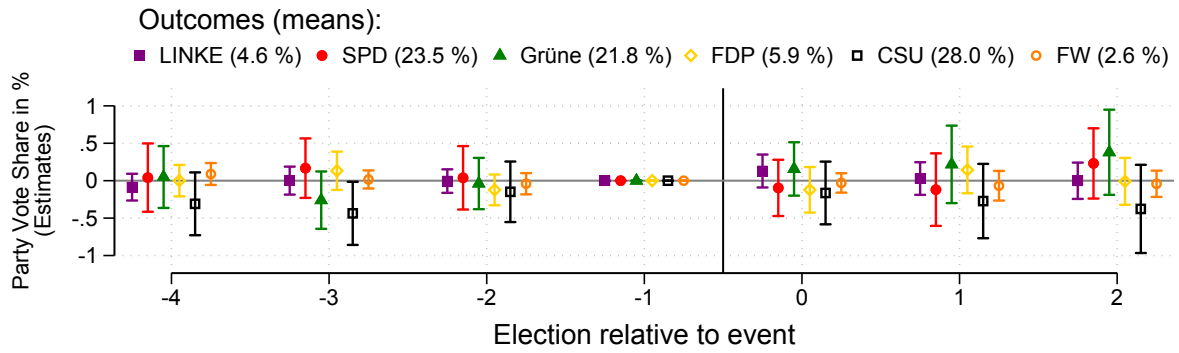
Notes: The figure presents event study results based on the triple difference estimator introduced in Equation 10 conditional on log street distance. Each panel uses a different heterogeneity dimension Z_p and plots the triple-difference coefficients $\hat{\gamma}^k$ for the three outcomes, polling place turnout, mail-in turnout, and overall 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 4.1. 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 A.14: Differential Effects of Reassignments on Party Outcomes

Panel A. Effect on Party Turnout



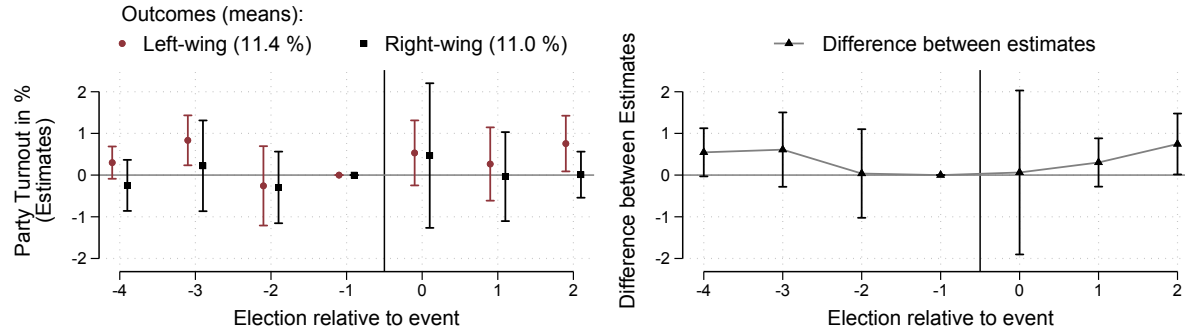
Panel B. Effect on Party Vote Shares



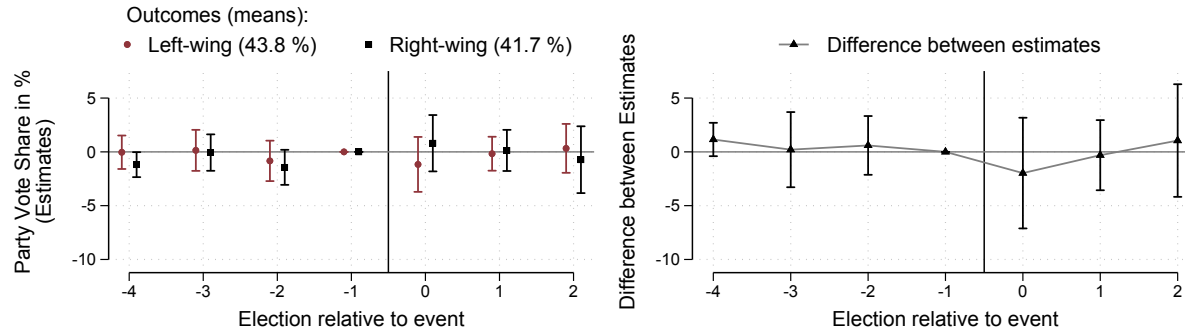
Notes: The figure presents event study results based on Equation 8. The outcomes in Panel A are party turnout defined as the number of votes relative to the number of eligible voters for the six largest parties that stood election in every election included in our panel, respectively. Dependent variables 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. All specifications include time-varying covariates listed in Section 4.1. 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 A.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 the at least 70 percent of the district is reassigned to a different polling place. All specifications include time-varying covariates listed in Section 4.1. 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 B. Tables

Table B.1: 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
Overall Turnout	63.15	14.57	15.10	51.20	65.27	75.26	91.72
Variables of Interest							
Avg. Street Distance to the Polling Place (km)	0.71	0.34	0.16	0.47	0.63	0.87	2.83
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							
Number of Residents	2,428	403	758	2,169	2,325	2,591	6,272
% 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	21.69	4.45	6.80	18.53	21.72	24.51	45.11
Avg. Quoted Rent per sqm	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 held between 2013 and 2020). The statistics are *not* weighted and might therefore differ from values reported in the main text.

Table B.2: Balancing Test on 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. Street Distance
#residents	0.009	-0.005	0.022	0.012	0.010	-0.002
	(0.013)	(0.018)	(0.014)	(0.011)	(0.013)	(0.012)
#single residents	0.010	0.005	0.030*	0.019	0.012	0.008
	(0.016)	(0.021)	(0.016)	(0.013)	(0.015)	(0.015)
#married residents	0.001	-0.022	0.015	0.000	0.014	-0.012
	(0.015)	(0.024)	(0.018)	(0.012)	(0.016)	(0.016)
#native German residents	0.005	-0.018	0.007	-0.005	0.012	-0.002
	(0.010)	(0.015)	(0.012)	(0.007)	(0.011)	(0.012)
#non-native German residents	0.015	-0.012	0.028	0.008	0.020	-0.029*
	(0.020)	(0.028)	(0.021)	(0.015)	(0.018)	(0.017)
#foreign residents	0.009	0.012	0.028	0.026	0.002	0.007
	(0.019)	(0.020)	(0.018)	(0.016)	(0.014)	(0.015)
#inhabitants eligible to vote	0.009	-0.004	0.008	-0.008	0.017	-0.005
	(0.013)	(0.016)	(0.012)	(0.008)	(0.011)	(0.012)
#eligible voters aged 18-24	0.009	-0.004	0.004	0.001	0.003	0.012
	(0.010)	(0.012)	(0.010)	(0.006)	(0.009)	(0.008)
#eligible voters aged 25-34	0.003	0.011	0.016	-0.007	0.023*	0.017
	(0.012)	(0.015)	(0.012)	(0.007)	(0.012)	(0.013)
#eligible voters aged 35-44	-0.005	-0.006	0.008	-0.002	0.010	-0.003
	(0.009)	(0.012)	(0.009)	(0.006)	(0.009)	(0.008)
#eligible voters aged 45-59	0.015	-0.017	0.013	-0.002	0.015	-0.008
	(0.010)	(0.013)	(0.010)	(0.007)	(0.009)	(0.009)
#eligible voters aged 60+	0.010	-0.006	-0.003	0.001	-0.004	-0.021*
	(0.013)	(0.015)	(0.013)	(0.010)	(0.011)	(0.011)
#German eligible voters	0.010	-0.006	0.011	-0.003	0.014	-0.009
	(0.009)	(0.012)	(0.009)	(0.005)	(0.009)	(0.010)
#EU-foreign eligible voters	0.003	-0.003	0.008	-0.002	0.010	0.009
	(0.010)	(0.015)	(0.010)	(0.007)	(0.010)	(0.008)
% households with children	-0.009	-0.007	0.019	0.016	0.004	0.026
	(0.020)	(0.026)	(0.022)	(0.013)	(0.020)	(0.020)
Avg. quoted rent per sqm	0.015	-0.003	0.005	-0.006	0.011	0.007
	(0.012)	(0.015)	(0.012)	(0.008)	(0.011)	(0.011)
Avg. duration of residence	0.002	-0.003	-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	0.57 [0.91]	0.66 [0.84]	0.51 [0.95]	1.04 [0.42]	0.53 [0.94]	1.07 [0.38]
Precinct FE	×	×	×	×	×	×
Election FE	×	×	×	×	×	×

Notes: Each cell in Columns (1) through (6) reports an OLS estimate from a separate univariate regression on precinct characteristics (in rows), conditional an 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 adjustment to precinct boundaries (Column 4), the share of addresses reassigned due to the recruitment of a different polling place (Column 5), and the log of average street distance to the polling location (Column 6), respectively. $F-$tests for the null that coefficients are jointly equal to zero are reported with p values in parentheses. Regressions are weighted by the number of eligible voters. Standard errors are clustered at the precinct level and reported in parentheses. $^{***}p < 0.01,^{**}p < 0.05,^{*}p < 0.1$.

Table B.3: Baseline Event Study Results

	(1)	(2)	(3)	(4)	(5)
Panel A: Effect on Turnout at the Polling Place [Mean outcome=33.7]					
Reassignment ($t-4$)	-0.02	0.03	0.02	-0.11	-0.16
	(0.19)	(0.18)	(0.18)	(0.20)	(0.16)
Reassignment ($t-3$)	-0.06	-0.04	-0.05	-0.03	-0.30*
	(0.18)	(0.17)	(0.17)	(0.21)	(0.16)
Reassignment ($t-2$)	-0.12	0.02	0.02	0.16	-0.07
	(0.14)	(0.12)	(0.12)	(0.14)	(0.11)
Reassignment ($t+0$)	-1.12***	-1.00***	-1.02***	-1.07***	-1.25***
	(0.25)	(0.24)	(0.23)	(0.24)	(0.20)
Reassignment ($t+1$)	-0.97***	-0.89***	-0.80***	-0.87***	-1.42***
	(0.25)	(0.23)	(0.21)	(0.25)	(0.21)
Reassignment ($t+2$)	-0.75***	-0.75***	-0.53**	-0.70***	-1.19***
	(0.28)	(0.26)	(0.22)	(0.27)	(0.23)
R^2	0.97	0.97	0.97	0.96	0.97
Panel B: Effect on Turnout via Mail [Mean outcome=28.7]					
Reassignment ($t-4$)	-0.21	-0.24	-0.22	-0.11	-0.06
	(0.18)	(0.16)	(0.16)	(0.17)	(0.15)
Reassignment ($t-3$)	0.08	-0.01	-0.00	-0.12	0.06
	(0.16)	(0.15)	(0.15)	(0.20)	(0.14)
Reassignment ($t-2$)	-0.17	-0.05	-0.04	-0.15	-0.07
	(0.13)	(0.12)	(0.12)	(0.14)	(0.11)
Reassignment ($t+0$)	0.52**	0.59***	0.60***	0.54**	0.68***
	(0.23)	(0.22)	(0.22)	(0.23)	(0.19)
Reassignment ($t+1$)	0.87***	0.90***	0.73***	0.87***	1.15***
	(0.24)	(0.23)	(0.21)	(0.24)	(0.21)
Reassignment ($t+2$)	0.90***	1.05***	0.72***	0.98***	1.34***
	(0.29)	(0.26)	(0.23)	(0.28)	(0.23)
R^2	0.95	0.96	0.96	0.95	0.96
Panel C: Effect on Total Turnout [Mean outcome=62.4]					
Reassignment ($t-4$)	-0.23	-0.21	-0.20	-0.21	-0.23
	(0.20)	(0.17)	(0.17)	(0.17)	(0.15)
Reassignment ($t-3$)	0.02	-0.05	-0.05	-0.15	-0.24*
	(0.19)	(0.16)	(0.16)	(0.17)	(0.14)
Reassignment ($t-2$)	-0.29	-0.03	-0.02	0.00	-0.14
	(0.18)	(0.13)	(0.13)	(0.15)	(0.12)
Reassignment ($t+0$)	-0.60***	-0.41**	-0.42**	-0.54***	-0.57***
	(0.20)	(0.16)	(0.16)	(0.17)	(0.16)
Reassignment ($t+1$)	-0.10	0.01	-0.07	0.00	-0.27
	(0.25)	(0.20)	(0.19)	(0.20)	(0.19)
Reassignment ($t+2$)	0.15	0.30	0.19	0.27	0.16
	(0.30)	(0.22)	(0.21)	(0.24)	(0.22)
R^2	0.98	0.99	0.99	0.99	0.99
Observations	4,672	4,672	4,944	4,672	4,528
Controls		×	×	×	×
Precinct FE	×	×	×	×	×
Election-District FE	×	×	×		×
Election FE				×	
Full sample			×		
Event: 100% reassigned	×	×	×	×	
Event: >50% reassigned					×

Notes: The table presents event study results based on Equation 8. The dependent variables are voter turnout (0–100) at the polling place (Panel A), by mail (Panel B), and overall (Panel C). In Columns (1)–(4), the event is defined as the first time in which the *entire* precinct is reassigned to a different polling place; in Column (5) the event occurs when at least 50 percent of addresses are reassigned. Columns (2)–(5) include time-varying covariates listed in Section 4.1. Except in Column (3), observations are dropped after a second reassignment (if any). Regressions are weighted by the number of eligible voters. Standard errors are clustered at the precinct level and reported in parentheses. $^{***}p < 0.01$,$^{**}p < 0.05$,$^{*}p < 0.1$.

Table B.4: Robustness of Event Study Results to Different Levels of Clustering

	(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 ($t-4$)	0.03	0.03	0.03	0.03	-0.11
	(0.18)	(0.19)	[0.865]	[0.870]	[0.561]
Reassignment ($t-3$)	-0.04	-0.04	-0.04	-0.04	-0.03
	(0.17)	(0.19)	[0.820]	[0.837]	[0.872]
Reassignment ($t-2$)	0.02	0.02	0.02	0.02	0.16
	(0.12)	(0.14)	[0.904]	[0.886]	[0.342]
Reassignment ($t+0$)	-1.00***	-1.00***	-1.00***	-1.00***	-1.07***
	(0.24)	(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]	[0.030]	[0.052]
Panel B: Effect on Turnout via Mail					
Reassignment ($t-4$)	-0.24	-0.24	-0.24	-0.24	-0.11
	(0.16)	(0.16)	[0.133]	[0.221]	[0.497]
Reassignment ($t-3$)	-0.01	-0.01	-0.01	-0.01	-0.12
	(0.15)	(0.16)	[0.957]	[0.949]	[0.604]
Reassignment ($t-2$)	-0.05	-0.05	-0.05	-0.05	-0.15
	(0.12)	(0.14)	[0.712]	[0.691]	[0.438]
Reassignment ($t+0$)	0.59***	0.59**	0.59**	0.59**	0.54*
	(0.22)	(0.23)	[0.013]	[0.020]	[0.065]
Reassignment ($t+1$)	0.90***	0.90***	0.90***	0.90***	0.87**
	(0.23)	(0.25)	[0.001]	[0.002]	[0.014]
Reassignment ($t+2$)	1.05***	1.05***	1.05***	1.05***	0.98**
	(0.26)	(0.27)	[0.000]	[0.000]	[0.012]
Panel C: Effect on Total Turnout					
Reassignment ($t-4$)	-0.21	-0.21	-0.21	-0.21	-0.21
	(0.17)	(0.17)	[0.214]	[0.256]	[0.229]
Reassignment ($t-3$)	-0.05	-0.05	-0.05	-0.05	-0.15
	(0.16)	(0.16)	[0.739]	[0.766]	[0.388]
Reassignment ($t-2$)	-0.03	-0.03	-0.03	-0.03	0.00
	(0.13)	(0.13)	[0.806]	[0.839]	[0.993]
Reassignment ($t+0$)	-0.41**	-0.41**	-0.41**	-0.41**	-0.54***
	(0.16)	(0.18)	[0.022]	[0.022]	[0.003]
Reassignment ($t+1$)	0.01	0.01	0.01	0.01	0.00
	(0.20)	(0.21)	[0.951]	[0.955]	[0.982]
Reassignment ($t+2$)	0.30	0.30	0.30	0.30*	0.27
	(0.22)	(0.21)	[0.187]	[0.094]	[0.399]
Observations	4,672	4,672	4,672	4,672	4,672
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 8. 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. All specifications include time-varying covariates listed in Section 4.1. Regressions are weighted by the number of eligible voters. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

Table B.5: Effect Heterogeneity by Change in Proximity to the Polling Location

	(1) Polling Place Turnout	(2) Mail-in Turnout	(3) Total Turnout
$\mathbb{1}$(Distance decrease)×			
Reassignment $(t-4)$	-0.19	-0.17	-0.36
	(0.24)	(0.22)	(0.24)
Reassignment $(t-3)$	-0.24	0.24	-0.00
	(0.24)	(0.22)	(0.23)
Reassignment $(t-2)$	-0.16	-0.02	-0.17
	(0.18)	(0.18)	(0.21)
Reassignment $(t+0)$	0.47	-0.40	0.07
	(0.35)	(0.31)	(0.24)
Reassignment $(t+1)$	0.55*	-0.35	0.20
	(0.32)	(0.31)	(0.28)
Reassignment $(t+2)$	0.47	0.07	0.54*
	(0.34)	(0.35)	(0.30)
$\mathbb{1}$(Distance increase)×			
Reassignment $(t-4)$	0.14	-0.26	-0.12
	(0.21)	(0.20)	(0.20)
Reassignment $(t-3)$	0.07	-0.15	-0.08
	(0.20)	(0.18)	(0.19)
Reassignment $(t-2)$	0.13	-0.09	0.05
	(0.14)	(0.14)	(0.15)
Reassignment $(t+0)$	-1.87***	1.18***	-0.68***
	(0.27)	(0.26)	(0.20)
Reassignment $(t+1)$	-1.96***	1.83***	-0.14
	(0.27)	(0.27)	(0.25)
Reassignment $(t+2)$	-1.63***	1.76***	0.12
	(0.31)	(0.33)	(0.28)
R^2	0.97	0.96	0.99
Observations	4,672	4,672	4,672
Mean outcome	33.7	28.7	62.4

Notes: The table reports point estimates and standard errors underlying the plots presented in Figure 11. Estimations are based on Equation 9. 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. All specifications include precinct fixed effects, election×district fixed effects, and time-varying covariates listed in Section 4.1. Regressions are weighted by the number of eligible voters. Standard errors are clustered at the precinct level and reported in parentheses. $^{***}p < 0.01$,$^{**}p < 0.05$,$^{*}p < 0.1$.

Table B.6: Effect Heterogeneity by Change in Proximity to the Polling Location

	(1) Polling Place Turnout	(2) Mail-in Turnout	(3) Total Turnout
𝟙(Distance decrease)×			
Reassignment $(t-4)$	-0.03	-0.31	-0.34
	(0.28)	(0.24)	(0.28)
Reassignment $(t-3)$	-0.21	0.23	0.02
	(0.29)	(0.25)	(0.26)
Reassignment $(t-2)$	-0.10	-0.11	-0.21
	(0.19)	(0.21)	(0.24)
Reassignment $(t+0)$	1.01**	-0.71**	0.29
	(0.40)	(0.36)	(0.27)
Reassignment $(t+1)$	0.97***	-0.58*	0.39
	(0.35)	(0.34)	(0.33)
Reassignment $(t+2)$	0.93**	-0.27	0.65*
	(0.37)	(0.38)	(0.33)
𝟙(Little change in distance)×			
Reassignment $(t-4)$	0.19	-0.50*	-0.31
	(0.27)	(0.26)	(0.25)
Reassignment $(t-3)$	-0.03	-0.02	-0.05
	(0.24)	(0.27)	(0.25)
Reassignment $(t-2)$	0.06	-0.15	-0.09
	(0.21)	(0.19)	(0.21)
Reassignment $(t+0)$	-0.60*	-0.02	-0.62**
	(0.31)	(0.30)	(0.25)
Reassignment $(t+1)$	-0.94***	0.56*	-0.38
	(0.32)	(0.33)	(0.34)
Reassignment $(t+2)$	-0.46	0.35	-0.11
	(0.34)	(0.39)	(0.33)
𝟙(Distance increase)×			
Reassignment $(t-4)$	-0.03	0.01	-0.01
	(0.25)	(0.23)	(0.24)
Reassignment $(t-3)$	0.08	-0.15	-0.07
	(0.27)	(0.21)	(0.23)
Reassignment $(t-2)$	0.13	0.02	0.15
	(0.18)	(0.18)	(0.19)
Reassignment $(t+0)$	-2.73***	2.00***	-0.73***
	(0.33)	(0.30)	(0.26)
Reassignment $(t+1)$	-2.55***	2.53***	-0.01
	(0.35)	(0.32)	(0.30)
Reassignment $(t+2)$	-2.46***	2.77***	0.31
	(0.39)	(0.39)	(0.37)
R^2	0.97	0.96	0.99
Observations	4,672	4,672	4,672
Mean outcome	33.7	28.7	62.4

Notes: The table presents event study results based on a version of Equation 9 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. All specifications include precinct fixed effects, election×district fixed effects, and time-varying covariates listed in Section 4.1. Regressions are weighted by the number of eligible voters. Standard errors are clustered at the precinct level and reported in parentheses. $^{***}p < 0.01$,$^{**}p < 0.05$,$^{*}p < 0.1$.

Table B.7: Difference between Event-Time Indicators in Period 1 and Period 0

	(1) Mail-in turnout	(2) Polling place turnout	(3) Overall turnout
Panel A: Differences based on event study estimates restricted to precincts with increased distance			
BJS (2021)	0.73***	-0.21	0.52**
dChDH (2020)	0.87***	-0.33	0.54**
TWFE-OLS	0.72***	-0.05	0.67***
SA (2020)	0.33	0.14	0.48**
CS (2021)	0.98***	-0.31	0.67**
Panel B: Differences based on event study estimates after absorbing transportation effect			
BJS (2021)	0.45**	-0.06	0.39**
dChDH (2020)	0.53***	-0.13	0.40**
TWFE-OLS	0.48***	0.01	0.50***
SA (2020)	0.13	0.20	0.34**
CS (2021)	0.32*	0.06	0.38*

Notes: The table reports the difference between the event study estimates in period 1 and period 0 relative to reassignment ($\hat{\mu}_1 - \hat{\mu}_0$) for mail-in, in-person, and overall turnout according to the TWFE-OLS estimator and the four novel estimators proposed by Borusyak et al. (2022) (BJS, 2021), Callaway and Sant'Anna (2021) (CS, 2020), Sun and Abraham (2021) (SA, 2020), and de Chaisemartin and D'Haultfœuille (2020) (dChDH, 2020), respectively. Event study estimates in Panel A are obtained on a sample restricted to never-treated precincts and precincts in which reassignments resulted in an increase in average distance. Estimates in Panel B are obtained controlling for the log of street distance to absorb the distance effect. $^{***}p < 0.01$,$^{**}p < 0.05$,$^{*}p < 0.1$.

Table B.8: Effect Heterogeneity by Precinct Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
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	Polling place turnout
$Z_p\times$							
Reassignment $(t-4)$	0.26	-0.31**	-0.23	0.04	-0.03	0.29*	0.49***
	(0.17)	(0.13)	(0.16)	(0.15)	(0.19)	(0.16)	(0.17)
Reassignment $(t-3)$	0.24	-0.13	-0.17	0.05	0.12	0.23	0.23
	(0.18)	(0.12)	(0.18)	(0.12)	(0.19)	(0.16)	(0.15)
Reassignment $(t-2)$	0.18	-0.20	-0.11	-0.01	-0.00	0.15	0.13
	(0.12)	(0.13)	(0.15)	(0.11)	(0.14)	(0.12)	(0.12)
Reassignment $(t+0)$	-0.45*	0.37**	0.37	-0.17	0.74***	-0.30	-0.66***
	(0.23)	(0.19)	(0.26)	(0.26)	(0.26)	(0.23)	(0.23)
Reassignment $(t+1)$	-0.49**	0.60***	0.26	-0.02	0.55***	-0.59***	-0.76***
	(0.21)	(0.19)	(0.21)	(0.26)	(0.20)	(0.20)	(0.23)
Reassignment $(t+2)$	-0.17	0.46**	0.16	-0.05	0.70***	-0.48**	-1.05***
	(0.26)	(0.22)	(0.36)	(0.24)	(0.25)	(0.23)	(0.22)
R^2	0.97	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	Polling place turnout
$Z_p\times$							
Reassignment $(t-4)$	-0.02	-0.12	0.16	-0.10	0.20	0.08	0.06
	(0.16)	(0.17)	(0.18)	(0.14)	(0.19)	(0.15)	(0.15)
Reassignment $(t-3)$	-0.21	0.03	0.23	-0.08	0.11	-0.05	0.02
	(0.17)	(0.13)	(0.20)	(0.12)	(0.20)	(0.15)	(0.15)
Reassignment $(t-2)$	0.06	0.17	0.25*	-0.12	0.35***	0.34***	-0.00
	(0.13)	(0.12)	(0.15)	(0.09)	(0.14)	(0.10)	(0.12)
Reassignment $(t+0)$	-0.22	-0.14	-0.34*	0.30	-0.65***	-0.12	0.52**
	(0.20)	(0.18)	(0.21)	(0.21)	(0.19)	(0.20)	(0.21)
Reassignment $(t+1)$	-0.28	-0.42**	-0.11	0.02	-0.64***	0.15	0.99***
	(0.22)	(0.19)	(0.20)	(0.21)	(0.21)	(0.22)	(0.23)
Reassignment $(t+2)$	-0.58**	-0.10	-0.11	0.03	-0.83***	-0.25	1.10***
	(0.23)	(0.19)	(0.28)	(0.21)	(0.20)	(0.24)	(0.24)
R^2	0.96	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	Polling place turnout
$Z_p\times$							
Reassignment $(t-4)$	0.24	-0.43**	-0.08	-0.06	0.16	0.38*	0.55***
	(0.17)	(0.20)	(0.16)	(0.15)	(0.21)	(0.19)	(0.17)
Reassignment $(t-3)$	0.03	-0.10	0.05	-0.03	0.24	0.18	0.25*
	(0.18)	(0.12)	(0.15)	(0.11)	(0.17)	(0.15)	(0.14)
Reassignment $(t-2)$	0.24*	-0.03	0.14	-0.14	0.35***	0.49***	0.13
	(0.14)	(0.10)	(0.11)	(0.10)	(0.12)	(0.13)	(0.14)
Reassignment $(t+0)$	-0.68***	0.23*	0.03	0.13	0.09	-0.42**	-0.13
	(0.15)	(0.13)	(0.16)	(0.15)	(0.18)	(0.16)	(0.16)
Reassignment $(t+1)$	-0.78***	0.18	0.15	-0.00	-0.09	-0.44**	0.23
	(0.18)	(0.14)	(0.14)	(0.17)	(0.20)	(0.19)	(0.21)
Reassignment $(t+2)$	-0.76***	0.36**	0.05	-0.02	-0.13	-0.73***	0.04
	(0.19)	(0.15)	(0.24)	(0.16)	(0.25)	(0.21)	(0.24)
R^2	0.99	0.99	0.99	0.99	0.99	0.99	0.99
Observations	4,672	4,672	4,672	4,672	4,672	4,672	4,672

Notes: The table reports point estimates and standard errors underlying the plots presented in Figure 13. Results are based on the triple-difference estimator presented in Equation 10. 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. All specifications include precinct fixed effects, election×district fixed effects, and time-varying covariates listed in Section 4.1. Regressions are weighted by the number of eligible voters. Standard errors are clustered at the precinct level and reported in parentheses. $^{***}p < 0.01$,$^{**}p < 0.05$,$^{*}p < 0.1$.

Appendix C. 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, and the other half 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, drawn up by the respective party caucus. Each constituency is represented by one seat in the *Bundestag*, with the remaining seats being allocated based on the second votes to achieve proportionality.

Bavarian State Elections. Similar to the federal parliament, 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 distinct 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.

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 for registration on the electoral roll before each election.

Appendix D. Effect Heterogeneity by Reassignment Reason

In this section, we investigate effect heterogeneity by reason of reassignment using the event study framework introduced in Section 4.1. Precinct reconfigurations are less likely to lead to *entire* precincts being reassigned (see Appendix Figure A.5). To ensure enough precision of our point estimates, we define the event as the first time that 50 percent or more residential addresses of a precinct are reassigned. Formally, let R_p be an indicator equal to 1 for precincts where reassignment occurred because of recruitment of a new polling venue and let B_p denotes an analogous indicator for cases in which reassignments are due to reconfiguration of precincts. Then, the modified event study specification takes the following form:

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

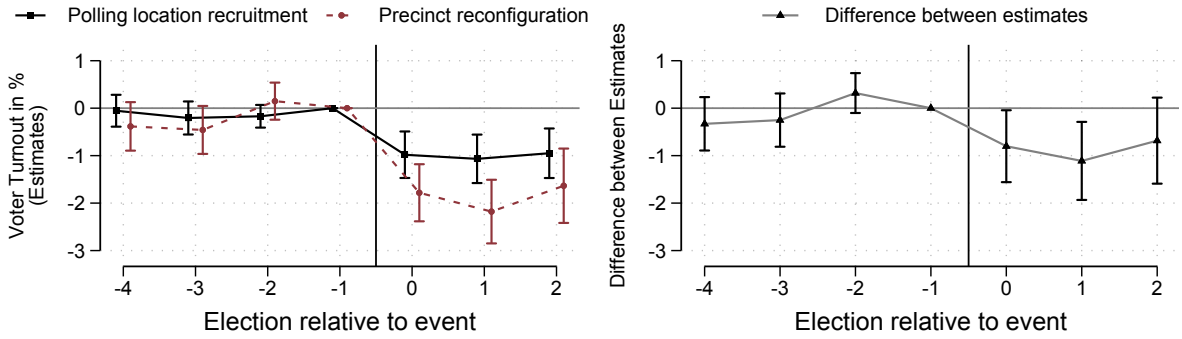
where the coefficients $\hat{\beta}^k$ and $\hat{\alpha}^k$ trace the differential time path of turnout separately for the two groups defined by R_p and B_p. As in our main specification, we include election×district fixed effects, a vector of precinct indicators, and time-varying controls.

The results are presented in Figure D.1. 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.

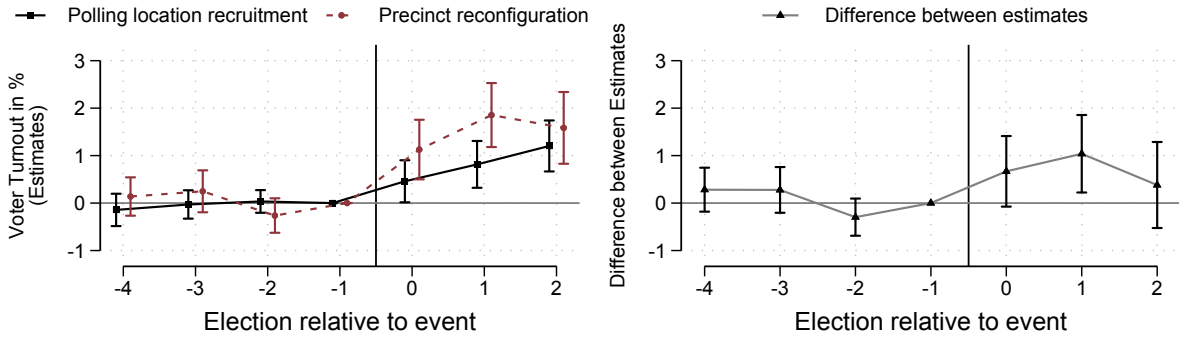
Reassuringly, pre-event estimates for both reassignment types are insignificant for all outcomes. Post-reassignment estimates follow a very similar trajectory. Treatment effects after a precinct reconfiguration seem slightly more pronounced; yet out of nine pairs of point estimates, only three are statistically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

Figure D.1: Effect Heterogeneity by Reassignment Reason

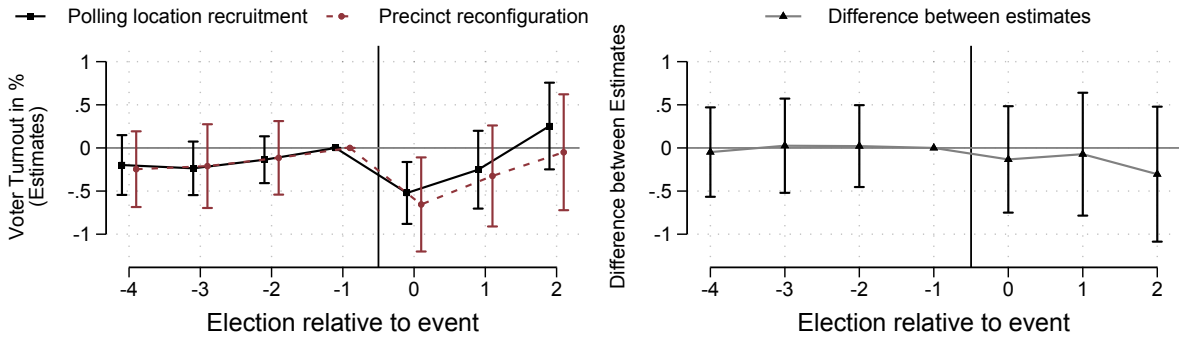
Panel A. Effect on Polling Place Turnout



Panel B. Effect on Mail-in Turnout



Panel C. Effect on Total Turnout



Notes: The figure presents event study results based on Equation D.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.