No surprises, please: Voting Costs and Electoral Turnout

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Abstract

We study how exogenous shocks to voting costs affect electoral turnout. Individuals whose polling place is relocated experience changes to their voting costs due to altered distance (transportation effect) and unfamiliarity with the new polling location (search effect). Using precinct-level data on eight elections in Munich (Germany), we find that polling place relocations reduce turnout by .46 percentage points (p.p.) on average: in-person voting declines by .75 p.p. and is only partly compensated by an .29 p.p. increase in mail-in voting. However, the turnout drop appears transitory as mail-in votes balance the decline in in-person votes in subsequent elections. This finding suggests inattentiveness to relocations, causing individuals to miss the deadline for requesting mail-in ballots. Some inattentive voters switch to nonvoting today but revert to mail-in voting in ensuing elections. The pattern is consistent with rational choice models of voting and incompatible with the hypothesis that voting is habit forming.

Keywords: Voter turnout; Germany; Habit formation; Elections; Election Administration; Precincts

JEL Codes: D72; D73; D83

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1. Introduction

Voting is the backbone of democracy. Yet, many democratic countries have experienced conspicuous declines in voter turnout in the past decades, prompting concerns about fading representativeness of electoral outcomes (Figure A.1). Early theories of electoral turnout have pointed out that the fact that people vote at all poses a paradox as the likelihood of a pivotal vote is dwarfed by any reasonable cost of casting a ballot (Downs, 1957). To rationalize positive turnout rates, scholars have extended the trade-off to include factors such as a consumption value of voting, ethical considerations, and social rewards (Riker and Ordeshook, 1968; Feddersen, 2004; Ali and Lin, 2013; Funk, 2010). Yet, the tension between voting as the essence of democracy and the insignificance of an individual vote remains unresolved, begging the question whether small increases in voting costs constitute a source of declining turnout rates.

We address this question by studying the effect of a seemingly innocuous shock to voting costs: the relocation of polling places. We use a natural experiment in Munich, the third largest city in Germany, where voters may be reassigned to a new polling place for two reasons. First, for administrative purposes, the boundaries of some voting precincts are redrawn between election years so that a portion of the electorate is reassigned to a different polling location. Secondly, polling venues (typically schools) need to be newly recruited for every election. Although the electoral office seeks to retain previously operated polling venues, new requirements, construction work, and other circumstances might render some locations unavailable, producing variation in precincts' assigned polling place over time. We show that turnout is unrelated to reassignments in future elections and that sociodemographic differences between treated and untreated precincts are negligible once we partial out election-specific shocks and time-constant variation at the precinct level.

We expect polling place reassignments to impact the costs of voting in person via two distinct mechanisms: *i)* through a "transportation effect" and *ii)* through a "search effect" (Brady and McNulty, 2011; McNulty et al., 2009). The transportation effect captures the increase or reduction in travel time resulting from the change in proximity to the polling location. The search effect refers to the cost of searching for the new polling place and going to an unfamiliar location (holding proximity constant). If the net increase in the costs of voting in person is sufficiently large, individuals will switch to mail-in voting or abstain from turning out.

As a key novelty of our study, we evaluate the persistence of the "relocation shock". Since reassignments typically produce lasting changes to voting costs, we expect behavioral adjustments to

carry over to subsequent elections. Persistence in voting patterns may also reflect habit formation, in the sense that today's act of voting increases the likelihood of voting in the future (Fujiwara et al., 2016). Thus, to the extent that nonvoting is internalized into a new habit, this channel represents another driver of lasting changes in turnout.

To empirically evaluate these predictions, we geo-locate the residential addresses of eligible voters and their designated polling place in the eight elections that took place between 2013 and 2020. We identify changes as well as the walking distance between each polling location-address pair, before harmonizing precinct boundaries over our observation period. This leaves us with a panel of 618 precincts with time-constant delineations for which we know the fraction of reassigned residential addresses, the average distance to the polling location, official election results (turnout at the polling place, turnout via mail, and overall participation), and time-varying sociodemographic characteristics.

We find that polling place reassignments engender a partial substitution of in-person for mail-in voting. On average, contemporaneous turnout declines by .46 ($SE = .12$) percentage points (p.p.)—or .74 percent, evaluated at the mean. Polling place voting declines by .75 ($SE = .13$) p.p. and is only partly compensated by an .29 ($SE = .13$) p.p. increase in mail-in voting. About 80 percent of the overall decline is driven by the search effect. To counterbalance the negative impact of the search effect on overall turnout, a polling place would have to move approximately 38 percent or .35 km closer to the voter, on average. The results are insensitive to including lag terms of reassignment and distance to the polling location, accounting for potential serial correlation in reassignments, and do not yield different results when distinguishing between relocations due to polling venue turnover and due to adjusted precinct boundaries.

To investigate the persistence of the relocation shock, we conduct an event study focusing on voting behavior around the first time a precinct is treated in our panel. We find no evidence of differential trends preceding the treatment, supporting the assumption that polling place reassignments occur randomly, conditional on precinct and election fixed effects. The event study results further show that a relocation leads to a significant drop in overall turnout in the treatment year; however, mail-in votes completely offset the decline in polling place votes in the two subsequent elections. This pattern is consistent with the presence of inattentive voters, who only notice the polling place reassignment after the closing date for requesting mail-in ballots. Inattentive voters who would have turned to mail-in voting as their preferred choice will either turn out at the new polling place anyway or abstain from voting upon reassignment. But conscious about the change, these voters

revert to mail-in voting in the ensuing elections, recovering the temporary drop in overall participation. This result is at odds with the hypothesis that voting is habit forming. Instead, the persistent substitution of in-person for mail-in voting is consistent with rational choice models of electoral turnout. The event study results are robust to accounting for the staggered timing of the treatment using novel estimators by Roth and Sant'Anna (2021a), Callaway and Sant'Anna (2020), and Sun and Abraham (2020).

Our evaluation of the causal effects of polling place reassignments on turnout relates to two previous studies. Brady and McNulty (2011) exploit the consolidation of voting precincts in the 2003 gubernatorial election in Los Angeles, which resulted in a reduction in the number of polling places. To account for non-random reassignment of individuals to polling locations, the authors employ statistical matching of registered voters in consolidated and unconsolidated precincts. They find a decrease in polling place turnout among reassigned voters, which was only partially offset by increased absentee voting. Using a similar strategy, McNulty et al. (2009) analyze the effect of trimming the number of polling places in the context of a 2006 school budget referendum in New York. The results show a lower turnout among voters who were reassigned to a new polling place. Both studies find that increased search costs and higher transportation costs jointly drive the decline in turnout. Causal identification in these settings rests on the assumption that matching on observables makes voters with new and unchanged polling locations comparable in all relevant characteristics.

Our identification strategy instead hinges on the elimination of all residual variation that may confound our estimates by partialling out precinct and election fixed effects. Moreover, polling place reassignments in Munich are not the result of cost-cutting policies but due to administrative reasons (adjustment to precinct boundaries and turnover in polling venues). Consequently, the extent to which reassignments in Munich result in closer or farther travel distances is similar. Our setting also allows us to examine the persistence of the treatment effects over subsequent elections and to shed light on habit formation in voting.

Several other studies also document the negative correlation between polling place reassignments or greater travel distance to the polls and electoral turnout. Amos et al. (2017) emphasize that re-precincting in the US is rarely a purely bureaucratic matter but prone to political influence. Against this backdrop, the authors find that the reduction of polling places for the 2014 General Election in Manatee County (FL) disproportionately affected minorities, younger voters and Democrats, and that reassigned voters turned out significantly less. Exploiting individual-level variation for the

2001 mayoral race in the city of Atlanta, Haspel and Knotts (2005) show that citizens who have to travel longer distances are less likely to vote. The results are consistent with cross-sectional evidence from other contexts (Fauvelle-Aymar and François, 2018; Gibson et al., 2013; Bhatti, 2012; Dyck and Gimpel, 2005; Gimpel and Schuknecht, 2003). However, these studies do not account for potential endogeneity, thus leaving room for biased estimates due to unobserved confounders or selection problems. One notable exception is Cantoni (2020), who studies the effect of distance to the polling location by exploiting geographic discontinuities at precinct borders in the US. Cantoni argues that citizens on opposite sides of precinct borders are identical on average, except for their assigned polling place. Comparing parcels of land and census blocks located near adjacent precincts, the author finds that a higher distance to the polling location significantly reduces the total number of votes. A key difference with our setting is that identification stems from cross-sectional variation. Instead, we estimate the effect of distance from *changes* in the proximity to the polling location within voting precincts.

Our study also contributes to the empirical literature on habit formation in voting. Habitual voting implies that the act of voting itself raises the probability of voting in the future. Scholars have long been aware that differences in turnout tend to persist over time (see e.g. Plutzer, 2002; Green and Shachar, 2000; Brody and Sniderman, 1977) but causal evidence for habit formation remains mixed. Meredith (2009) demonstrates that voters who just turned 18 at the time of the 2000 US general election (and thus are barely eligible to vote) are also more likely to cast their ballot in the subsequent election than their peers who just missed the age threshold. Gerber et al. (2003) provide evidence from a field experiment, suggesting that get-out-the-vote (GOTV) campaigns increase turnout in subsequent elections. By contrast, compulsory voting in Switzerland and Austria showed no persistent effects on turnout once it was abolished (Bechtel et al., 2018; Gaebler et al., 2020). Similarly, Potrafke and Roesel (2020) find that longer opening hours of polling places increased contemporaneous voter participation but did not affect turnout in subsequent elections when opening hours were no longer prolonged. Fujiwara et al. (2016) emphasize that, to appropriately identify habit formation, shocks that alter voting behavior in one election must not affect the costs or benefits of voting in the future. Specifically, the authors call into question that experiencing a presidential campaign at a young age or receiving information and emotional messaging from a GOTV campaign would not influence a person's tastes, sense of civic duty, or cost of voting in a lasting way. Instead, they propose election-day rainfall as a transitory and unexpected shock to voting costs and show that the decrease in turnout induced by rainfall also reduces turnout in subsequent US presidential elections. In our setting, the relocation of a polling place, although

plausibly unexpected, is clearly correlated with future voting costs (e.g., when the new polling place is located farther away). Thus, distinguishing whether a persistently lower turnout reflects habit formation or a lasting shift of voting costs may be impossible. However, we are able to test the necessary condition for habit formation: if voting is habit forming, then a decline in turnout due to the relocation shock must carry over to subsequent elections. We show that the necessary condition for habit formation can be rejected as (inattentive) voters who abstain from voting when subject to reassignment return to voting in the ensuing elections, thus recovering the drop in aggregate turnout.

2. Institutional Background

2.1. Elections in Munich

Our panel covers the outcomes of eight elections that were held in Munich between the years 2013 and 2020. These include elections to four legislative bodies that reflect the federal system of Germany: the *Bundestag* (German federal parliament), which constitutes the main body of the central government, the Bavarian *Landtag*, one of sixteen state parliaments, the *Stadtrat* (Munich city council), which governs the city alongside the mayor, and the European Parliament, which practically exercises some powers of the federal government since Germany is a member of the European Union. All elections follow the principles of proportional representation (PR) but differ with respect to the electoral rules used to achieve PR. In Appendix C, we briefly describe the key features and differences of the electoral processes.

Figure 1 illustrates the timeline of the eight elections included in our panel. Depicted are the number of eligible voters on the electoral rolls (vertical bars, left axis) as well as total turnout and the share of votes cast at the designated polling place on the right axis. Two elections were held in both 2013 and 2014 (yet not on the same day), and one election took place in each year from 2017 to 2020. Total turnout tends to increase over time when comparing the same election type. In general, eligible voters are automatically entered on the electoral roll without having to make a specific request. The number of eligible voters is distinctively greater in municipal elections, in which EU-foreigners with residence in Munich are also entitled to vote and entered on the electoral roll.[1] Foreign EU-citizens who wish to vote in Munich instead of their country of origin in European Elections must lodge a registration request. Every person on the roll receives an election notification via mail (no later than 21 days before any election) containing information about the

[1]For instance, in the 2020 Municipal Elections, 17.5 percent of eligible voters were foreign EU-citizens.

election date, voting time, the location of the polling place, whether it offers barrier-free access for the disabled or the elderly, and on the possibility of requesting a polling card (*Wahlschein*). There is no explicit information about any *changes* to the polling locations—neither in the election documents nor in any separate notification. This contrasts with the US, where changes in precinct borders typically trigger the requirement to notify affected voters (Cantoni, 2020). Eligible voters may cast a ballot in person at their assigned polling place on Election Day or request a polling card, which entitles them to vote by mail. A polling card must be requested no later than two days before the election. In principle, the polling card also entitles to vote at another polling place in the city (e.g., if the original polling place does not provide barrier-free access), but typically more than 98 percent of ballots cast using polling cards are votes by mail. And more than 90 percent of voters requesting a polling card actually end up casting a vote. In our observation period, the share of polling place votes ranges between 50 and 60 percent of all ballots and appears very stable over time. With more than half of all votes cast by mail, the 2020 Municipal Election held during the Covid-19 pandemic marks an exception.

2.2. Precincts and Polling Places

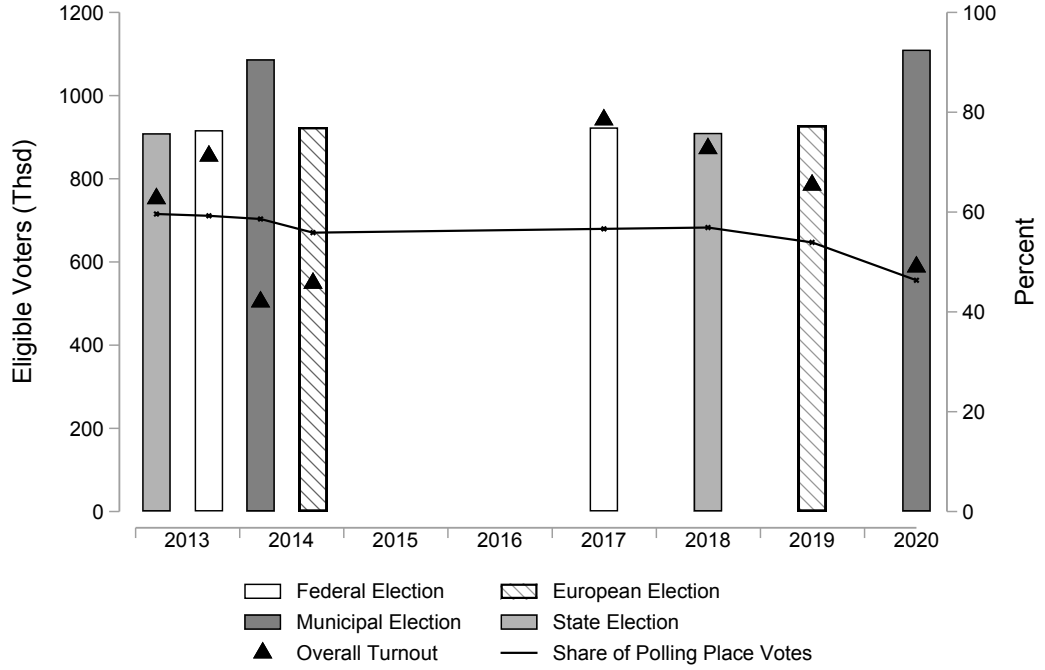
Every election is organized and administered by the Munich Electoral Office (*Wahlamt*) following a strict legal framework. Employees of the Electoral Office are nonpartisan civil servants and have no direct incentives to manipulate the electoral process. In every election, the electorate is geographically partitioned into several hundred voting precincts based on eligible voters' registered residential addresses.[2] Precincts constitute the smallest administrative unit in German elections and serve to enable a manageable election process and to facilitate the exercise of citizens' franchise, e.g., by preventing overcrowded polling places.

Figure 2 illustrates the electoral map for the 2018 State Election. The black boundaries identify the 618 precincts, the blue lines delineate the 25 city districts.[3] There is one polling place (depicted by a black star) for every precinct but it is not uncommon that a single venue, typically a school, hosts several polling places for neighboring precincts (four on average). The straight gray lines pair the addresses of residents on the official electoral roll with their assigned polling place.

[2]Citizens are required by law to notify the relevant registration office of the city within two weeks of moving into a flat. This also applies to citizens who move within a municipality.

[3]City districts have constant borders over time. Unlike precincts, districts are directly contested in some elections; for instance, adjacent districts cluster into 4 single-member constituencies in Federal Elections. In Municipal Elections, voters elect a local district committee (*Bezirksausschuss*).

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

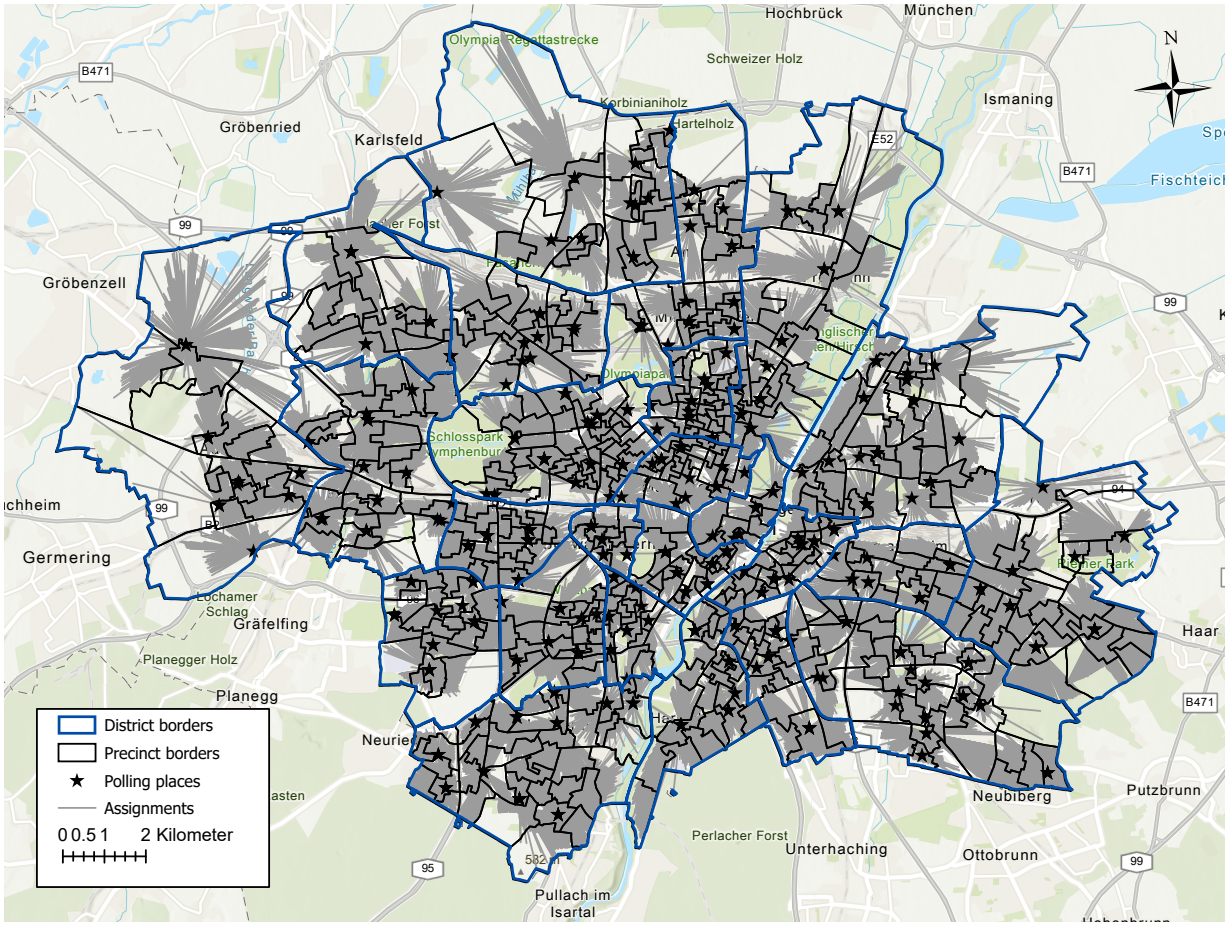


Notes: The figure presents the number of eligible voters (vertical bars) as well as total turnout (triangles) and the share of polling place votes (solid line) for the eight elections included in our sample. The shading of the bars reflect the different election types. Between 2013 and 2020, two State Elections, two Federal Elections, two European Elections, and two Municipal Elections were held in Munich. The data are from the Munich Electoral Office (*Wahlamt*).

Redrawing Precinct Boundaries. One source of variation in the assignment of voters to polling places results from adjustments to precinct boundaries. The law requires that voting precincts be delimited according to local conditions in a manner that participation in the election is "facilitated as much as possible for all eligible voters".[4] It further specifies that a precinct may not accommodate more than 2,500 eligible voters in any election. In practice, the city has aimed at an average number of 1,500 eligible voters per precinct during the elections included in our panel (see Appendix Figure A.2 for a density plot of precinct sizes over all elections). In every election year, the electoral office evaluates whether a change in the number of eligible voters, population growth, or new housing units requires adapting the number of precincts or precinct boundaries to maintain a

[4]The legal requirements are outlined in the federal, state, and European election law, LWO §10, BWO §12, EUW §12, GLKrWO §13.

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



Notes: The map delineates the boundaries of the 618 precincts (black lines) and the boundaries of the 25 city districts (blue lines) as of 2018. The locations of polling places are marked by a black star. Gray lines link the residential addresses of eligible voters in the 2018 State Election with their designated polling place.

decent access to the polls. Overall, the total number of precincts stayed at 702 in 2013 and 2014 before declining to 617 in 2017, due to the introduction of a new urban planning technology, which allowed for a more granular spatial monitoring of the electorate and thus for a more precise delineation of precincts. This resulted in a major redivision of the city area and a significant reduction in the variance of precinct sizes.[5] The number of precincts stayed at 618 in 2018 and 2019 and increased again to 755 in 2020 to accommodate a larger number of eligible voters during municipal elections.

[5] Anecdotally, the Electoral Office addressed changes in the number of eligible voters by adjusting the number of poll workers at the polling locations before prior to 2017.

Recruitment of Polling Places. A second source of variation in the assignment of voters to polling places emerges from the recruitment of the venues hosting the polling places. In every election year, the electoral office prepares an information sheet containing the delineation of voting precincts and updated requirements for polling places. These requirements include, for instance, an adequate power supply and sufficient mobile network connection in the polling venues. Since 2017, the city has also placed priority on recruiting venues with barrier-free access for the elderly and the disabled. Based on these guidelines, district inspectors (*Bezirksinspektoren)* are charged with the actual recruitment of potential venues, including their localization, verification, and the coordination with third parties. Polling places are typically recruited from public or municipal properties, usually schools (about 70 percent), but also retirement homes (15 percent), and ecclesiastical facilities (5 percent)—see Appendix Figure A.3 for an overview of venue types. While recruitment usually focuses on venues which have already been operated in the past, new polling place requirements, competing events on Election Sundays, building closures, or ongoing construction work may render certain locations unavailable.[6] Overall, we observe 293 distinct venues that hosted polling places in at least one election between 2013 and 2020. The number of operated venues is typically around 200 in any given election.

Despite the changes to precinct boundaries and to operated polling venues, election officials have maintained accessibility in terms of proximity to the polls fairly constant over time. Appendix Figure A.4 depicts the median and interquartile range of the street (walking) distance between residential addresses of eligible voters and their designated polling place. The median distance to the polls stays at about 715 meters before slightly increasing to roughly 760 meters in 2017.

2.3. Polling Place Reassignments

Figure 3 illustrates two instances of polling place reassignments which exemplify the two sources of reassignments in our setting. Gray lines link residential addresses to their corresponding polling place in the 2017 Federal Election. The black lines connect the addresses to their polling place in the 2018 State Election. The solid black border lines delineate the precinct of interest. In Panel (a), all voters living in a northern Munich precinct experienced a relocation of their 2017 polling place as the hosting elementary school, marked by the black star, underwent a general renovation and became inoperable for the 2018 election. The new polling place was hosted by a vocational

[6]There is no documentation of the reasons why venues become inactive. Anecdotal evidence suggests that, for instance, Munich's school construction program, which included investments of more than 3.8 billion Euros in the refurbishment of educational facilities starting from 2017, affected several polling venues. It is also possible that ecclesiastical institutions schedule religious events on Election Sundays.

school (marked by the white star) situated a six-minute walking distance (500 meters) apart from the old polling place. The example shows that the recruitment of a new polling venue—or the change in the activity status of a venue in general—typically means that all eligible voters living in the affected precinct have their polling place relocated relative to the previous election. In this particular instance, the average distance to the polling place increased for the affected electorate.

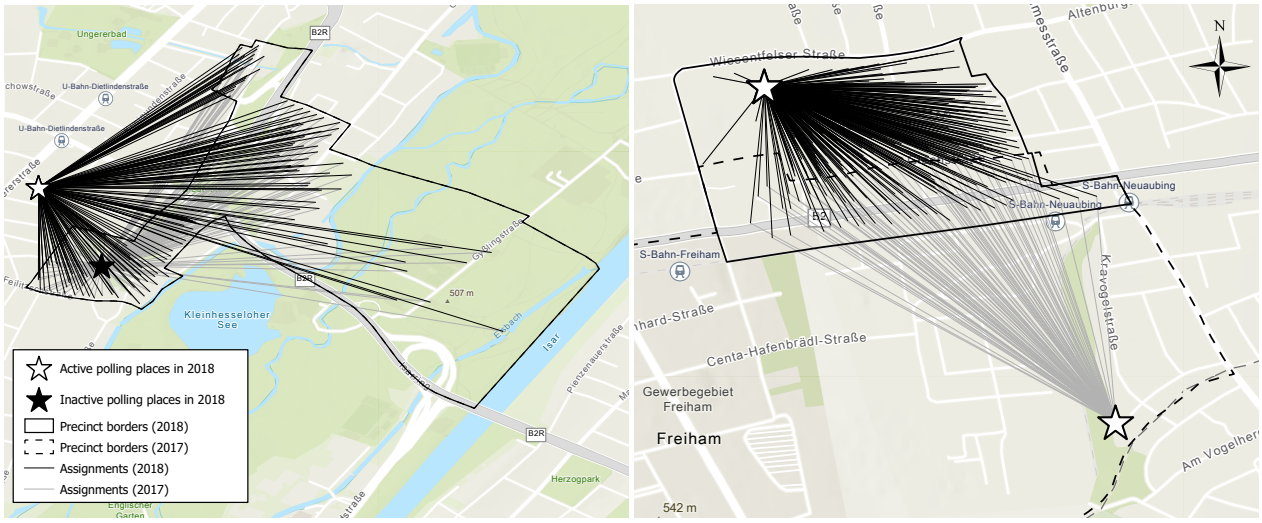
By contrast, Panel (b) illustrates an instance in which only a fraction of a precinct's electorate is treated due to the reconfiguration of its boundaries. The solid black lines demarcate the borders of the precinct of interest as of 2018. The dashed lines delineate the boundaries of another precinct as of 2017. Hence, citizens living at addresses located at the intersection of these two shapes were reassigned from one precinct to the other, and consequently experienced a change to the location of their designated polling place. The fraction of voters living to the north of the dashed line were assigned to the same polling place in 2017 and in 2018 and are therefore considered untreated in our experimental setting. Unlike in the preceding example, both polling places remained in operation in 2018 (white stars).

Figure 4 documents the fraction of residential addresses reassigned to a new polling place relative to the previous election. There are zero reassignments in the 2013 Federal Election and the 2014 European Election as other elections were held earlier in the same year. In 2017, more than 40 percent of addresses were reassigned to another polling place, as the city performed a major consolidation of precincts and updated requirements for polling venues.

Figure 5 reports the distribution of street distances between residential addresses and polling places (left panel), and the distribution of proximity *changes* conditional on a polling place relocation over all elections (right panel). Negative values indicate that the new polling place is situated at a closer distance to an address (relative to the location in the previous election), positive values correspond to a relocation farther away.[7] For 90 percent of residential addresses, the polling place is located no more than 1.4 kilometers away, which roughly corresponds to a 17-minutes walk (median: 735 meters). The median difference in proximity to the polling place following a reassignment is 30 meters (mean: 55 meters) and the distribution has a skewness of .1. Hence, the distribution is fairly symmetrical with polling places not systematically located closer or farther away upon reassignment.

[7] Figure A.5 in the Appendix reports the distributions for straight-line distances. Notice that by definition straight-line distances are no greater than street distances.

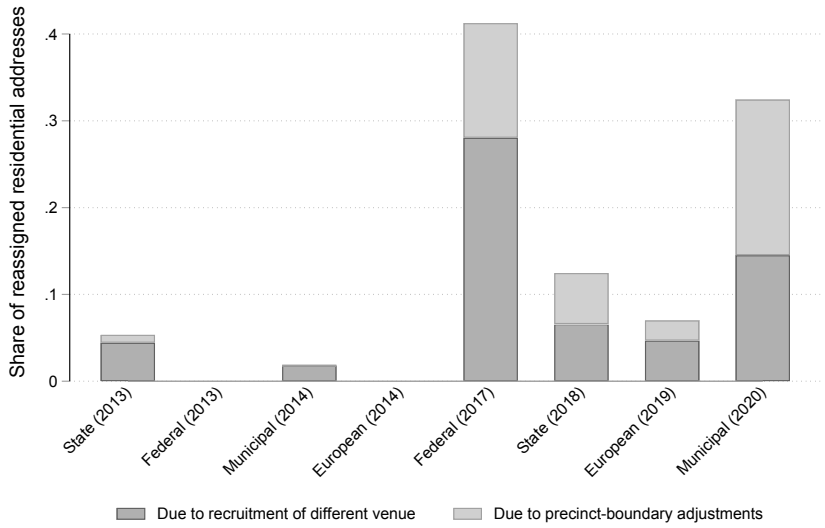
Figure 3: Illustration of Treatment



(a) Recruitment of different polling venue **(b)** Redrawing precinct boundaries

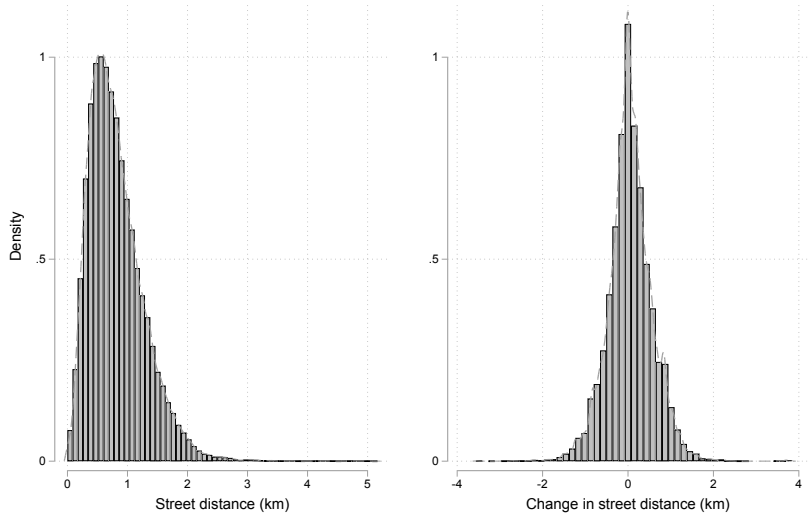
Notes: The figure illustrates two instances of polling place reassignments between the 2017 Federal Election and the 2018 State Election. Residential addresses of eligible voters are liked to their assigned polling place in 2017 via gray lines and to their 2017 polling location via black lines. Precincts as of 2018 are delineated by the solid black borders. In Panel (a), all residential addresses are reassigned due to the recruitment of a different polling venue: from the location marked by a black star to a new location marked by a white star. Panel (b) illustrates a reassignment due to an adjustment to precinct boundaries: the subset of residential addresses at the intersection of the 2018 precinct delineation (solid black boundaries) and the 2017 delineation (dashed black boundaries) were reassigned from the polling place located at the south of the map to the polling place to the north of the map.

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



Notes: The figure presents the share of addresses of residents on the official electoral rolls, which are reassigned to a different polling place relative to the previous election. Reassignment can be due to adjustment of precinct boundaries or due to recruitment of a different polling venue.

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 designated polling place (left plot, $N = 1,133,136$) and the *change* in distance conditional on reassignment to a new polling place relative to the previous election (right plot, $N = 142,062$) over the eight elections held between 2013 and 2020.

3. Conceptual Framework

To guide our empirical analysis, we present a simple theoretical illustration drawing on the "calculus of voting" framework, in which citizens ground their voting decision in a rational evaluation of their options (Riker and Ordeshook, 1968; Downs, 1957). We omit individual and election indices for notational simplicity in the following. Denote $V \in \{N, P, M\}$ a citizen's voting decision in the election. She can either vote at the polling place (P) incurring cost c_p or vote by mail (M), which produces cost c_m. She may also entirely abstain from voting (N), which generates no costs nor benefits. Voting yields utility B, which may include the direct benefits from the act of voting itself, e.g., from fulfilling a civic duty, as well as the expected gain if the preferred party achieves a greater number of votes in the election. The citizen abstains from voting if and only if her net benefits of voting are (weakly) negative, i.e., $B \leq c_p$ *and* $B \leq c_m$. In contrast, she votes by mail if and only if her net benefit of mail-in voting is positive and polling place voting is relatively more costly, i.e., $B/c_m > 1$ *and* $c_p/c_m > 1$. Similarly, she decides to vote at the polling place when $B/c_p > 1$ *and* $c_p/c_m < 1$. The left diagram in Figure 6 plots the utility of turning out relative to the cost of voting by mail against the relative costs of polling place voting. A citizen will turn out to vote if and only if her cost-benefit vector lies above the 45-degree line, where $B > c_p$, *or* above the horizontal unity line, where $B > c_m$. If additionally she lies to the right of the vertical unity line, where $c_p > c_m$, then she will vote by mail (M), and chose polling place voting (P) otherwise. In the area below the intersection of the 45-degree and the horizontal unity line, the net benefits of voting are always negative and the citizen will not vote (N). The shaded areas in the figure illustrate the voting decisions according to different cost-benefit configurations. If one imagines a distribution of the Munich population over the depicted plane, then historically roughly 38 percent of eligible voters lie somewhere in the nonvoting area (N), 33 percent in polling place voter area (P) and the remaining 29 find themselves in the mail-in voter area (M).

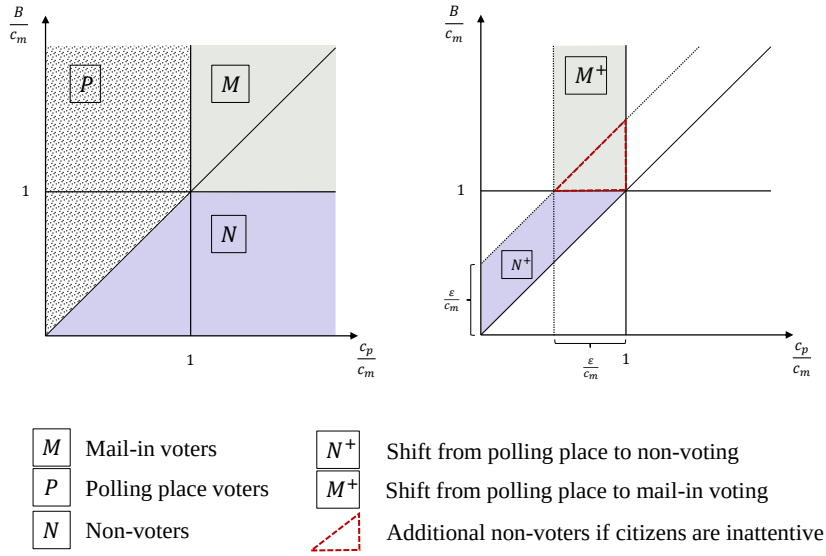
Now, suppose that the electorate is subject to a relocation of the polling place. We anticipate that reassignments of voters to polling places impact the costs of turning out at the polling place via two distinct mechanisms: *i)* through a "transportation effect" and *ii)* through a "search effect" (Brady and McNulty, 2011; McNulty et al., 2009). The transportation effect captures the change in travel costs on Election Day resulting from the change in proximity to the polling place. In Munich, where polling places are usually located within walking distance, travel costs mainly correspond to the time to walk to the polling place. The search effect refers to the additional costs of searching for and learning about the new polling place (holding proximity constant). Search costs may also capture the psychological barrier to engage with the unfamiliar environment.

For illustrative purposes, suppose that the search and the transportation effect (or a combination of both) produce a (net) positive shock to the cost of voting at the polling place, $\varepsilon > 0$. Accordingly, c_p increases to $c'_p = c_p + \varepsilon$. The shock thus increases the *absolute* costs of voting at the polling place and decreases the *relative* costs of voting by mail. Graphically, this corresponds to an upward parallel shift of the diagonal line and a leftward shift of the vertical line, as illustrated in the right diagram of Figure 6. As a result, some voters will switch from polling place to mail-in voting (area denoted by M^+). This is the case when the reduction in relative cost of voting by mail is large enough that $c'_p/c_m > 1$ and the net benefit of casting a mail-in ballot is positive, $B > c_m$. When the benefit of voting by mail is not sufficient to outweigh the costs, the citizen will switch to nonvoting if the cost shock is large enough to render polling place voting unattractive, i.e., $c'_p > B$ and $c_m > B$. The area denoted N^+ depicts the shift from polling place to nonvoting. Therefore, the model predicts that the cost shock engenders a substitution effect between mail-in and polling place voting and a decline in overall turnout. A special circumstance arises, when voters are inattentive to polling place reassignments by not or only carelessly reading the election notification, which is mailed several weeks before Election Day. Upon realizing a polling place relocation, these voters may have missed the closing date for requesting mail-in ballots so that the cost of switching to mail-in voting is practically infinity. Inattentiveness attenuates the shift from in-person to mail-in voting—as some voters will choose to go to the new polling place anyway—and amplify the shift towards nonvoting, as some voters who would have voted by mail abstain from casting a ballot. The additional portion of nonvoters is highlighted by the red triangle in the right diagram.

To what extent do these adjustments carry over to subsequent elections? The theory offers two mechanisms that may be at play. First, polling place reassignments may alter the cost of voting at the polling place permanently. This is obvious, for instance, when transportation costs increase because a polling place is moved farther away. Similarly, search costs are likely to persist unless people familiarize themselves with the new location between two elections. Thus, the relative cost reduction of mail-in compared to in-person voting is likely to persist and thus to maintain the substitution effect. If the absolute cost increase for voting at the polling place is sufficiently high, then voters may entirely abstain from voting today and in the future. However, the initial election may be different from subsequent ones due to inattentive voters. Some inattentive voters will initially abstain from voting or cast their ballot at the new polling location but revert to mail-in voting in ensuing elections. Consequently, a drop in aggregate turnout may be (partly) recovered and the substitution of in-person for mail-in voting reinforced over time.

A second mechanism that could drive persistent changes in voting pattern is habit formation. Habit

Figure 6: Effect of Increased Cost of Polling Place Voting on Voting Behavior



Notes: The left diagram illustrates citizens' possible voting behavior—turning out at a polling place (P), by mail (M), and not voting (N)—as a function of (individual) benefits (B) and costs of turning out at a polling place (c_p) and via mail (c_m). The right diagram illustrates how a positive shock to the cost of voting at the polling place (ε) affects voting behavior. M^+ marks the additional portion of mail-in voters, N^+ marks the additional portion of nonvoters, and the red triangle highlights the additional portion of nonvoters when citizens are inattentive to polling place reassignments.

formation means that the act of voting itself affects the probability of voting in the future—holding voter traits, such as the sense of civic duty or (individual) voting costs, constant (Fujiwara et al., 2016). Applied to our setting, habit formation would imply that a decline in overall turnout due to polling place reassignments would carry over to subsequent elections even if the costs of voting were completely restored to pre-treatment conditions. As there is compelling reason to anticipate that polling place relocation shocks are not transitory but alter voting costs in a lasting way, our setting is not suited to disentangle the effects of habit formation from increased costs. Yet, we are able to test the necessary condition for habit formation; namely, if (non)voting is habit-forming, then any initial decline in voter turnout must persist in the subsequent election(s). Empirically, the magnitudes of these effects depend on the distribution of the population over different cost-benefit vectors and the size of the reassignment shock(s).

4. Empirical Strategy

4.1. Data

All information on polling locations, residential addresses, and voter turnout (by mail and in-person at the polling place) stem from administrative sources including official electoral rolls and official election results. We geo-reference polling locations and residential addresses in the eight elections that took place in our panel as well as in the 2009 Federal Election, which serves as a reference to identify changes to polling place assignments relative to the 2013 state election (the first election in our panel). We identify 152,026 residential addresses from the 2018 electoral roll, of which we are able to match 143,278 to a unique precinct in every election (94.2 percent). 141,612 of these addresses were successfully geo-located (99.0 percent). We also calculate the street distance, defined as the shortest walking distance using the public road network, and the straight-line (Euclidean) distance between every pair of residential address and polling place in every election.[8]

In addition, we leverage time-varying administrative data on structural indicators at the precinct level.[9] These include information on the age structure of the electorate, average duration of resi-

[8]We use the geodist STATA package (Picard, 2019) to compute straight-line distances and the osrmtime package (Huber and Rust, 2016), which make use of *Open Source Routing Machine (OSRM)* and of *OpenStreetMaps (OSM)* to find the shortest route (by foot or other means), to calculate street distances.

[9]Precinct-level structural indicators and turnout data are available for download from the city's election review website (*Wahlatlas*): https://www.muenchen.de/rathaus/Stadtinfos/Statistik/Wahlen.html [accessed August 8, 2021]. Official electoral rolls including residential and polling place addresses are provided by the Munich Election Office (*Wahlamt*) upon request.

dence in Munich, marital status of inhabitants and their citizenship (German, non-German EU, or non-EU citizenship). We also aggregate yearly real estate rental price information compiled by the RWI Institute for Economic Research from square grids with a 1 km length to the precinct level to capture socioeconomic differences among precincts.[10] Unfortunately, mail-in ballots are recorded at the level of administrative delineations that do not coincide with precinct borders. Thus, we are confined to relying on *requests* of polling cards as a proxy for mail-in votes in our empirical analysis. As noted above, about 90 percent of the requested cards are returned as ballots, and more than 98 percent of these are mail-in votes. We will address different approximations in our robustness section.

To obtain a panel of precincts suitable for estimation, we account for changes in precinct delineation over time. To this end, we harmonize precinct borders to the 2018 configuration, i.e., the share of polling place reassignments and the average distance to the polling place in every election are computed supposing the 2018 (instead of the contemporaneous) precinct borders. Likewise, election-specific precinct characteristics, such as the age structure, the size of the electorate, or the number of votes cast, are converted to precinct borders as of 2018 using conversion keys provided by the Munich Statistical Office (*Statistisches Amt der Landeshauptstadt München*).[11] This leaves us with a panel of 618 precincts with constant borders, which we observe over eight elections. Appendix Figure A.6 plots the distribution of treatment intensities, i.e., the share of reassigned addresses, over all precinct-election observations in our panel in which a positive share of residential addresses are reassigned to a new polling place. It is apparent that in the modal case, a precinct is fully treated, i.e., all its citizens are subject to reassignment (39.8 percent of all instances). Table B.1 in the Appendix reports summary statistics of our precinct-level variables.

[10]The RWI - Leibniz Institute for Economic Research (formerly Rheinisch-Westfälisches Institut für Wirtschaftsforschung) and its research data center compile granular real estate data obtained from the Internet platform *ImmobilienScout24* for research purposes.

[11]The conversion of variables is essentially performed using population or electorate weights. A key assumption is that characteristics are evenly distributed within a precinct. For example, if a precinct is divided in two in 2018 (in terms of its electorate), it is assumed that past voting behavior did not differ systematically between the two parts.

4.2. Main Specifications

We estimate the contemporaneous search and transportation effect by relating turnout to polling place reassignments and changes in average walking distance in the following model:

$$\begin{aligned} Turnout^{s}_{pe(t)} = {} & \gamma_1 Reassigned_{pe(t)} + \gamma_2 Distance_{pe(t)} + \gamma_3 Reassigned_{pe(t-1)} + \gamma_4 Distance_{pe(t-1)} \\ & + \mathbf{X}'_{pe(t)}\lambda + \alpha_p + \alpha_{e(t)} + \varepsilon_{pe(t)}, \end{aligned} \tag{1}$$

where $Turnout^{s}_{pe(t)}$ measures the percentage turnout in precinct p in election e held at date t, with $e(t) = 1, 2, ..., 8$, so that elections are ordered chronologically. The superscript s indicates whether turnout refers to participation at the polling place, via mail, or overall (given as the sum of polling place and mail-in turnout). The variable *Reassigned* denotes the share of residential addresses reassigned to a new polling place relative to the previous election. Thus, the estimate for γ_1 captures the contemporaneous search effect. *Distance* is the natural logarithm of the average street distance between residential addresses and the designated polling place. By including precinct fixed effects, α_p, we identify the effect of *Distance* from precinct-specific deviations from the mean, which are uniquely driven by polling place reassignments. Thus, the transportation effect is captured by the estimate of γ_2. We also control for the lag terms of reassigned and distance to account for potential serial correlation in treatment that may bias our results. Intuitively, if a voter persistently modifies her behavior following a polling place reassignment—for instance, by switching to mail-in voting—a second polling place relocation will not induce further behavioral adjustment. Thus, to the extent that voters are repeatedly subject to reassignments over our observation period, we may underestimate behavioral adjustments to voting cost shocks. $\mathbf{X}$ is a vector of time-varying covariates at the precinct level: the precinct size (log of 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, 45-59, respectively), 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. We also include election fixed effects, $\alpha_{e(t)}$, to control for election-specific shocks, such as differences in voting propensity due to varying perceived stakes or weather on Election Day. Precinct fixed effects further account for time-invariant precinct characteristics, such as its size (in terms of area), remoteness, or its settlement structure (to the extent that it remains stable over our observation period).

The two main identifying assumptions for interpreting the estimation of contemporaneous treat-

ment effects in Specification (1) as causal are *(i)* that polling place reassignments and distance changes are uncorrelated with other unaccounted factors that may affect turnout, and *(ii)* that polling place reassignments are not themselves driven by the expectation of changes in turnout. Although these assumptions are not directly testable, we provide a number of robustness checks including a balancing exercise, a placebo test, and an analysis of pretrends, suggesting that our results can be interpreted as causal.

To investigate the persistence of behavioral changes from polling place reassignment, we conduct an event study focusing on the window around the *first* time a precinct is treated in our sample. The event study design allows us to examine to what degree voters may be persistently dissuaded from turning out and whether there are lasting substitutive effects between in-person and mail-in voting. Let E_p denote the election in which precinct p is treated for the first time (the event). We regress turnout on election dummies $D^k_{pe(t)}$ relative to the event E_p, control variables, as well as precinct and election fixed effects $(\delta_p, \delta_{e(t)})$:

$$Turnout^s_{pe(t)} = \sum_{k=-K}^{-2} \mu_k^{lead} D^k_{pe(t)} + \sum_{k=0}^{L} \mu_k^{lag} D^k_{pe(t)} + \mathbf{X}'_{pe(t)}\phi + \delta_p + \delta_{e(t)} + v_{pe(t)}, \tag{2}$$

with the event study dummies $D^k_{pe(t)} = \mathbb{1}\{e(t) - E_p = k\}$ and $e(t) = 1, 2, ..., 8$. In our baseline estimates, E_p corresponds to the first election in which the *entire* electorate in a precinct is subject to a polling place reassignment. In the baseline, we also trim precincts' time series from the moment a second relocation occurs so that we make sure to capture the impact of a single instance of reassignment instead of a series of changes. We test our results for robustness to alternative specifications in the subsequent section.

We weight precinct-level observations with the number of eligible voters. This allows us to recover the conditional mean association between turnout and polling place reassignments at the individual level. In the baseline specifications, we cluster standard errors at the precinct level to account for the correlation of model errors over time. We also test the robustness of our results to alternative assumptions about the variance-covariance matrix in Section 5.3.

As a number of recent contributions have pointed out, two-way fixed effect (TWFE) event study (or difference-in-difference) approaches, similar to the specification in Equation (2), may still yield biased estimates when treatment effects vary over time (see e.g., Athey and Imbens, 2021; de Chaisemartin and D'Haultfœuille, 2020; Borusyak et al., 2021; Goodman-Bacon, 2019; Sun and Abraham, 2020). The main reason is that the TWFE estimator uses already-treated precincts as

control group for newly-treated precincts, causing a violation of the parallel trend assumption in the presence of treatment effect dynamics. To account for this threat to identification, we also perform alternative approaches proposed by Callaway and Sant'Anna (2020), Roth and Sant'Anna (2021a), and Sun and Abraham (2020). For instance, Callaway and Sant'Anna (2020) suggest a two-step estimation strategy by first estimating "group-time average treatment effects", where groups are defined by when units (precincts) are first treated, before aggregating the treatment effects by relative time using a propensity-score weighting method.

4.3. Balancing Test

Under our identifying assumption, precincts with and without polling place reassignments share similar determinants of voter participation, on average. Consequently, the correlation between observable precinct characteristics and reassignments should be negligible and statistically insignificant *conditional* on election and precinct fixed effects. We test this in Table 1. Each cell reports OLS estimates from a separate regression, where rows correspond to precinct characteristics. The dependent variable in Column (1) is a dummy identifying precincts with a nonzero share of reassignments. The estimates are very small and not statistically significant, suggesting that the likelihood of having *any* number of voters reassigned to a different polling location is unrelated to observables. The depended variable in Column (2) is the share of addresses reassigned to a different polling place. Only one estimate appears marginally significant. Columns (3) and (4) distinguish between the reason for reassignment, i.e., due to revisions of precinct boundaries or due to the recruitment of a different polling place, respectively. The estimates indicate no evidence that precinct characteristics are systematically related to the likelihood of reassignment due to either reason. Finally, Column (5) regresses the log of average street distance on precinct characteristics. Out of seventeen estimates, only two estimates cross the threshold of statistical significance. Nonetheless, F-tests cannot reject the hypotheses that estimates are jointly equal to zero in any column, indicating that the fixed effects perform well in eliminating residual correlation between treatment and precinct characteristics. Therefore, the balancing test supports our identifying assumption.

5. Results

5.1. Search and Transportation Costs

Table 2 reports the estimation results of Equation (1). Panels A and B show the results for polling place turnout and turnout via mail, respectively. Panel C reports the net effect on total turnout.

Table 1: Balance Test on Precinct Characteristics

	(1)	(2)	(3)	(4)	(5)
	Dummy (Reassigned >0)	Share Reassigned	Share Reassigned (Precinct Boundaries)	Share Reassigned (Recruitment)	Log Street Distance
Residents (thsd)	-0.013	0.055	0.030	0.025	-0.004
	(0.045)	(0.035)	(0.028)	(0.031)	(0.031)
Single residents (thsd)	0.016	0.108*	0.068	0.040	0.037
	(0.076)	(0.060)	(0.046)	(0.056)	(0.055)
Married residents (thsd)	-0.103	0.070	0.002	0.067	-0.059
	(0.113)	(0.085)	(0.057)	(0.076)	(0.075)
Native German residents (thsd)	-0.133	0.035	-0.033	0.067	-0.001
	(0.098)	(0.077)	(0.044)	(0.071)	(0.081)
Non-native German residents (thsd)	-0.065	0.165	0.050	0.115	-0.185*
	(0.169)	(0.125)	(0.087)	(0.108)	(0.102)
Foreign residents (thsd)	0.040	0.083	0.076	0.008	0.022
	(0.060)	(0.053)	(0.046)	(0.043)	(0.044)
Residents eligible to vote (thsd)	-0.017	0.038	-0.039	0.077	-0.029
	(0.074)	(0.057)	(0.040)	(0.054)	(0.054)
Eligible voters aged 18-24 (thsd)	-0.073	0.067	0.014	0.054	0.255
	(0.250)	(0.203)	(0.131)	(0.177)	(0.165)
Eligible voters aged 25-34 (thsd)	0.105	0.149	-0.061	0.209*	0.142
	(0.138)	(0.113)	(0.067)	(0.108)	(0.115)
Eligible voters aged 35-44 (thsd)	-0.075	0.130	-0.030	0.160	-0.052
	(0.173)	(0.139)	(0.085)	(0.129)	(0.123)
Eligible voters aged 45-59 (thsd)	-0.253	0.156	-0.030	0.186	-0.114
	(0.175)	(0.144)	(0.103)	(0.127)	(0.122)
Eligible voters aged 60+ (thsd)	-0.046	-0.026	0.006	-0.033	-0.167**
	(0.113)	(0.095)	(0.071)	(0.078)	(0.084)
Germans in the electorate (thsd)	-0.061	0.067	-0.020	0.088	-0.058
	(0.084)	(0.066)	(0.039)	(0.062)	(0.069)
EU-foreigners in the electorate (thsd)	0.001	0.063	-0.014	0.077	0.036
	(0.093)	(0.067)	(0.046)	(0.066)	(0.050)
households with children (%)	-0.001	0.003	0.003	0.001	0.004
	(0.004)	(0.004)	(0.002)	(0.003)	(0.003)
Average quoted rent per sqm	0.000	0.002	-0.001	0.003	0.001
	(0.003)	(0.003)	(0.002)	(0.003)	(0.002)
Average duration of residence	-0.001	-0.001	0.000	-0.002	-0.003
	(0.003)	(0.003)	(0.002)	(0.002)	(0.003)
F-test [p-value]	0.66 [0.85]	0.49 [0.96]	1.04 [0.42]	0.55 [0.93]	1.09 [0.36]
Observations	4,944	4,944	4,944	4,944	4,944
Precinct FE	×	×	×	×	×
Election FE	×	×	×	×	×

Notes: Each cell in Columns (1) through (5) report OLS estimates from a separate regression on precinct characteristics (in rows). All regressions include precinct and election fixed effects. The dependent variables are a dummy identifying precincts with a nonzero share of reassignments (Column 1), the share of addresses reassigned to a different polling place (Column 2), the share of reassignment due to adjustment to precinct boundaries (Column 3), the share of reassignments due to the recruitment of a different polling place (Column 4), and the log of average street distance to the polling location (Column 5). Regressions are weighted with the number of eligible voters. Standard errors are clustered at the precinct level and reported in parentheses. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

Column (1) includes only the share of reassigned residential addresses and the fixed effects. Column (2) adds precinct covariates. Column (3) further includes the lag term of reassignment. The estimate of *Reassigned* in this column thus captures the average impact of a relocation on turnout. Column (4) reports the full specification including log street distance and the lag terms of reassignment and distance. Column (5) removes the lag terms to test the sensitivity of the estimates of contemporaneous reassignment and distance. Finally, we run a falsification test by relating contemporaneous turnout to *future* reassignments and distance to the polling place in addition to the current and past values. It may be the case that contemporaneous and future reassignments share some causes that also determine voter participation. For instance, population growth may prompt more readjustments of precinct boundaries, and perhaps citizens in these precincts have a systematically different voting behavior. Thus, an association between future reassignments and current turnout would suggest that these persistent confounders afflict our core estimates. The results of the placebo treatment are presented in Column (6).

In line with our expectations, the effect of reassignment on polling place turnout is negative and significant at the one percent level across all specifications (Panel A). Controlling for lagged reassignments and covariates, the relocation of a polling place reduces in-person voting by .75 percentage points on average (Column 3). Evaluated at the mean, this corresponds to a reduction by roughly 2.2 percent. Adding distance in Column (4) breaks down this reduction into the search effect and the transportation effect. Holding distance to the polling place and other factors constant, polling place relocation reduce in-person voting by .46 percentage points (1.4 percent at the mean), on average. The transportation effect also appears statistically significant: increasing the street distance to the polling place by 10 percent (corresponding to roughly 71 meters at the mean) reduces polling place turnout by .34 percentage points (equivalent to a one-percent decline at the mean). Thus, about 60 percent of reduction can be attributed to the search effect. The estimates also imply that a polling place would have to move approximately 13 percent closer to the voter to counterbalance the negative impact of the search effect for in-person voting, on average. The estimates of contemporaneous search and transportation effect are insensitive to excluding the lag terms, suggesting that serial correlation in reassignments does not bias our results (Column 5). The placebo treatment estimates reported in Column (6) further show that future polling place relocations do not affect current turnout in any panel. Thus, we find no evidence for unobserved persistent confounders.

The impact on mail-in turnout in Panel B mirrors the effect on polling place voting. On average, reassignments increase mail-in turnout by .29 percentage points (Column 3 of Panel B). However,

only the transportation effect appears statistically significant in the full specification (Column 4). Increasing the distance to the polling place by 10 percent raises mail-in voting by 2.4 percentage points (equivalent to 8.4 percent at the mean). Thus, we find evidence for a substitution of in-person for mail-in votes following a polling place relocation. Yet, holding distance constant, the search cost effect only marginally compensates the drop in polling place turnout by increasing participation via mail. Similarly, a hypothetical relocation that results in a greater distance to the polling place reduces polling place turnout more than it increases mail-in turnout. This is in line with the theory predicting only a partial substitution as some voters will switch to nonvoting because the (individual) costs of voting by mail are higher than the perceived benefits or because inattentiveness regarding polling place relocations causes some voters to miss the deadline for requesting mail-in ballots.

The net effect of polling place reassignment on overall participation is indeed sizable and statistically significant. On average, turnout declines by .46 percentage points (Column 3 of Panel C). Both search and transportation costs drive the effect: holding distance constant, a polling place reassignment reduces overall turnout by .38 percentage points, equivalent to .6 percent at the mean, (Column 4 of Panel C). Thus, about 80 percent of the overall effect is due to the search effect. Increasing the distance to the polling place by 10 percent depresses voter turnout by approximately .1 percentage points, which corresponds to a .2 percentage reduction at the mean. The estimates imply that the magnitude of the search cost effect on overall participation is equivalent to an increase in travel distance by 38 percent. Notice that the estimate of the contemporaneous search effect on overall turnout also reflects inattentiveness, i.e., votes that would have been cast by mail if individuals had noticed their polling place relocation in time. For instance, the estimates of the lag terms of *Reassigned* suggest that there is some increase in mail-in voting stemming from relocations in the past. This could indicate that inattentive voters revert to mail-in voting in the election *after* the reassignment. The event study analysis in the subsequent section allows to shed more light on this potential driver of declining turnout. In sum, the evidence so far shows marked transportation and search effects in the short-run, consistent with theoretical predictions and previous research (Brady and McNulty, 2011; McNulty et al., 2009).

5.2. Pretrends and Persistence of the Relocation Shock

The key assumption of our empirical analysis maintains that polling place reassignments occur randomly conditional on precinct and election fixed effects. A central threat to validity are differential trends in turnout among precincts depending on whether or not they experience polling place changes. Hypothetically, the election office may systematically consolidate adjacent precincts that

Table 2: Search and Transportation Costs—Baseline Specification

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Turnout at the Polling Place						
Reassigned	-0.76***	-0.74***	-0.75***	-0.46***	-0.44***	-0.60***
	(0.14)	(0.12)	(0.13)	(0.11)	(0.11)	(0.14)
Reassigned, $t-1$			-0.62***	-0.44***		-0.58***
			(0.12)	(0.12)		(0.13)
Log Street Distance				-3.44***	-3.48***	-3.60***
				(0.23)	(0.23)	(0.34)
Log Street Distance, $t-1$				-0.02		0.24
				(0.19)		(0.24)
Reassigned, $t+1$						0.04
						(0.13)
Log Street Distance, $t+1$						-0.15
						(0.21)
R^2	0.96	0.96	0.96	0.97	0.97	0.96
Panel B: Turnout via Mail (requested)						
Reassigned	0.26*	0.28**	0.29**	0.08	0.06	0.31**
	(0.15)	(0.13)	(0.13)	(0.12)	(0.12)	(0.15)
Reassigned, $t-1$			0.52***	0.36***		0.52***
			(0.13)	(0.13)		(0.15)
Log Street Distance				2.41***	2.59***	2.59***
				(0.24)	(0.23)	(0.33)
Log Street Distance, $t-1$				0.35*		0.10
				(0.18)		(0.23)
Reassigned, $t+1$						0.05
						(0.12)
Log Street Distance, $t+1$						-0.06
						(0.17)
R^2	0.93	0.94	0.94	0.95	0.95	0.95
Panel C: Overall Turnout						
Reassigned	-0.51***	-0.45***	-0.46***	-0.38***	-0.38***	-0.29**
	(0.16)	(0.12)	(0.12)	(0.12)	(0.11)	(0.14)
Reassigned, $t-1$			-0.10	-0.08		-0.06
			(0.13)	(0.13)		(0.15)
Log Street Distance				-1.03***	-0.90***	-1.00***
				(0.20)	(0.21)	(0.25)
Log Street Distance, $t-1$				0.33*		0.33
				(0.19)		(0.21)
Reassigned, $t+1$						0.09
						(0.12)
Log Street Distance, $t+1$						-0.20
						(0.16)
R^2	0.98	0.99	0.99	0.99	0.99	0.99
Observations	4,944	4,944	4,944	4,944	4,944	4,326
Controls		×	×	×	×	×

Notes: Dependent variables are voter turnout (0–100) at the polling place (Panel A), by mail (Panel B), and overall (Panel C). Mail-in voting is approximated with requests of polling cards (*Wahlscheine*). All specifications include election and precinct fixed effects. Precinct controls include the log of the number of residents, the share of residents eligible to vote, the share of eligible voters aged 18-24, 25-34, 35-44, 45-59, respectively, the share of EU-foreigners in the electorate, the share of native German residents, the share of non-native German resident, 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. Regressions are weighted with the number of eligible voters. Standard errors are clustered at the precinct level and reported in parentheses. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

displayed a stronger shift from in-person towards mail-in voting in the past to reduce costs of operating polling places. In this case, our OLS estimate for the effect of reassignments may simply pick up a pre-existing trend instead of the substitution effect induced by a cost shock to voting at the polling place. The parallel-trend assumption is not directly testable. However, the event study approach allows us to examine the existence of differential trends preceding the treatment.

Figure 7 plots the event study results for turnout at the polling place, via mail, and overall. The event is defined as the first election a precinct is treated in our sample. In the baseline, we consider this to be the case when all residential addresses are reassigned to a new polling place. As emphasized above, we exclude all precinct-election observations beyond any second relocation so that we pick up the effects of only one instance of reassignments in every precinct. Of our 618 precincts, 278 are treated at some point. For most treated precincts the event occurs in the 2017 Federal Election (60 percent), 14 percent (13 percent) experience the reassignment shock in the 2020 Municipal Election (2018 State Election), and the remainder are treated in other elections.

Reassuringly, the results do not show evidence of pretrends in any of our outcome variables: all pre-event dummies are very small in magnitude and statistically indistinguishable from zero. By contrast, we find that polling place turnout falls by 1.15 ($SE = .24$) and mail-in turnout increases by .58 ($SE = .24$) percentage points right after a polling place reassignment. This is in line with the substitution effect ensuing a reduction in relative costs of mail-in voting due to a polling place relocation. The bottom plot shows that the effect is not strong enough to completely offset the reduction in overall participation: total turnout declines on average by .57 ($SE = .17$) percentage points in the event election. Thus, compared with the earlier results estimated on the full sample, the event study estimates on contemporaneous turnout are slightly more pronounced, suggesting a greater reduction in polling place turnout, a stronger substitution towards mail-in voting and a slightly greater overall reduction of aggregate turnout.

The estimates further show that the substitution of polling place for mail-in voting carries over to the two subsequent elections. This is consistent with the theory predicting a persistent substitution effect resulting from lasting changes to relative costs of voting. Interestingly, the net effect on total turnout appears statistically indistinguishable from zero in all elections following the event. While a portion of treated voters switch to nonvoting upon reassignment, the decline in turnout is already recovered in the ensuing election. One interpretation is that the initial shock to the costs of polling place voting fades over time. For instance, the search cost effect may wane, as voters become familiar with the new polling place, reducing uncertainty about its location and

accessibility. Another explanation is that the initial decline is largely driven by inattentive voters, who do not (or not carefully) read the election notification and miss the deadline for requesting mail-in ballots before noticing a polling place change. Inattentive voters who would have switched to mail-in voting as their preferred choice will either decide to turn out at the new polling place anyway or abstain from voting in the event election. But conscious about the reassignment, these voters revert to mail-in voting in ensuing elections. The estimates support this interpretation as total turnout recovers after the event and mail-in (polling place) voting exhibits a slight upward (downward) trend in the subsequent elections.

Finally, our results reject the hypothesis of habit formation in voting behavior. If (non)voting were actually habit forming, we would expect a lasting decline in turnout following the initial drop—even when the costs of voting were entirely restored to pre-event levels. Our estimates clearly do not support this pattern. However, in our setting, the decline in turnout—and consequently the test of the habit formation hypothesis—is likely disproportionately driven by inattentive voters. As this subset of the population is not necessarily representative of the general electorate, we cannot definitely rule out that habit formation still constitutes a relevant determinant of voting behavior for the average citizen.

The full set of our event study results are reported in Table 3. We first verify that our baseline estimates of the search and transportation effects (Equation 1) on turnout hold in the subsample used in the event study (Column 1). In Column (2), we present the event-study results corresponding to estimates reported in Figure 7. In Column (3), we additionally control for the log of street distance to absorb the transportation effect resulting from the polling place relocation. Since on average, a reassignment tends to increase the distance to a citizen's polling place, it is not surprising that post-event estimates now appear slightly closer to zero. Yet, the coefficients remain statistically significant with the exception of the event-dummy in Panel (B), which captures the initial impact of a polling place relocation on mail-in votes. Thus, it appears that, holding transportation costs constant, a polling place relocation reduces polling place voting but does not affect mail-in turnout. The shift towards mail-in voting only occurs in the election(s) ensuing the event. This result lends further support to the hypothesis of inattentive citizens, who would have switched to mail-in voting, yet notice their reassignment only after the closing date for polling card requests. We also estimate the event study on the full sample instead of trimming the time series once a second treatment occurs. The estimates presented in Column (4) show that results remain robust. In Column (5), we consider a different definition of the event. Specifically, the event corresponds to the first election in which at least 50 percent of a precinct is affected by polling place reassignments. The

effect size magnitudes are slightly attenuated but maintain statistical significance. Finally, we estimate the model on a balanced sample. This reduces the number of observations by roughly 500 and the number of treated precincts from 278 to 114, of which 90 percent occur in the 2017 Federal Election and 10 percent in the 2018 State Election. The results reported in Column (6) confirm the previous estimations. Only the negative treatment effect on overall turnout in Panel (C) appears statistically insignificant, possibly due to the loss of statistical power from the restricted sample.

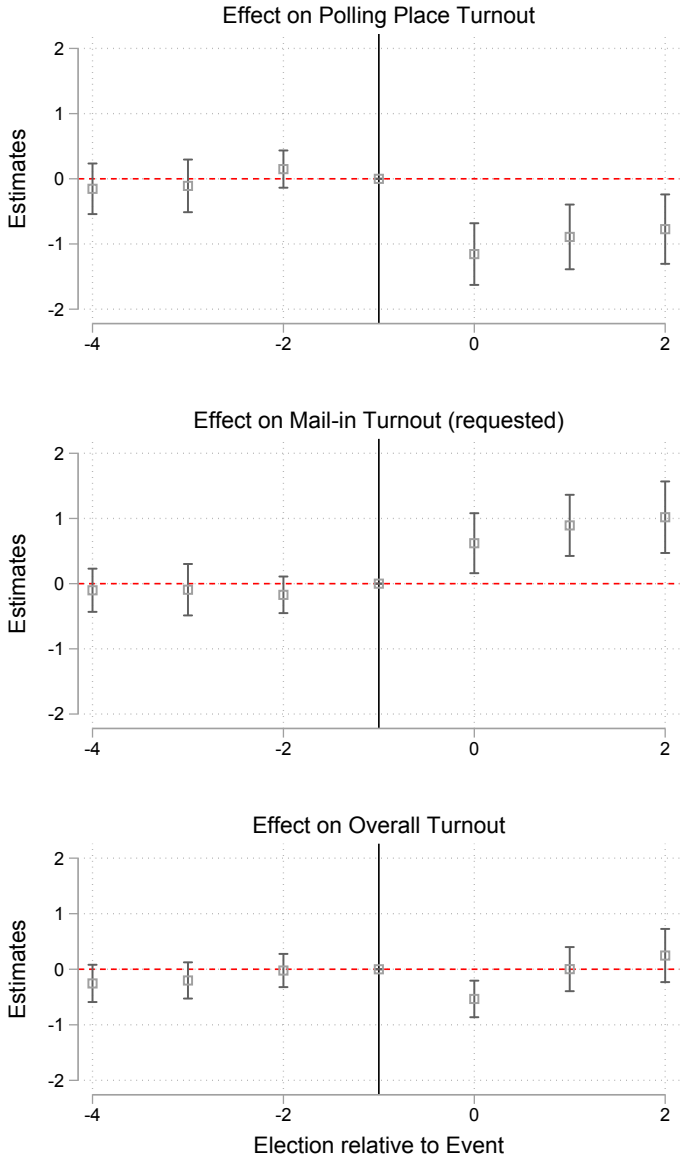
In Appendix Figure A.7, we replicate the results of Table 3, Column (4) with several novel difference-in-differences estimators for staggered timing of the treatment.[12] Column (1) of Figure A.7 shows the results applying the estimator suggested by Roth and Sant'Anna (2021a), Column (2) reports the estimators proposed by Callaway and Sant'Anna (2020), and Column (3) the one by Sun and Abraham (2020). In our setting, treatment accumulates in the 2017 Federal Election, which is right in the middle of our observation period. Hence, estimators give a high weight to this cohort and heterogeneity of treatment is only a minor concern.

5.3. Robustness of the results

Reason for Reassignment. one potential concern is that the different reasons for polling place reassignments yield systematically different behavioral responses. This would suggest that voters anticipate changes due to a reconfiguration of precinct boundaries and changes due to the recruitment of a different venue to varying degrees. Or that some part of the electorate is systematically more prone to experiencing one type of reassignment, casting doubt on the (quasi)randomness of treatment. It may also be the case that voters living near precinct borders are more likely to experience reassignments due to revisions of precinct boundaries. If these voters differ systematically with respect to other determinants of electoral turnout, this could in turn afflict our estimates of interest. To test whether the different sources for reassignments could be a source of concern, we re-estimate Equation (1) differentiating the reassignment by reason of reassignment. The results appear in Table 4. Column (1) reports the baseline results for comparison. The estimates in Column (2) show that the different reasons for polling place reassignment do not unequally drive the effect of a reassignment. The *t*-tests for equality of estimates (p-values reported in square brackets) indicate that the estimates are not statistically different from each other with respect to all outcomes (Panels A, B and C). This substantiates the assumption that voters do not anticipate or react differently to polling place reassignments depending on the source of the change.

[12] We used the staggered R-package by Roth and Sant'Anna (2021b).

Figure 7: Event Study Illustration



Notes: The figure presents the event study results from regressing turnout (at the polling place, via mail, and overall, respectively) on a set of election-date dummies around the event defined as the first time the entire precinct is reassigned to a new polling place (Equation 2). Regressions are weighted with the number of eligible voters. Confidence intervals reported at the 95% level. The full results of the underlying regressions appear in Column (2) of Table 3.

Table 3: Event Study

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Turnout at the Polling Place						
Reassigned	-0.63***					
	(0.15)					
Log Street Distance	-3.45***		-3.43***			
	(0.31)		(0.26)			
$t-4$		-0.18	-0.22	-0.19	-0.13	-0.55*
		(0.20)	(0.19)	(0.20)	(0.16)	(0.28)
$t-3$		-0.09	-0.15	-0.11	-0.01	-0.49
		(0.21)	(0.21)	(0.21)	(0.18)	(0.30)
$t-2$		0.14	0.15	0.14	0.21	0.21
		(0.14)	(0.14)	(0.14)	(0.15)	(0.20)
t		-1.15***	-0.74***	-1.16***	-0.97***	-1.85***
		(0.24)	(0.22)	(0.24)	(0.17)	(0.40)
$t+1$		-0.91***	-0.67***	-0.86***	-0.79***	-1.70***
		(0.25)	(0.22)	(0.22)	(0.22)	(0.36)
$t+2$		-0.77***	-0.51**	-0.60***	-0.59**	-0.99***
		(0.27)	(0.24)	(0.22)	(0.26)	(0.35)
R^2	0.97	0.96	0.97	0.96	0.96	0.96
Panel B: Turnout via Mail (requested)						
Reassigned	0.08					
	(0.16)					
Log Street Distance	2.57***		2.64***			
	(0.29)		(0.27)			
	(0.24)					
$t-4$		-0.09	-0.06	-0.07	-0.02	0.20
		(0.17)	(0.17)	(0.17)	(0.14)	(0.21)
$t-3$		-0.11	-0.07	-0.09	-0.06	0.30
		(0.21)	(0.20)	(0.20)	(0.18)	(0.29)
$t-2$		-0.17	-0.18	-0.17	-0.08	-0.03
		(0.15)	(0.14)	(0.14)	(0.17)	(0.20)
t		0.58**	0.26	0.59**	0.44**	1.51***
		(0.24)	(0.23)	(0.24)	(0.18)	(0.35)
$t+1$		0.89***	0.71***	0.79***	0.76***	1.40***
		(0.24)	(0.22)	(0.21)	(0.22)	(0.34)
$t+2$		1.01***	0.81***	0.70***	0.91***	1.39***
		(0.28)	(0.26)	(0.24)	(0.27)	(0.36)
R^2	0.95	0.95	0.95	0.95	0.95	0.95
Panel C: Overall Turnout						
Reassigned	-0.55***					
	(0.14)					
Log Street Distance	-0.88***		-0.79***			
	(0.24)		(0.25)			
$t-4$		-0.27	-0.28	-0.26	-0.15	-0.35
		(0.17)	(0.17)	(0.17)	(0.15)	(0.24)
$t-3$		-0.20	-0.22	-0.20	-0.07	-0.19
		(0.17)	(0.17)	(0.17)	(0.16)	(0.26)
$t-2$		-0.03	-0.03	-0.03	0.13	0.18
		(0.16)	(0.16)	(0.16)	(0.15)	(0.26)
t		-0.57***	-0.48***	-0.57***	-0.53***	-0.34
		(0.17)	(0.17)	(0.17)	(0.14)	(0.27)
$t+1$		-0.02	0.04	-0.07	-0.03	-0.30
		(0.20)	(0.20)	(0.19)	(0.19)	(0.30)
$t+2$		0.24	0.30	0.10	0.32	0.39
		(0.24)	(0.24)	(0.21)	(0.25)	(0.30)
R^2	0.99	0.99	0.99	0.99	0.99	0.99
Observations	4,350	4,350	4,350	4,500	4,090	3,518
Event: 100% reassigned		×	×	×		×
Full sample				×		
Event: >50% reassigned					×	
Balanced panel						×

Notes: Dependent variables are voter turnout (0–100) at the polling place (Panel A), by mail (Panel B), and overall (Panel C). Mail-in voting is approximated with requests of polling cards (*Wahlscheine*). All specifications include election and precinct fixed effects and control for the following precinct covariates: the log of the number of residents, the share of residents eligible to vote, the share of eligible voters aged 18-24, 25-34, 35-44, 45-59, respectively, the share of EU-foreigners in the electorate, the share of native German residents, the share of non-native German resident, 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. The specification in Column (1) additionally controls for the lag of *Reassigned* and the lag of *Log Street Distance* (output suppressed). Regressions are weighted with the number of eligible voters. Standard errors are clustered at the precinct level and reported in parentheses. $^{***}p<0.01,^{**}p<0.05,^{*}p<0.1$.

Error Correlation within Election-Districts. Another potential concern is that model errors are correlated within city districts. This may be the case because adjustments to boundaries of adjacent precincts do not occur across but solely within districts. Moreover, it is not uncommon that several precincts (within a district) have their polling place hosted by the same venue. In these cases, a change to the venue's activity status will simultaneously affect multiple precincts at once. To take this into account, we re-estimate Equation (1) correcting standard errors 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). Column (3) presents the estimates with two-way cluster-robust standard errors. The standard errors of our variables of interest increase slightly but their statistical significance remains unaffected. We also re-estimate our event study specification with two-way cluster-robust standard errors, which does not reduce the statistical significance of the estimates compared to the baseline specification.

Accounting for Constituencies. Unlike precincts, city districts are directly contested in some elections. In state and federal elections, for instance, the 25 districts cluster into several single-member constituencies, which are contested by party candidates for seats in the respective parliament. In municipal elections, citizens are invited to elect their local district committee (in addition to the city council and the major). If there are systematic differences regarding the incentives to vote across districts—for instance, because citizens anticipate very close races in some constituencies—this may pose a threat to validity of our estimates of interest. Thus, we account for potential cross-district variation by estimating Equation (1) including a full set of district-election fixed effects. This assures that comparisons occur only within district-election cells. The results reported in Column (4) show that our estimates of interest and their statistical significance are insensitive to the alternative specification.

Linear Time Trends. We also test the robustness of our results against the inclusion of precinct-specific time trends. In the aggregate, we observe a slight shift towards mail-in voting over time, which has been somewhat reinforced by the introduction of a simplified online application procedure for requesting polling cards in 2017. To account for possible differential trends among precincts, we re-estimate Equation (1) including a linear precinct-specific yearly trend. The results presented in Column (5) suggest that our results remain robust against this specification.

Excluding Election during Covid-19 Pandemic. We also estimate the model excluding the 2020 Municipal Election, which was held at the onset of the Covid-19 pandemic in March. Uncertainty about contagion risks and limited hygiene concepts led to a historically low polling place turnout.

As precincts may be hit by varying degrees by the crisis and voting behavior may not adapt uniformly in the city, we estimate the baseline equations without the 2020 election. Our results still hold, as shown in Column (6).

Alternative distance measures. We also consider alternative measures of the transportation cost effect in Appendix Table C.2. In our baseline, we use the logarithmic street distance (walking distance) between residential addresses and their assigned polling place (replicated in Column 1). Column (2) uses the linear street distance and Column (3) includes the linear street distance together with a quadratic term. The logarithmic and the linear street distance in Columns (1) and (2) show very similar estimates in all panels. Hence, the effect of an additional kilometer and of a doubled distance are comparable. This indicates that the effect is not driven by precincts with a very high or very low average distance to the polls. The quadratic distance in Column (3) shows that an additional meter tends to reduce the effect size. In Columns (4) to (6), we perform the same exercise but replace the street distance with the average straight-line (Euclidean) distance between the residential addresses and the polling place. In all but the first specification the estimates increase slightly as the straight-line distance is by definition shorter than the street distance. Importantly, the search cost effect (*Reassigned*) remains robust against alternative measurement of the transportation effect across all specifications.

6. Discussion

6.1. Comparison with previous research

Previous findings on the effect of polling place reassignments on voting behavior provide an important benchmark for our results. There exist no other studies investigating how lasting these effects are. We thus focus on contemporaneous effects in the following. We estimate that, on average, reassignments result in a decline of in-person voting by .75 p.p. which is partially offset by an .29 p.p. increase in mail-in voting, leading to an overall decline of about half a percentage point–or .74 percent evaluated at the mean. Brady and McNulty (2011) find a similar partial substitution of mail-in voting for in-person voting following polling place reassignments in the 2003 gubernatorial recall election in Los Angeles. However, the estimated effect magnitudes are significantly higher, with polling place turnout declining by 3.0 p.p. and overall turnout falling by 1.8 p.p, or 3 percent relative to an overall turnout of 61.2 percent. As Brady and McNulty analyze a setting in which the number of polling places was significantly reduced (and thus distances to the polls increased) the greater decline in turnout unsurprising. Still, we cannot rule out that our estimates suffer from attenuation bias due to imperfect measurement as we rely on the share of reassigned addresses

Table 4: Search and Transportation Costs—Robustness

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Polling Place Turnout						
Reassigned	-0.46***		-0.46***	-0.39***	-0.59***	-0.61***
	(0.11)		(0.12)	(0.12)	(0.15)	(0.14)
Log Street Distance	-3.38***	-3.37***	-3.38***	-3.41***	-3.50***	-3.57***
	(0.23)	(0.23)	(0.23)	(0.22)	(0.30)	(0.30)
Reassigned (Precinct Boundaries)		-0.54***				
		(0.18)				
Reassigned (Recruitment)		-0.42***				
		(0.14)				
R^2	0.97	0.97	0.97	0.97	0.98	0.96
T-test for equality of estimates		-0.56 [0.57]				
Panel B: Turnout via Mail (requested)						
Reassigned	0.08		0.08	0.07	0.20	0.32**
	(0.12)		(0.17)	(0.12)	(0.16)	(0.15)
Log Street Distance	2.37***	2.36***	2.37***	2.46***	2.55***	2.47***
	(0.24)	(0.24)	(0.25)	(0.22)	(0.30)	(0.31)
Reassigned (Precinct Boundaries)		0.13				
		(0.20)				
Reassigned (Recruitment)		0.06				
		(0.15)				
R^2	0.95	0.95	0.95	0.96	0.96	0.95
T-test for equality of estimates		0.31 [0.76]				
Panel C: Overall Turnout						
Reassigned	-0.38***		-0.38***	-0.31***	-0.40***	-0.29**
	(0.12)		(0.14)	(0.12)	(0.14)	(0.14)
Log Street Distance	-1.01***	-1.00***	-1.01***	-0.95***	-0.96***	-1.10***
	(0.20)	(0.20)	(0.20)	(0.19)	(0.25)	(0.25)
Reassigned (Precinct Boundaries)		-0.41**				
		(0.20)				
Reassigned (Recruitment)		-0.36***				
		(0.13)				
R^2	0.99	0.99	0.99	0.99	0.99	0.99
T-test for equality of estimates		-0.22 [0.82]				
Observations	4,944	4,944	4,944	4,944	4,944	4,326
Election FE	×	×	×		×	×
Precinct FE	×	×	×	×	×	×
2-way Cluster			×			
Election-District FE				×		
Linear Trend					×	
Excluding 2020 Election						×

Notes: Dependent variables are voter turnout (0–100) at the polling place (Panel A), by mail (Panel B), and overall (Panel C). Mail-in voting is approximated with requests of polling cards (*Wahlscheine*). All specifications control for lag of *Reassigned* and the lag of *log Street Distance* in addition to the following precinct covariates: the log of the number of residents, the share of residents eligible to vote, the share of eligible voters aged 18-24, 25-34, 35-44, 45-59, respectively, the share of EU-foreigners in the electorate, the share of native German residents, the share of non-native German resident, 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. Regressions are weighted with the number of eligible voters. Standard errors are clustered at the precinct level (except in Column 3) and reported in parentheses. In Column (3), standard errors are corrected for two-way clusters at the level of precincts (to account for model error correlation over time) and at the level of districts in each election (to account for within-district-election correlation). $^{***}p < 0.01$,$^{**}p < 0.05$,$^{*}p < 0.1$.

instead of reassigned individuals. Accounting for changes in distance, Brady and McNulty find that about 60 percent of the reduction in polling place turnout is due to the search effect. This estimate is almost identical to our finding. Keeping in mind that our setting also features relocations that result in a closer distance to the polling place, this result indicates that the search effect is stronger overall in their setting. In fact, the authors find that the magnitude of the search effect is approximately equivalent to moving the polling place about one mile (1.6 km) further away. By contrast, our estimates imply that the size of the search effect is comparable to an increased distance by about 100 meters and thus more than an order of magnitude smaller than Brady and McNulty's estimate. One explanation for the discrepancy is that voters' means of transportation to the polls differ, with Los Angeles voters primarily driving while Munich voters typically walking to the polls. Thus, the effects in terms of travel time are much closer. Another explanation is that the magnitude of the search effect itself is influenced by the distance to the polling place. If the new polling place is moved farther away, it is more likely located in an unfamiliar neighborhood. And consequently, the costs of getting acquainted with the new environment are higher. Since Brady and McNulty estimate the search effect in combination with *greater* distances to the polls, the search effect is thus probably more pronounced than in Munich, where distance increases and reductions are roughly at par.

McNulty et al. (2009) analyze a 2006 school budget referendum and estimate that the trimming of polling places caused a turnout decline of 7 p.p. Due to the negligible number of mail-in ballots, the authors focus on polling place voting only. Again, this substantial drop in turnout may be due to the fact that the travel distances to the polls increased. At the same time, the results suggest that the effects of reassignments crucially depend on the context. An extra cost of voting in less salient or lower-stake elections such as a school referendum may affect the voting decision more strongly than in higher-stake elections.

6.2. Policy implications

Election administrators' goal in Munich is to facilitate access to polling places as much as possible. Accessibility has been primarily understood in terms of precinct sizes, proximity to the polls, and (in more recent years) barrier-free access for individuals with physical impairments. Our results suggest that changing polling locations, even for the purpose of improving accessibility, constitutes an overlooked hurdle to voting. On average, reassigned voters are less likely to cast a ballot leading to a drop in aggregate turnout. We identified two main reasons for this result. First, the decision to vote appears only marginally affected by the change in the distance to the polls and primarily driven by the mere change in polling location (search effect). Secondly, inattentiveness

to reassignments push individuals to temporarily abstain from turning out. Both channels could be mitigated by minimizing the number of reassignments by actively considering reassignments a threat to accessibility. Moreover, if voters choose to abstain from turning out because they missed the deadline for requesting a mail-in ballot, an information treatment could alleviate effect; for instance, by notifying citizens of polling place relocations separately from the usual election notification. In a correlational study in the context of the 2001 mayoral race in the city of Atlanta (US), Haspel and Knotts (2005) provide suggestive evidence that postcards sent to voters whose polling place had moved increased the likelihood to turn out by reminding citizens to vote.

7. Conclusion

Voting is the backbone of democracy. Yet, the likelihood of a pivotal vote is dwarfed by any positive cost of voting. Thus, even small and seemingly innocuous shocks to voting costs may affect aggregate electoral turnout. We exploit a natural experiment in the city of Munich (Germany) to evaluate how the relocation of polling places affects democratic participation. We find that moving a polling place has a disenfranchising effect, depressing precinct-level turnout by .46 percentage points, on average. The decline in polling place turnout by .75 percentage points is partially compensated by an increase in mail-in votes by .29 percentage points. These effects can be explained by a combination of increased search costs due to unfamiliarity with the new polling place and transportation costs due to altered proximity to the polls. Further analyses show that the decline in overall turnout is transitory while the substitution of polling place for mail-in voting persists after the relocation of the polling place. This finding is consistent with the presence of inattentive voters, who only notice the polling place reassignment after the closing date for requesting mail-in ballots. Inattentive voters who would have switched to mail-in voting as their preferred choice either turn out at the new polling place anyway or abstain from voting. But with the awareness about the change, these voters revert to mail-in voting in ensuing elections, recovering the temporary drop in overall participation. Thus, rather than producing a (non)voting habit, reassignments provoke a persistent substitution of in-person for mail-in voting, consistent with rational choice models of electoral turnout.

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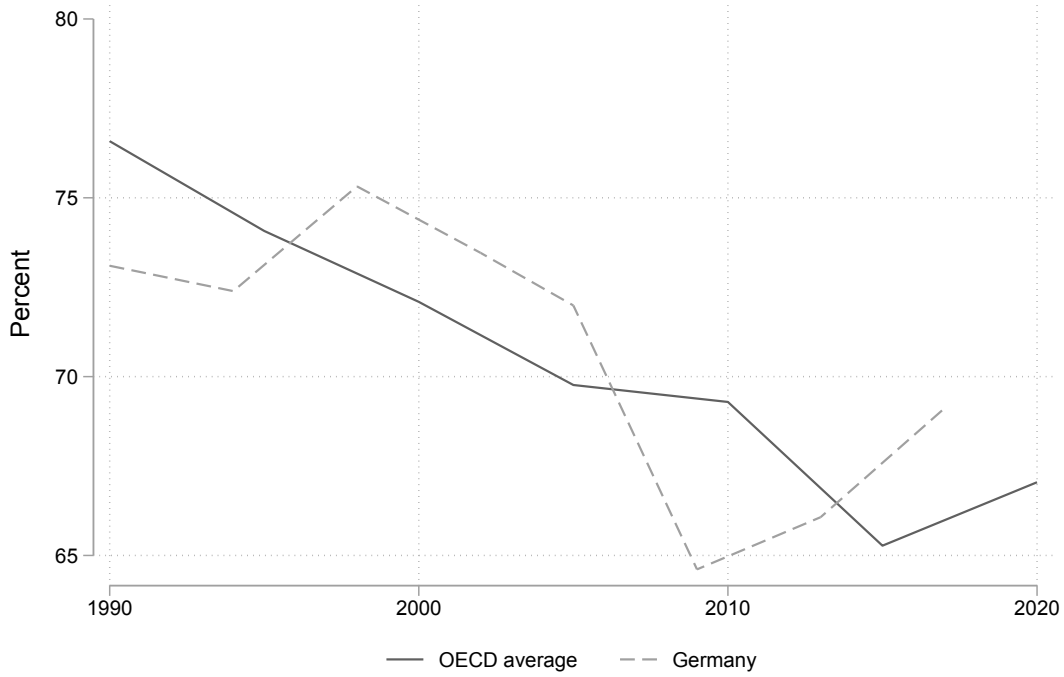
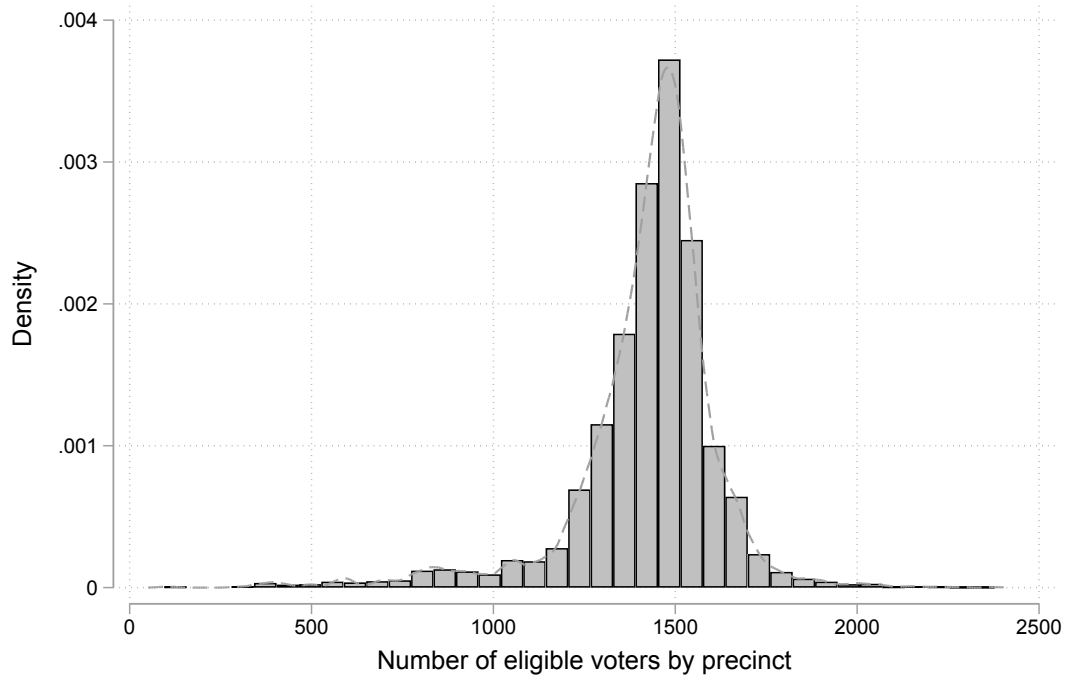
Appendix A. Figures

Figure A.1: Voter Turnout in the OECD and Germany



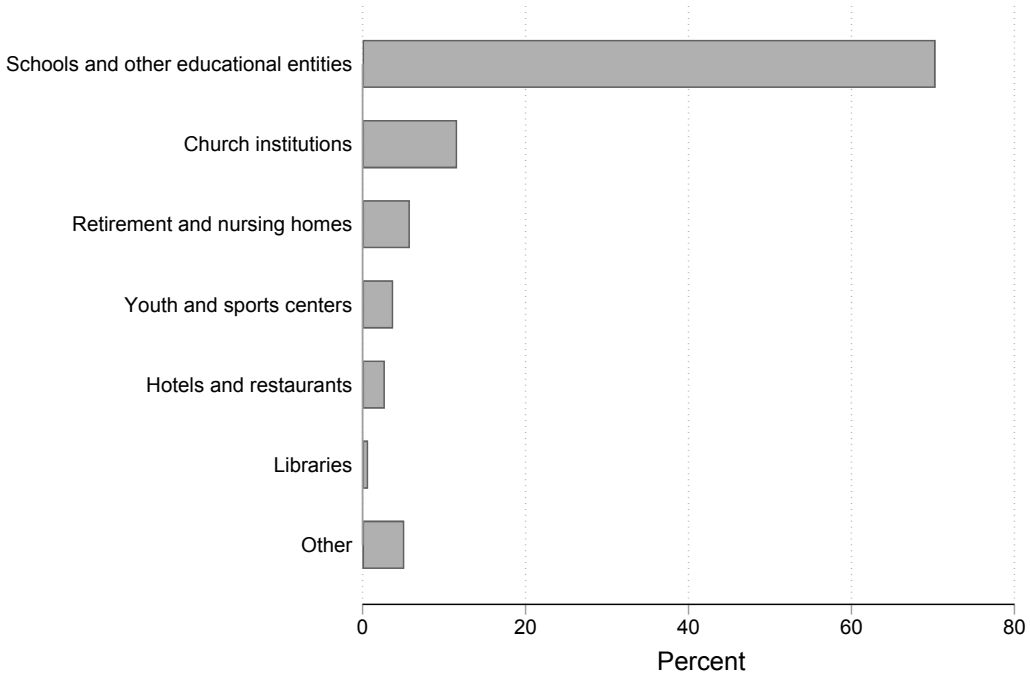
Notes: The figure plots voter turnout in Federal Elections in Germany and average voter turnout in national elections across OECD countries (5-year average). Data are from the International Institute for Democracy and Electoral Assistance.

Figure A.2: Distribution of Precinct Sizes



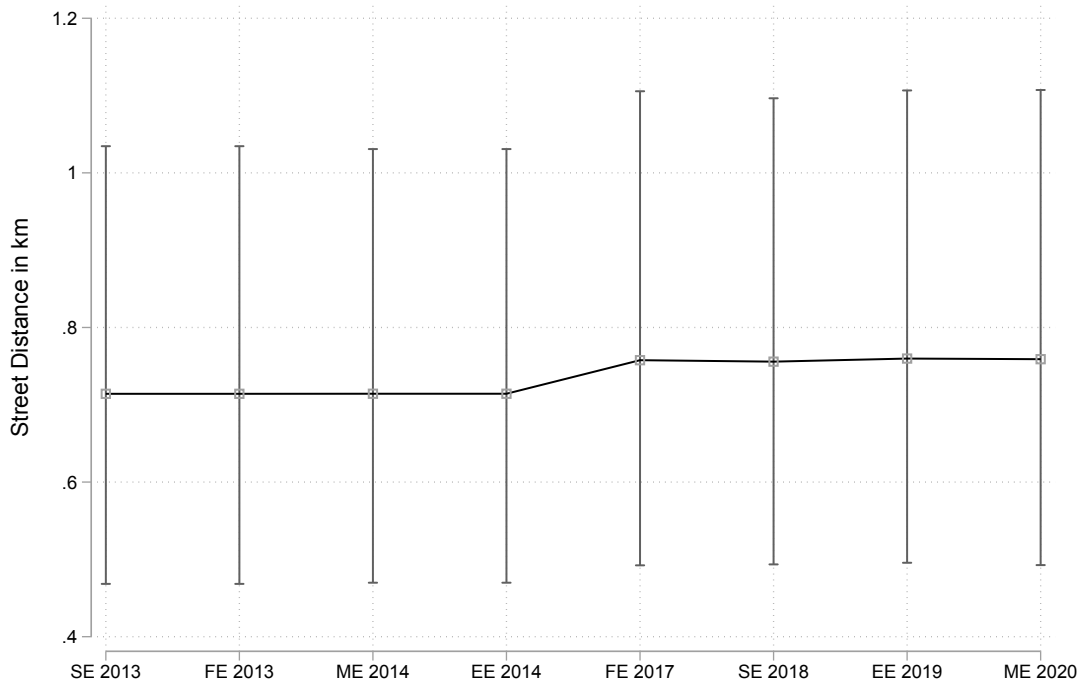
Notes: The figure plots the density precinct of sizes (number of eligible voters) over all elections. Precincts are delineated according to their election-specific boundaries (i.e., before harmonization of precinct borders).

Figure A.3: Types of polling venues



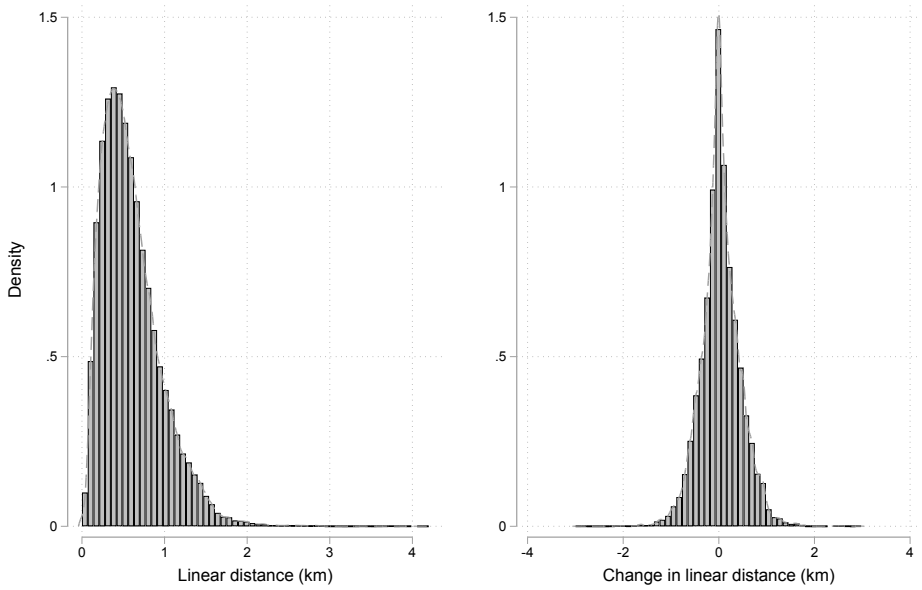
Notes: The figure depicts the frequency of types of polling places venues over the eight elections held in Munich between 2013 and 2020 (293 distinct venues in total).

Figure A.4: Median and Interquartile Range of Distance to the Polling Place



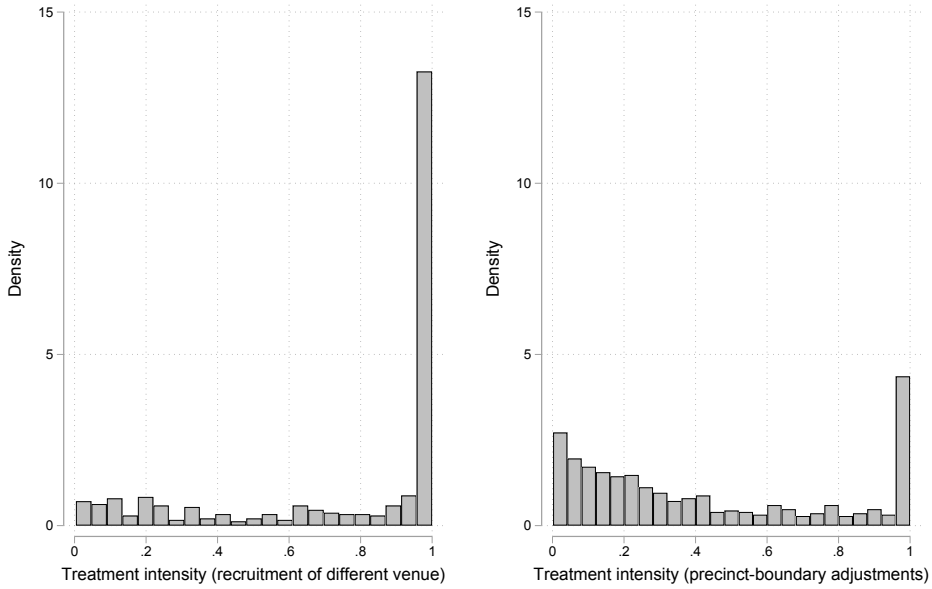
Notes: The figure plots the median and interquartile range (75th and 25th percentile) of the street distance between residential addresses of eligible voters and their designated polling place in each election between 2013 and 2020. SE = State Election, FE = Federal Election, ME = Municipal Election, EE = European Election.

Figure A.5: Density of Straight-line Distance and Distance Change to Polling Place



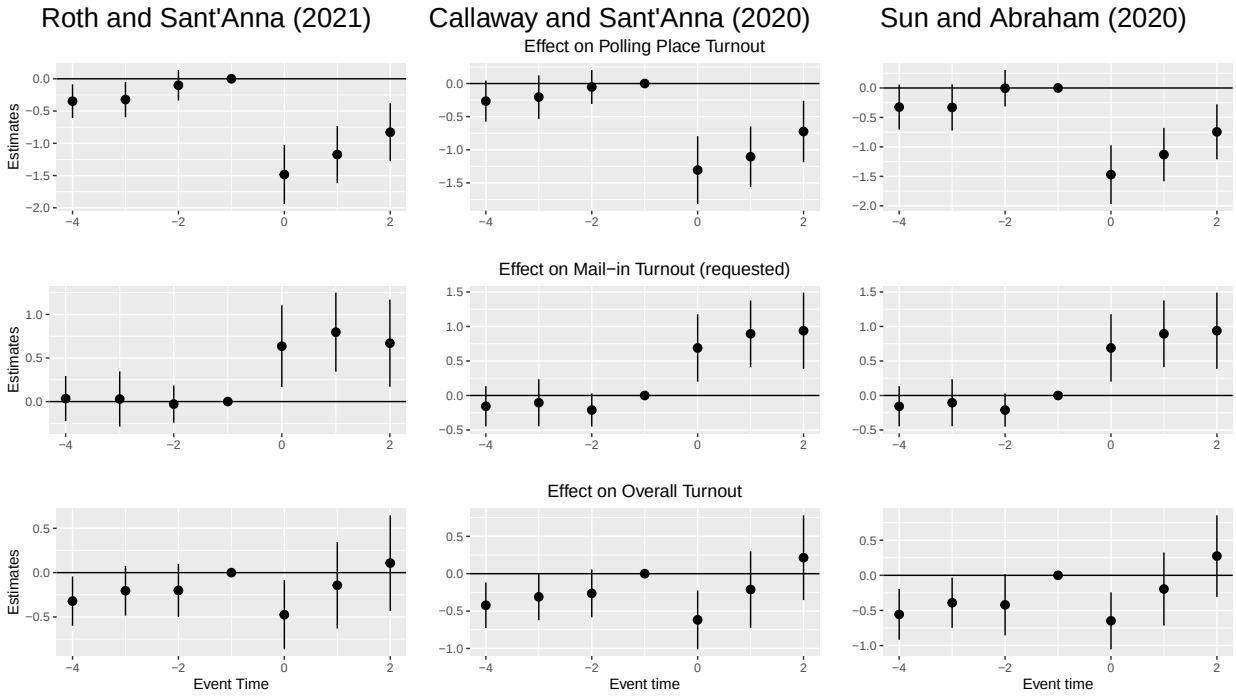
Notes: The figures present density plots for the straight-line (Euclidean) distance between residential addresses of eligible voters and their designated polling place (left plot) and the *change* in distance conditional on reassignment to a new polling place relative to the previous election (right plot) over the eight elections held between 2013 and 2020.

Figure A.6: Density of Treatment Intensity at the Precinct Level



Notes: The figure shows the density of treatment intensity (share of residential addresses reassigned to different polling place) by reason of reassignment. The left panel reports the density for polling place changes due to recruitment of a different venue, the right panel reports the density for changes due to precinct boundary adjustments. Observations with zero reassignments are excluded.

Figure A.7: Robustness: Event Study Illustration



Notes: The figure presents the event study results estimate with the suggested procedures by Roth and Sant'Anna (2021a), Callaway and Sant'Anna (2020), and Sun and Abraham (2020). Control variables are not included. Confidence intervals reported at the 95% level. Results replicate the specification of Column (4) in Table 3.

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)	28.92	7.64	4.01	23.10	29.46	34.70	51.99
Overall turnout (requested)	63.15	14.57	15.10	51.20	65.27	75.26	91.72
Variables of interest							
Avg. linear distance to the polling place (km)	0.52	0.27	0.11	0.32	0.46	0.64	2.19
Avg. street distance to the polling place (km)	0.71	0.34	0.16	0.47	0.63	0.87	2.57
Share of reassigned residential addresses	0.14	0.32	0.00	0.00	0.00	0.00	1.00
Reassigned (precinct boundary adjustments)	0.05	0.19	0.00	0.00	0.00	0.00	1.00
Reassigned (recruitment of polling place venue)	0.09	0.27	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
% Germans in the electorate	91.71	9.13	53.61	84.19	97.30	100	100
% EU-foreigners in the electorate	8.29	9.13	0.00	0.00	2.70	15.81	46.39
% households w/ children	17.53	6.08	5.31	13.35	16.69	20.43	58.75
Average duration of residence	21.69	4.45	6.80	18.53	21.72	24.51	45.11
Average quoted rent per sqm	17.42	4.54	6.69	13.67	16.45	20.30	43.92
Area in sqkm	0.50	0.85	0.06	0.16	0.29	0.49	10.69

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).

Table C.2: Robustness to Alternative Distance Measures

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Turnout at the Polling Place						
Reassigned	-0.46***	-0.46***	-0.45***	-0.50***	-0.51***	-0.49***
	(0.11)	(0.12)	(0.11)	(0.11)	(0.12)	(0.11)
Log Street Distance	-3.44***					
	(0.23)					
Street Distance		-4.38***	-7.49***			
		(0.33)	(0.88)			
Street Distance (squared)			1.60***			
			(0.43)			
Log Linear Distance				-3.15***		
				(0.21)		
Linear Distance					-5.39***	-8.88***
					(0.39)	(1.05)
Linear Distance (squared)						2.35***
						(0.66)
R^2	0.97	0.97	0.97	0.97	0.97	0.97
Panel B: Turnout Postal (requested)						
Reassigned	0.08	0.07	0.07	0.11	0.12	0.11
	(0.12)	(0.13)	(0.13)	(0.13)	(0.13)	(0.13)
Log Street Distance	2.41***					
	(0.24)					
Street Distance		3.27***	4.36***			
		(0.32)	(1.06)			
Street Distance (squared)			-0.56			
			(0.55)			
Log Linear Distance				2.08***		
				(0.23)		
Linear Distance					3.74***	5.15***
					(0.40)	(1.37)
Linear Distance (squared)						-0.96
						(0.91)
R^2	0.95	0.95	0.95	0.95	0.95	0.95
Panel C: Overall Turnout						
Reassigned	-0.38***	-0.39***	-0.39***	-0.38***	-0.39***	-0.38***
	(0.12)	(0.12)	(0.12)	(0.11)	(0.11)	(0.12)
Log Street Distance	-1.03***					
	(0.20)					
Street Distance		-1.11***	-3.13***			
		(0.27)	(0.85)			
Street Distance (squared)			1.04**			
			(0.42)			
Log Linear Distance				-1.07***		
				(0.18)		
Linear Distance					-1.66***	-3.73***
					(0.34)	(0.99)
Linear Distance (squared)						1.39**
						(0.65)
R^2	0.99	0.99	0.99	0.99	0.99	0.99
Observations	4,944	4,944	4,944	4,944	4,944	4,944

Notes: Dependent variables are the percentage voter turnout at the polling place (Panel A), by mail (Panel B), and overall (Panel C). Mail-in voting is approximated with requests of polling cards (*Wahlscheine*). All specifications include the lag terms of *Reassigned* and the respective distance variable and include the following controls: log of the number of residents, the share of residents eligible to vote, the share of eligible voters aged 18-24, 25-34, 35-44, 45-59, respectively, the share of EU-foreigners in the electorate, the share of native German residents, the share of non-native German resident, 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. Regressions are weighted with the number of eligible voters. Standard errors are clustered at the precinct level and reported in parentheses.

Appendix C. Elections in Munich

Federal Elections. The German *Bundestag* is elected for a four-year term by German citizens over the age of eighteen. Elections are based on a mixed-member proportional representation system, where half of the members of parliament are elected directly from 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 based on a plurality rule, and a second vote for a party list, which is established by the respective party caucus. Each constituency is represented with one seat in the *Bundestag*, with the remaining seats allocated to achieve proportionality based on the second votes only.

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

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 adult Germans are eligible to vote in the European Election. 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.

Munich City Council Elections. Municipal Elections in Munich comprise three distinct elections which are held every six years on the same day: 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 who are elected based on (open) party lists and the mayor as the chairperson. In addition to adult German citizens, EU-foreigners are also eligible to vote in municipal elections.