Polling Place Relocations and Democratic Participation

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Abstract

To what extent does the electoral process affect democratic participation? We exploit a natural experiment in the city of Munich (Germany) to evaluate the effect of a seemingly innocuous bureaucratic procedure: the relocation of polling places. We find that reassignments depress electoral return by approximately half a percentage point. The effect can be explained by the altered proximity to the polls and a search cost resulting from uncertainty about the new location. The decline in turnout is recovered in the subsequent election as mail-in voting offsets the reduction in polling place voting. Thus, rather than producing a (non)voting habit, reassignments provoke a persistent substitution of in-person for mail-in voting in line with rational choice models of electoral turnout.

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

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1. Introduction

Leading up to the 2020 US presidential election, a controversy surrounding a series of managerial decisions at the US Postal Office drew national attention. Louis DeJoy, the acting Postmaster General and a major donor to the Trump campaign, was accused of politicizing the national postal service by implementing a number of reforms, including the removal of collection boxes, the reduction of mail-sorting machines and the elimination of employee overtime. Many observers voiced concerns over whether the reduced accessibility of postal services served to disenfranchise Democratic voters, who were anticipated to predominantly turn out by mail amid the Covid-19 pandemic. Former President Barack Obama even accused President Trump of attempting to "actively kneecap" the Postal Service to affect mail-in voting in the 2020 election. Although there is no evidence to date that Postmaster General DeJoy's interventions systematically shifted vote shares in favor of the Republican Party, the controversy brought attention to a pertinent question: to what extent does the electoral process and its supporting infrastructure affect electoral return?

We address this question by evaluating the effect of a seemingly innocuous bureaucratic procedure on democratic participation: the relocation of polling places. If sizable, the burden of being reassigned to a different polling place could induce a worrisome source of disenfranchisement. This is particularly relevant in an environment in which the administration of elections is prone to political influence (Amos et al., 2017). But even—or especially—in contexts, in which facilitating electoral turnout is the primary objective, understanding to what extent the electoral process hampers voter participation is a matter of public importance.

Our natural experiment takes place in Munich, the third largest city in Germany and capital of the State of Bavaria, where voters may experience an unexpected relocation of their polling place for two reasons. First, due to administrative purposes, the boundaries of some voting precincts are redrawn between any two election years so that a portion of the electorate is reassigned to a different polling place. Secondly, polling place venues (typically schools) need to be newly recruited for every election. And although electoral administrators seek to keep the location of polling places unaltered, competing events on Election Day, construction works, or other circumstances may render some locations unavailable so that different venues need to be recruited, thus producing variation in the location of a precincts' polling places over time.

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 increase or reduction in travel time to the polls resulting from the change in proximity to the polling place. The search effect refers to the additional costs from searching for and learning about a new polling place (holding proximity constant). If the net increase in the cost of voting at the polls is sufficiently large, then voters will either switch to mail-in voting or completely abstain from turning out upon reassignment. Since a reassignment is likely to produce a lasting change to the costs of voting, we expect that behavioral adjustments carry over to subsequent elections. As a number of empirical studies have pointed out, persistence in voting patterns can also be the product of habit formation in the sense that current voting behavior affects future voting decisions, all else equal. Thus, to the extent that initial behavioral adjustments are internalized into a new habit, this mechanism provides an additional driver of persistent effects of a reassignment "shock".

To empirically evaluate these predictions, we geo-locate residential addresses and their designated polling places for the eight elections that took place between 2013 and 2020 in Munich and determine the changes as well as the distance between each pair. Harmonizing precinct boundaries over our observation period, we build a panel of 618 precincts with official election returns (turnout via mail, at the polling station, and overall participation), time-varying sociodemographic precinct characteristics as well as the share of reassigned residential addresses of eligible voters and the average proximity to the designated polling place.

We find support for the search and the transportation effect following polling place reassignments: holding proximity to the polling place constant, relocating the polling place depresses contemporaneous voter turnout by .38 ($SE = .12$) percentage points (p.p.)—or .61 percent, evaluated at the mean. The decline in polling place turnout by .46 p.p. ($SE = .11$) is partially compensated by an increase in mail-in votes by .08 p.p. ($SE = .12$). The estimates imply that to counterbalance the negative impact of the search effect on overall turnout, a polling place would have to move approximately 38 percent closer to the voter on average. The validity of our estimates depends on whether "treatment" in a given election is plausibly random. We show that turnout is unrelated to reassignments in future elections and that differences between treated and untreated precincts regarding sociodemographic characteristics are negligible once we partial out election-specific shocks and time-constant variation at the precinct level. To investigate the persistence of the relocation shock, we conduct an event study in which we focus on the voting behavior around the first time a precinct is treated in our panel. The dynamic analysis allows us also to address the concern that voters with declining propensity to vote may be more likely to be reassigned to a new polling station, causing spurious correlations between reassignment and turnout. We find no evidence of differential trends preceding the treatment, further supporting the assumption that polling place reassignments occur

quasi-randomly. The event study results further show that the relocation-shock leads to a significant drop in overall turnout in the treatment year, but mail-in votes 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 switched to mail-in voting as their preferred choice will either turn out at the new polling place anyway or completely abstain from voting upon reassignment. But with the awareness about the change, these voters revert to mail-in voting in the ensuing elections, recovering the temporary drop in overall participation. Rather than producing a new (non)voting habit, the reassignment is thus more likely to have persistently increased the costs of in-person voting relative to mail-in voting.

Our empirical evaluation of the causal effects of polling place reassignments on electoral return builds on two previous studies. Brady and McNulty (2011) exploit the consolidation voting precincts due to cost cutting purposes in the in the 2003 gubernatorial election in Los Angeles, which resulted in a reduction in the number of polling places. To account for the non-random reassignment of voters to polling stations, the authors employ statistical matching of treated to non-treated individuals. They find a decrease in polling place turnout among reassigned voters, which was only partially offset by increased absentee voting. McNulty et al. (2009) use a similar approach to 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.

Our setting deviates from these studies in several ways. First, our observation period covers several elections, so that we are able to examine the persistence of the treatment effect over subsequent elections and to shed light on the habit-forming effects of a reassignment shock. If voters are dissuaded from turning out when burdened with a new polling place, this might translate into a new (non)voting habit. Such an analysis goes beyond the short-time effects that have been investigated by scholars so far. Secondly, because of the motive of the policy interventions (cutting administrative costs), the aforementioned studies estimate the treatment effects uniquely from reductions in the number of polling places and thus from *increases* in the distance to the polls. In our setting, election official seek to maintain the accessibility to the polling places constant over time so that the number of instances and the degree to which a reassignment results in a closer or farther travel distance to the poll is comparable. Thirdly, unlike in the U.S., local election administrators in Germany are not elected, but typically represent nonpartisan civil servants (Kimball et al., 2006).

And sub-municipal precincts in Munich do not correspond to electoral districts, i.e., they are not directly contested, but represent mere administrative subdivisions of the electorate. In effect, the recruitment of polling places and the redrawing of precinct boundaries in Munich are essentially devoid of political influence. These processes rather constitute bureaucratic acts aimed at facilitating the administration of elections while preserving a decent access to the polls for voters. Thus, our natural experiment carries a significantly lower risk that changes to the assignment of voters to polling places are confounded by (unobserved) political interference, as has been noted in several U.S. settings (Amos et al., 2017). Our panel structure additionally allows us to exploit the variation within precincts and to account for possible non-randomness in treatment by partialling out time-constant and election-specific factors that may confound our treatment effects.

Several correlational studies also document the negative association between reassignments or travel distance to the polls and electoral turnout. Amos et al. (2017) emphasize that reprecincting in the U.S. is unlikely a purely bureaucratic matter, but often influenced by political considerations regarding electoral consequences. Against this backdrop, the authors find that the reduction of Election Day 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. Haspel and Knotts (2005) exploit individual-level variation for the 2001 mayoral race in the city of Atlanta, and show that individuals 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). The findings typically suggest a nonlinear effect (diminishing marginal impact) of larger distances on the propensity to vote.

Our study also contributes to the empirical literature on habit formation and voting behavior. 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 the formation of voting habits remains mixed. Meredith (2009) estimate that voters who just turned 18 at the time of the 2000 U.S. general election, and thus are barely eligible to vote, are also significantly 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 based on face-to-face and direct mail canvassing increase turnout in subsequent elections. By contrast, Bechtel et al. (2018) and Gaebler et al. (2020) show that compulsory voting had no persistent effects on turnout once it was abolished in Switzerland and

Austria, respectively. Potrafke and Roesel (2020) find that longer opening hours of polling stations 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 an arguably transitory and unexpected shock to voting costs and show that the decrease in turnout induced by higher precipitation also reduces turnout in subsequent U.S. presidential elections. In our setting, the relocation of a polling station, although plausibly unexpected, is clearly correlated with future voting costs (especially when the new polling place is moved farther away). Thus, distinguishing whether persistently lower turnout rates reflect habit formation or a permanent shift of voting costs may be impossible. However, we are able to test the necessary condition for habit formation; namely, that the reduced turnout as a consequence of polling station relocation carries over into subsequent elections. We will show that the necessary condition for habit formation can be rejected as affected voters are not permanently dissuaded from voting. The results suggest instead that polling place voting becomes (persistently) more costly relative to mail-in voting and that voters accordingly switch from in-person to mail-in votes in subsequent elections.

Finally, our study also speaks to the empirical literature concerned with assessing the role of institutional barriers for democratic participation, including arduous registration procedures (Corvalan and Cox, 2018; Braconnier et al., 2017; Burden et al., 2014; Brians and Grofman, 2001), postage costs (Schelker and Schneiter, 2017) and limited opening hours of polling places (Potrafke and Roesel, 2020; Garmann, 2017).

The remainder of this paper is organized as follows. Section 2 describes the institutional background, the experimental setting, and the data used in the study. Section 4 outlines the empirical strategy. Section 5 presents the results. In Section 5.3, we discuss potential threats to the validity of our estimates and present several robustness checks. Section 7 concludes.

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 the following, we briefly describe the key features and differences of the electoral processes.

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 vote 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 districts 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, while the specific voting systems vary across EU-member states. 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. In principle, it is also possible for non-German EU citizens living in Munich to vote in the city, but they must 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 the municipal elections.

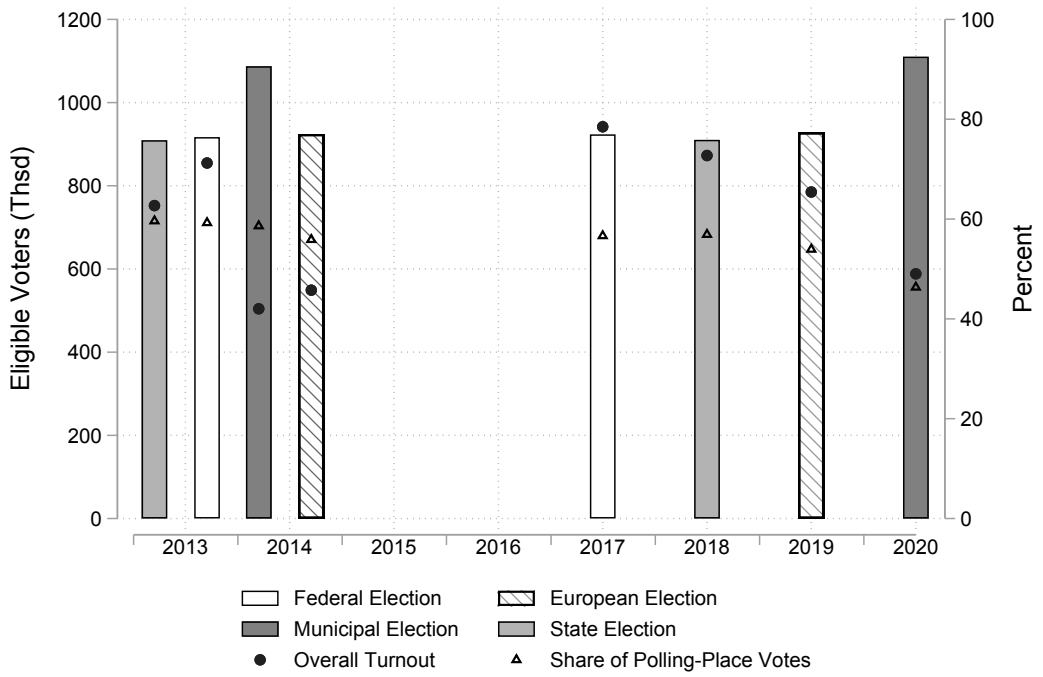
In general, all entitled voters registered as residents of the municipality are automatically entered on the electoral roll without having to make a specific request. Every person on the roll receives an election notification via mail (no later than 21 days before any election) containing information about the Election Day and 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*). Importantly, there is no explicit information about any *changes* to the polling place location. Eligible voters may vote in person at the designated polling place on Election Day. Alternatively, they can file a request for 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 allows to vote at another polling place in the city (e.g., if the original polling station 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.

Figure 1 illustrates the timeline of the eight elections included in our panel. Depicted are the total number of eligible voters in each election (vertical bars on the 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. The number of eligible voters is distinctively greater in municipal elections, as EU-foreigners are automatically entered on the electoral roll. The share of polling place votes typically ranges between 50 and 60 percent of all votes, with small declines between any pair of elections of the same type 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. Overall turnout tends to increase over time when comparing the same election type. Most notably, voter participation surged by nearly 20 percentage points in the 2019 European Election compared to the previous election in 2014.

2.2. Precincts and Polling Places

All elections in the city are organized and administered by the Munich Electoral Office (*Wahlamt*) following a strict legal framework. The electorate is geographically subdivided into several hundred voting precincts in every election. We geo-reference polling place locations and residential

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 percentage turnout (circles) and the share of polling place votes (triangles) 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.

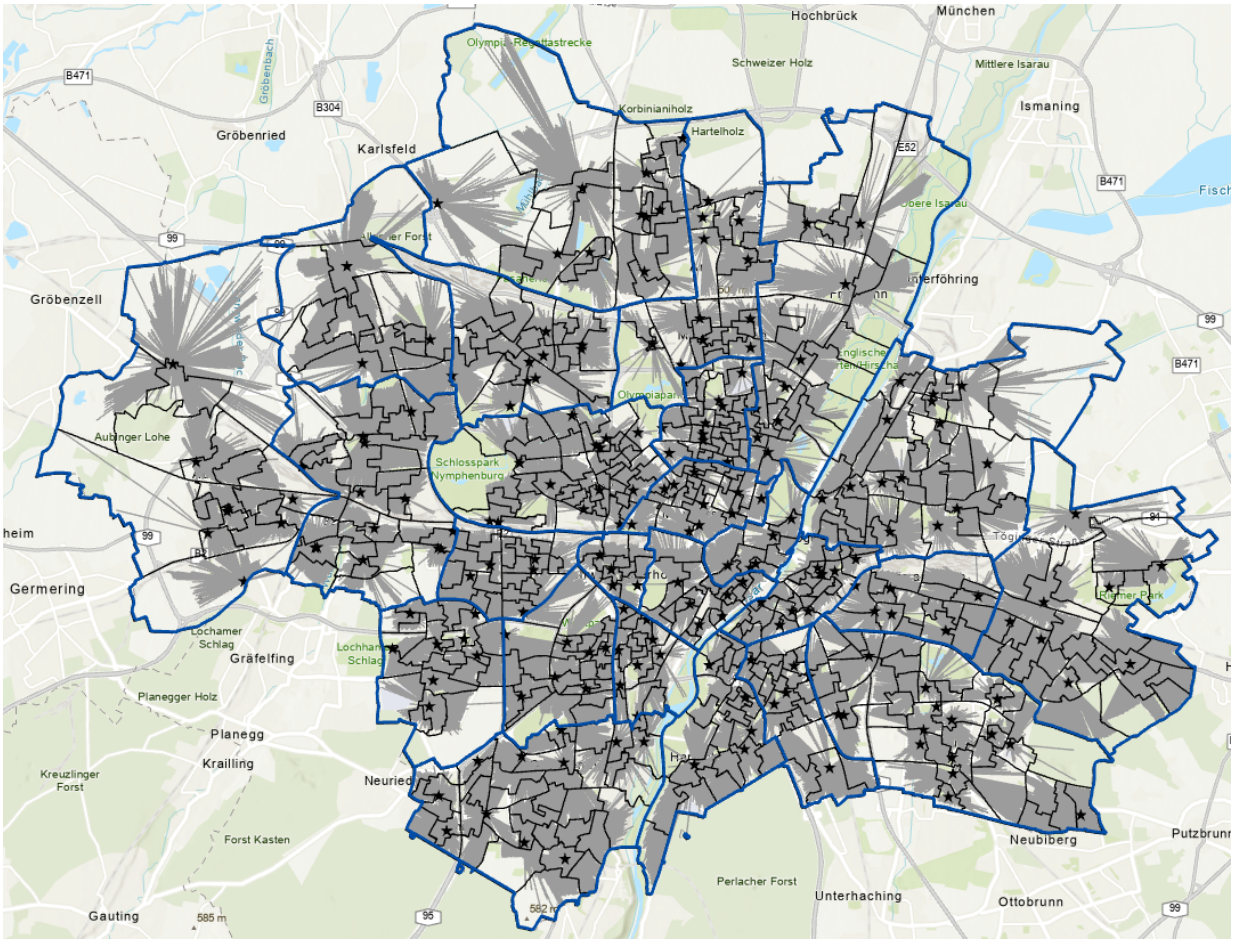
addresses in the eight elections that took place between 2013 and 2020 as well as in the 2009 federal election, which serves as reference to identify changes to polling station assignments relative to the 2013 state election (the first election in our panel). We also calculate the euclidean (linear) distance and the street distance, defined as the shortest walking distance using the public road network, between every pair of residential address and polling station in every election.[1]

Figure 2 illustrates the electoral map for the State Election in 2018. The black boundaries identify the 618 precincts, the blue boundaries delineate the 25 city districts. There is one polling place (depicted by a black star) for every precinct, but it is not uncommon that a single venue, typically

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

a school, hosts polling stations for several neighboring precincts. The straight gray lines pair the residential addresses of eligible voters on the official electoral roll with the corresponding polling place.

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.

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".[2] 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

[2]See LWO §10, BWO §12, EUW §12, GLKrWO §13

number of 1,500 eligible voters per precinct during the elections included in our panel (see Appendix Figure A.1 for a density plot of precinct sizes over all elections). In every election year, the electoral office evaluates whether population growth and new housing units requires adapting the number of precincts or precinct boundaries to maintain the aforementioned aims. Adjustment of boundaries including precinct consolidations are performed only *within* city district borders, which remain unaltered over time. 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 city management technology, which allowed for a more granular spatial monitoring of the electorate and thus a more precise delineation of precincts. This resulted in a major redivision of the city area and a significant reduction in overall precinct size variance. The number stayed at 618 in 2018 and 2019 and increased again to 755 in 2020 as the city accounted for a larger number of eligible voters during municipal elections.

Recruitment of Polling Places. A second source of (temporal) variation in the assignment of voters to polling places emerges from the recruitment of the venues hosting in-person voting on Election Day. 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 a sufficient mobile network connection in the polling place 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), churches and other ecclesiastical institutions (5 percent) – see Appendix Figure A.2 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 or ongoing construction work may render certain locations unavailable.[3] Overall, we observe 293 distinct venues that hosted polling stations 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 place venues, election officials

[3]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 place venues. It is also possible that ecclesiastical institutions schedule religious events on Election Sundays.

have maintained the accessibility of the polls fairly constant over time. Appendix Figure A.3 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 is 800 meters before slightly increasing to 840 meters in 2017, which roughly corresponds to a 10 minutes walk.

3. Conceptual Framework

To guide our empirical strategy, 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 in person 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 both 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 seats 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 postal 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 3 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 (non)voting decisions according to different cost-benefit configurations. If one imagines a distribution of the Munich population over the depicted plane, then historically roughly 40 percent of eligible voters would lie somewhere in the nonvoting area (N), 35 percent in polling place voter area (P) and the remaining 25 would find themselves in the mail-in voter area (M).

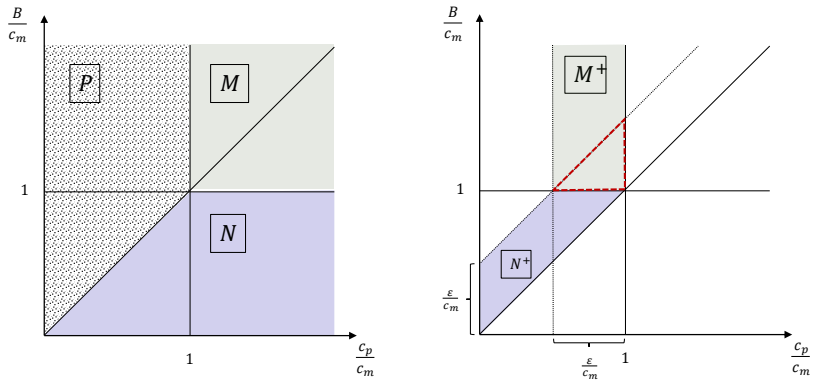
Now, suppose that the electorate is affected by a relocation of the polling place. We anticipate that reassignments of voters to polling places impact the costs of turning out at the polling place via two distinct mechanisms: *i)* through a "transportation effect" and *ii)* through a "search effect"

(Brady and McNulty, 2011; McNulty et al., 2009). The transportation effect captures the increase or reduction in transportation 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, transportation costs mainly correspond to the travel time between the home and the polling place. The search effect refers to the additional costs from searching for and learning about a new polling place (holding proximity constant). Search costs may also capture uncertainty about the new location and the psychological barrier to engage with the new 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 3. 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, a 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 may arise, 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 application deadline for mail-in ballots so that the cost of switching to mail-in voting is essentially infinity. As a result, an additional portion of voters with cost-benefit vectors within the highlighted red triangle will join nonvoters and further decrease overall turnout. To the degree that inattentiveness is independent from the proximity to the polling place, the mechanism is likely to amplify the contemporaneous search cost effect.

To what extent may 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 concerning finding and learning about the new polling place may still carry over into the future. Thus, the relative cost reduction of mail-in relative to in-person voting is likely to persist and accordingly shift the voting

Figure 3: Effect of Increasing Cost of Polling Place Voting on Voting Behavior



Notes: The left diagram illustrates citizens' possible voting behavior—turning out at a polling station (P), by mail (M), and not voting (N)—as a function of (individual) benefits (B) and costs of turning out at a polling station (c_p) and via mail (c_m). The right diagram illustrates the effect of an increase to the costs of voting at the polling place. The red triangle marks the additional

behavior. If the absolute cost increase for voting at the polling place is sufficiently high, then voters may entirely abstain from voting. This is the case when the costs of switching to mail-in voting and casting a ballot outweigh the perceived benefits. However, the initial election may be different from subsequent ones due to inattentive voters. While these voters initially miss the deadline for mail-in voting, they will be aware of the relocation in the ensuing election so that the cost shock fades over time, increasing the overall electoral return again.

A second mechanism that could drive persistent changes in voting pattern is habit formation. In its narrowest sense, habit 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

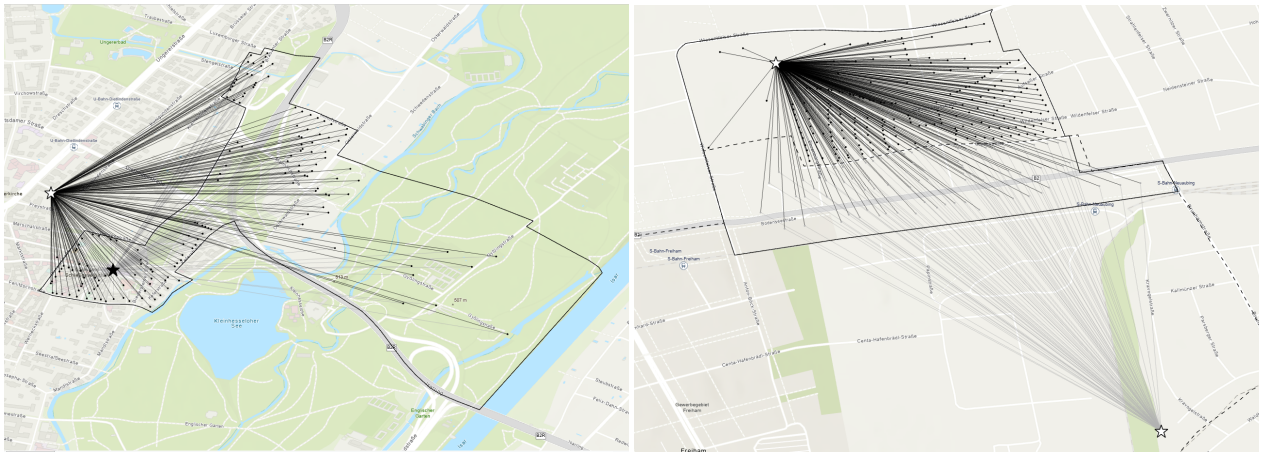
4.1. Treatment Variables

Polling place reassignments can occur due to changes to the activity status of a polling place venue or due to modifications of precinct boundaries. Figure 4 illustrates two instances of polling place reassignments which exemplify the treatment in our natural experiment. Black dots depict residential addresses, which are connected via gray lines to the 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 boundaries 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 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 place 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 redrawing of its boundaries. The solid black lines demarcate the borders of the precinct of interest as of 2018. The dotted lines delineate the boundaries of another precinct as of 2017. Hence, citizens living at residential addresses located at the intersection of these two shapes were effectively reassigned from one precinct to the other, and consequently experienced a change to the location of their designated polling place. By contrast, the fraction of voters living to the north of the dotted line were assigned to the same polling place in 2017 and in 2018 and are therefore considered untreated in our experimental setting. In contrast to the example in Panel (a), both polling places remained operational in 2018 (white stars).

Figure 4: Illustration of Treatment



(a) Recruitment of different polling place venue **(b)** Redrawing of 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 marked by black dots and the precinct boundaries as of 2018 are delineated by a solid black line. In Panel (a), all residential addresses are reassigned due to the recruitment of a different polling place venue: from the location marked by a black star to a new location marked by a white star. Panel (b) illustrates reassignments due to adjustments to precinct boundaries: the subset of residential addresses at the intersection of the 2018 precinct delineation (solid black boundaries) and the 2017 delineation (dotted 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 5 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 place venues.

Figure 6 plots 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 proximity to an address (relative to the location in the previous election), positive values correspond to a relocation farther away.[4] For 90 percent of residential addresses, the polling station is located no more than 1.4 kilometers away, which roughly corresponds to a 17-minutes walk (median: 735 meters, mean: 817 meters, max: 5,164 meters). The median difference in proximity to the polling place following a reassignment is 30 meters (mean: 55 meters), the 25th (75th) percentile is -237 meters (349 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.

4.2. Precinct-level Data

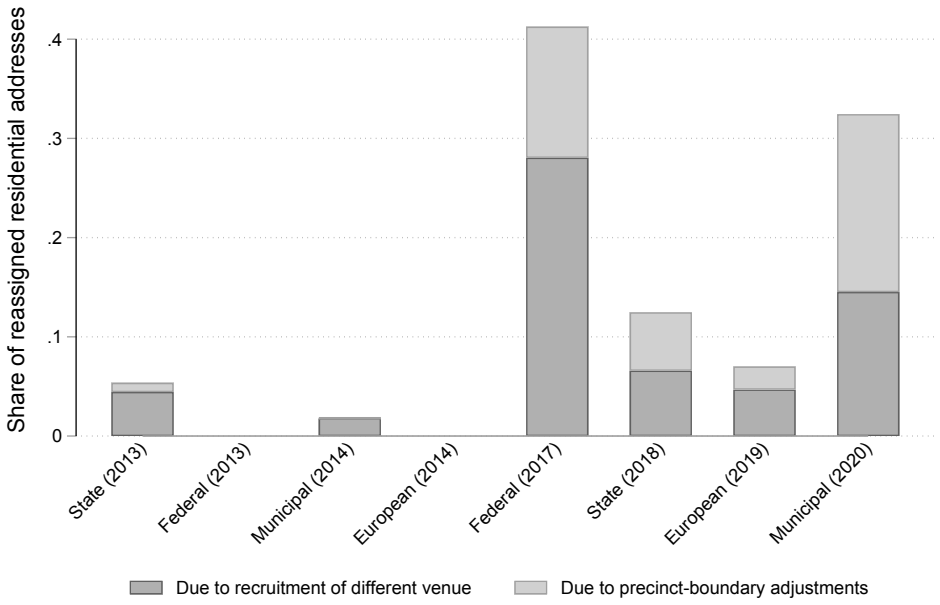
All information on polling place locations, residential addresses, and voter turnout (by mail and in-person at the polling place) stem from administrative sources including official electoral rolls and official election results. In addition, we leverage time-varying administrative data on structural indicators at the precinct level.[5] These include information on the age structure of the electorate, average duration of residence 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.[6] 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

[4]Figure A.4 in the Appendix reports the distributions for linear distances. Notice that by definition linear distances are no greater than street distances.

[5]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/Indikatoren-und-Monatszahlen/Wahlatlas.html. Official electoral rolls including residential and polling place addresses are provided by the Munich Election Office (*Wahlamt*) upon request.

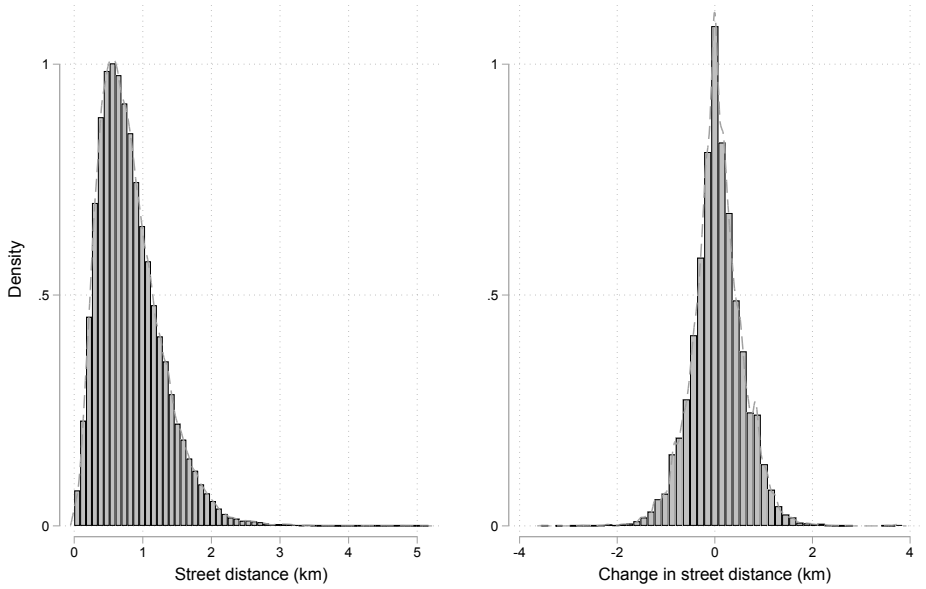
[6]The RWI - Leibniz Institute for Economic Research (formerly Rheinisch-Westfälisches Institut für Wirtschaftsforschung) and its research data center compile granular real estate data obtained from the Internet platform *ImmobilienScout24* for research purposes.

Figure 5: Share of Addresses Reassigned to Different Polling Place Relative to Previous Election



Notes: The figure presents the share of residential addresses of eligible voters 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 place venue.

Figure 6: Density of Street Distance and Change in Proximity to the Polling Station



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

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*).[7] This leaves us with a panel of 618 precincts with constant borders, which we observe over eight elections. Appendix Figure A.5 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), whereas in 40.4 percent of all instances the treatment intensity is below 50 percent.

Table B.1 in the Appendix reports summary statistics of our precinct-level variables.

4.3. Main Specifications

We estimate the contemporaneous search and transportation effect by relating turnout to polling place reassignments and changes in 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 polling place. By including precinct fixed effects,

[7] 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.

α_p, we identify the effect of *Distance* from precinct-specific deviations from the mean, which are precisely driven by polling place reassignments. Thus, the transportation effect is captured by the estimate of γ_2. We also control for the lags of the search and the transportation effect 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 global election-specific shocks such as differences in the propensity to vote due to varying perceived stakes of electoral outcomes 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 treatment 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 balance tests 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, i.e., is affected by reassignments, 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-date dummies $D^k_{pe(t)}$ relative to the date

of 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 reassignments. We also trim precincts' time series from the moment a second event 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 assumptions in the subsequent section.

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 primary 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 the estimation approach proposed by Callaway and Sant'Anna (2020). The authors 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.

5. Polling Place Relocation and Democratic Participation

We estimate Equations (1) and (2) using OLS. Precinct-level observations are weighted 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.

5.1. Search and Transportation Costs

Estimates of the search effect and transportation effect are reported in Table 1. Panels A and B show the results for polling place turnout and turnout via mail, respectively. Panel C reports the net effect

on overall participation. Column (1) includes only the share of reassigned residential addresses and the fixed effects. Column (2) adds the log of the average distance to the polling place, and Column (3) further includes precinct covariates. Column (4) reports the full specification including the lag terms of reassignment and average street 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. The results are presented in Column (5).

In line with our expectations, the search effect on polling place turnout is negative and significant at the one percent level across all specifications. Controlling for contemporaneous average distance, covariates and the lag terms, the effect magnitude attenuates from $-.76$ to $-.46$: holding distance to the polling place and other factors constant, the relocation of a polling place reduces in-person voting by .46 percentage points. Evaluated at the mean, this corresponds to a reduction by roughly 1.4 percent. The estimate for current average distance is negative and 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, which is equivalent to a one-percent decline at the mean. 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 in Column (3). In addition, the placebo estimates reported in Column (5) show that future polling place relocations do not affect current turnout, suggesting that serial correlation in reassignments does not meaningfully bias our results.

The impact on mail-in turnout in Panel B mirrors the effect on polling place voting. The estimates of the search effect and the transportation effect have positive signs; however, the full specification in Column (4) suggests that only the transportation effect is statistically significant. Increasing the distance to the polling place by 10 percent raises mail-in voting by 2.4 percentage points (equivalent to .83 at the mean). Thus, we find evidence for a substitution of in-person for mail-in votes following a polling place relocation. Yet, holding the 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. Therefore, only the transportation effect is statistically significant. Mail-in voters did not miss the

deadline for requesting mail-in ballots. If they have voted by mail in the prior elections, a reduction in distance to the new polling place might shift them to voting in person again. However, a prior in-person voter who noticed a change in the polling place several days before the election might reevaluate her potentially increased transportation costs to her benefits and decide to vote by mail.

In fact, the net effects of increasing search costs and transportation costs on overall participation are indeed sizable and statistically significant. According to the full specification in Column (4) of Panel C, relocating the polling place reduces polling place overall turnout by .38 percentage points (equivalent to .61 percent at the mean), all else equal. And increasing the proximity to the polling place by 10 percent depresses voter turnout by approximately .1 percentage points, which corresponds to a .16 percentage reduction at the mean. The estimates imply that, on average, the magnitude of the search cost effect on overall participation is equivalent to an increase in travel distance by 38 percent.

Overall, we find evidence for marked transportation and search effects in the short-run, consistent with theoretical predictions and previous research (Brady and McNulty, 2011; McNulty et al., 2009). To what extent did polling place reassignments affect turnout in our observation period? Hypothetically, relocations that result in closer proximity to the polling place could counterweight search shocks to the degree that overall turnout is not depressed. A simple counterfactual analysis suggests, however, that reassignment actually produced additional (positive) costs to in-person voting on average. Total participation would have been .17 percent higher in elections in which a nonzero share of the electorate experienced polling place changes (.78 for in-person voting).

5.2. Pretrends and Persistence of the Relocation Shock

The key assumption in our natural experiments asserts that polling place reassignments are quasi random conditional on precinct and election fixed effects. A central threat to validity occurs when there 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 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 estimation results of our event study specification for turnout at the polling place,

Table 1: Search and Transportation Costs—Baseline Specification

	(1)	(2)	(3)	(4)	(5)
Panel A: Turnout at the Polling Place					
Reassigned	-0.76***	-0.47***	-0.44***	-0.46***	-0.60***
	(0.14)	(0.13)	(0.11)	(0.11)	(0.14)
Log Street Distance		-3.43***	-3.48***	-3.44***	-3.60***
		(0.25)	(0.23)	(0.23)	(0.34)
Reassigned, $t-1$				-0.44***	-0.58***
				(0.12)	(0.13)
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	0.97	0.97	0.96
Panel B: Turnout via Mail (requested)					
Reassigned	0.26*	0.03	0.06	0.08	0.31**
	(0.15)	(0.14)	(0.12)	(0.12)	(0.15)
Log Street Distance		2.70***	2.59***	2.41***	2.59***
		(0.25)	(0.23)	(0.24)	(0.33)
Reassigned, $t-1$				0.36***	0.52***
				(0.13)	(0.15)
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	0.93	0.95	0.95	0.95
Panel C: Overall Turnout					
Reassigned	-0.51***	-0.45***	-0.38***	-0.38***	-0.29**
	(0.16)	(0.17)	(0.11)	(0.12)	(0.14)
Log Street Distance		-0.73**	-0.90***	-1.03***	-1.00***
		(0.30)	(0.21)	(0.20)	(0.25)
Reassigned, $t-1$				-0.08	-0.06
				(0.13)	(0.15)
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	0.98	0.99	0.99	0.99
Observations	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. Standard errors are clustered at the precinct level and reported in parentheses. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

via mail and overall. We regress turnout on election-date dummies relative to the event election and time-varying controls. 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 event election 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.

The estimates further show that the substitution of polling place for mail-in voting indeed carries over to the two subsequent elections. This is consistent with the theory predicting a persistent substitution effect resulting from a lasting alteration in the 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 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 completely abstain from voting in the event election. But with the awareness about the reassignment, these voters will revert to mail-in voting in ensuing elections. The estimates support this interpretation as total turnout recovers after the event and mail-in voting exhibits a slight upward trend in the subsequent elections.

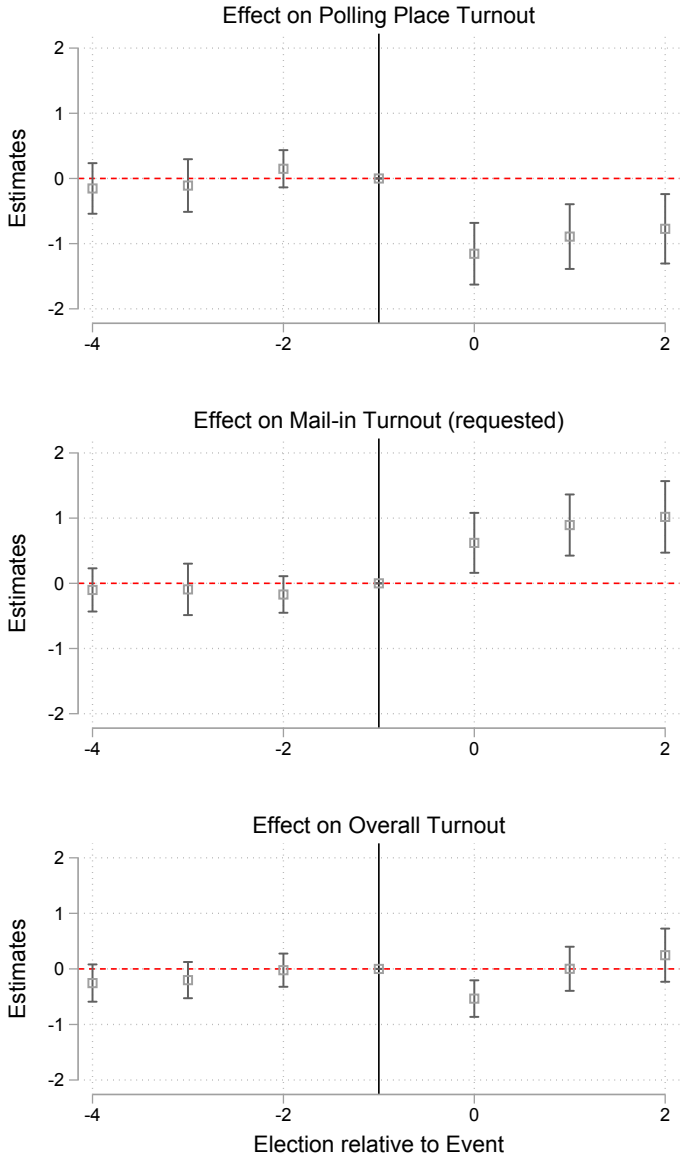
Finally, our results reject the hypothesis of habit formation in voting behavior. If (non)voting were actually habit forming, we would expect a lasting decline in turnout following the initial drop—even when the costs of voting were entirely restored to pre-event levels. Our estimates clearly do not 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 2. We first verify that our baseline estimates of the search and transportation effects 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 but not 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 Table B.2, we replicate the results, where the first treatment is defined as a reassignment to a new polling place for all residential addresses (Columns (2), (3), (5), and (6)), applying

the Callaway and Sant'Anna (2020) propensity score weighting method. The results stay robust to the alternative estimation approach.

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). Confidence intervals reported at the 95% level. The full results of the underlying regressions appear in Column (2) of Table 2.

Table 2: 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). Standard errors are clustered at the precinct level and reported in parentheses. $^{***}p < 0.01$,$^{**}p < 0.05$,$^{*}p < 0.1$.

5.3. Addressing identification challenges

Can our estimates be interpreted as causal? In this section, we present evidence indicating that polling place reassignments are plausibly exogenous. We corroborate the findings by performing a number of robustness checks against alternative assumptions.

Balance Test. One threat to the validity of our results is endogeneity of treatment due to confounding covariates. Although our precinct and election fixed effects partial out a large portion of relevant variation, time-varying confounders may still produce spurious correlation between turnout and our variables of interest. We can test if the likelihood of polling place reassignments is related to precinct characteristics by estimating the conditional correlation between treatment and our set of observable covariates. Table 3 reports the results. In Column (1), we regress the share of reassigned addresses on election fixed effects only. The R-squared suggests that the fixed effects already explain 24 percent of the variation in reassignments. This is not surprising as the scope of reassignments is specific to certain elections. In Column (2), we add one set of time-varying covariates as well as a precinct's surface areas (which is time-consistent as we use harmonized boundaries). Column (3) adds another set of time-varying variables. The coefficients on the precinct characteristics are at most marginally significant and the R-squared increases only slightly, suggesting that the additional covariates add little explanatory power to the model. Column (4) further includes precinct fixed effects, rendering all estimates on time-varying precinct characteristics insignificant. The R-squared increases to .39, suggesting that some of the variation in reassignments is indeed determined by unobservable, yet time-constant factors. In Columns (5) to (8), we repeat the exercise using the log of street distance as the dependent variable. With the inclusion of precinct fixed effects in Column (8), time-varying covariates appear at most barely statistically significant. The R-squared is .86, indicating that the average distance to the polling place is largely constant across elections. Overall, the results lend credibility to the assumption that potentially confounding precinct characteristics do not vary significantly over time or appear specific to elections, so that our fixed effects can account for most concerning influences.

Reason for Reassignment. Another 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 ones due to the recruitment of a different venue to varying degrees. Or that some part of the electorate is systematically more prone to experience one type of reassignment, casting doubt on the (quasi)randomness of treatment. We address the concern by re-estimating conditional correlations between precinct characteristics and reassignments separately for reassignments due to precinct-boundary reconfiguration and reas-

Table 3: Balance Test

	Share Reassigned				Log Street Distance			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log number of residents		0.09	0.07	0.09		0.31*	-0.03	-0.17
		(0.07)	(0.07)	(0.12)		(0.19)	(0.18)	(0.12)
Share native German residents		0.15	-0.17	-0.10		0.05	-0.07	0.12
		(0.12)	(0.26)	(0.38)		(0.32)	(0.50)	(0.29)
Share non-native German residents		0.29	-0.14	-0.12		-2.00***	-3.33***	-0.89*
		(0.21)	(0.34)	(0.62)		(0.56)	(0.66)	(0.54)
Share single residents		0.12	0.15	0.23		1.25**	-2.04***	1.16
		(0.19)	(0.34)	(0.83)		(0.58)	(0.76)	(0.80)
Share married residents		-0.32	-0.28	-0.49		4.48***	1.14	0.28
		(0.21)	(0.31)	(0.80)		(0.65)	(0.78)	(0.79)
Share eligible voters			0.24	0.20			0.06	-0.20
			(0.25)	(0.27)			(0.48)	(0.19)
Share EU-foreigners in the electorate			-0.13	-0.02			0.07	0.11
			(0.15)	(0.15)			(0.22)	(0.08)
Share electorate aged 18-24			-0.36	-0.49			2.31***	0.37
			(0.26)	(0.39)			(0.57)	(0.31)
Share electorate aged 25-34			-0.06	0.07			0.37	0.33
			(0.18)	(0.24)			(0.44)	(0.21)
Share electorate aged 35-44			-0.08	-0.15			-0.04	-0.27
			(0.25)	(0.32)			(0.49)	(0.25)
Share electorate aged 45-59			-0.17	0.28			-0.24	0.02
			(0.19)	(0.31)			(0.48)	(0.23)
Average duration of residence			0.00	-0.00			-0.01***	-0.00
			(0.00)	(0.00)			(0.00)	(0.00)
Share households w/ children			0.35	0.33			1.98***	0.83*
			(0.23)	(0.55)			(0.51)	(0.48)
Average quoted rent per sqm			0.00*	0.00			-0.00	0.00
			(0.00)	(0.00)			(0.00)	(0.00)
R^2	0.24	0.25	0.25	0.39	0.00	0.19	0.24	0.86
Observations	4,944	4,944	4,944	4,944	4,944	4,944	4,944	4,944
Election FE	×	×	×	×	×	×	×	×
Precinct FE				×				×

Notes: Dependent variables are the share of residential addresses reassigned to a new polling place (Columns 1–4) and the log of average street distance between residential addresses and the polling place (Columns 5–8). Standard errors are clustered at the precinct level and reported in parentheses. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

signments due to recruitment of a different polling place venue. The results reported in Appendix Table B.3 show no evidence that precinct characteristics are systematically related to the likelihood of reassignment due to either reason. We also re-estimate Equation (1) differentiating the search effect 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 disparately drive the search effect. The t-tests for equality of estimates (p-values reported in square brackets) indicate that the estimates are not statistically different from each other for outcomes (Panels A, B and C). This lends credibility to the presumption that voters do not anticipate or react differently to polling place reassignments depending on the source of the change.

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) of Table 4 presents the estimates with two-way cluster-robust standard errors. The magnitudes of the standard errors of our variables of interest only increase slightly. And the statistical significance of our estimates of interest remain unaffected.[8]

Accounting for Constituencies. As described earlier, city districts are directly contested in some elections. In state and federal elections, for instance, the 25 city 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 re-

[8]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.

ported in Column (4) of Table 4 show that our estimates of interest and their statistical significance are not particularly affected.

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 continuous shift towards more postal 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) of Table 4 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 B.4. In our baseline, we use the logarithmic street distance (walking distance) between voters and their polling station (replicated in Column 1). Column (2) uses the simple street distance and Column (3) includes the simple 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 similar. This indicates that the effect is not driven by precincts with a high average distance as for very high distances a doubled distance is less than one kilometer. The quadratic distance in Column (3) shows that an additional meter reduces the effect size. Therefore, the estimator of the linear street distance overestimates the effect. One should note, that the quadratic term is not significant in all panels. However, for the polling place vote it is strongly statistically significant, where we also think of the strongest impact of the distance. In Columns (4) to (6), we perform the same exercise but replace the street distance with the average linear (euclidean) distance between the residential addresses and the polling station. In all but the first specification the estimates increase slightly as the linear 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.

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. 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). $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

6. Discussion

6.1. Comparison with previous research

Previous estimates of search and transportation costs for voting at the polling place or via mail-in serve for comparison with our results. For the combined effect of search and transportation costs, we find that reassignments depress turnout at the polling station by .76 to 1.16 percentage points and overall by approximately half a percentage point. By comparison, Brady and McNulty (2011) estimate a decrease of three percentage points at the polling station and one and a half percentage points in overall turnout. McNulty et al. (2009) estimate the highest decrease with seven percentage points. However, they do not investigate mail-in voting as there are only very few casts for mail-in voting. Not having the possibility to substitute to mail-in voting might be the first explanation for their very high estimate. Another important difference in McNulty et al. (2009) is that they investigate a school budget referendum. We argue that the benefits of voting are smaller than when voting in a federal or state election. Therefore, an increase in voting costs might have stronger effects.

Similar to us, Brady and McNulty (2011) find that a drop in polling place turnout is partially offset by an increase in mail-in turnout. However, they are not able to investigate subsequent elections in this regard. Their slightly higher estimates, in comparison to ours, can partly be explained by the design of the natural experiment: In our setting, accessibility is still an administration's aim. Therefore, the number of polling places is fairly constant over time and for about half of the treated eligible voters the new polling place is even closer. In contrast, as two-thirds of the original polling places were closed to reduce administrative costs, in Brady and McNulty (2011), for almost all treated voters their new polling place is further away. Hence, in our setting many voters do not have to go to an unfamiliar neighborhood and a closer located polling place might even increase their turnout.[9] at the polling station, offset by an increase in mail-in turnout of one and half a percentage points, results in an overall effect of one percentage point. Results are upon request.

Next, we discuss the importance of the search effect and the transportation effect. We estimate that a 10 percent greater distance to a polling place decreases turnout by .1 percentage points overall and by .3 percentage points at the polling place.[10] Our estimates suggest that a polling place would

[9] Investigating a sub-sample where the distance increases on average, we find higher estimates which are very similar to Brady and McNulty (2011): An effect of two and half a percentage points

[10] Assuming a linear effect, an additional kilometer (mile) in street distance has a negative effect of 1.11 (1.79) overall and 4.38 (7.05) at the polling place and in linear distance the negative effects are little higher (1.66 (2.66) and 5.39 (8.68) respectively). However, the effect is non-linear and diminishes with increasing distance.

need to move 38 percent (.35 kilometers (.22 miles) in street distance or .23 kilometers (.15 miles) in linear distance) closer to the voter to offset the negative impact of the search effect. Brady and McNulty (2011) estimate a hypothetical shift in distance of about a mile, while McNulty et al. (2009) find a distance of about 3.5 miles. These values might differ simply because they are relative to different average distances and do not account for the non-linearity in the transportation costs. However, the distinct settings should be accounted for again. In our setting, search costs might be lower since the number of polling places is fairly constant over time. Therefore, there are less distance increases in comparison to the other settings. Hence, a voter rarely needs to go to an unfamiliar neighborhood. Turning again to the subsample where the distance increases on average, we also find a higher importance of the search effect in comparison to the transportation effect. We argue that voters finding the new polling place to be located closer to them as before have very little search costs as the new polling place is located in the neighborhood they know very well. Additionally, with the decrease in marginal transportation costs, a decrease in distance weights strongly in their decision making. In contrast, voters finding the new polling place to be located further away from them as before have high search costs as in McNulty et al. (2009) and Brady and McNulty (2011) since the new polling place might be located in a neighborhood, they do not know that well. Additionally, for them an additional distance is less important following again the argumentation of the non-linearity in the transportation costs.

6.2. Policy implications

In Munich, the officials' goal was to have the highest possible accessibility of polling places for all eligible voters. However, we showed that reassigning polling places, even for the purpose of improving accessibility, reduced voter turnout. Therefore, our first policy implication is that bureaucratic agencies should consider reducing the number of changes in polling place assignment to avoid imposing unintended costs on voters at the cost of a small reduction in accessibility. Accessibility could instead be increased by increasing the number of voting booths and poll workers at polling places.

In cases where the old polling place is inaccessible due to construction, for example, we hypothesize that the cost of the shock of relocation (search costs) could be reduced by informing voters more clearly, earlier, and visibly about the change. If voters note before Election Day that their polling place had changed, they might substitute to mail-in voting directly, rather than in the subsequent election, given the costs of in-person voting are higher than the benefits of voting. Another way to increase turnout among this group could be to facilitate mail-in voting even further. For example, voting from home could still be allowed on Election Day, e.g, via a secure Internet con-

nection instead of mail-in voting, which is no longer possible on Election Day. We cannot analyze the exact mechanism of why people do not go to the newly assigned polling place and vote by mail in the subsequent election. Therefore, we can only speculate whether the search cost to find out where the new polling place is located and how to get there is a barrier. If this was the case, authorities could include a map or a description of the new polling place with the election notification.

7. Conclusion

Exploiting a natural experiment, the unexpected reassignment of polling places to citizens by a seemingly innocuous bureaucratic procedure, we found, holding the proximity to the polling place constant, that relocating the polling place depresses voter turnout by approximately half a percentage point. This impact on turnout can be explained by two distinct mechanisms: a "search effect" and a "transportation effect". The search cost arises from the cost of finding the new polling place, while the transportation cost results from a change in proximity to the polling place. Our estimates suggest that a polling place would need to move about 38 percent closer to the voter to offset the negative impact of the search effect. We contributed by providing the first causal estimate in a proportional representation system on the cost of voting, building on Brady and McNulty (2011) and McNulty et al. (2009), who have previously studied the two mechanisms.

Voters in Munich can apply for mail-in voting quite easily. Therefore, it is an important substitution channel for voters who experienced greatly increased cost of voting at a polling station. We showed that the decline in polling place turnout is partially compensated by an increase in mail-in votes. Especially in the subsequent elections after the relocation, we found a strong increase of mail-in votes.

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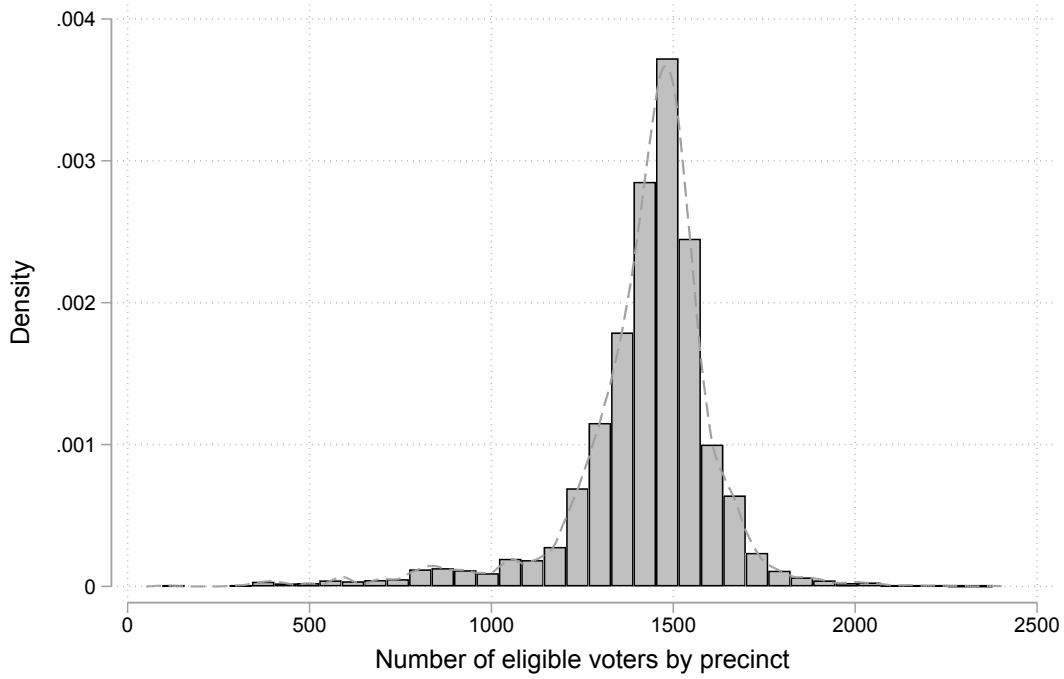
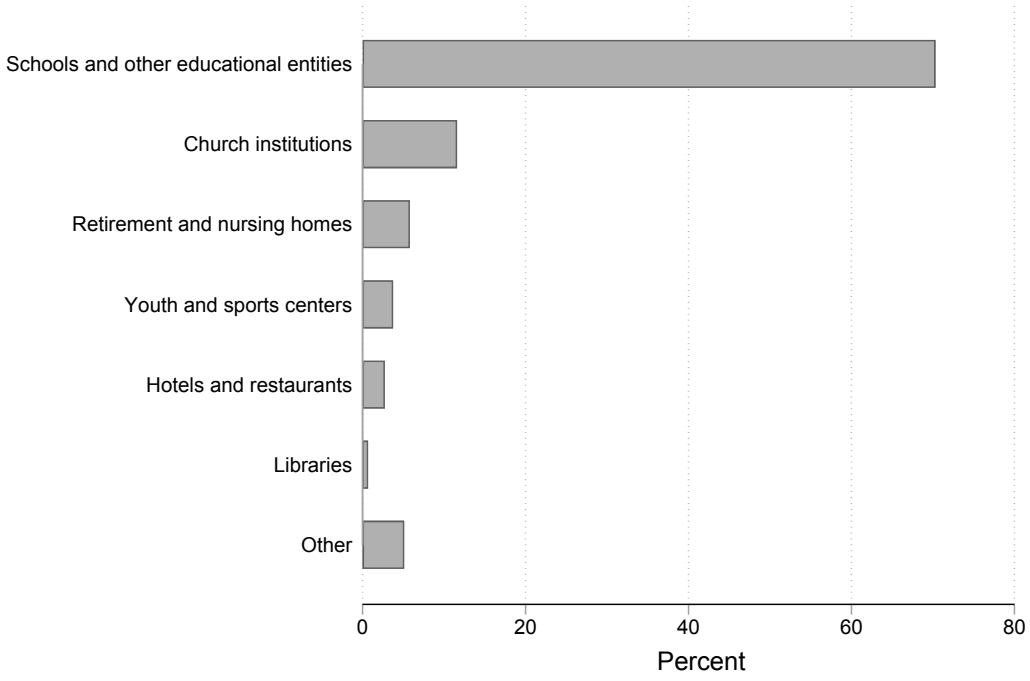
Appendix A. Figures

Figure A.1: Distribution of Precinct Sizes



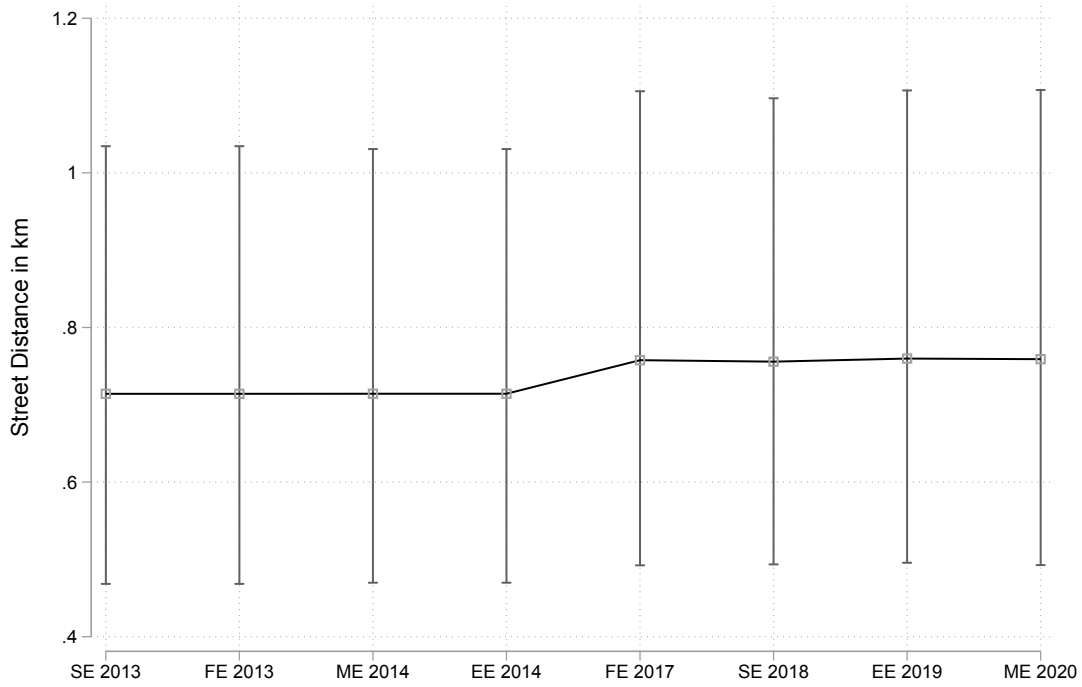
Notes: The figure plots the density precinct sizes (number of eligible voters) over all elections. Precincts are delineated according to their election-specific boundaries (not harmonized).

Figure A.2: Types of Polling Place Venues



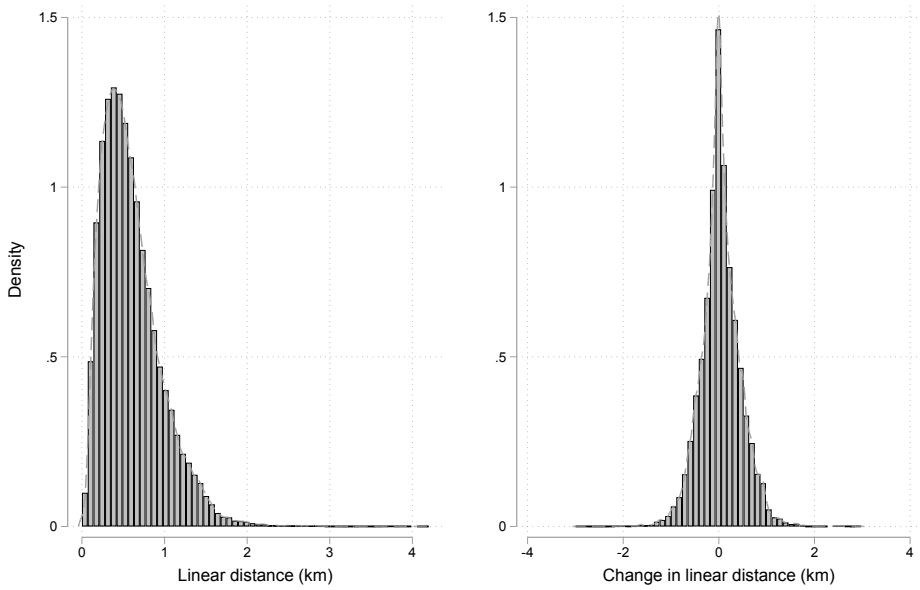
Notes: This Figure depicts the frequency of types of polling stations venues over the eight elections held in Munich between 2013 and 2020 (293 distinct venues in total).

Figure A.3: Median and Interquartile Range of Distance to the Polling Place



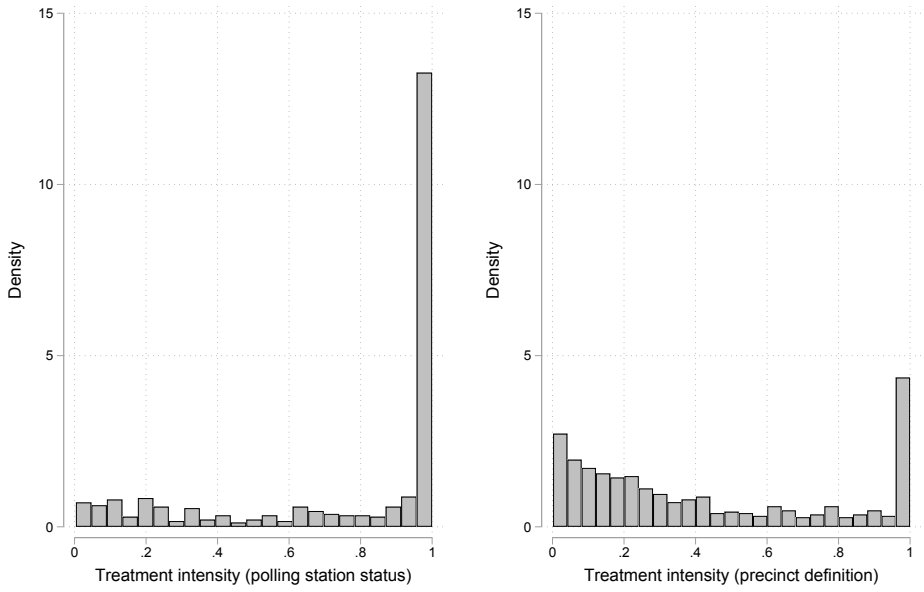
Notes: The figure plots the median and interquartile range (75th and 25th percentile) of the street distance between residential addresses of eligible voters and their designated polling place in each election between 2013 and 2020. SE = State Election, FE = Federal Election, ME = Municipal Election,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

Figure A.4: Density of Linear Distance and Distance Change to Polling Station



Notes: xxx

Figure A.5: Density of Treatment Intensity at the Precinct Level



Notes: The treatment intensity for each precinct is shown by the type by which treatment is induced. On the left the activity status of the polling stations has changed, while on the right the precinct was redefined.

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 precincts characteristics							
Number of residents	2,428	403	758	2,169	2,325	2,591	6,272
Number of eligible voters	1,560	172	495	1,468	1,517	1,614	2,821
% 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 B.2: Robustness to the event study estimates

	(1)	(2)	(3)	(4)
Panel A: Turnout at the Polling Place				
Period -3	−0.05	−0.05	−0.04	0.06
	(0.13)	(0.12)	(0.13)	(0.12)
Period -2	0.26	0.26	0.26	0.33
	(0.17)	(0.18)	(0.17)	(0.19)
Period -1	−0.01	0.07	−0.02	−0.13
	(0.16)	(0.15)	(0.15)	(0.18)
Period 0	−1.37***	−0.93***	−1.39***	−1.57***
	(0.26)	(0.24)	(0.26)	(0.31)
Period 1	−1.44***	−1.15***	−1.19***	−1.54***
	(0.31)	(0.26)	(0.23)	(0.32)
Period 2	−1.11***	−0.78*	−0.86***	−1.11***
	(0.34)	(0.31)	(0.24)	(0.36)
Panel B: Turnout Postal (requested)				
Period -3	0.25	0.27*	0.23	0.11
	(0.12)	(0.12)	(0.12)	(0.11)
Period -2	−0.36	−0.41	−0.36	−0.42
	(0.19)	(0.19)	(0.20)	(0.23)
Period -1	0.16	0.16	0.16	0.21
	(0.13)	(0.14)	(0.14)	(0.16)
Period 0	0.74**	0.16	0.77**	1.06***
	(0.24)	(0.25)	(0.26)	(0.28)
Period 1	0.71	0.28	0.86***	0.87*
	(0.33)	(0.32)	(0.25)	(0.35)
Period 2	1.10*	0.52	0.93***	1.12*
	(0.46)	(0.48)	(0.26)	(0.46)
Panel C: Overall Turnout				
Period -3	0.19	0.23	0.19	0.18
	(0.14)	(0.14)	(0.14)	(0.13)
Period -2	−0.10	−0.15	−0.10	−0.09
	(0.21)	(0.20)	(0.22)	(0.23)
Period -1	0.15	0.23	0.14	0.08
	(0.21)	(0.20)	(0.21)	(0.24)
Period 0	−0.63**	−0.77**	−0.62**	−0.51
	(0.20)	(0.27)	(0.21)	(0.25)
Period 1	−0.73	−0.87	−0.33	−0.67
	(0.42)	(0.40)	(0.28)	(0.39)
Period 2	−0.02	−0.26	0.07	0.01
	(0.49)	(0.58)	(0.31)	(0.56)
Event: 100% reassigned	×	×	×	×
Full sample			×	
Balanced panel				×

*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*). Precinct-level controls are not added, besides when in Column (2) we control for log street distance. Standard errors are bootstrapped and reported in parentheses. $^{***}p < 0.01$,$^{**}p < 0.05$,$^{*}p < 0.1$.

Table B.3: Balance Test by Reason of Reassignment

	Reassignment (Boundaries)				Reassignment (Recruitment)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log number of residents		-0.00	-0.02	-0.04		0.09	0.10	0.13
		(0.04)	(0.04)	(0.07)		(0.06)	(0.06)	(0.12)
Share native German residents		0.07	-0.30	-0.43*		0.09	0.13	0.33
		(0.07)	(0.20)	(0.25)		(0.09)	(0.21)	(0.31)
Share non-native German residents		0.16	-0.21	-0.42		0.14	0.07	0.30
		(0.13)	(0.26)	(0.47)		(0.17)	(0.28)	(0.52)
Share single residents		-0.17	0.26	0.41		0.29*	-0.11	-0.17
		(0.13)	(0.28)	(0.81)		(0.15)	(0.26)	(0.63)
Share married residents		-0.29**	-0.13	-0.36		-0.02	-0.15	-0.13
		(0.15)	(0.24)	(0.75)		(0.17)	(0.24)	(0.64)
Share eligible voters			0.25	0.09			-0.01	0.11
			(0.19)	(0.20)			(0.22)	(0.24)
Share EU-foreigners in the electorate			-0.15	-0.10			0.02	0.08
			(0.12)	(0.10)			(0.13)	(0.14)
Share electorate aged 18-24			-0.37*	-0.16			0.01	-0.32
			(0.19)	(0.28)			(0.24)	(0.34)
Share electorate aged 25-34			-0.40***	-0.22			0.35**	0.29
			(0.12)	(0.17)			(0.16)	(0.21)
Share electorate aged 35-44			0.01	-0.04			-0.09	-0.11
			(0.17)	(0.23)			(0.20)	(0.26)
Share electorate aged 45-59			-0.16	0.08			-0.01	0.20
			(0.13)	(0.22)			(0.16)	(0.24)
Average duration of residence			0.00	0.00			-0.00	-0.00
			(0.00)	(0.00)			(0.00)	(0.00)
Share households w/ children			0.16	0.40			0.20	-0.07
			(0.16)	(0.27)			(0.19)	(0.52)
Average quoted rent per sqm			-0.00	-0.00			0.00**	0.00
			(0.00)	(0.00)			(0.00)	(0.00)
R^2	0.14	0.14	0.14	0.30	0.12	0.13	0.13	0.28
Observations	4,944	4,944	4,944	4,944	4,944	4,944	4,944	4,944
Election FE	×	×	×	×	×	×	×	×
Precinct FE				×				×

Notes: Dependent variables are the share of residential addresses reassigned as a result of adjustments to precinct boundaries (Columns 1–4) and the share of residential addresses reassigned to the recruitment of a different polling place (Columns 5–8). Standard errors are clustered at the precinct level and reported in parentheses. $^{***}p < 0.01$,$^{**}p < 0.05$,$^{*}p < 0.1$.

Table B.4: Robustness 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)
R2	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)
R2	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)
R2	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*). Precinct-level 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. Standard errors are clustered at the precinct level and reported in parentheses.