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No Surprises, Please: Voting Costs and Electoral Turnout

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No Surprises, Please: Voting Costs and Electoral Turnout

Abstract

We study how shocks to voting costs affect electoral turnout. Individuals whose polling place is relocated face changes to their cost of voting in person due to altered distance and unfamiliarity with the new polling place. Using address-level and precinct-level data, we find that polling place relocations depress turnout by 0.5–0.6 percentage points (p.p.): in-person turnout declines by 0.8–1.1 p.p. and is only partly compensated by a 0.3–0.5 p.p. increase in mail-in voting. However, the drop in turnout is only transitory as mail-in votes balance the decline in in-person votes in subsequent elections. This finding is consistent with inattentiveness to relocations, causing individuals to miss the deadline for requesting mail-in ballots. Some inattentive voters forgo voting today but turn to mail-in voting in ensuing elections. Our results are in line with rational choice models of voting and incompatible with the hypothesis that voting is habit forming.

JEL-Codes: D720, D730, D830, R410.

Keywords: voter turnout, habit formation, elections, election administration, precincts, polling places.

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1. Introduction

Classical theories of electoral participation emphasize that small voting costs can dissuade rational voters from exercising their democratic franchise (“paradox of voting”). Even as pivotal voting models have been expanded to include intrinsic and strategic motivations for turning out, the notion that voting is costly remains a prominent feature, highlighting the importance of electoral practices for voter participation (Riker and Ordeshook, 1968; Feddersen, 2004; Ali and Lin, 2013). In most Western democracies, where voter registration is automatic and mail-in voting unavailable, provisions governing in-person voting at the polling place are therefore central for electoral considerations. Yet, while observational research provides ample evidence that polling place accessibility (e.g., in terms of proximity to the polling location or their number) constitutes an important factor for turnout, very few studies establish causality (Cantoni, 2020; Brady and McNulty, 2011).

In this paper, we investigate the effect of a seemingly innocuous practice on voter turnout: the relocation of polling places. We provide causal evidence that polling place reassignments induce a persistent shift from in-person to mail-in voting and temporarily depress overall turnout. We analyze a natural experiment in Munich, the third-largest city in Germany, where election administrators aim to “facilitate [voting] as much as possible” (Federal Election Code, Section 12:2). Maintaining this goal typically involves avoiding congestion at polling places and ensuring that they are accessible on Election Day. In practice, the measures include adjusting boundaries of voting precincts and recruiting new polling places when venues become unavailable. A by-product of these efforts is that some eligible voters are assigned to vote at a different polling location than in previous elections. We provide empirical evidence that these reassignments occur “as-good-as-randomly” in the eight elections held between 2013 and 2020. Specifically, we show that turnout (in person, by mail, and overall) is unrelated to reassignments in future elections and that sociodemographic differences between treated and untreated precincts are negligible.

Using a static difference-in-differences (DiD) design which relates contemporaneous turnout to reassignments in the same election at the precinct level, we find that reassignments reduce turnout at the polling place by 0.8 percentage points (p.p.), on average. This effect can be decomposed into two separate mechanisms, *i*) a “transportation effect” and *ii*) a “search effect” (Brady and McNulty, 2011). The transportation effect captures the change in travel time resulting from the change in proximity to the polling location. The search effect captures all additional costs resulting from reassignments when holding proximity constant, including the cost of searching for the new polling place and going to an unfamiliar location. Our estimates imply that 60 percent of the decline in in-person turnout is driven by the search effect. Furthermore, we find that the drop in in-person

turnout is only partly compensated by a 0.3 p.p. increase in mail-in voting, resulting in a decline in
35 total turnout by 0.5 p.p.—or 0.8 percent, evaluated at the mean. Doubling the distance to a polling
place reduces turnout by 1.0 p.p., on average. The results are insensitive to including lag terms
of reassignment and distance to the polling location to account for potential serial correlation in
reassignments and do not yield different results when distinguishing between reassignments due
to changes in polling venues and due to adjusted precinct boundaries.

40 A key novelty of our study is the evaluation of *persistent* effects of relocation shocks. Since
reassignments typically produce lasting changes to voting costs (e.g., due to greater distance),
persistence may reflect a rational adjustment of voting behavior. Another driver of persistence may
be habit formation, in the sense that today’s act of voting (or abstaining) increases its consumption
value and therefore the likelihood of voting (or abstaining) in the future (Fujiwara et al., 2016).¹

45 Specifically, if nonvoting as a consequence of reassignment is internalized into a new habit, this
will cause a lasting decline in turnout. On the other hand, eligible voters in Germany are not
explicitly notified of *changes* to their assigned polling place. This introduces the possibility that
inattentive voters are surprised by reassignments *after* the deadline for requesting mail-in ballots
has passed. Some inattentive voters who would have switched from in-person to mail-in voting
50 will subsequently abstain from voting in the current election and only turn to mail-in voting in
the subsequent election. Consequently, inattentiveness will *temporarily* amplify the shift from
in-person to nonvoting captured in the search effect.

To examine the persistence of the effects, we conduct an event study focusing on voting behavior
around the first time a precinct is treated in our panel. We find no evidence of differential trends
55 preceding the treatment, bolstering our confidence that the parallel trend assumption holds and that
election officials do not take into consideration historical voting patterns when modifying precinct
boundaries. We find that reassignments lead to a significant drop of 0.6 p.p. in overall turnout in
the treatment year; however, mail-in votes completely offset the decline in polling place votes in
the subsequent election. This pattern is consistent with inattentiveness, causing some individuals to
60 temporarily abstain from voting before turning to mail-in voting in the ensuing election. Moreover,
the results are at odds with the hypothesis that (non)voting is habit forming as the decline in overall
turnout is only transitory. Instead, the persistent substitution of in-person for mail-in voting is
consistent with rational choice models of electoral turnout.

¹We follow the existing literature and study habit formation in voting only regarding the decision whether or not
to vote, abstracting from the mode of voting (i.e., in person or by mail).

65 Unlike contentious practices such as gerrymandering and instances of reprecincting in the US, the
reassignment of polling places in Munich is a routine and transparent process. Election administra-
tors are nonpartisan civil servants and have no incentives to tamper with the electoral process. We
show that the Elections Office has indeed maintained precinct size and distance to polling places
fairly constant over time and that reassignments do not systematically skew towards greater or
70 closer proximity to the polls. However, we find that the effects of reassignments have an unequal
impact on the propensity to participate in the election among different electoral groups. Specif-
ically, heterogeneity analyses reveal that the effects of reassignment are strongly attenuated in
precincts with a larger share of first-time voters. While the decline in turnout at the polling place
is similar, precincts with a higher proportion of elderly eligible voters are more likely to abstain
rather than switch to mail-in voting. The opposite is true in more affluent precincts (measured
75 by the average quoted rent), where individuals are more likely to switch to mail-in voting rather
than abstain when affected by reassignment. We find no statistical differences regarding the effect
of reassignments on overall turnout between historically conservative-leaning precincts and left-
leaning precincts. This finding is consistent with results on the impact of reassignments on party
outcomes, suggesting declines in party turnout across almost all parties and negligible effects on
80 party shares.

Our article contributes to two branches of literature. First, we contribute to the empirical literature
assessing the role of institutional barriers to democratic participation.² In particular, we build on
three closely related articles that study the effects of polling place reassignments and distance to
the polling location on turnout in the US. [Brady and McNulty \(2011\)](#) exploit the consolidation
85 of voting precincts in the 2003 Los Angeles gubernatorial recall election, which resulted in a *re-*
duction in the number of polling places. To account for nonrandom reassignment of individuals
to polling locations, the authors employ statistical matching of registered voters in consolidated
and unconsolidated precincts.³ They find a lower polling place turnout among reassigned voters,
which is only partially offset by higher mail-in voting. [Clinton et al. \(2021\)](#) use individual-level
90 data to analyze the impact of polling place reassignments in the 2012 and 2016 presidential elec-

²For instance, previous studies have examined the role of 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](#)).

³The prevalence of nonrandom polling place reassignments in the US is also emphasized by [Amos et al. \(2017\)](#), who argues that reprecincting is rarely a purely bureaucratic matter but prone to political influence. Against this backdrop, the authors find that the reduction of polling places for the 2014 General Election in Manatee County (FL) disproportionately affected minorities, younger voters, and Democrats, and that turnout was significantly lower among reassigned voters.

tions in North Carolina. Using a static DiD approach, the authors find evidence for an increase in turnout via early voting that completely compensates the decline in polling place turnout. The authors suggest that the absence of a negative effect on total turnout is likely due to the fact that affected individuals were informed of the polling place change by mail. Both studies find that
95 increased search costs and transportation costs jointly drive the effects. While causal identification in [Brady and McNulty \(2011\)](#) rests on the assumption that matching on observables makes voters with new and unchanged polling locations comparable in all relevant characteristics, [Clinton et al. \(2021\)](#) rely on individual and year fixed effects to assure comparability. Similarly, our identification strategy hinges on the validity of the parallel trend assumption, conditional on precinct and
100 election fixed effects. Unlike [Clinton et al. \(2021\)](#), we can bolster this assumption by exploiting the length of our panel to test for the presence of pretrends. Moreover, our event study takes into account treatment timing and explicitly accounts for “forbidden comparisons” of newly treated with already treated units ([Borusyak et al., 2021](#)). Finally, our setting also allows us to examine the persistence of the treatment effects over subsequent elections, an aspect that is lacking in the
105 existing literature.

Our study also relates to [Cantoni \(2020\)](#), who focuses on the effect of distance to the polling location on electoral participation by exploiting geographic discontinuities at precinct borders in the US. [Cantoni](#) argues that citizens living in close proximity on opposite sides of precinct borders are identical on average, except for their assigned polling place. Comparing parcels of land and
110 census blocks located near adjacent precincts, the study finds that a greater distance to the polling location significantly reduces the total number of votes.⁴ A key difference with our setting is that identification stems from cross-sectional variation. Instead, we estimate the effect of distance using *changes* in the distance to the polling location within voting precincts. Studying reassignments allows us to shed light on several other potential determinants of turnout besides distance to the
115 polling location, including inattentiveness, search costs, and habit formation. Finally, we are the first to investigate the impact of reassignments in a context outside the US; in particular, in a system with proportional representation achieved mainly by party list votes.

Secondly, we contribute to the empirical literature on habit formation in voting. Habitual voting implies that the act of voting itself increases its consumption value and thus the likelihood of

⁴[Cantoni](#)’s results are consistent with observational research ([Haspel and Knotts, 2005](#); [Fauvelle-Aymar and François, 2018](#); [Gibson et al., 2013](#); [Bhatti, 2012](#); [McNulty et al., 2009](#); [Dyck and Gimpel, 2005](#); [Gimpel and Schuknecht, 2003](#)). However, these studies do not account for potential endogeneity, leaving room for biased estimates due to unobserved confounders or selection problems.

120 voting in the future. Scholars have long been aware that differences in turnout tend to persist
over time (see e.g., [Plutzer, 2002](#); [Green and Shachar, 2000](#); [Brody and Sniderman, 1977](#)) but
causal evidence for habit formation remains ambiguous.⁵ [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. They propose election-day rainfall as a transitory
125 and unexpected shock to voting costs and show that the decrease in turnout induced by rainfall also
reduces turnout in subsequent US presidential elections. In our setting, while the relocation of a
polling place is plausibly unexpected, it is typically not transitory and thus correlated with future
voting costs (e.g., if individuals are permanently assigned to a new polling place that is located
further away). Thus, distinguishing whether persistently altered turnout reflects habit formation or
130 a lasting shift in voting costs may be impossible. However, we are able to test the key implication
of habit formation. Specifically, if (non)voting is habit forming, then a change in turnout due to
the relocation shock must carry over to subsequent elections. We show that the hypothesis of habit
formation in the decision to vote is not supported as (inattentive) voters who abstain from voting
when subject to reassignment return to voting in the ensuing elections, thus recovering the drop in
135 aggregate turnout.

The next section describes the institutional setting. Section 3 outlines the conceptual framework
guiding our empirical analysis. Section 4 describes how we build our estimation panel and outlines
our empirical strategies. We present our main results in Section 5. In Section 6, we discuss poten-
tial threats to the validity of our estimates and present several robustness checks. Section 7 analyzes
140 heterogeneous effects across precinct characteristics and explores potential partisan consequences
of reassignments. Section 8 relates our findings to results of previous studies and discusses policy
implications. Section 9 concludes.

⁵[Meredith \(2009\)](#) demonstrates that voters who had just turned eighteen at the time of the 2000 US general elec-
tion (and thus had just become eligible to vote) are also more likely to cast their ballot in the subsequent election than
their peers who fell just short of the age threshold. [Gerber et al. \(2003\)](#) provide evidence from a field experiment,
suggesting that get-out-the-vote campaigns increase turnout in subsequent elections. By contrast, compulsory voting
in Switzerland and Austria showed no persistent effects on turnout after its abolition ([Bechtel et al., 2018](#); [Gaebler
et al., 2020](#)). Similarly, [Potrafke and Roesel \(2020\)](#) find that longer opening hours of polling places increased contem-
poraneous voter participation but did not affect turnout in subsequent elections when opening hours were no longer
prolonged.

2. Institutional Background: Elections and Polling Place Reassignments

2.1. Elections in Munich

145 Our panel covers the eight elections held in Munich between 2013 and 2020. These include elec-
tions to the four legislative bodies that reflect the federal system in Germany: the *Bundestag* (fed-
eral parliament), which constitutes the main body of the central government, the Bavarian *Landtag*
(state parliament), the *Stadtrat* (Munich city council), which governs the city alongside the mayor,
and the European Parliament, which effectively exercises some of the power of the federal gov-
150 ernment since Germany is a member of the European Union. All elections follow the principles of
proportional representation but differ with respect to the electoral rules. In [Appendix C](#), we briefly
describe the key features of the different electoral processes.

In all elections, eligible voters 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
155 than 21 days before the election. The letter contains information about the election, including the
date, the location and opening hours of the polling place, and whether it offers barrier-free access
for the disabled or the elderly. There is no explicit information about any *changes* to the polling
location—neither in the election documents nor in any separate notification. This contrasts with the
US, where changes to precinct borders typically trigger the requirement to notify affected voters
160 ([Cantoni, 2020](#); [Clinton et al., 2021](#)). Eligible voters may cast a ballot in person at their assigned
polling place on Election Day or vote by mail, in which case a polling card (*Wahlschein*) must be
requested no later than two days before the election.⁶

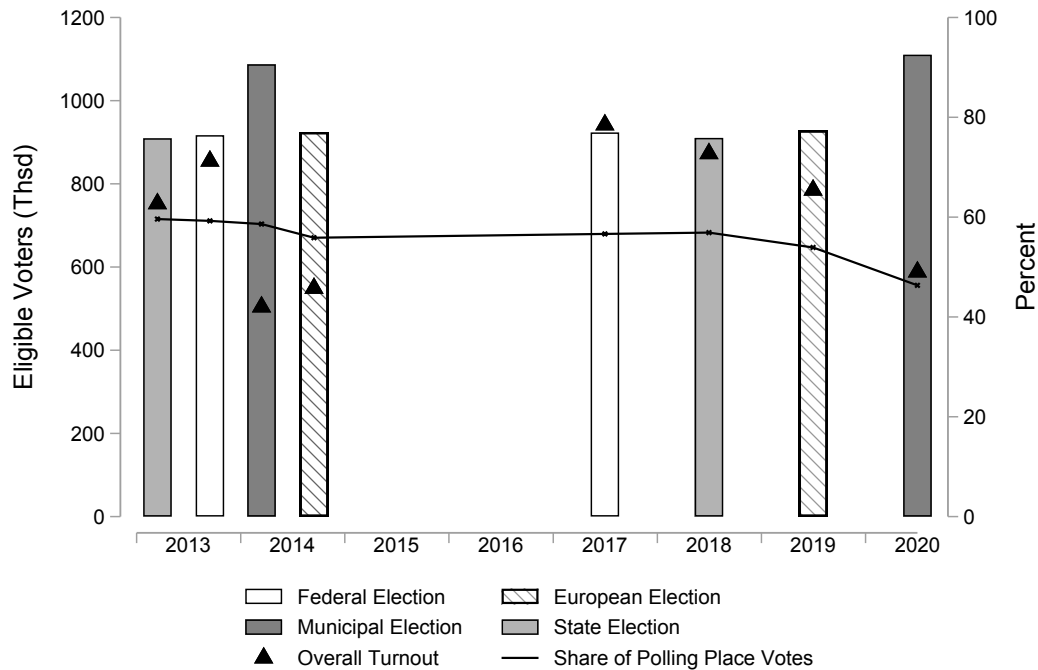
Figure 1 illustrates the timeline of the eight elections included in our panel. Two elections were
held in both 2013 and 2014 (but not on the same day), and one election took place every year
165 between 2017 and 2020. The vertical bars illustrate the number of eligible voters on the electoral
roll (left axis). The triangles and the solid line show total turnout and the share of votes cast at the
designated polling place in every election, respectively (right axis). The number of eligible voters
is distinctively higher in municipal elections, in which EU foreigners with residence in Munich are
also entitled to vote.⁷ Total turnout tends to increase over time when comparing the same election

⁶In Germany, mail-in voting is available to all eligible voters and does not require separate photo identification. In principle, the polling card also entitles to vote at another polling place in the city (e.g., if the original polling place does not provide barrier-free access), but typically more than 98 percent of ballots cast using polling cards are mail-in votes. And more than 90 percent of voters requesting a polling card actually end up casting a vote.

⁷For instance, in the 2020 Municipal Elections, 17.5 percent of eligible voters were foreign EU citizens. Foreign EU citizens who wish to vote in European elections in Munich instead of their country of origin must lodge a registration request.

170 type, while the share of in-person votes ranges between 50 and 60 percent of all ballots and shows a slight decline over time.⁸

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



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

2.2. Polling Place Reassignments

175 Elections are organized and administered according to a strict legal framework by the Munich Elections Office (*Wahlamt*). Employees of the Elections Office are nonpartisan civil servants and have no direct incentives to manipulate the electoral process. In every election, the electorate is geographically partitioned into more than 600 voting precincts based on eligible voters' registered residential addresses.⁹ Precincts constitute the smallest administrative unit in elections and serve

⁸With more than half of all votes cast by mail, the 2020 Municipal Election held during the Covid-19 pandemic marks an exception.

⁹Citizens are required by law to notify the relevant registration office of the city within two weeks of moving into a new residence. This also applies to citizens who move within a municipality.

to enable a manageable election process and to facilitate the exercise of citizens' franchise, e.g., by preventing congestion at polling places. A typical electoral map is illustrated in Figure 2. The black boundaries delineate the 618 precincts, and the blue lines delineate the 25 city districts, which have constant borders over time.¹⁰ In every election, a polling place, depicted by a black star, is assigned to each precinct; however, it is not uncommon that a single venue, typically a school, accommodates several polling stations for neighboring precincts (three on average). The straight gray lines illustrate the assignment of the residential addresses of eligible voters to their polling place.

Reconfiguration of Precinct Boundaries. One source of variation in the assignment of voters to polling places comes from adjustments to precinct boundaries. The law requires that voting precincts be drawn according to local conditions in a manner that “participation in the election is facilitated as much as possible for all eligible voters” (Federal Election Code, Section 12:2).¹¹ Achieving this objective has been primarily understood in terms of maintaining precinct sizes at an average of 1,500 eligible voters, monitoring the proximity to the polling locations, and (in more recent years) recruiting polling venues with barrier-free access for individuals with special needs. In every election year, the Elections Office evaluates whether changes to the number of precincts or to precinct boundaries are required to maintain these goals (e.g., due to a different number of eligible voters, population growth, or new housing units).¹² As a result of these adjustments, some eligible voters are reassigned to a different (or new) precinct with a different polling place. Overall, the total number of precincts remained at 702 in 2013 and 2014 before declining to 617 in 2017, due to the introduction of a new urban planning technology, which allows for a more granular spatial monitoring of the electorate and thus for a more precise delineation of precincts. This resulted in a comprehensive redivision of the city and a significant reduction in the variance of precinct sizes (see Appendix Figure A.1).¹³ The number of precincts remained at 618 in 2018 and 2019 and increased again to 755 in 2020 to accommodate a larger number of eligible voters during municipal elections and to account for social distancing provisions during the Covid-19 pandemic.

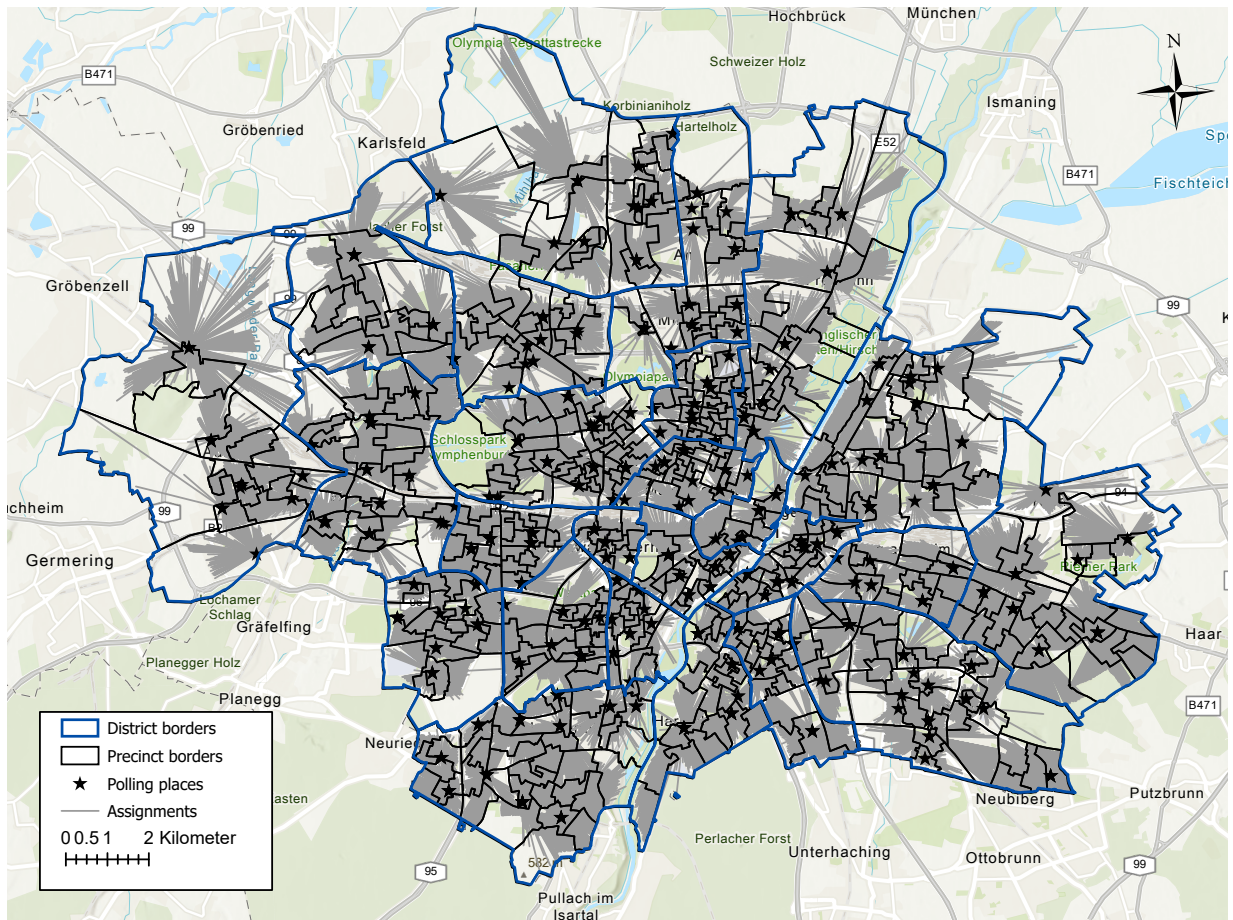
¹⁰Unlike precincts, districts are directly contested in some elections; for instance, adjacent districts cluster into four single-member constituencies in federal elections. In municipal elections, voters elect a local district committee (*Bezirksausschuss*).

¹¹The legal requirements are outlined in the federal, state, and European election codes, including LWO §10, BWO §12, EUW §12, GLKrWO §13.

¹²The law specifies that a precinct may not accommodate more than 2,500 eligible voters in any election. See Appendix Figure A.1 for a density plot of precinct size across all elections.

¹³Before 2017, the Elections Office addressed changes in precinct size mainly by adjusting the number of poll workers at the polling locations.

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 connect the residential addresses of eligible voters in the 2018 State Election to the assigned polling places.

205 *Recruitment of Polling Venues.* The second source of variation in the assignment of voters to polling places comes from the recruitment of the venues used as polling places. In each election year, the Elections Office prepares an information sheet that includes the delineation of the voting precincts and updated requirements for polling places. These requirements include, for instance, an adequate power supply and a sufficient mobile network connection. Following a resolution of the city council (*Stadtrat*), the Elections Office has placed priority on selecting venues with barrier-free

210 access for elderly and disabled people since 2017.¹⁴ Based on these guidelines, district inspectors
(*Bezirksinspektoren*) are charged with the actual recruitment of potential venues, including their
localization, verification, and coordination with third parties. Polling venues are typically public
or municipal properties, usually schools (71 percent of all venues), but also Church-affiliated fa-
cilities (11 percent), and retirement homes (5 percent)—see Appendix Figure A.2 for an overview
215 of venue types. While recruitment usually focuses on venues which have already been used in
the past, new polling place requirements, competing events on Election Day, building closures or
ongoing construction work may leave certain locations unavailable. There is no documentation of
the reasons why venues become unavailable. Correspondence with the Elections Office suggests
that especially Munich’s school construction program (*Schulbauoffensive*), which involved invest-
220 ments of more than 3.8 billion Euros in the refurbishment of educational facilities starting in 2016,
affected several polling venues. Indeed, a review of the public documents on the investment plan
revealed that in 70 percent of the cases in which schools were no longer used as polling venues, the
election date fell within the specified construction period. Further correspondence with affected
establishments revealed that there were some instances in which Church-affiliated facilities hosted
225 religious events on Election Sundays and became unavailable to serve as polling places. Overall,
we observe 293 distinct venues that were used in at least one election between 2013 and 2020. The
number of operated venues is typically around 200 in any given election. Appendix Figure A.3
illustrates the activity status of polling venues over time.

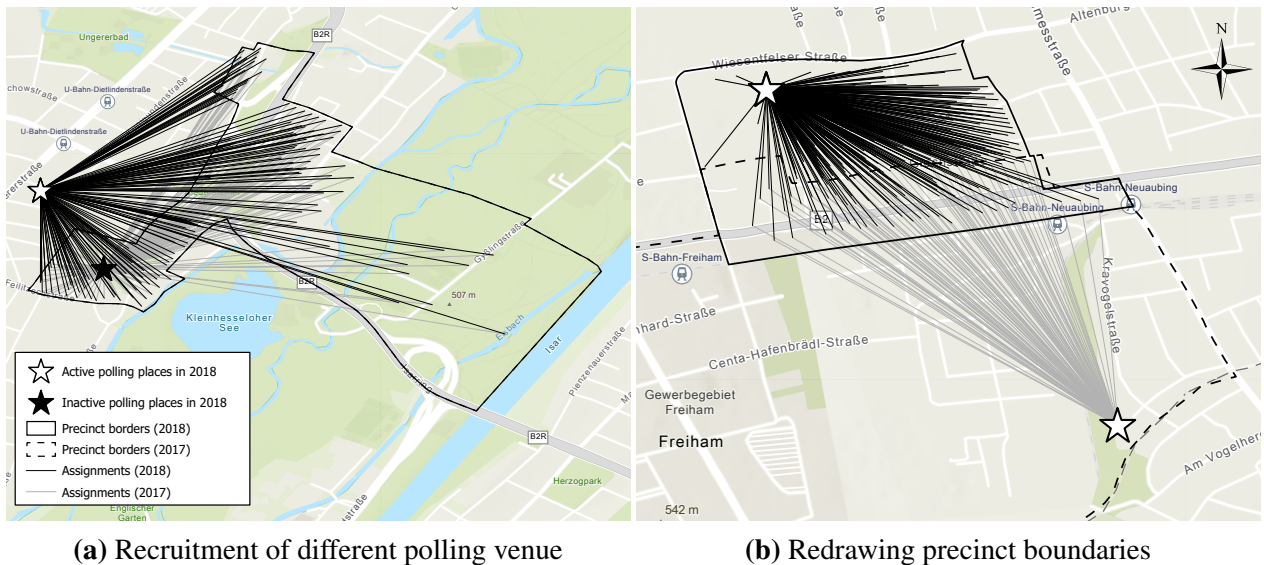
Illustration of Polling Place Reassignments. To exemplify the two sources of reassignments in our
230 setting, we illustrate two instances of polling place reassignments in Figure 3. Gray lines connect
eligible voters’ addresses to their assigned polling place in the 2017 Federal Election. The black
lines connect the addresses to their polling place assigned in the 2018 State Election. The solid
black borders delineate the precinct of interest. In Panel (a), all voters living in a northern precinct
experienced a relocation of their 2017 polling place as the elementary school, marked by the black
235 star, underwent a general renovation and became inoperable for the 2018 election. The new polling
station was hosted by a vocational school (indicated by the white star) located six walking minutes
from the old polling place. The example shows that newly recruiting or dropping a polling venue
typically means that all eligible voters living in the affected precinct are reassigned to a different

¹⁴The city council’s resolution specifies that the number of barrier-free polling places be doubled between 2014 and 2017 and that a share of at least 75 percent should be reached by 2020. According to documents of the Elections Office, a share of 80 percent was achieved in the 2018 State Election.

polling location. In the example, the relocation led to an increase in the average distance to the
240 polling place.

Panel (b) illustrates an instance in which only a fraction of a precinct's electorate is treated due
to the reconfiguration of its boundaries. The solid black lines mark the borders of the precinct of
interest in 2018. The dashed line delineates the boundaries of another precinct in 2017. Due to the
precinct boundary reconfiguration, individuals living at the intersection of these two shapes were
245 reassigned from the polling place at the bottom of the map to the polling place at the top of the
map. The fraction of voters living north of the dashed line were assigned to the same polling place
in 2017 and in 2018 and are therefore considered untreated in our setting. Unlike in the preceding
example, both polling places remained in operation in 2018 (white stars).

Figure 3: Illustration of Treatment



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

Figure 4 documents the fraction of residential addresses that were reassigned to a different polling
250 place relative to the previous election. There were no reassignments in the 2013 Federal Election

and the 2014 European Election as other elections were held earlier in the same year. Before 2017, the Elections Office addressed changes in precinct size mainly by adjusting the number of poll workers at the polling locations so that reassignments due to precinct border adjustments were limited. In 2017, 41 percent of residential addresses were assigned to a different polling place,
255 mainly caused by the major consolidation of precincts (enabled by a new urban planning technology) and due to updated requirements for polling places (especially regarding barrier-free venues). Munich’s school construction program contributed to the turnover of polling venues starting in 2017. In 2020, reassignments were primarily the result of the increased number of precincts and the recruitment of suitable venues to meet social distancing provisions during the Covid-19 pandemic.
260 Overall, 42 percent of all addresses are never subject to reassignments between 2013 and 2020, 26 percent are reassigned once, and 24 percent twice (see Appendix Figure A.4).¹⁵

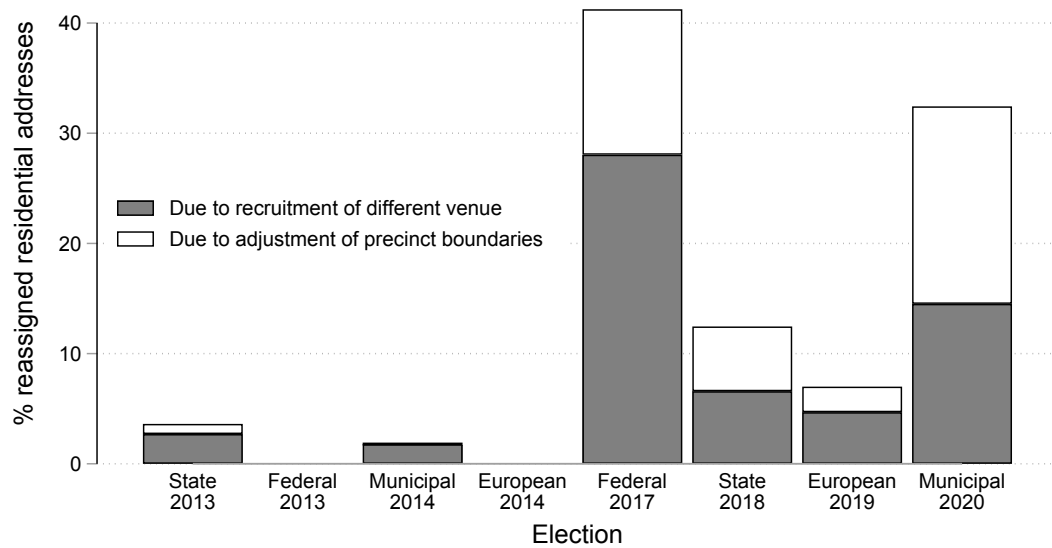
Figure 5 reports the distribution of street (walking) distances between residential addresses and polling places (left panel), and the distribution of distance *changes* conditional on a polling place relocation across all elections (right panel). Negative values indicate that the new polling place
265 is located at a closer distance (relative to the previous election), positive values correspond to a relocation further away. For 90 percent of residential addresses, the polling place is no further than 1.4 kilometers away, which roughly corresponds to a 17-minute walk (median: 0.74 kilometers). The distribution of distance changes is closely centered around zero (median: +0.04 kilometers, mean: +0.06 kilometers) and approximately symmetric (skewness: 0.2), indicating that
270 polling places are not systematically located closer or further away after reassignment. Despite the changes to precinct boundaries and polling venues, election officials maintained accessibility in terms of distance to polling places nearly constant over time. Appendix Figure A.5 depicts the median and interquartile range of the street distance between eligible voters’ addresses and their assigned polling places. The median distance remains at 0.71 kilometers before slightly increasing
275 to 0.76 kilometers in 2017.

3. Conceptual Framework

To guide our empirical analysis, we present a simple theoretical model drawing on the “calculus of voting” framework, in which citizens base their voting decision on a rational evaluation of their options (Riker and Ordeshook, 1968; Downs, 1957). We restrict this section to a summary of the
280 arguments and relegate the details to Appendix D.

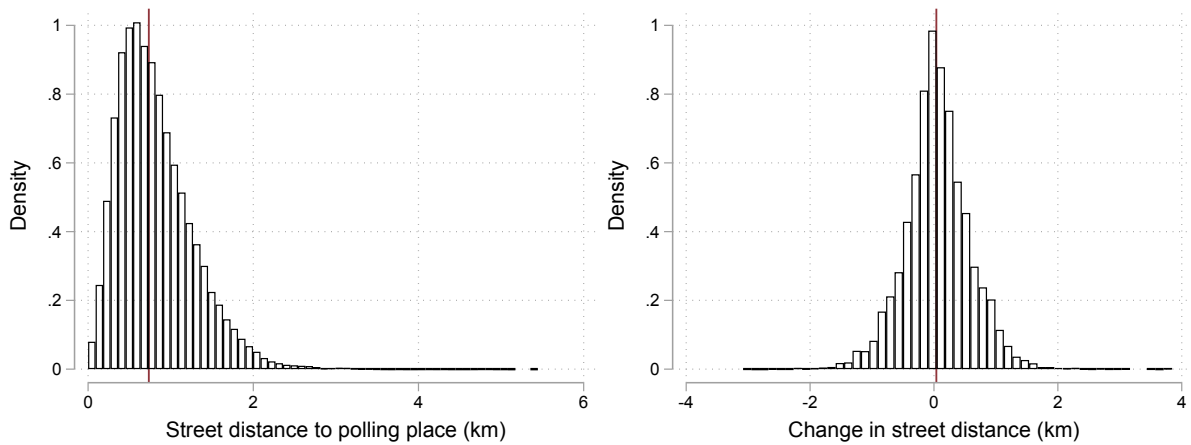
¹⁵On average, an address is reassigned once during our observation period. When an address is reassigned more than once, the median period between the first and second reassignment is three elections.

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



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

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



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

Individuals can vote in person at their assigned polling location, by mail, or abstain from voting. Voting yields some benefit, capturing the expected utility if the preferred party wins a greater number of seats and any direct utility from the act of voting itself (e.g., from fulfilling a civic duty). The individual abstains from voting if and only if her benefits do not outweigh the costs of voting. The chosen mode of voting depends primarily on the costs of voting in person relative to voting by mail.

Now, suppose that the electorate is assigned to a new polling place. We anticipate that the reassignment impacts the costs of voting in person via two mechanisms: *i*) a “transportation effect” and *ii*) a “search effect” (Brady and McNulty, 2011). The transportation effect captures the change in travel costs on Election Day resulting from the change in proximity to the polling place. In Munich, where polling places are usually located within walking distance, travel costs mainly correspond to the time to walk to the polling place. The search effect captures all additional costs resulting from reassignments when holding proximity constant, including the cost of searching for the new polling place and engaging with an unfamiliar location. Unlike in the US, eligible voters in Germany are not explicitly notified of *changes* to their assigned polling place. This introduces the possibility that *inattentive voters* are surprised by the reassignment after the deadline for requesting mail-in ballots has passed. Inattentive voters who would have switched to voting by mail can only do so in the ensuing election.

Suppose that the search and the transportation effect (or a combination of both) cause a positive shock to the cost of voting at the polling place. The shock increases the *absolute* costs of voting at the polling place and decreases the *relative* costs of voting by mail. As a result, some voters will switch from polling place to mail-in voting. If the benefit of voting by mail is not sufficient to outweigh the costs, individuals will switch to nonvoting if the cost shock is large enough to make polling place voting unattractive. Therefore, the model predicts that the shock causes a substitution effect between mail-in and polling place voting and a decline in overall turnout. If voters are inattentive to polling place reassignments, they miss the deadline for requesting mail-in ballots. Consequently, inattentiveness attenuates the shift from in-person to mail-in voting—as some voters choose to go to the new polling place anyway—and amplifies the shift towards nonvoting, as some voters who would have voted by mail entirely abstain from casting a ballot.

To what extent do these adjustments carry over to subsequent elections? The theory suggests two mechanisms that may be at play. First, relocating polling places may permanently alter the cost of in-person voting. This is obvious, for instance, when the electorate is permanently reassigned

to a polling place that is located further away. Similarly, search costs are likely to persist unless individuals familiarize themselves with the new location between two elections. Thus, the relative cost reduction of mail-in compared to in-person voting is likely to persist and induce a lasting substitution effect. If the absolute cost increase for voting at the polling place is sufficiently high, then voters may entirely abstain from voting today and in the future. However, the initial election may be different from subsequent ones due to inattentive voters. Some inattentive voters will only temporarily abstain from voting or cast their ballot at the new polling location and turn to mail-in voting in the following elections. Consequently, a drop in aggregate turnout may be (partly) recovered and the substitution of in-person for mail-in voting reinforced over time.

A second mechanism that could drive persistent changes in voting behavior is habit formation. Habit formation means that the act of (non)voting itself increases its consumption value and therefore increases the likelihood of nonvoting in the future—holding individual traits and voting costs constant (Fujiwara et al., 2016). Applied to our setting, habit formation would imply that voters who switch to nonvoting as a consequence of the reassignment, experience an increase in their utility of nonvoting. As there are compelling reasons to anticipate that relocations permanently alter voting costs, it is not possible to empirically separate the effect of habit formation from increased costs in our setting. However, we are able to test the key prediction of habit formation, namely: if (non)voting is habit forming, then any change in voter turnout must persist in the subsequent election(s), even in a hypothetical scenario in which the cost of voting was completely restored to pre-treatment conditions. Empirically, the magnitudes of these effects depend on the distribution of the population over different cost-benefit vectors and the magnitude of the reassignment shock.

4. Empirical Strategy

4.1. Data: Precinct-Level Panel

All information on polling locations, residential addresses, and turnout comes from administrative sources including official electoral rolls and official election results provided by the Munich Elections Office. We geo-reference polling locations and residential addresses in the eight elections in our panel, as well as in the 2009 Federal Election, which serves as a reference to identify changes in polling place assignments relative to the 2013 State Election (the first election in our panel). We identify 152,026 residential addresses from the 2018 electoral roll, of which we are able to match 143,278 to a unique precinct in every election (94.2 percent). We successfully geolocate 141,612 of these addresses (99.0 percent). We calculate the street distance, defined as the shortest walking

distance using the public road network, and the straight-line (Euclidean) distance between every
345 pair of residential address and polling place in every election.¹⁶

We complement the data with time-varying structural indicators obtained from administrative
sources.¹⁷ These include information on the age structure of the electorate, the average dura-
tion of residence in Munich, the marital status of residents, and their citizenship (German, non-
German EU, or non-EU citizenship) at the precinct level. We also aggregate annual real estate
350 rental price information compiled by the RWI Institute for Economic Research from square grids
with a one kilometer length to the precinct level to capture socioeconomic differences among
precincts.¹⁸ Because votes by mail are recorded at a level of administrative delineations that does
not coincide with precinct borders, we are confined to relying on *requests* of polling cards as a
proxy for mail-in votes. As noted above, about 90 percent of the requested cards are returned as
355 ballots, and more than 98 percent of these ballots are mail-in votes.

To obtain a panel of precincts that is 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 are
computed assuming 2018 (instead of the contemporaneous) precinct borders. Likewise, election-
360 specific precinct characteristics, such as the age structure, the size of the electorate, or the number
of votes cast, are converted to 2018 precinct borders using conversion keys provided by the Munich
Statistical Office (*Statistisches Amt der Landeshauptstadt München*).¹⁹ This leaves us with a panel
of 618 precincts with constant borders, which we observe over eight elections. Appendix Table B.1
reports summary statistics of our precinct-level variables.

¹⁶We use the geodist STATA package (Picard, 2019) to compute straight-line distances and the osrmtime package (Huber and Rust, 2016), which make use of *Open Source Routing Machine (OSRM)* and of *OpenStreetMaps (OSM)* to find the shortest route (by foot or other means), to calculate street distances.

¹⁷Precinct-level structural indicators and turnout data are available for download from the city's election review website (*WahlAtlas*): <https://www.muenchen.de/rathaus/Stadtfinfos/Statistik/Wahlen.html> [accessed August 8, 2021].

¹⁸The RWI - Leibniz Institute for Economic Research and its research data center compile granular real estate data obtained from the Internet platform *ImmobilienScout24* for research purposes.

¹⁹The variables are converted using population or electorate weights. A key assumption is that characteristics are evenly distributed within a precinct. For example, if a precinct is divided into two parts in 2018 (in terms of its electorate), it is assumed that voting behavior has not differed systematically between the two parts in the past.

365 4.2. Main Specifications

We estimate the contemporaneous search and transportation effect by relating turnout to polling place reassignments and changes in average street distance in a static DiD model:

$$\begin{aligned} Turnout_{pt}^s = & \gamma_1 Reassigned_{pt} + \gamma_2 Distance_{pt} + \gamma_3 Reassigned_{p,t-1} + \gamma_4 Distance_{p,t-1} \\ & + \mathbf{X}'_{pt} \boldsymbol{\lambda} + \alpha_p + \alpha_t + \varepsilon_{pt}, \end{aligned} \quad (1)$$

where $Turnout_{pt}^s$ measures the percentage turnout in precinct p in period t , with $t = 1, 2, \dots, 8$, so that elections are ordered chronologically. The superscript s indicates whether turnout refers to
 370 turnout at the polling place, via mail, or in total (given as the sum of polling place and mail-in turnout). The variable *Reassigned* denotes the share of residential addresses assigned to a different polling place compared 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 assigned polling place. By including precinct fixed effects, α_p , we
 375 identify the effect of *Distance* from *within*-precinct variation, which arises from reassignments only. Thus, the transportation effect is captured by γ_2 . We also control for the lag terms of re-assigned and distance to account for potential serial correlation in reassignments that may bias our results. Intuitively, if voters persistently change their behavior after a polling place reassignment—for instance, by permanently switching to mail-in voting—a second polling place relocation will
 380 not result in further behavioral adjustments. Thus, to the extent that some voters are repeatedly reassigned during our observation period, we may underestimate the effect of the shocks. \mathbf{X} is a vector of time-varying covariates at the precinct level: the precinct size (log of the number of residents and the share of residents eligible to vote), the age structure of the electorate (share of eligible voters aged 18–24, 25–34, 35–44, 45–59), the share of EU foreigners in the electorate,
 385 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, α_t , to control for election-specific shocks, such as differences in voting propensity due to varying perceived stakes or the weather on Election Day. Precinct fixed effects further ac-
 390 count for time-invariant precinct characteristics, such as its size (in terms of area), its remoteness, or its settlement structure (to the extent that it remains stable over our observation period).

We weigh observations with the number of eligible voters. This allows us to recover the conditional mean association between turnout and polling place reassignments at the individual level. In the baseline, we cluster standard errors at the precinct level to account for the correlation of model

395 errors over time. We also test the robustness of our results to alternative assumptions about the variance-covariance matrix in Section 6.

The two main identifying assumptions for interpreting our estimates of interests as causal are that (i) polling place reassignments and changes in distance are uncorrelated with other omitted factors that may affect turnout, and that (ii) polling place reassignments themselves are not driven
400 by the expectation of changes in turnout. Although these assumptions are not directly testable, we provide a number of robustness checks, including a balancing exercise, a placebo test, and a pretrend analysis, bolstering our confidence that our results can be interpreted as causal.

To investigate the persistence of behavioral changes due to polling place reassignment, we conduct an event study focusing on voting behavior around the *first* time a precinct is treated in our sample.
405 The event study design allows us to examine to what extent voters may be permanently dissuaded from voting and whether there are lasting substitution effects between in-person and mail-in voting. Let E_p denote the election in which precinct p is treated for the first time (the event). We regress turnout on election dummies relative to the event E_p , control variables, as well as precinct and election fixed effects (δ_p, δ_t):

$$Turnout_{pt}^s = \sum_{k=K}^L \mu_k \mathbb{1}(\text{Elections since } E_p = k) + \mathbf{X}_{pt}' \phi + \delta_p + \delta_t + v_{pt}, \quad (2)$$

410 where $\mathbb{1}(\cdot)$ is an indicator function and k indexes the number of periods before and after a precinct is treated for the first time. In the baseline, E_p corresponds to the first election in which the *entire* electorate in a precinct is affected by a polling place reassignment. Full reassignment constitutes the modal case of reassignment intensities, capturing 40 percent of all instances (see Appendix Figure A.6). In the baseline, we also trim precinct time series from the point at which a second
415 treatment occurs to ensure that we capture the impact of a single reassignment rather than a series of changes. Among all treated precincts, 54 percent are treated exactly once (Appendix Figure A.7). We test our results for robustness to alternative specifications in the subsequent section.

As a number of recent contributions have pointed out, two-way fixed effect (TWFE) event study estimates, may still yield biased results under staggered treatment and heterogeneous effects.²⁰
420 The main reason for this is that the TWFE estimator uses already-treated precincts as controls for

²⁰See e.g., [Athey and Imbens \(2022\)](#); [de Chaisemartin and D’Haultfœuille \(2020\)](#); [Borusyak et al. \(2021\)](#); [Goodman-Bacon \(2021\)](#); [Sun and Abraham \(2021\)](#)

newly-treated precincts, thereby violating the parallel trend assumption in the presence of treatment effect dynamics. Appendix Figure A.8 illustrates the treatment timing for the 618 precincts in our sample. Following our baseline definition in which treatment corresponds to 100 percent reassignments, we observe 340 never-treated precincts and 278 precincts which are treated at least once. For most treated precincts, the first treatment occurs in the 2017 Federal Election (62 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. To account for the staggered timing of treatment, we also estimate the event study using the estimators proposed by Borusyak et al. (2021), Callaway and Sant’Anna (2021), Sun and Abraham (2021), and de Chaisemartin and D’Haultfoeuille (2020). For instance, Callaway and Sant’Anna (2021) suggest a two-step estimation strategy by first estimating “group-time average treatment effects”, where groups are defined according to the first time units are treated, before aggregating the treatment effects by relative time using a propensity-score weighting method.

4.3. *Balancing Test*

Under our identifying assumption, the timing of reassignments is uncorrelated with other determinants of turnout. One approach to shed light on the comparability of treated and untreated precincts is to examine whether precinct characteristics are balanced once we account for time-constant factors and election-specific shocks. To this end, we estimate correlations between observable precinct characteristics and reassignments, conditional on election and precinct fixed effects. We report the results of the balance test in Table 1. Each cell contains an OLS estimate from a separate regression, with rows corresponding to precinct characteristics. The dependent variable in Column (1) is a dummy identifying precincts with a nonzero share of reassignments. The estimates are very small and not statistically significant, suggesting that the likelihood of *any* number of voters being reassigned to a different polling location is unrelated to observables. The dependent variable in Column (2) is the share of addresses assigned to a different polling place. Only one estimate appears marginally significant. Columns (3) and (4) distinguish between the reasons for reassignment, i.e., change in precinct boundaries or recruitment of a different polling venue, respectively. The estimates indicate no evidence that precinct characteristics are systematically related to the likelihood of reassignment for either reason. Finally, Column (5) regresses the log of average street distance on precinct characteristics. Out of seventeen estimates, only two are (marginally) significant. Nonetheless, *F*-tests cannot reject the hypotheses that the estimates are jointly equal to zero in any column, indicating that the fixed effects perform well in eliminating the correlation

between treatment and precinct characteristics. The balancing test thus supports our identifying assumption.

Table 1: Balance Test on Precinct Characteristics

	(1) Dummy (Reassigned >0)	(2) Share Reassigned	(3) Share Reassigned (Precinct Boundaries)	(4) Share Reassigned (Recruitment)	(5) Log Street Distance
Residents (thsd)	-0.012 (0.044)	0.056 (0.035)	0.030 (0.028)	0.026 (0.031)	-0.006 (0.031)
Single Residents (thsd)	0.019 (0.075)	0.111* (0.060)	0.068 (0.046)	0.042 (0.056)	0.031 (0.055)
Married Residents (thsd)	-0.103 (0.113)	0.070 (0.085)	0.002 (0.057)	0.067 (0.076)	-0.057 (0.075)
Native German Residents (thsd)	-0.119 (0.097)	0.044 (0.077)	-0.033 (0.044)	0.077 (0.071)	-0.011 (0.081)
Non-native German Residents (thsd)	-0.068 (0.168)	0.167 (0.125)	0.050 (0.087)	0.116 (0.108)	-0.172* (0.102)
Foreign Residents (thsd)	0.037 (0.060)	0.081 (0.053)	0.076 (0.046)	0.006 (0.043)	0.020 (0.044)
Inhabitants Eligible to Vote (thsd)	-0.018 (0.074)	0.039 (0.057)	-0.039 (0.040)	0.078 (0.054)	-0.022 (0.055)
Electorate Aged 18–24 (thsd)	-0.078 (0.250)	0.073 (0.203)	0.014 (0.131)	0.059 (0.177)	0.247 (0.166)
Electorate Aged 25–34 (thsd)	0.101 (0.137)	0.147 (0.112)	-0.061 (0.067)	0.207* (0.108)	0.155 (0.117)
Electorate Aged 35–44 (thsd)	-0.086 (0.171)	0.123 (0.138)	-0.030 (0.085)	0.153 (0.129)	-0.045 (0.123)
Electorate Aged 45–59 (thsd)	-0.231 (0.175)	0.175 (0.144)	-0.030 (0.103)	0.206 (0.127)	-0.109 (0.122)
Electorate Aged 60+ (thsd)	-0.045 (0.113)	-0.025 (0.095)	0.006 (0.071)	-0.031 (0.078)	-0.157* (0.084)
Germans in the Electorate (thsd)	-0.046 (0.084)	0.078 (0.066)	-0.020 (0.039)	0.098 (0.062)	-0.062 (0.069)
EU Foreigners in the Electorate (thsd)	-0.020 (0.092)	0.052 (0.066)	-0.014 (0.046)	0.066 (0.065)	0.056 (0.053)
Households w/ Children (%)	-0.001 (0.004)	0.003 (0.004)	0.003 (0.002)	0.001 (0.003)	0.004 (0.003)
Avg. Quoted Rent per sqm	-0.001 (0.003)	0.001 (0.003)	-0.001 (0.002)	0.002 (0.002)	0.002 (0.002)
Avg. Duration of Residence	-0.001 (0.003)	-0.001 (0.003)	0.000 (0.002)	-0.001 (0.002)	-0.003 (0.003)
<i>F</i> -test [<i>p</i> -value]	0.66 [0.84]	0.51 [0.95]	1.04 [0.42]	0.53 [0.94]	1.07 [0.38]
Observations	4944	4944	4944	4944	4944
Precinct FE	×	×	×	×	×
Election FE	×	×	×	×	×

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

455 5. Main Results

5.1. Search and Transportation Costs

Table 2 reports the estimation results of Equation (1). Panels A and B show the results for polling place turnout and turnout via mail, respectively. Panel C presents the results for total turnout. Column (1) includes only the share of reassigned residential addresses and the fixed effects. Column (2) adds precinct covariates. Column (3) further includes the lag term of reassignment. The estimate of *Reassigned* in this column thus captures the average impact of a relocation on turnout. Column (4) reports the full specification including log street distance and the lag terms of reassignment and distance. Column (5) removes the lag terms to test the sensitivity of the estimates of contemporaneous reassignment and distance. Finally, we run a falsification test by relating contemporaneous turnout to *future* reassignments and distance to the polling place in addition to current and past values. It may be that current and future reassignments share common causes that also determine voter participation. For instance, population growth may necessitate additional adjustments to precinct boundaries, and perhaps citizens in these precincts have a systematically different voting behavior. Thus, a relation between future reassignments and current turnout would suggest that these persistent confounders afflict our estimates. The results of the placebo treatment are presented in Column (6).

In line with our expectations, the effect of reassignment on polling place turnout is negative and significant at the one percent level in all specifications (Panel A). Controlling for lagged reassignments and covariates, the relocation of a polling place reduces in-person voting by 0.78 p.p. on average (Column 3). Evaluated at the mean, this corresponds to a reduction of roughly 2.3 percent. Adding distance in Column (4) breaks down the reduction into the search effect and the transportation effect. Holding distance to the polling place and other factors constant, polling place relocations reduce in-person voting by 0.48 p.p. (1.7 percent at the mean), on average. The transportation effect also appears statistically significant: increasing the street distance to the polling place by 10 percent (equivalent to roughly 75 meters at the mean) reduces polling place turnout by 0.34 p.p. (equivalent to a one-percent decline at the mean). Thus, about 60 percent of reduction can be attributed to the search effect. The estimates also imply that a polling place would have to move approximately 14 percent closer to the voter to counterbalance the negative impact of the search effect, on average. The estimates of the search and transportation effect are insensitive to excluding the lag terms, suggesting that serial correlation in reassignments does not bias our results (Column 5). The placebo treatment estimates reported in Column (6) further show that future

polling place relocations do not affect current turnout in any panel, bolstering our confidence that persistent unobserved confounders do not afflict our estimates of interest.

The impact on mail-in turnout in Panel B mirrors the effect on polling place voting. On average, 490 reassignments increase mail-in turnout by 0.30 p.p. (Column 3). However, only the transportation effect is statistically significant in the full specification (Column 4). Increasing the distance to the polling place by 10 percent raises mail-in voting by 0.24 p.p. (equivalent to 0.82 percent at the mean). Thus, we find evidence for the substitution of in-person voting for mail-in voting after a polling place relocation. Yet, holding distance constant, the search cost effect only slightly 495 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 leads to a larger decrease in polling place turnout than it increases mail-in turnout. This is in line with the theory predicting only a partial substitution as some voters switch to nonvoting because the (individual) costs of voting by mail are higher than the perceived benefits or because inattentiveness regarding 500 polling place relocations causes some voters to miss the deadline for requesting mail-in ballots.

The effect of polling place reassignment on overall participation is indeed sizable and statistically significant. On average, turnout declines by 0.48 p.p. (Column 3 of Panel C). Both search and transportation costs drive the effect: holding distance constant, a polling place reassignment reduces overall turnout by 0.40 p.p., which is equivalent to 0.65 percent at the mean (Column 4 of 505 Panel C). Thus, about 83 percent of the overall effect is due to the search effect. Increasing the distance to the polling place by 10 percent depresses turnout by 0.10 p.p., which corresponds to a 0.16 percent reduction at the mean. The estimates imply that the magnitude of the search cost effect on overall participation is equivalent to an increase in travel distance by 39 percent. Notice that the estimate of the contemporaneous search effect on overall turnout also reflects inattentive- 510 ness, i.e., votes that would have been cast by mail if individuals had noticed their polling place relocation in time. For instance, the estimates of the lag terms of *Reassigned* suggest that there is some increase in mail-in voting stemming from relocations in the past. This could indicate that inattentive voters revert to mail-in voting in the election *after* the reassignment. The event study analysis in the subsequent section sheds more light on this potential driver of declining turnout. 515 Overall, the evidence so far shows pronounced transportation and search effects in the short-run, consistent with theoretical predictions and previous research (Brady and McNulty, 2011).

Table 2: Search and Transportation Costs—Baseline Specification

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Turnout at the Polling Place						
Reassigned	-0.80*** (0.14)	-0.77*** (0.13)	-0.78*** (0.13)	-0.47*** (0.11)	-0.46*** (0.11)	-0.64*** (0.14)
Reassigned, $t - 1$			-0.65*** (0.12)	-0.47*** (0.12)		-0.62*** (0.14)
Log Street Distance				-3.37*** (0.24)	-3.42*** (0.23)	-3.60*** (0.34)
Log Street Distance, $t - 1$				-0.04 (0.20)		0.26 (0.24)
Reassigned, $t + 1$						0.04 (0.13)
Log Street Distance, $t + 1$						-0.12 (0.20)
R^2	0.96	0.96	0.96	0.97	0.97	0.96
Panel B: Turnout via Mail						
Reassigned	0.26* (0.15)	0.29** (0.13)	0.30** (0.13)	0.07 (0.13)	0.06 (0.13)	0.32** (0.16)
Reassigned, $t - 1$			0.53*** (0.13)	0.37*** (0.13)		0.55*** (0.15)
Log Street Distance				2.35*** (0.24)	2.54*** (0.23)	2.58*** (0.33)
Log Street Distance, $t - 1$				0.38** (0.19)		0.09 (0.23)
Reassigned, $t + 1$						0.05 (0.12)
Log Street Distance, $t + 1$						-0.05 (0.17)
R^2	0.93	0.94	0.94	0.95	0.95	0.95
Panel C: Overall Turnout						
Reassigned	-0.53*** (0.17)	-0.48*** (0.12)	-0.48*** (0.12)	-0.40*** (0.12)	-0.40*** (0.12)	-0.32** (0.14)
Reassigned, $t - 1$			-0.12 (0.13)	-0.10 (0.13)		-0.08 (0.15)
Log Street Distance				-1.02*** (0.19)	-0.89*** (0.21)	-1.02*** (0.25)
Log Street Distance, $t - 1$				0.34* (0.19)		0.35* (0.21)
Reassigned, $t + 1$						0.08 (0.12)
Log Street Distance, $t + 1$						-0.17 (0.16)
R^2	0.98	0.99	0.99	0.99	0.99	0.99
Observations	4,944	4,944	4,944	4,944	4,944	4,326
Controls		×	×	×	×	×

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

5.2. *Pretrends and Treatment Effect Persistence*

A central threat to validity is a violation of the parallel trend assumption, namely, that the trend in turnout among precincts subject to reassignments would have differed from the trend of other precincts absent reassignments. For instance, this could be the case if the Elections Office systematically consolidated neighboring precincts that showed greater shifts from in-person to mail-in voting in the past to reduce the costs of operating polling places. In this case, the estimate for the effect of reassignments may simply reflect a pre-existing trend rather than the substitution effect resulting from a shock to the cost of 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 parallel trends preceding the treatment.

Figure 6 plots the event study results for turnout at the polling place, via mail, and overall using the TWFE-OLS estimator. The event is defined as the first election in which *all* residential addresses are reassigned to a new polling place. As emphasized above, we exclude all precinct-election observations beyond any second event so that we pick up the effects of only one instance of treatment in every precinct.

Reassuringly, the results do not show evidence of pretrends in any of our outcome variables, bolstering our confidence that the parallel trend assumption holds. All pre-reassignment dummies are very small and statistically indistinguishable from zero. By contrast, we find that polling place turnout falls by 1.09 (SE=0.24) p.p. and mail-in turnout increases by 0.53 (SE=0.24) p.p. immediately after a polling place relocation. This is in line with the substitution effect resulting from the reduction in relative costs of mail-in voting due to reassignment. The bottom plot shows that the substitution is not large enough to completely offset the reduction in overall participation: on average, total turnout declines by 0.56 (SE=0.17) p.p. immediately after reassignment. Thus, compared to the earlier results estimated for the full sample, the event study estimates on contemporaneous turnout are slightly more pronounced, suggesting a greater reduction in polling place turnout, a stronger substitution towards mail-in voting, and a slightly larger decline in aggregate turnout.

The estimates further show that the shift from in-person to mail-in voting persists in the two subsequent elections. This is consistent with the theory predicting a lasting substitution effect resulting from a permanent change in the relative costs of voting. Interestingly, the net effect on total turnout is statistically indistinguishable from zero in all periods following the event. While a portion of treated voters switches to nonvoting upon reassignment, the decline in turnout is already recovered in the following election. One possible mechanism is that the initial shock to the costs of voting

weakens over time, inducing temporary abstainers to turn out at their new polling location. For
550 instance, the search cost effect may wane as voters become familiar with the new polling place
and uncertainty about its location and accessibility decreases. Another explanation is that the ini-
tial decline is driven by inattentive voters, who notice the reassignment only after the deadline
for requesting a mail-in ballot has passed. Inattentive voters who would have switched to mail-in
voting will either vote at the new polling place anyway or forgo voting in the treatment election.
555 But aware of the reassignment, these voters turn to mail-in voting in subsequent elections. In Sec-
tion 5.3, we make the case that the temporary decline in overall turnout is indeed explained by
inattentiveness rather than waning search costs. The argument is that the recovery in turnout is
demonstrably driven by an increase in mail-in turnout rather than in-person turnout.

Finally, our results do not support the hypothesis that (non)voting is habit forming. If abstaining
560 from voting was habit forming, the initial decline in turnout would carry over to subsequent elec-
tion, even if the costs of voting were entirely restored to pre-treatment 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 to be disproportionately driven by inattentive vot-
ers. As this subset of the population is not necessarily representative of the general electorate, we
565 cannot rule out that habit formation is still a relevant determinant of voting behavior for the average
citizen.

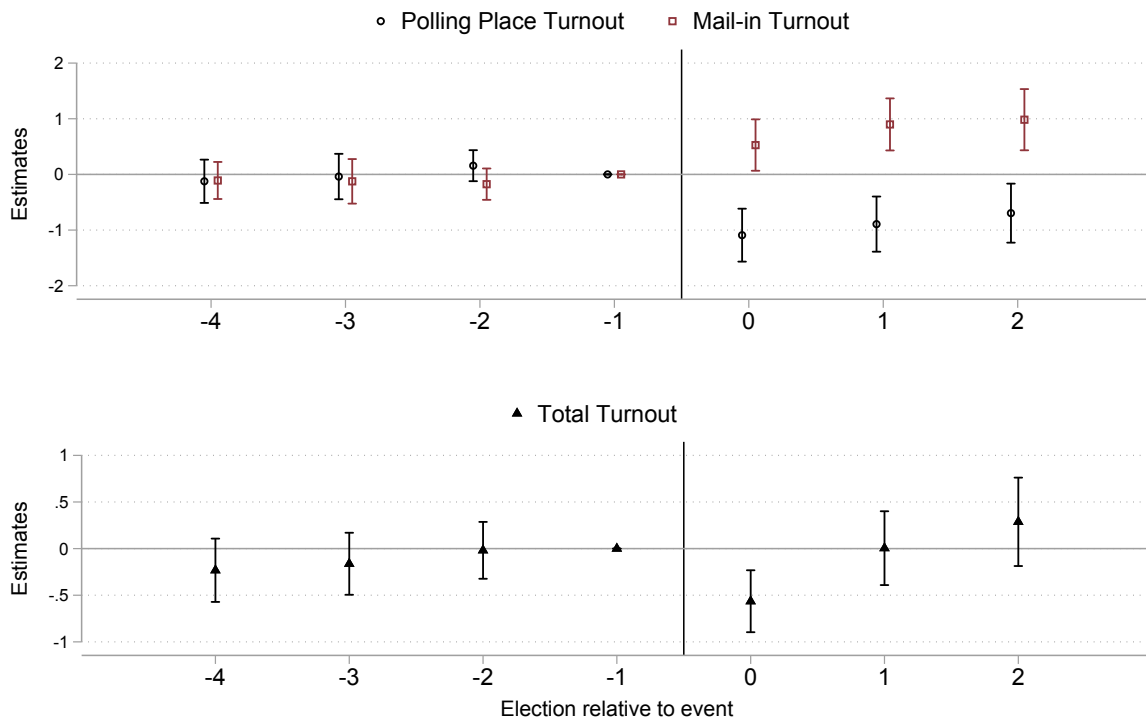
The full set of our event study results is reported in Table 3. We first verify that our baseline
estimates of the search and the transportation effects (Equation 1) hold in the subsample used for
the event study (Column 1). In Column (2), we present the event study results corresponding to
570 estimates reported in Figure 6. We also estimate the event study using the full sample instead of
trimming the time series once a second treatment occurs. The estimates presented in Column (3)
show that the results remain robust. In Column (4), the event corresponds to the first election in
which at least 50 percent of a precinct is affected by polling place reassignments.²¹ The effect sizes
are slightly attenuated but remain statistically significant. Finally, we estimate the model with a
575 balanced sample in the four periods before and two periods after the relocation. This reduces the
number of observations by roughly 800 and the number of treated precincts from 278 to 115, of
which 89 percent are treated in the 2017 Federal Election and 11 percent in the 2018 State Election.
The estimates presented in Column (5) are consistent with previous results; however, the negative

²¹Using this treatment definition, we capture 60 percent of all instances in which a positive share of addresses is
reassigned (see Appendix Figure A.6).

580 treatment effect on overall turnout in Panel C is imprecisely estimated, likely due to the restricted sample.

In Appendix Figure A.9, we replicate the event study results of Column (2) with several novel estimators which account for staggered treatment timing (Borusyak et al., 2021; Callaway and Sant’Anna, 2021; Sun and Abraham, 2021; de Chaisemartin and D’Haultfœuille, 2020). The estimates are very similar and consistent with the TWFE-OLS results.

Figure 6: Event Study Illustration



Notes: The figure presents the event study results estimated using the TWFE-OLS estimator. The event is defined as the first time in which the entire precinct is reassigned to a new polling place (Equation 2). Regressions are weighted with the number of eligible voters. Confidence intervals reported at the 95% level. The full results of the underlying regressions appear in Column (2) of Table 3.

Table 3: Event Study Results

	(1)	(2)	(3)	(4)	(5)
Panel A: Turnout at the Polling Place					
Reassigned	-0.63*** (0.16)				
Log Street Distance	-3.35*** (0.31)				
$t - 4$		-0.12 (0.20)	-0.14 (0.20)	-0.11 (0.16)	-0.49* (0.29)
$t - 3$		-0.04 (0.21)	-0.05 (0.21)	0.02 (0.18)	-0.43 (0.31)
$t - 2$		0.16 (0.14)	0.15 (0.14)	0.19 (0.15)	0.24 (0.21)
t		-1.09*** (0.24)	-1.10*** (0.24)	-0.97*** (0.18)	-1.79*** (0.40)
$t + 1$		-0.89*** (0.25)	-0.81*** (0.22)	-0.81*** (0.23)	-1.67*** (0.36)
$t + 2$		-0.70** (0.27)	-0.51** (0.23)	-0.55** (0.26)	-0.96*** (0.34)
R^2	0.97	0.96	0.96	0.96	0.96
Panel B: Turnout via Mail					
Reassigned	0.05 (0.16)				
Log Street Distance	2.46*** (0.30)				
$t - 4$		-0.11 (0.17)	-0.08 (0.17)	-0.01 (0.14)	0.19 (0.21)
$t - 3$		-0.12 (0.20)	-0.10 (0.20)	-0.05 (0.18)	0.28 (0.29)
$t - 2$		-0.18 (0.14)	-0.17 (0.14)	-0.06 (0.17)	-0.04 (0.20)
t		0.53** (0.24)	0.55** (0.23)	0.44** (0.18)	1.48*** (0.35)
$t + 1$		0.90*** (0.24)	0.78*** (0.21)	0.80*** (0.22)	1.41*** (0.34)
$t + 2$		0.98*** (0.28)	0.66*** (0.24)	0.91*** (0.27)	1.36*** (0.35)
R^2	0.95	0.95	0.95	0.95	0.95
Panel C: Overall Turnout					
Reassigned	-0.58*** (0.14)				
Log Street Distance	-0.89*** (0.23)				
$t - 4$		-0.23 (0.17)	-0.22 (0.17)	-0.13 (0.15)	-0.30 (0.24)
$t - 3$		-0.16 (0.17)	-0.16 (0.17)	-0.04 (0.16)	-0.14 (0.26)
$t - 2$		-0.02 (0.16)	-0.01 (0.15)	0.13 (0.15)	0.20 (0.26)
t		-0.56*** (0.17)	-0.56*** (0.17)	-0.54*** (0.14)	-0.31 (0.27)
$t + 1$		0.00 (0.20)	-0.03 (0.19)	-0.01 (0.19)	-0.26 (0.30)
$t + 2$		0.29 (0.24)	0.16 (0.21)	0.36 (0.25)	0.40 (0.30)
R^2	0.99	0.99	0.99	0.99	0.99
Observations	4,360	4,360	4,514	4,109	3,525
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 by the number of polling cards requested. 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*. Regressions are weighted with the number of eligible voters. Standard errors are clustered at the precinct level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

585 *5.3. Mechanism: What Drives the Recovery in Overall Turnout?*

The transitory drop in overall turnout demands further examination of the possible mechanisms explaining the recovery. We discuss two potential channels, *i*) inattentiveness to reassignments and *ii*) waning search costs. As outlined above, inattentiveness implies that voters delay switching to mail-in voting and temporarily abstain from turning out. Waning search costs, on the other hand, 590 imply that voters temporarily abstain from turning out and return to voting in person because they familiarized themselves with their new polling place. Thus, while inattentiveness implies that the recovery in the subsequent election is driven by an increase in *mail-in* voting, waning search costs imply that the recovery is driven by an increase in *in-person* turnout.

We can empirically test these implications by assessing whether the differences between the treat- 595 ment and the first post-treatment dummy ($\hat{\mu}_1 - \hat{\mu}_0$) are greater than zero for mail-in and in-person turnout, respectively. To rule out ambiguity due to cases in which reassignments resulted in a *negative* shock to voting cost due to closer proximity to the polling place, we estimate two modified event study specifications. In one specification, we additionally control for the log of street distance to the assigned polling place, absorbing the transportation effect. Second, we estimate the 600 event study on a sample excluding all precincts in which the reassignment resulted in a decrease in average distance to the polling place. Again, the event corresponds to the first time in which the entire precinct is assigned to a different polling place.

The event study results are plotted in Appendix Figures [A.10](#) and [A.11](#). We report the impact on in-person turnout, mail-in turnout, and overall turnout using the TWFE-OLS estimator as well as the 605 four novel estimators which account for staggered treatment timing ([Borusyak et al., 2021](#); [Callaway and Sant'Anna, 2021](#); [Sun and Abraham, 2021](#); [de Chaisemartin and D'Haultfœuille, 2020](#)). The patterns are consistent with the results of the main specifications, confirming the persistent shift from in-person toward mail-in voting and the transitory drop in overall turnout. However, the magnitudes of the coefficients differ. Unsurprisingly, the effects are amplified when focusing only 610 on precincts in which reassignments resulted in greater distance to the polling place and somewhat attenuated when absorbing the transportation effect.

Table 4 reports the difference between the event study estimates in period 1 and period 0 ($\hat{\mu}_1 - \hat{\mu}_0$) for mail-in, in-person, and overall turnout according to the five estimators. The coefficients estimated on the restricted sample (Panel A) indicate that mail-in turnout *further* increases in the 615 election following the treatment: the difference ($\hat{\mu}_1 - \hat{\mu}_0$) is positive according to all five estimators and statistically significant based on all but one estimator. By contrast, in-person turnout

further *declines* in the subsequent election according to four out of five estimators; though, the differences between the treatment dummies $\hat{\mu}_1$ and $\hat{\mu}_0$ on in-person turnout are not statistically significant.²² Hence, the results strongly support the hypothesis that the recovery in overall turnout in period 1 is driven by inattentive voters switching from nonvoting (and partly in-person voting) to mail-in voting.

Table 4: Difference between Event Study Estimates in Period 1 and Period 0

	(1)	(2)	(3)
	Mail-in turnout	Polling place turnout	Overall turnout
<i>Panel A: Differences based on event study estimates restricted to precincts with increased distance</i>			
BJS (2021)	0.65***	-0.17	0.48**
TWFE-OLS	0.70***	-0.04	0.67***
SA (2020)	0.29	0.19	0.48**
dChDH (2020)	0.87***	-0.33	0.54**
CS (2021)	0.79***	-0.47*	0.32
<i>Panel B: Differences based on event study estimates after absorbing transportation effect</i>			
BJS (2021)	0.37**	-0.02	0.36**
TWFE-OLS	0.47***	0.03	0.51***
SA (2020)	0.11	0.20	0.30*
dChDH (2020)	0.53***	-0.13	0.40**
CS (2021)	0.33*	0.07	0.39*

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

The results obtained when absorbing the transportation effect bolster this interpretation (Panel B). Again, all estimators show that mail-in turnout further increases in the post-treatment election, the change being statistically significant in four out of five cases. Two (three) estimators suggest that in-person voting further decreases (increases) in the election following the treatment; however,

²²Only the event study estimates proposed by [Sun and Abraham \(2021\)](#) suggest an increase of in-person turnout in the first post-treatment period; still, 60 percent of the recovery of overall turnout is explained by the increase in mail-in voting.

none of these changes are statistically significant.²³ Thus, the results do not support the hypothesis that the recovery is explained by waning search costs.

6. Robustness of the Results

Different Reassignment Reasons. One potential concern is that the different reasons for polling place reassignments yield systematically different behavioral responses. This would suggest that voters anticipate changes due to a reconfiguration of precinct boundaries and changes due to the recruitment of a different venue to varying degrees. It may also indicate that a part of the electorate is systematically more likely to experience one type of reassignment, casting doubt on the (conditional) randomness of treatment. Moreover, voters living near precinct borders may be more likely reassigned due to revisions of precinct boundaries. If these voters differ systematically with respect to other determinants of electoral turnout, this could afflict our estimates of interest. To test whether the different causes of reassignments could be a source of concern, we re-estimate Equation (1) differentiating by reason of reassignment. The results are shown in Table 5. Column (1) reports the baseline results for comparison. The estimates in Column (2) show that the different reasons for polling place reassignment do not have different effects on turnout. The *t*-tests for equality of the estimates (p-values reported in square brackets) indicate that the estimates are not statistically different from each other with respect to all outcomes (Panels A, B, and C). This supports the view that voters do not anticipate or react differently to polling place reassignments depending on the reason for the change.

Error Correlation within Election-Districts. Another potential concern is that model errors are correlated within city districts. This may happen because adjustments to the boundaries of adjacent precincts are not made across but solely within a district. Moreover, it is not uncommon that polling stations of several precincts (within a district) are located in the same building. In these cases, dropping a venue will affect multiple precincts simultaneously. To account for this, we re-estimate Equation (1), correcting standard errors for two-way clusters at the level of precincts (to account for error correlation over time) and at the level of districts in each election (to account for within-district-election correlation). Column (3) presents the estimates with two-way cluster-robust standard errors. The standard errors of our variables of interest increase slightly but the statistical significance of the estimates remains unaffected. We also show the robustness of our

²³The increases of in-person turnout according to the OLS estimates and the approaches proposed by Sun and Abraham (2021) and Callaway and Sant’Anna (2021) explain 0.7 percent, 60 percent, and 17 percent of the recovery of overall turnout, respectively.

655 event study results to alternative levels of clustering, including two-way clustering, clustering at the district level, and wild bootstrapped clustering as suggested by [MacKinnon et al. \(2022\)](#) in Appendix Table [B.2](#).

Accounting for Constituencies. Unlike precincts, city districts are directly contested in some elections. In State and Federal Elections, for instance, the 25 districts are combined into several single-
660 member constituencies, where residents directly elect their representatives for parliament. In Municipal Elections, citizens also elect a local district committee (in addition to the city council and the mayor). If there are systematic differences in voting incentives across districts—for instance, because citizens anticipate close races in some constituencies ([Bursztyn et al., 2022](#))—this may pose a threat to the validity of our estimates of interest. Thus, we account for potential cross-
665 district variation by estimating Equation (1) including a full set of district-election fixed effects. This ensures that comparisons are only made within district-election cells. The results in Column (4) show that our estimates of interest are insensitive to the alternative specification.²⁴

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

Excluding Election during COVID-19 Pandemic. We also estimate the model excluding the 2020
675 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 in-person 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).

²⁴We also re-estimate our baseline event study including district-election fixed effects. The result reported in Column (5) of Appendix Table [B.2](#) show that the estimates are robust.

Table 5: Search and Transportation Costs—Robustness

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Polling Place Turnout						
Reassigned	-0.47*** (0.11)		-0.47*** (0.12)	-0.39*** (0.12)	-0.62*** (0.14)	-0.65*** (0.14)
Log Street Distance	-3.31*** (0.23)	-3.30*** (0.23)	-3.31*** (0.23)	-3.34*** (0.23)	-3.43*** (0.30)	-3.55*** (0.30)
Reassigned (Precinct Boundaries)		-0.56*** (0.18)				
Reassigned (Recruitment)		-0.43*** (0.14)				
R^2	0.97	0.97	0.97	0.97	0.98	0.96
T -test for equality of estimates		-0.58 [0.57]				
Panel B: Turnout via Mail						
Reassigned	0.07 (0.13)		0.07 (0.18)	0.07 (0.12)	0.20 (0.16)	0.32** (0.16)
Log Street Distance	2.31*** (0.24)	2.31*** (0.24)	2.31*** (0.24)	2.41*** (0.22)	2.46*** (0.30)	2.46*** (0.31)
Reassigned (Precinct Boundaries)		0.14 (0.20)				
Reassigned (Recruitment)		0.05 (0.15)				
R^2	0.95	0.95	0.95	0.96	0.96	0.95
T -test for equality of estimates		0.38 [0.70]				
Panel C: Overall Turnout						
Reassigned	-0.40*** (0.12)		-0.40*** (0.14)	-0.32*** (0.12)	-0.42*** (0.14)	-0.32** (0.14)
Log Street Distance	-0.99*** (0.20)	-0.99*** (0.20)	-0.99*** (0.19)	-0.92*** (0.18)	-0.97*** (0.25)	-1.09*** (0.25)
Reassigned (Precinct Boundaries)		-0.42** (0.20)				
Reassigned (Recruitment)		-0.39*** (0.13)				
R^2	0.99	0.99	0.99	0.99	0.99	0.99
T -test for equality of estimates		-0.16 [0.88]				
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 by the number of polling cards requested. All specifications control for lag of *Reassigned* and the lag of *Log Street Distance* in addition to the following precinct covariates: the log of the number of residents, the share of residents eligible to vote, the share of eligible voters aged 18–24, 25–34, 35–44, 45–59, respectively, the share of EU foreigners in the electorate, the share of native German residents, the share of non-native German resident, the share of single residents, the share of married residents, the average duration of residence (in years), the share of households with children, and the average quoted rent per square meter. Regressions are weighted with the number of eligible voters. Standard errors are clustered at the precinct level (except in Column 3) and reported in parentheses. In Column (3), standard errors are corrected for two-way clusters at the level of precincts (to account for model error correlation over time) and at the level of districts in each election (to account for within-district-election correlation). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

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

7. Heterogeneity across Precinct Characteristics and Party Outcomes

695 *Heterogeneity across Precinct Characteristics.* Who responds to reassignment shocks? To shed light on this question, we explore heterogeneous effects of polling place reassignments across precinct characteristics in Appendix Table B.4. Column (1) reports the baseline results from Column (4) of Table 2 for comparison. We re-estimate Equation (1) adding an interaction term between the share of reassigned addresses and each precinct characteristic individually in Columns (2) through (8). Panels A, B, and C present the results for in-person turnout, mail-in turnout, and overall turnout, respectively. All continuous covariates in the interaction terms are transformed to z-scores (i.e., zero mean and unit standard deviation). We also examine heterogeneity across political leanings by estimating interactions with binary indicators identifying precincts with above-median vote shares for conservative parties (*CSU* and *Freie Wähler*) and left-wing parties (*SPD*, *DIE LINKE*, and *Grüne*) in 2013, respectively.²⁵

The results of the exercise suggest that the effects of reassignments are attenuated in precincts with a larger share of younger eligible voters (aged 18–24). This is unsurprising as a higher share of first-time voters means a higher proportion of individuals who do not experience reassignments. Precincts with a higher share of middle-aged individuals (25–59) in the electorate do not react

²⁵Due to data limitations, we are confined to determining party shares using in-person votes only.

710 differently at the polling place, but shift more strongly to mail-in voting when reassigned, resulting in a lower overall decline (Column 3). The opposite is true for precincts with a higher share of elderly eligible voters (aged 60 and above), where individuals are more likely to switch to nonvoting rather than mail-in voting. As a result, the decline in total participation is significantly more pronounced in these precincts (Column 4).

715 We find no different effect of reassignments on overall turnout in precincts with a higher share of households with children; yet, the substitution between in-person and mail-in voting is weaker in these precincts. In more affluent precincts (measured by the average quoted rent), individuals are more likely to switch to mail-in voting rather than abstain, resulting in a smaller decline in total turnout. Finally, we find no statistical differences regarding the effect of reassignments on total
720 turnout for historically conservative-leaning precincts and left-leaning precincts. Interestingly, however, conservative precincts are more likely to turn out at the new polling location, while left-leaning precincts switch more strongly to mail-in voting.

Heterogeneous Effects on Party Outcomes. What are the consequences of reassignments for electoral outcomes? To examine this question, we estimate the impact of reassignments on party
725 results. Unfortunately, party outcomes at the precinct level are only recorded for votes cast in person, so that we cannot analyze the role of mail-in votes in compensating for votes lost at the polling place. Nevertheless, the results can be interpreted as upper bound estimates for the effect on party outcomes. In Appendix Table B.5, we report results from estimating Equation (1) using as dependent variables party votes relative to the number of eligible voters (Panel A) and party
730 votes relative to the number of total votes (Panel B) for the seven biggest parties in the State of Bavaria.²⁶ Odd columns do not control for distance, even columns show the full specification. The columns' ordering reflects the political leaning of parties from the extreme left (*DIE LINKE*) to the extreme right (*AfD*).

The results in Panel A show that the decline in in-person turnout due to reassignments is mirrored
735 by a decline in party turnout across almost all parties. On average, party turnout falls by 0.12–0.17 p.p. for all parties except *DIE LINKE* (extreme left) and *Freie Wähler* (conservative-right), for which the effect is very close to zero. The impact of distance is negative for all parties, but again appears somewhat attenuated for *DIE LINKE* and *Freie Wähler* when comparing the even columns.

²⁶To compute party outcomes, we use votes cast for the City Council in Municipal Elections, and the party list votes in European, Federal, and State Elections.

740 Panel B presents the results on party shares relative to all votes cast at the polling place. Since this is the relevant metric for determining the composition of parliament, the estimates are better suited to gauge the effect on electoral outcomes. On average, two parties gain from reassignments, the extreme-left *DIE LINKE* and the conservative *CSU*. However, the gains appear rather moderate, with increases by 0.18 and 0.27 p.p., respectively. On the other hand, the left-wing *SPD* and 745 the conservative-right *Freie Wähler* lose by 0.25 and 0.15 p.p., respectively. The effects for the remaining parties are small and statistically insignificant. Thus, even assuming none of the votes lost at the polling place are offset by mail-in votes, the likelihood of reassignments affecting a parliament's composition by altering the proportion of party shares is negligible.

8. Discussion: Magnitudes and Policy Implications

750 8.1. Comparison with Previous Research

Previous findings on the effect of polling place reassignments on voting behavior provide an important benchmark for our results. There exist no other studies investigating how lasting these effects are. Thus, we focus on contemporaneous effects in the following. The existing evidence originates solely from US elections using a candidate-based plurality voting system. By contrast, elections in 755 Munich are (mainly) based on party list votes and yield proportional representation. We find that, on average, reassignments result in a decline of in-person voting by 0.8–1.1 p.p. which is partially offset by an 0.3–0.5 p.p. increase in mail-in voting, leading to an overall decline of 0.5–0.6 p.p., or 0.8 percent relative to the mean. [Brady and McNulty \(2011\)](#) find a similar partial substitution of mail-in voting for in-person voting following polling place reassignments in the 2003 Los Angeles gubernatorial recall election. However, the estimated effect magnitudes are significantly higher, 760 with polling place turnout declining by 3.0 p.p. and overall turnout falling by 1.8 p.p., or 3 percent. Given that [Brady and McNulty](#) analyze a setting in which the number of polling places was significantly reduced (and thus distances to the polls increased), the greater decline in turnout is unsurprising. Still, we cannot rule out the possibility that our estimates suffer from attenuation bias 765 due to imperfect measurement as we rely on the share of reassigned addresses instead of reassigned individuals. Accounting for changes in distance, [Brady and McNulty](#) find that about 60 percent of the reduction in polling place turnout is due to the search effect. This estimate is almost identical to our finding. Keeping in mind that our setting also features reassignments that result in a closer distance to the polling place, this result indicates that the search effect is stronger overall in their 770 setting. In fact, the authors find that the magnitude of the search effect is approximately equivalent to moving the polling place about one mile (1.6 kilometers) further away. By contrast, our estimates imply that the size of the search effect is comparable to increasing the distance by about

0.10 kilometers, which is more than an order of magnitude smaller than [Brady and McNulty](#)'s estimate. One explanation for this discrepancy is that voters use different modes of transportation to get to the polling locations, with Los Angeles voters primarily driving while Munich voters typically walking. Thus, the effects in terms of travel time are much closer. Finally, regarding the importance of the search effect on overall voter turnout, our estimates match the findings of [Brady and McNulty](#). Both studies find that roughly 80 percent of the decline in overall turnout is due to the search effect.²⁷

[Clinton et al. \(2021\)](#) use a static DiD approach to estimate the impact of polling place reassignments in the presidential elections of 2012 and 2016 in North Carolina. Similar to our setting, reassignments do not systematically yield shorter or longer travel distances. Their findings suggest that following reassignments, in-person turnout declines by 0.7 p.p. (2 percent at the mean) while early voting increases by roughly the same magnitude. Thus, overall turnout is not measurably affected by the change on average. While the size of the decline in in-person turnout is very close to our estimate, we find a significant drop in overall turnout. The probable explanation for the different results is that unlike voters in Munich, North Carolina voters are explicitly notified of changes to their polling location. This may alleviate the shift toward nonvoting due to inattentiveness, while also encouraging turnout by inducing previous nonvoters to cast a ballot.

We can also compare our estimated transportation effect with the impact of distance to the polling location estimated by [Cantoni \(2020\)](#). [Cantoni](#) uses data from Massachusetts and Minnesota to estimate a spatial regression discontinuity model at precinct borders. The estimates suggest that increasing the distance to the polling place by one standard deviation (SD), or 0.25 miles, reduces turnout by 1 to 3 p.p. In our setting, the SD of the street distance to the polling place is slightly lower (0.34 kilometers or 0.21 miles). Using the estimate for *Street Distance* from Column (2) of Appendix Table [B.3](#), we find that a one SD increase in distance reduces in-person turnout by 1.4 p.p. and contemporaneous overall turnout by 0.36 p.p., which is clearly smaller than [Cantoni](#) estimate.

8.2. Policy Implications

Election administrators' goal in Munich is to facilitate access to polling places as much as possible. Accessibility has been primarily understood in terms of precinct sizes, proximity to the polls, and (in more recent years) barrier-free access for individuals with physical impairments. Our results

²⁷[Brady and McNulty \(2011\)](#) report that 1.4 p.p. of the 1.85 p.p. reduction is due to the search cost effect.

suggest that changing polling locations, even for the purpose of improving accessibility, constitutes an overlooked hurdle to voting. While our estimates imply that polling place reassignments depress turnout only marginally and not persistently, and are therefore unlikely to impact a parliament's composition by altering the proportion of party votes, it is conceivable that they may impact direct mandates. Direct mandates are contested on a plurality rule in single-member constituencies during State and Federal Elections and are highly competitive at times. For example, in the 2018 State Election, the direct mandate in the constituency *Munich-Moosach* was won by a margin of only 63 votes (0.09 p.p.) by the Green candidate.

Our findings suggest two drivers of the decline in overall turnout. First, the decision to vote appears only marginally affected by the change in distance to the polls and primarily driven by the change in polling location itself (search effect). Second, inattentiveness to reassignments pushes individuals to temporarily abstain from turning out. Both channels could be mitigated by actively considering reassignments as a threat to accessibility and aiming at minimizing the number of reassignments.

Moreover, if voters choose not to vote because they missed the deadline for requesting mail-in ballots, an additional notification could alleviate the effect; for instance, by informing citizens of polling place relocations separately from the usual election notification. Corroborating the results of Clinton et al. (2021), Haspel and Knotts (2005) present correlational evidence from the 2001 mayoral race in Atlanta (US) suggesting that notification letters sent to voters whose polling place has been moved increase the likelihood of casting a ballot.

Our results also highlight the role of convenience voting in compensating for the persistent decline in turnout at the polling place. Convenience voting is rather uncommon by international comparison, with only 17 percent of European countries, including Germany, enabling mail-in voting for all eligible voters (Heinmaa, 2020).²⁸ Thus, in contexts in which substitution between modes of voting is limited, the negative consequences of reassignments for overall participation are likely to be larger and persistent, underscoring the importance of monitoring reassignments.

9. Conclusion

Voting is the backbone of democracy. Yet, the likelihood of a pivotal vote is negligible, raising the possibility that even small and seemingly innocuous changes to voting costs affect electoral turnout. We exploit a natural experiment in the city of Munich (Germany) to evaluate how the

²⁸Of the 48 European countries studied by Heinmaa (2020), 29 percent enable mail-in or early voting for all eligible voters.

relocation of polling places affects electoral turnout. Using precinct-level and address-level data from eight elections held between 2013 and 2020, we find that moving a polling place depresses turnout by 0.5–0.6 p.p., on average. The decline in polling place turnout by 0.8–1.1 p.p. is only partially compensated by an increase in mail-in votes by 0.3–0.5 p.p. These effects can be explained by a combination of search costs due to unfamiliarity with the new polling place and altered transportation costs due to the change in proximity to the polls. We show that treated and untreated precincts do not differ significantly in their sociodemographic characteristics once conditioning on election and precinct fixed effects. We also find no evidence of pretrends, suggesting that our estimates can be interpreted as causal.

Event study results show that the decline in overall turnout is transitory while the substitution of polling place for mail-in voting persists after the relocation of the polling place. This finding is consistent with the presence of inattentive voters, who only notice the reassignment after the deadline for requesting mail-in ballots has passed. Inattentive voters who would have switched to mail-in voting as their preferred choice either turn out at the new polling place anyway or abstain from voting. But aware of the change, these voters switch to mail-in voting in ensuing elections, recovering the temporary drop in overall participation. Thus, rather than producing a nonvoting habit, reassignments provoke a persistent substitution of in-person for mail-in voting, consistent with rational choice models of electoral turnout.

850 **References**

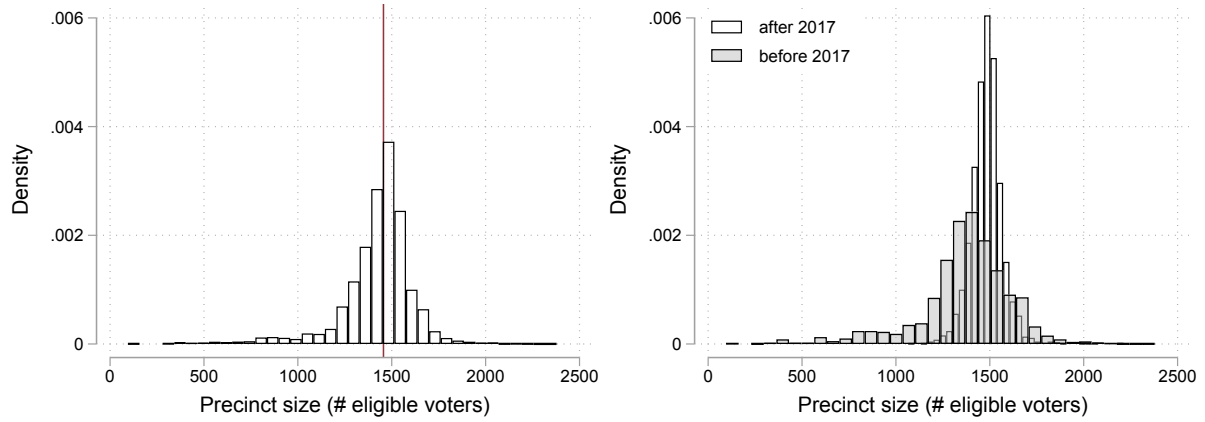
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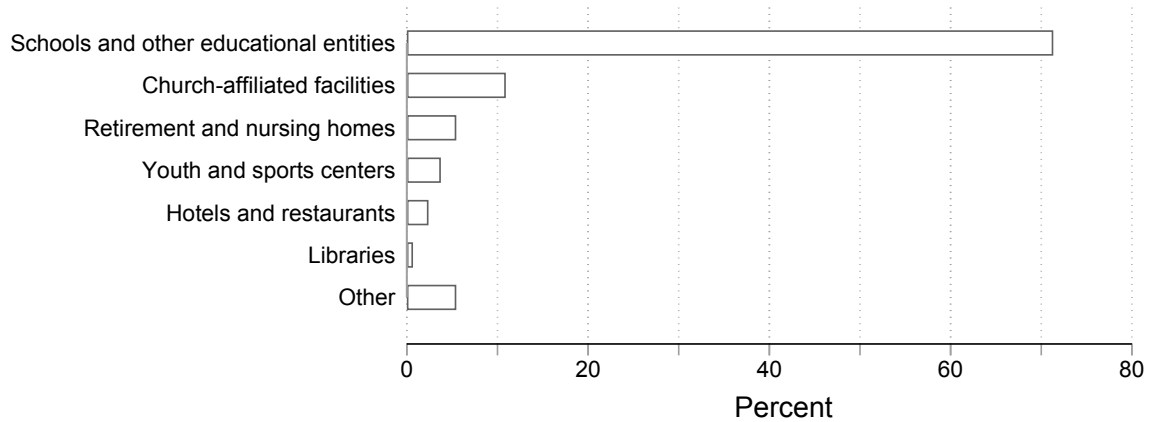
Appendix A. Figures

Figure A.1: Distribution of Precinct Sizes



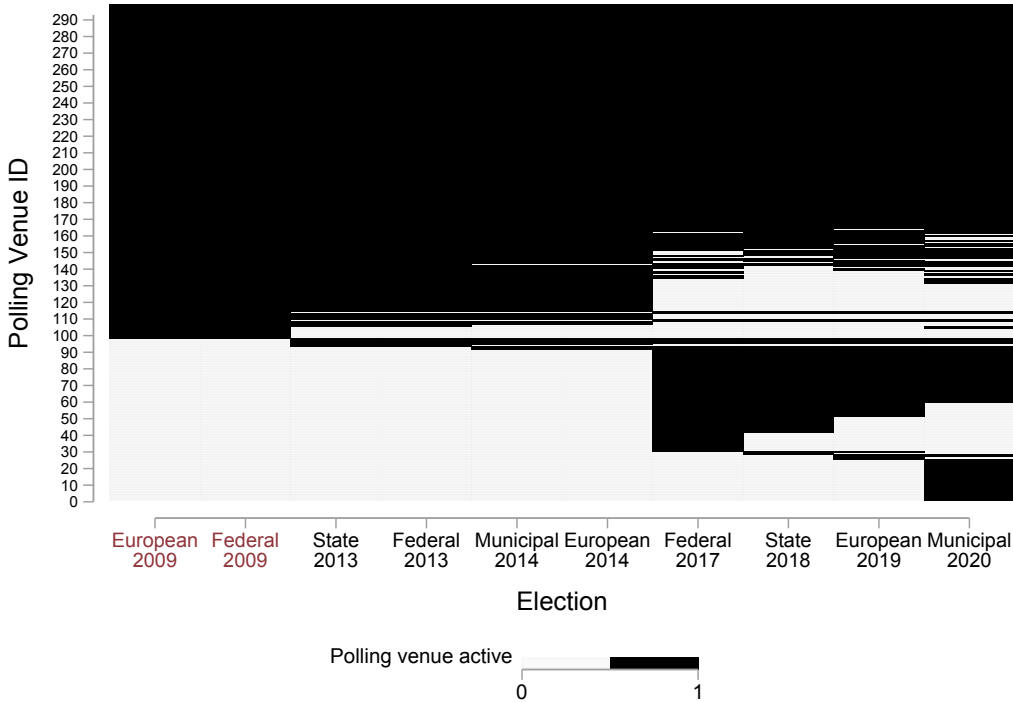
Notes: The figure plots the distribution of precinct of size (number of eligible voters) over all elections (left plot) and before and after 2017 when the Elections Office performed a major reconfiguration of precinct boundaries (right plot). Precincts are delineated according to their election-specific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

Figure A.2: Types of Polling Venues



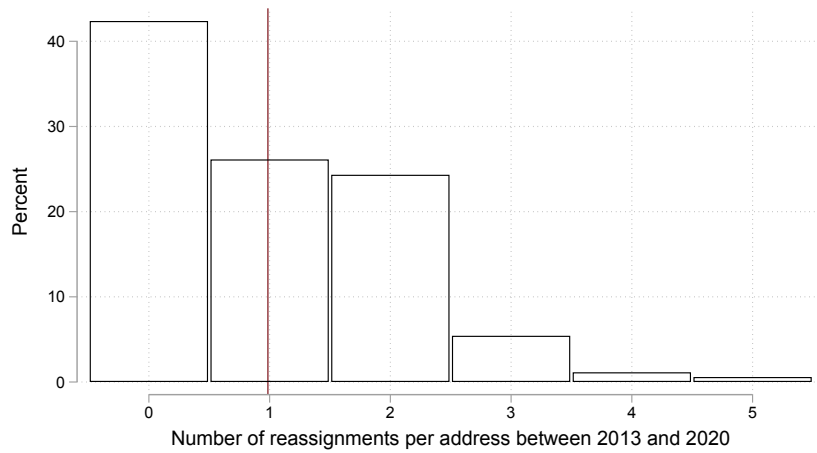
Notes: The figure shows the distribution polling venues over different categories in the eight elections held in Munich between 2013 and 2020 (293 distinct venues in total).

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



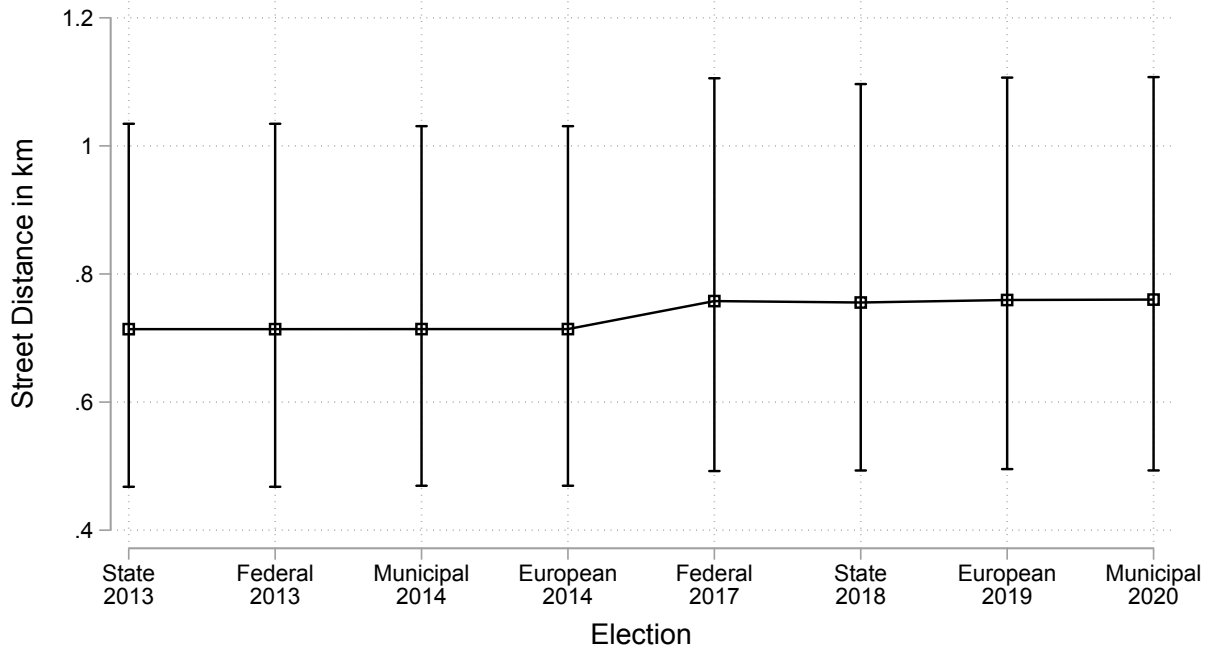
Notes: The figure illustrates in which elections venues were used as polling places. We observe 293 distinct venues between 2013 and 2020. The 2009 European and Federal Elections are not part of our estimation sample (highlighted). Six venues were active only in 2009.

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



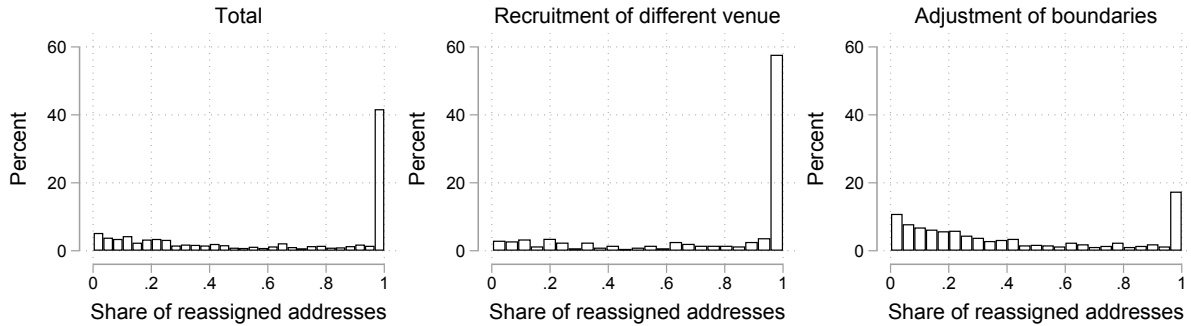
Notes: The figure plots the frequency of polling places reassignments (relative to the previous election) for residential addresses between 2013 and 2020. The vertical line highlights the mean.

Figure A.5: Median and Interquartile Range of Distance to the Polling Place



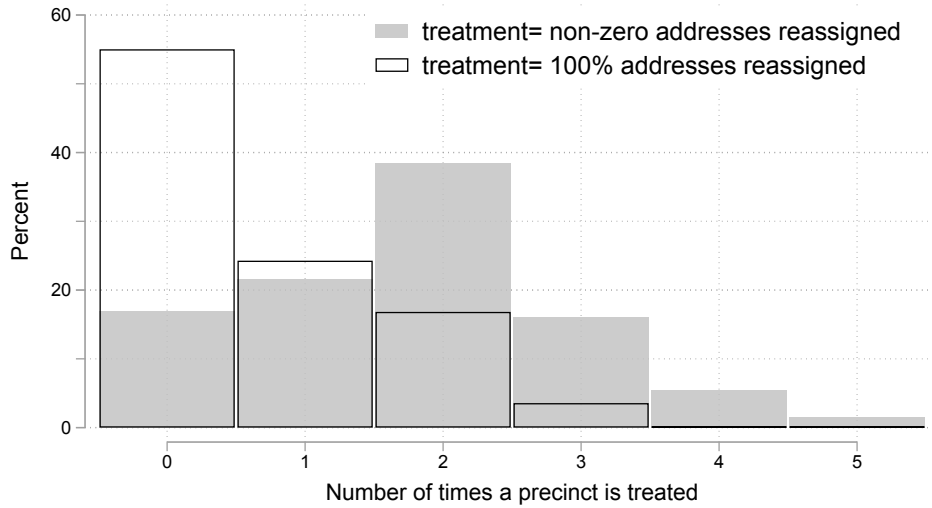
Notes: The figure plots the median and interquartile range (75th and 25th percentile) of the street distance between residential addresses of eligible voters and their designated polling place in each election between 2013 and 2020.

Figure A.6: Treatment Intensity at the Precinct Level



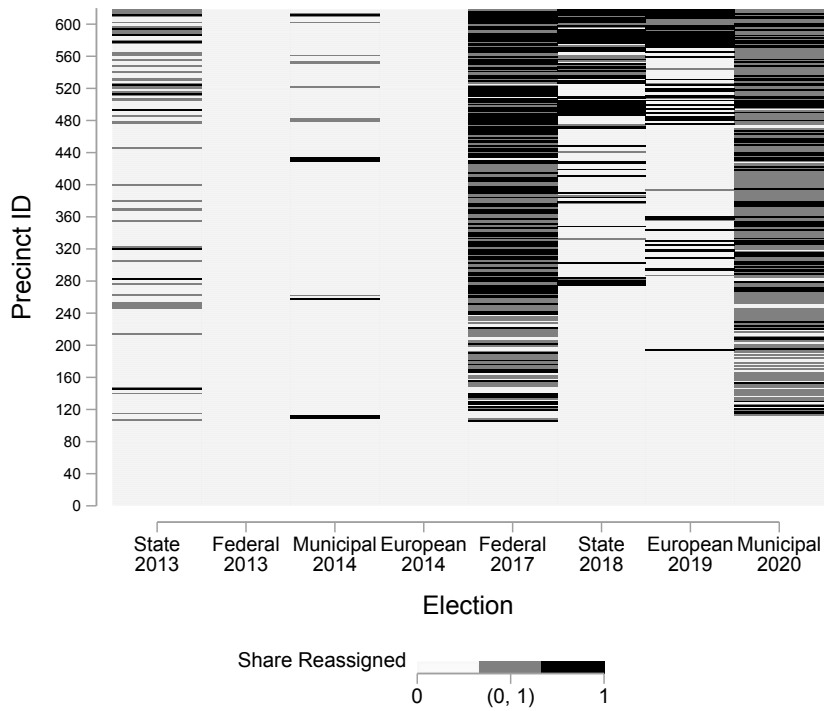
Notes: The figure shows the distribution of treatment intensity (share of residential addresses reassigned to different polling place) overall (left plot) and by reason of reassignment, i.e., due to recruitment of a different polling venue (middle) or due to reconfiguration of precinct boundaries (right). Observations with zero reassignments are excluded.

Figure A.7: Treatment Frequency



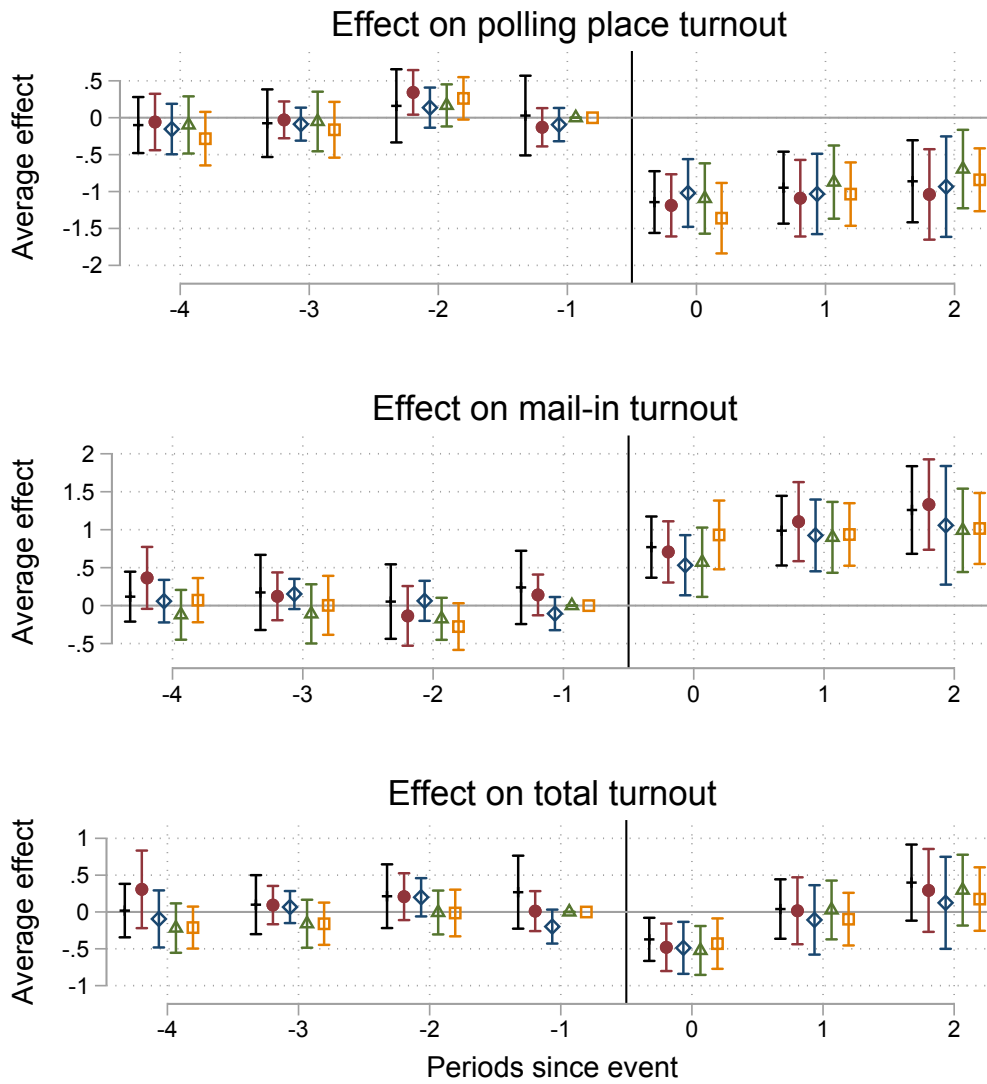
Notes: The figure shows how frequently a precinct is “treated” between 2013 and 2020 according to two treatment definitions, *i*) any positive share reassigned addresses (gray bars), and *ii*) 100 percent reassigned addresses (white bars). For instance, 55 percent of precincts are never treated and 0 percent are always treated according to the latter definition.

Figure A.8: Timing of Treatment



Notes: The figure illustrates the timing of reassignments (relative to the previous election) for the 618 precincts in our sample. Black cells indicate that the entire precinct is assigned to a different polling place. Gray cells highlight partial reassignments.

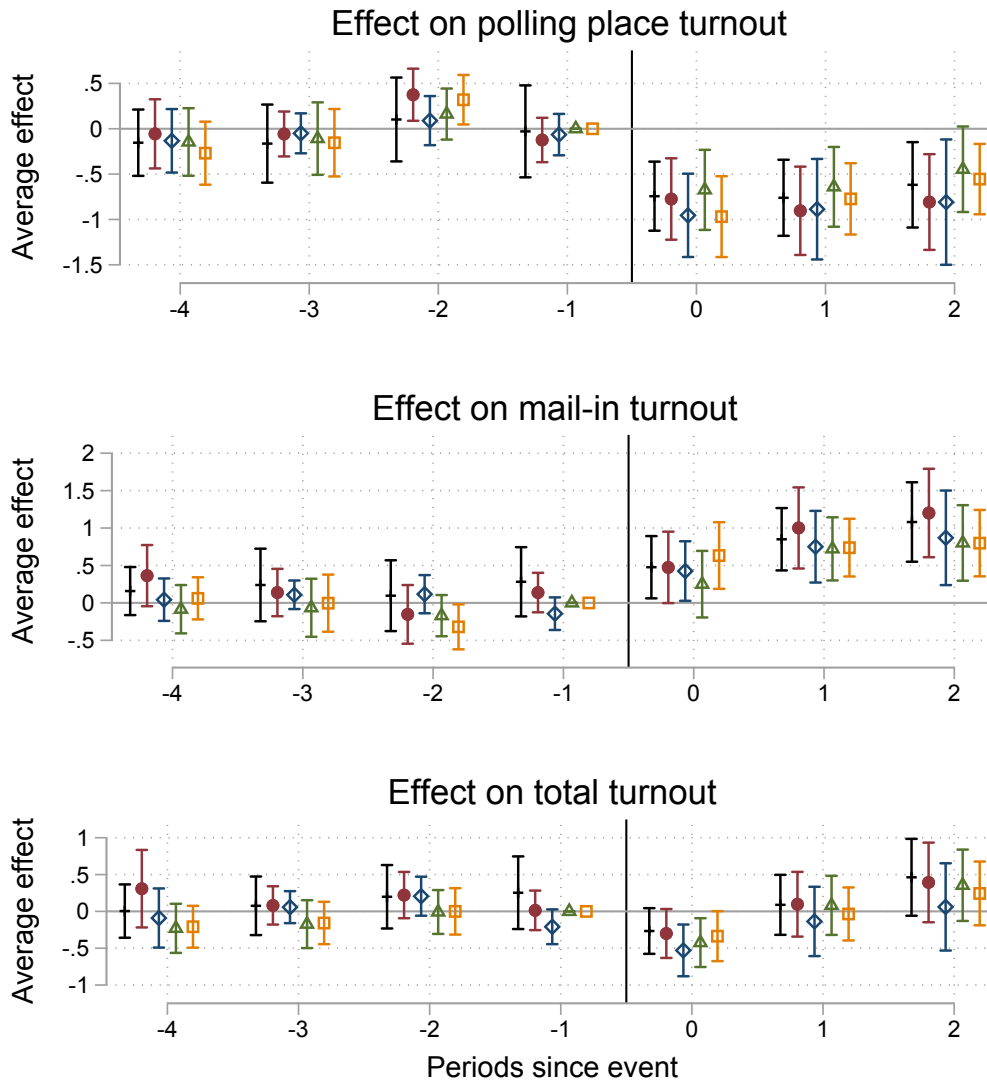
Figure A.9: Robustness of Event Study Results to Novel Estimators



- + BJS (2021)
- ◆ Callaway-Sant'Anna (2021)
- ◻ Sun-Abraham (2020)
- de Chaisemartin-D'Haultfoeuille (2020)
- ▲ TWFE OLS

Notes: The figure presents event study results estimated using TWFE-OLS as well as the estimators suggested by [Borusyak et al. \(2021\)](#), [Callaway and Sant'Anna \(2021\)](#), [Sun and Abraham \(2021\)](#), and [de Chaisemartin and D'Haultfoeuille \(2020\)](#). The event is defined as the first time in which the entire precinct is reassigned to a different polling place (Equation 2). All specifications include controls listed in Section 4.2. Confidence intervals reported at the 95% level.

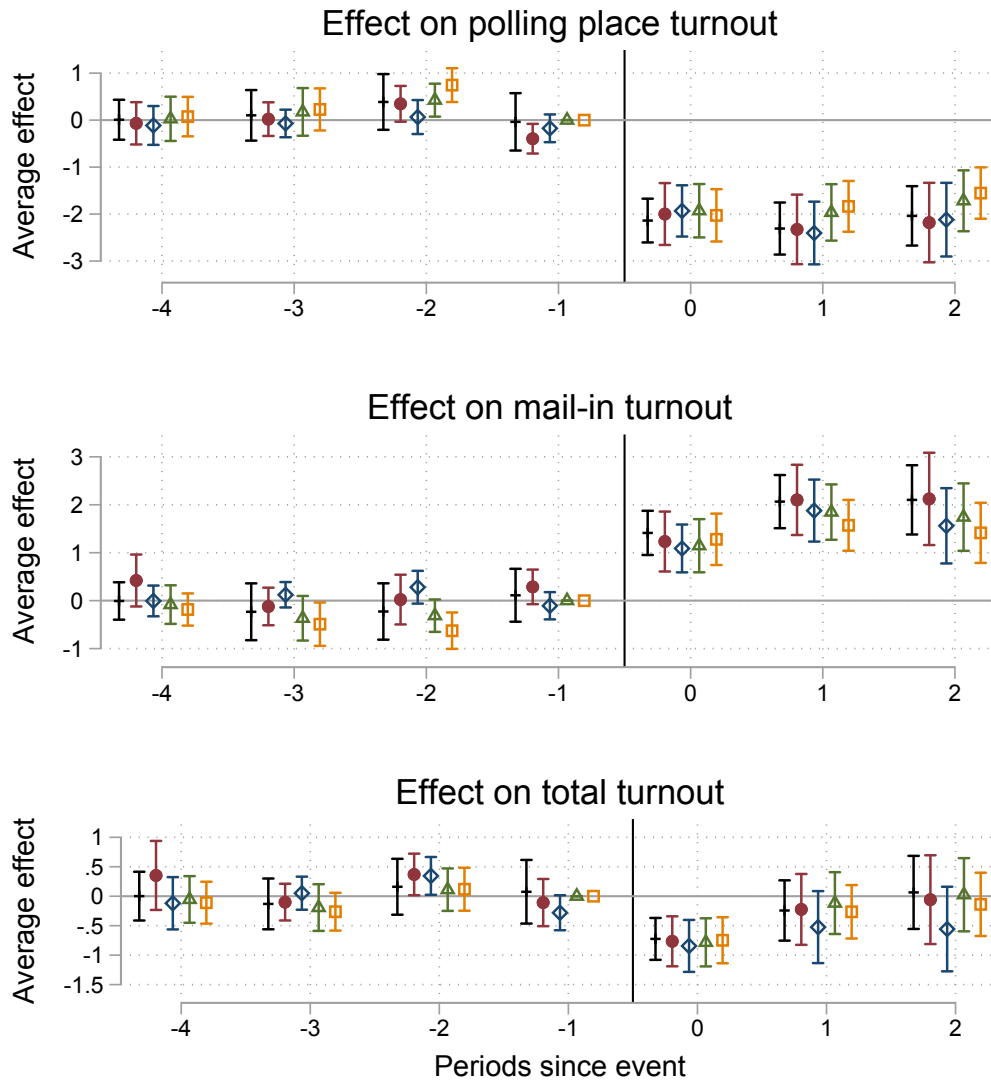
Figure A.10: Event Study Results Absorbing the Transportation Effect



- + BJS (2021)
- ◆ Callaway-Sant'Anna (2021)
- ◻ Sun-Abraham (2020)
- de Chaisemartin-D'Haultfoeuille (2020)
- ▲ TWFE OLS

Notes: The figure presents event study results controlling for the log of street distance to absorb the transportation effect. The event is defined as the first time in which the entire precinct is reassigned to a different polling place (Equation 2). Estimates are obtained using TWFE-OLS as well as the estimators suggested by [Borusyak et al. \(2021\)](#), [Callaway and Sant'Anna \(2021\)](#), [Sun and Abraham \(2021\)](#), and [de Chaisemartin and D'Haultfoeuille \(2020\)](#). All specifications include controls listed in Section 4.2. Confidence intervals reported at the 95% level.

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



- + BJS (2021)
- ◆ Callaway-Sant'Anna (2021)
- ◻ Sun-Abraham (2020)
- de Chaisemartin-D'Haultfoeuille (2020)
- ▲ TWFE OLS

Notes: The figure presents event study results estimated on a sample restricted to never-treated precincts and precincts for which the polling place reassignment resulted in an *increase* in average distance. The event is defined as the first time in which the entire precinct is reassigned to a different polling place (see Equation 2). Estimates are obtained using TWFE-OLS as well as the estimators suggested by [Borusyak et al. \(2021\)](#), [Callaway and Sant'Anna \(2021\)](#), [Sun and Abraham \(2021\)](#), and [de Chaisemartin and D'Haultfoeuille \(2020\)](#). All specifications include controls listed in Section 4.2. Confidence intervals reported at the 95% level.

Table B.1: Summary Statistics of Precinct Characteristics

	Mean	Std. Dev.	Min	p25	Median	p75	Max
Outcome Variables							
Polling Place Turnout	34.24	9.04	9.94	26.18	35.54	41.70	55.86
Mail-in Turnout (Requested Polling Cards)	28.92	7.64	4.01	23.10	29.46	34.70	51.99
Overall Turnout	63.15	14.57	15.10	51.20	65.27	75.26	91.72
Variables of Interest							
Avg. Linear Distance to the Polling Place (in km)	0.52	0.27	0.11	0.32	0.46	0.64	2.69
Avg. Street Distance to the Polling Place (in km)	0.71	0.34	0.16	0.47	0.63	0.87	2.83
Share of Reassigned Residential Addresses	0.14	0.32	0.00	0.00	0.00	0.00	1.00
Reassigned (Precinct Boundary Adjustments)	0.05	0.19	0.00	0.00	0.00	0.00	1.00
Reassigned (Recruitment of Polling Venue)	0.08	0.26	0.00	0.00	0.00	0.00	1.00
Other Precinct Characteristics							
Number of Residents	2,428.04	403.37	757.91	2,169.00	2,324.62	2,590.74	6,272.00
% Residents Eligible to Vote	65.35	9.15	24.62	60.22	66.42	71.70	86.93
% Non-native German Residents	14.68	4.35	5.50	11.70	13.48	16.45	35.78
% Native German Residents	59.77	11.35	21.00	52.75	61.80	68.11	83.97
% EU Foreigners	12.90	3.97	4.00	10.13	12.38	14.99	36.05
% Non-EU Foreigners	12.66	6.18	1.91	7.97	11.49	16.06	50.82
% Single Residents	49.73	7.34	35.28	43.72	48.84	55.02	80.20
% Married Residents	37.29	6.49	15.50	32.28	37.43	42.77	51.84
% Electorate Aged 18–24	8.74	2.87	2.41	7.20	8.25	9.64	49.07
% Electorate Aged 25–34	21.15	6.57	7.40	15.73	20.83	26.01	42.30
% Electorate Aged 35–44	17.92	4.00	6.30	15.23	17.37	20.08	34.70
% Electorate Aged 45–59	24.62	3.97	4.85	21.97	24.40	27.25	45.32
% Electorate Aged 60+	27.57	8.39	2.61	21.30	27.57	33.29	63.80
% Germans in the Electorate	91.71	9.13	53.61	84.19	97.30	100.00	100.00
% EU Foreigners in the Electorate	8.29	9.13	0.00	0.00	2.70	15.81	46.39
% Households with Children	17.53	6.08	5.31	13.35	16.69	20.43	58.75
Avg. Duration of Residence	21.69	4.45	6.80	18.53	21.72	24.51	45.11
Avg. Quoted Rent per sqm	17.42	4.54	6.69	13.67	16.45	20.30	43.92
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 of Event Study Results to Different Levels of Clustering and Fixed Effects

	(1) Cluster Precinct (Baseline)	(2) TW Cluster Precinct + Election-District	(3) Wild Cluster Bootstrap Precinct	(4) Wild Cluster Bootstrap District	(5) Wild Cluster Bootstrap District
Panel A: Turnout at the Polling Place					
$t - 4$	-0.12 (0.20)	-0.12 (0.21)	-0.12 [0.563]	-0.12 [0.512]	0.04 [0.853]
$t - 3$	-0.04 (0.21)	-0.04 (0.23)	-0.04 [0.847]	-0.04 [0.853]	-0.03 [0.886]
$t - 2$	0.16 (0.14)	0.16 (0.16)	0.16 [0.265]	0.16 [0.334]	0.01 [0.931]
t	-1.09*** (0.24)	-1.09*** (0.26)	-1.09*** [0.000]	-1.09*** [0.000]	-1.00*** [0.001]
$t + 1$	-0.89*** (0.25)	-0.89*** (0.28)	-0.89*** [0.002]	-0.89** [0.018]	-0.91*** [0.001]
$t + 2$	-0.70** (0.27)	-0.70** (0.32)	-0.70** [0.018]	-0.70* [0.065]	-0.77** [0.030]
R^2	0.96	0.96	0.96	0.96	0.97
Panel B: Turnout via Mail					
$t - 4$	-0.11 (0.17)	-0.11 (0.20)	-0.11 [0.522]	-0.11 [0.444]	-0.26 [0.188]
$t - 3$	-0.12 (0.20)	-0.12 (0.24)	-0.12 [0.563]	-0.12 [0.583]	-0.03 [0.805]
$t - 2$	-0.18 (0.14)	-0.18 (0.20)	-0.18 [0.218]	-0.18 [0.367]	-0.07 [0.584]
t	0.53** (0.24)	0.53* (0.27)	0.53** [0.020]	0.53* [0.081]	0.58** [0.025]
$t + 1$	0.90*** (0.24)	0.90*** (0.26)	0.90*** [0.000]	0.90** [0.011]	0.92*** [0.001]
$t + 2$	0.98*** (0.28)	0.98*** (0.33)	0.98*** [0.000]	0.98*** [0.005]	1.08*** [0.000]
R^2	0.95	0.95	0.95	0.95	0.96
Panel C: Overall Turnout					
$t - 4$	-0.23 (0.17)	-0.23 (0.19)	-0.23 [0.183]	-0.23 [0.156]	-0.22 [0.221]
$t - 3$	-0.16 (0.17)	-0.16 (0.18)	-0.16 [0.330]	-0.16 [0.358]	-0.06 [0.721]
$t - 2$	-0.02 (0.16)	-0.02 (0.19)	-0.02 [0.899]	-0.02 [0.927]	-0.05 [0.721]
t	-0.56*** (0.17)	-0.56*** (0.20)	-0.56*** [0.001]	-0.56*** [0.002]	-0.42** [0.025]
$t + 1$	0.00 (0.20)	0.00 (0.22)	0.00 [0.986]	0.00 [0.983]	0.01 [0.960]
$t + 2$	0.29 (0.24)	0.29 (0.26)	0.29 [0.256]	0.29 [0.342]	0.32* [0.075]
R^2	0.99	0.99	0.99	0.99	0.99
Observations	4,360	4,360	4,360	4,360	4,360
Number of Clusters	618	618+200	618	25	25
Precinct FE	×	×	×	×	×
Election FE	×	×	×	×	×
Election-District FE					×

Notes: The table presents robustness checks for the baseline event study specification (Equation 2), estimated in Column (2) of Table 3. The event is defined as the first time in which the entire precinct is reassigned to a different polling place. Column (1) replicates the baseline results with standard errors (SE) clustered at the precinct level for comparison. Column (2) uses two-way clustered SE at the level of precincts and district-elections (reported in parentheses). Column (3) uses wild cluster bootstrap (WCB) at the precinct level. Column (4) uses WCB at the district level. Column (4) uses WCB at the district level and replaces election fixed effects with election-district fixed effects. All WCB p-values for the hypothesis that the estimate is equal to zero employ 1000 repetitions and are reported in square brackets. All specifications include controls listed in Section 4.2. Regressions are weighted with the number of eligible voters. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table B.3: Robustness to Alternative Distance Measures

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Turnout at the Polling Place						
Reassigned	-0.47*** (0.11)	-0.48*** (0.12)	-0.47*** (0.12)	-0.51*** (0.11)	-0.52*** (0.12)	-0.51*** (0.12)
Log Street Distance	-3.37*** (0.24)					
Street Distance		-4.11*** (0.40)	-7.90*** (0.82)			
Street Distance (Squared)			1.84*** (0.38)			
Log Linear Distance				-3.06*** (0.23)		
Linear Distance					-4.84*** (0.61)	-9.40*** (0.80)
Linear Distance (Squared)						2.73*** (0.39)
R^2	0.97	0.97	0.97	0.97	0.97	0.97
Panel B: Turnout via Mail						
Reassigned	0.07 (0.13)	0.07 (0.13)	0.06 (0.13)	0.11 (0.13)	0.11 (0.13)	0.11 (0.13)
Log Street Distance	2.35*** (0.24)					
Street Distance		3.05*** (0.37)	5.01*** (1.00)			
Street Distance (Squared)			-0.96* (0.50)			
Log Linear Distance				2.02*** (0.23)		
Linear Distance					3.33*** (0.52)	6.08*** (0.99)
Linear Distance (Squared)						-1.65*** (0.55)
R^2	0.95	0.95	0.95	0.95	0.95	0.95
Panel C: Overall Turnout						
Reassigned	-0.40*** (0.12)	-0.41*** (0.12)	-0.41*** (0.12)	-0.40*** (0.12)	-0.41*** (0.12)	-0.41*** (0.12)
Log Street Distance	-1.02*** (0.19)					
Street Distance		-1.06*** (0.25)	-2.88*** (0.73)			
Street Distance (Squared)			0.89*** (0.33)			
Log Linear Distance				-1.05*** (0.18)		
Linear Distance					-1.51*** (0.33)	-3.33*** (0.71)
Linear Distance (Squared)						1.08*** (0.36)
R^2	0.99	0.99	0.99	0.99	0.99	0.99
Observations	4,944	4,944	4,944	4,944	4,944	4,944

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

Table B.4: Heterogeneity across Precinct Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Polling Place Turnout								
Reassigned	-0.47*** (0.11)	-0.47*** (0.11)	-0.45*** (0.12)	-0.47*** (0.12)	-0.47*** (0.11)	-0.35** (0.14)	-0.68*** (0.14)	-0.35* (0.18)
Log Street Distance	-3.37*** (0.24)	-3.37*** (0.24)	-3.38*** (0.24)	-3.37*** (0.24)	-3.35*** (0.23)	-3.38*** (0.24)	-3.39*** (0.24)	-3.38*** (0.24)
Reassigned								
×Electorate Aged 18–24		0.32*** (0.09)						
×Electorate Aged 25–59			-0.13 (0.10)					
×Electorate Aged 60+				-0.02 (0.11)				
×Households w/ Children					0.40*** (0.11)			
×Avg. Quoted Rent per sqm						-0.17 (0.12)		
×Conservative Precinct							0.52** (0.22)	
×Left-wing Precinct								-0.20 (0.22)
R^2	0.97	0.97	0.97	0.97	0.97	0.97	0.97	0.97
Panel B: Turnout via Mail (requested)								
Reassigned	0.07 (0.13)	0.07 (0.13)	-0.00 (0.12)	-0.00 (0.13)	0.07 (0.13)	-0.22 (0.16)	0.39** (0.16)	-0.22 (0.18)
Log Street Distance	2.35*** (0.24)	2.35*** (0.24)	2.37*** (0.24)	2.37*** (0.24)	2.33*** (0.24)	2.36*** (0.24)	2.38*** (0.24)	2.37*** (0.24)
Reassigned								
×Electorate Aged 18–24		-0.19** (0.09)						
×Electorate Aged 25–59			0.43*** (0.10)					
×Electorate Aged 60+				-0.35*** (0.10)				
×Households w/ Children					-0.33*** (0.12)			
×Avg. Quoted Rent per sqm						0.42*** (0.12)		
×Conservative Precinct							-0.81*** (0.22)	
×Left-wing Precinct								0.48** (0.22)
R^2	0.95	0.95	0.95	0.95	0.95	0.95	0.95	0.95
Panel C: Overall Turnout								
Reassigned	-0.40*** (0.12)	-0.40*** (0.12)	-0.45*** (0.12)	-0.48*** (0.12)	-0.40*** (0.12)	-0.57*** (0.14)	-0.29** (0.14)	-0.57*** (0.16)
Log Street Distance	-1.02*** (0.19)	-1.02*** (0.19)	-1.00*** (0.19)	-1.00*** (0.19)	-1.02*** (0.19)	-1.01*** (0.19)	-1.01*** (0.19)	-1.01*** (0.19)
Reassigned								
×Electorate Aged 18–24		0.14* (0.07)						
×Electorate Aged 25–59			0.31*** (0.08)					
×Electorate Aged 60+				-0.37*** (0.09)				
×Households w/ Children					0.07 (0.11)			
×Avg. Quoted Rent per sqm						0.25** (0.11)		
×Conservative Precinct							-0.29 (0.19)	
×Left-wing Precinct								0.28 (0.19)
R^2	0.99	0.99	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	4,944	4,944

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 by the number of polling cards requested. *Reassigned* is separately interacted with z-transformed sociodemographic characteristics and binary indicators identifying precincts with above-median voting shares for conservative and left-wing parties in the 2013 State Election, respectively. All specifications control for lag terms of *Reassigned* and *Log Street Distance* in addition to precinct-level controls (see Section 4.2). Regressions are weighted with the number of eligible voters. Standard errors are clustered at the precinct level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table B.5: Effect of Reassignments on Party Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
	Party votes (at the polling place) relative to number of eligible voters													
	Party votes (at the polling place) relative to number of eligible voters													
Reassigned	0.02 (0.03)	0.03 (0.03)	-0.17*** (0.06)	-0.11* (0.06)	-0.12* (0.06)	-0.07 (0.06)	-0.16*** (0.04)	-0.14*** (0.04)	-0.17** (0.07)	-0.07 (0.06)	-0.04** (0.02)	-0.03* (0.02)	-0.14*** (0.04)	-0.12*** (0.05)
Log Street Distance						-0.50*** (0.11)		-0.28*** (0.08)		-1.17*** (0.11)		-0.15*** (0.03)		-0.20*** (0.08)
R ²	0.90	0.90	0.93	0.94	0.95	0.95	0.91	0.91	0.94	0.95	0.87	0.87	0.84	0.84
Mean dep. var.	1.62	1.62	7.52	7.52	7.20	7.20	2.23	2.23	9.59	9.59	0.87	0.87	2.09	2.09
	Panel B: Share of party votes in total votes (at the polling place)													
Reassigned	0.18** (0.07)	0.17** (0.07)	-0.25* (0.13)	-0.25* (0.13)	-0.03 (0.14)	-0.10 (0.14)	-0.12 (0.09)	-0.12 (0.09)	0.27* (0.14)	0.35** (0.14)	-0.12** (0.04)	-0.10** (0.05)	-0.11 (0.10)	-0.11 (0.10)
Log Street Distance					0.82*** (0.22)			0.02 (0.17)		-0.84*** (0.24)		-0.23*** (0.07)		0.11 (0.17)
R ²	0.87	0.87	0.94	0.94	0.96	0.96	0.90	0.90	0.94	0.94	0.84	0.84	0.82	0.82
Mean dep. var.	4.56	4.56	23.46	23.46	21.80	21.80	5.95	5.95	28.02	28.02	2.55	2.55	6.04	6.04
Observations	4,944	4,944	4,944	4,944	4,944	4,944	4,944	4,944	4,944	4,944	4,944	4,944	4,326	4,326

Notes: The dependent variable in Panel A is the number of party votes (at the polling place) relative to the number of eligible voters (0–100) for each party, including *DIE LINKE* (Socialists), *SPD* (Social Democrats), *Grüne* (Greens), *FDP* (Liberal Democrats), *CSU* (Conservatives), *Freie Wähler* (Conservatives), and *AfD* (Nationalists). The dependent variable in Panel B is the percentage share of party votes relative to all votes at the polling place for each party, respectively. Specifications control for lag terms of *Reassigned* (odd and even columns) and *Log Street Distance* (even columns) in addition to precinct-level controls (see Section 4.2). Regressions are weighted with the number of eligible voters. Standard errors are clustered at the precinct level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Appendix C. Elections in Munich

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

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

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

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

970 **Appendix D. A Calculus of Voting Framework**

We present a simple model of electoral participation drawing on the “calculus of voting” framework due to (Downs, 1957), Tullock (1967), and Riker and Ordeshook (1968). For simplicity, we omit individual and election indices in the following. Denote $V \in \{N, P, M\}$ an individual’s voting decision in an election. She can either vote in person at the polling place (P), which involves cost $c_p > 0$ or vote by mail (M), incurring cost $c_m > 0$. She may also abstain from voting (N), which generates utility $q \geq 0$. Voting by mail and in person yield utility $B_m \geq 0$ and $B_p \geq 0$, respectively. Voting benefits include the expected gain if the preferred party wins a greater number of seats in the election as well as any direct utility from the act of voting itself, e.g., from fulfilling a civic duty, which may differ between the modes of voting. The individual abstains from voting if and only if her net benefit of voting is (weakly) negative, i.e., $B_p/(c_p + q) \leq 1$ and $B_m/(c_m + q) \leq 1$. In contrast, she votes by mail if and only if her net benefit of mail-in voting is positive and polling place voting is less attractive than mail-in voting, i.e., $B_m/(c_m + q) > 1$ and $B_m/(c_m + q) > B_p/(c_p + q)$. Similarly, she decides to vote at the polling place if $B_p/(c_p + 1) > 1$ and $B_m/(c_m + q) < B_p/(c_p + q)$.

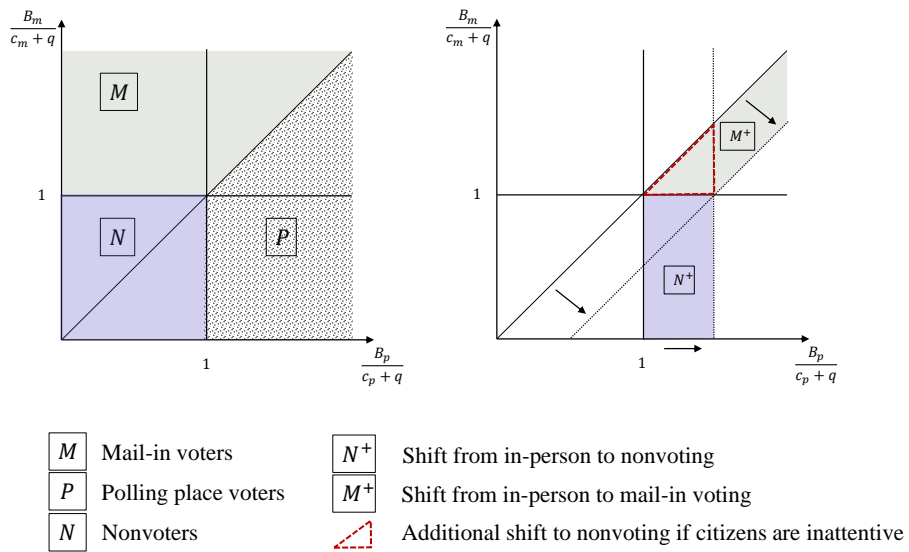
985 The left diagram in Figure D.1 plots the benefit of voting by mail against the benefits of voting in person relative to the costs of voting. An individual will vote if and only if her cost-benefit vector lies above the horizontal or the vertical unity lines, where $B_m/(c_m + q) > 1$ and $B_p/(c_p + 1) > 1$, respectively. If additionally, the vector lies above the 45-degree line, then she will vote by mail (M), and chose polling place voting (P) otherwise. In the area below the intersection of the unity lines, the benefits of voting are always negative and the individual will not vote (N). The shades in the figure illustrate the different voting decisions according to different cost-benefit configurations. If one imagines a distribution of Munich’s population over the depicted plane, then historically roughly 38 percent of eligible voters lie somewhere in the nonvoting area (N), 33 percent are in polling place voter area (P) and the remaining 29 find themselves in the mail-in voter area (M).

995 For illustrative purposes, suppose a positive shock to the cost of voting at the polling place, $\varepsilon > 0$, so that $c'_p = c_p + \varepsilon$. 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 a downward parallel shift of the diagonal line and a rightward shift of the vertical line, as illustrated in the right diagram of Figure D.1. As a result, some voters will switch from polling place to mail-in
1000 voting (area labeled M^+). This is the case if the reduction in relative cost is large enough to make voting by mail more attractive than voting in person, $B_m/B_p > (c_m + q)/(c'_p + q)$, and the benefit of casting a mail-in ballot outweighs its costs, $B_m/(c_m + q) > 1$. If the benefit of voting by mail is not sufficient to outweigh the costs, the individual will switch to nonvoting if the cost shock is large enough to make polling place voting unattractive, i.e., $B_p/(c'_p + q) \leq 1$ and $B_m/(c_m + q) \leq 1$. On
1005 the other hand, the area labeled N^+ represents 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.

If voters are inattentive to polling place reassignments by not or only carelessly reading the election notification, which is mailed several weeks before Election Day, they will miss the deadline for
1010 requesting mail-in ballots. Consequently, inattentiveness attenuates the shift from in-person to mail-in voting—as some voters choose to go to the new polling place anyway—and amplifies the shift towards nonvoting, as some voters who would have voted by mail entirely abstain from casting a ballot. The additional portion of nonvoters is highlighted by the red triangle in the right diagram.

1015 To what extent do these adjustments carry over to subsequent elections? The theory suggests two mechanisms that may be at play. First, relocating polling places may permanently alter the cost of in-person voting. This is obvious, for instance, when the electorate is permanently reassigned to a polling place that is located further away. Similarly, search costs are likely to persist unless people familiarize themselves with the new location between two elections. Thus, the relative
1020 cost reduction of mail-in compared to in-person voting is likely to persist and induce a lasting substitution effect. If the absolute cost increase for voting at the polling place is sufficiently high, then voters may entirely abstain from voting today and in the future. However, the initial election may be different from subsequent ones due to inattentive voters. Some inattentive voters will only temporarily abstain from voting or cast their ballot at the new polling location and revert to mail-
1025 in voting in the following elections. Consequently, a drop in aggregate turnout may be (partly) recovered and the substitution of in-person for mail-in voting reinforced over time.

Figure D.1: Effect of Increasing Cost of In-Person Voting



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

A second mechanism that could drive persistent changes in voting behavior is habit formation. Habit formation means that the act of (non)voting itself increases its consumption value and therefore increases the likelihood of nonvoting 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 voters who switch to nonvoting as a consequence of the reassignment experience an increase in the utility of nonvoting q . As there are compelling reasons to anticipate that relocations permanently alter voting costs, it is not possible to empirically separate the effect of habit formation from increased costs in our setting. However, we are able to test the key prediction of habit formation, namely: if (non)voting is habit forming, then any change in voter turnout must persist in the subsequent election(s), even in a hypothetical scenario in which the cost of voting was completely restored to pre-treatment conditions. Empirically, the magnitudes of these effects depend on the distribution of the population over different cost-benefit vectors and the magnitude of the reassignment shock.