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# **Living Together, Voting Together: Voters moving in together before an election have higher turnout**

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The register data for this study is stored on servers at Statistics Denmark. Due to security and privacy implications, the data cannot be made publicly available.

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# **Living Together, Voting Together: Voters moving in together before an election have higher turnout**

## **Abstract**

Scholars have long noted that couples are more likely to vote compared to individuals who live alone and that partners' turnout behavior is strongly correlated. With a large administrative dataset containing detailed information about validated turnout and the timing of individuals moving in together, we find evidence for a substantial and robust increase in turnout after cohabitation. We exploit the fact that two-voter households moving in together right before an election are comparable to two-voter households moving in together right after the election. Depending on model specification, turnout increases by 3.5 to 10.6 percentage points in the months after taking up cohabitation. Voters are mobilized regardless of their own and their cohabitant's turnout behavior in a previous election. The results are robust to several robustness checks, including benchmarking with singles who move to mitigate the cost of moving. The results highlight the importance of social norms and the household's essential role as a proximate social network that increases turnout.

**Keyword:** turnout; social norms; cohabitation; participation; social influence

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Scholars have for many years observed that eligible voters who share residence tend to vote or abstain jointly (Anderson 1943; Glaser 1959; Wolfinger and Rosenstone 1980; Stoker and Jennings 1995). For instance, Wolfinger and Rosenstone (1980) observed that married couples vote at higher rates and argued that marriage is the most important type of interpersonal influence. Stoker and Jennings (1995) argued that one should focus on the time of transition—when voters go from being unmarried to married—and showed that newly married couples tend to have concordant turnout behavior. In many modern societies, couples join households and build strong social ties a long time before getting married. Therefore, we focus on turnout of two-voter households regardless of their marital status in order to demonstrate a robust, substantial, and positive relationship between cohabitation and turnout.

Why should cohabitation affect turnout? It could be the case that cohabitation has an effect because inhabitants of a two-voter household affect each other regarding behavioral alignment; that is, either both vote, or both abstain from voting (Zuckerman 2005; Fowler 2005; Nickerson 2008; Klobstad 2010). It could also be the case that voters who live together are exposed to the same contextual factors such as distance to the polling location and strength of the local campaigning (Cho and Rudolph 2008). On the other hand, the correlation in two-voter households' turnout could simply be due to sorting where citizens with a taste for voting (or nonvoting) tend to live with people who share their preferences.

So far, most research has had to rely on data that assessed the concordance of attitudes between couples either before or after they formed (Stoker and Jennings 2005; Alford et al. 2011; Klobstad et al. 2012; Huber and Malhotra 2017). As an alternative, experiments have demonstrated how efforts to mobilize one household member to vote can spill over to other persons within a household (Nickerson 2008; Sinclair et al. 2012; Foos and de Rooij 2016). We have learned much from both types of studies, but there has been a call for studies that use stronger designs for studying

the transition into cohabitation (Alford et al. 2011). We argue that our identification strategy brings us closer to this goal by creating comparable groups of voters who do or do not live together.

We use rich administrative data to study how cohabitation affects turnout. In the Danish public records, we have validated turnout and voters' complete residential history. The latter allows us to know where a voter lived at any given time and with whom. We compare turnout of voters who moved together in two-voter households immediately before Election Day to turnout of voters who moved together in two-voter households immediately after Election Day. We find that voters who took up cohabitation just before the election are much more likely to vote. In our simplest specification, where we just compare couples moving before and after, the increase is 3.5 percentage points when we compare voters moving in together zero to 30 days before or after an election and 8.9 percentage points when comparing voters moving in together 31 to 60 days before or after an election. The latter estimate is comparable to the very strongest known get-out-the-vote efforts as well as strong predictors of turnout such as education (Gerber et al. 2009; Wolfinger and Rosenstone 1980). We also find higher turnout after cohabitation when we focus only on voters who had someone move in with them while remaining at the same address. Finally, both past voters and nonvoters who took up cohabitation became more likely to vote regardless of whether the other person in the household is a past voter or nonvoter.

For a causal interpretation of our results, we rely on the assumption that voters moving together before or after the election are comparable. For readers unwilling to accept this assumption, we run a number of alternative specifications, where we control for covariates including time trends and benchmark against singles who move before or after the Election Day. The relationship in the zero to 30 days window is stronger in each of our alternative specifications, whereas it is largely unchanged in the 31 to 60 days window. The results are also robust to a number of additional placebo and robustness tests. Ultimately, a key limitation of our design is that we are unable to rule out all

sources of potential biases as we can only rule out one source of bias by introducing another. I.e. contextual bias vs social influence, see below for further discussion. However, our results suggest that our most conservative estimates of the mobilizing effect of turnout, underestimates the true effect of cohabitation.

### **Sorting or social influence within households?**

Voters cohabit for all sorts of reasons: They can be life partners, siblings, parents and children, or simply acquaintances who choose to live together. We take an agnostic stance regarding why people would live together and focus on all pairs of eligible voters who move together into households of two eligible voters either before or after the election. We choose to do so even though the bulk of previous research has focused on (married) couples. However, we have no theoretical argument for why the social influence should be limited to certain households.

At least three factors could explain why the turnout of people who live together correlates. First, voters could sort into households with like-minded voters. Even if most individuals do not select housemates primarily based on political attitudes or behaviors, individuals select into social networks based on a variety of factors, which in turn could correlate with political attitudes and behaviors. Alford et al. (2011) use cross-sectional survey data of couples and find strong concordance in attitudes; Iyengar et al. (2018) find spousal correspondence in attitudes has increased over time for American couples and point to mate selection as the principal reason; and analyses based on data from dating sites also consistently find that political preferences correlate with mating preferences (Klofstad et al. 2012; Huber and Malhotra 2017).

Second, cohabitation might correlate with turnout because cohabitants are exposed to similar contextual factors (Cho and Rudolph 2008; Bhatti 2012; Enos 2016). In this paper, we understand context as all the external factors that influence the decision to vote, including influences

from social networks, except for the social influence of one's cohabitant. When voters take up cohabitation, they also start sharing environment such as the intensity of local campaigning, social surroundings, and distance to the polling place. If they live in a competitive district, they are both incentivized to vote; if they move closer to the polling station, it is easy for both to vote; and if they live on a street where everyone is voting, they are both exposed to the same contextual information about what constitutes appropriate behavior and campaign information. It could also be the case that context changes because people start living together. They could start visiting restaurants or shops that they would not have visited otherwise; they could change their media diet; they could see changes in their social networks; and in a political context, they could be targeted differently by political campaigns. Living together with another voter might also reduce the cost of voting. For instance, cohabitants could accompany each other to the polling station by sharing transport (Fieldhouse and Cutts 2012).

Third, social influence between the cohabitants could have a direct influence on turnout. Following Sinclair (2012: 2), we can think of two ways in which individuals may be influenced by their social environment. Information shared in their network could alter behavior through persuasion or some other mechanism, and members of a network could simply conform to expectation of social norms. In this paper, we constrain the social environment to include only the person being lived with and the network to only the members of a two-voter household.

To see how norm compliance may factor in, we can consider the civic duty to vote. It is a strong social norm to comply with and social pressure is a strong force, best exercised in close networks such as household relations (Blais et al. 2000; Blais 2000; Sinclair 2012; Bhatti et al. 2018). Voters prioritize stable social relations and learning that a housemate plans to vote could motivate one to avoid disapproval through breaking the norm (Coleman 1988). Indeed, experimental evidence supports that conformity makes individuals adapt their opinions to those of their environment

(Carlson and Settle 2016). Furthermore, living with another potential voter will make a decision to abstain more visible and bring non-compliance of the norm of voting out into the open (Fieldhouse and Cutts 2018; Dahlgaard 2018; Bhatti et al. 2017; Bhatti et al. 2020). The first component, information-sharing, could materialize in voters providing each other with low-cost, credible political information about the election, such as who is running, for whom to vote, and the voting process, such as where and how to vote. Alternatively, one of the voters in the household could persuade an otherwise reluctant voter to vote.

### **Is there a social influence on turnout?**

Our strategy is to compare the turnout of those eligible to vote in households that formed before and after the election in order to ascertain whether turnout increases when moving in with someone else. We argue that two-voter households who move in together right before an election are comparable to two-voter households who move in together immediately after. Theoretically, compliance with norms can affect turnout in either direction. In a household where one holds the norm of voting while the other does not, they could conceivably settle on either voting or abstaining. However, Blais (2000) shows that most people share a sense of civic duty to vote even if they do not live by it, and living together with another voter may activate the norm. Therefore, we expect turnout to increase when two-voter households move in together due to social influence.

When we compare voters in households formed before the election to voters in households formed after the election, we learn about the joint effect of social influence and contextual factors that change simultaneously with cohabitation. To get even closer to the effect of increased social influence following cohabitation, we can take a focused look at a group for whom the context is arguably most stable: voters who do not move when they take up cohabitation but have someone move in with them. We call this group “*Stayers*.” These are voters for whom the social context is

close to fixed, though some will have a housemate move in right before the election while others will have one move in right after. Consequently, they are most likely to be affected only through social influence from others who join their household.

### **Overview of the potential outcomes and quantities of interest**

To provide an overview of what we can identify using all voters as well as the subgroup of *Stayers*, we stipulate the two quantities of interest that we are able to identify, as well as how they relate to the potential outcomes of different groups of households or voters. For the effect of cohabitation on all voters moving in together before the election,  $\tau$ , we compare those moving in together immediately before the election to those moving in together immediately after. For voters who stay at the same address and have someone move in with them, we use  $\tilde{\tau}$  in order to emphasize that we are changing the subgroup for which the effect is identified. We understand the potential outcomes as functions of three factors<sup>6</sup>: Unobserved heterogeneity ( $X$ ), Contextual factors ( $C$ ), and Social influence ( $S$ ).

For the first quantity of interest, the total effect of cohabitation on turnout, we need only the assumption that we can make a valid comparison between households formed before and after the election. For the second quantity of interest, the effect of social influence on turnout, we need the additional assumption that the new housemate moving in with the *Stayer* does not bring with him or her any contextual change. This could be violated if, for instance, some municipalities send additional information to new inhabitants about the polling location or if the voter moving into the household induces a change in the contextual exposure of the *Stayer*. We could imagine the *Stayer* to start visiting different areas in his or her neighborhood with changing frequencies. Thus, we emphasize that the assumption of no contextual change is a stronger assumption, and for the reader unwilling to

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<sup>6</sup> Note that we implicitly assume that the factors are additively related to each other.

accept this assumption, the effect on the *Stayers* will instead be a joint effect of social influence and contextual changes, the latter being arguably smaller than for those who physically move.

Another potential contextual factor is a disruptive effect of moving in itself. Essentially, people who move are occupied with moving both before and after a move, leaving less time to vote. The cost of moving would threaten our identification strategy if it is higher before a move than after. In this case, our estimates could be biased by the cost of moving being higher for those that move after the election than those who move before the election. On the other hand, we would underestimate the true effect of cohabitation if the cost is higher after a move than before.

To gauge the potential bias from the cost of moving, we use singles who moves as a benchmark to complement our analyses. These are voters who lived alone before and after the move so they are not exposed to a change in within household social influence. At the same time, they incur a cost of moving. If we are willing to make the strong assumption that their cost of moving is comparable to the cost for couples, we can use them as a benchmark for the cost of moving. A weaker assumption is just that the cost they experience is in the same direction as for couples. Under this assumption, we can use singles to determine whether any bias from the cost of moving causes us to underestimate or overestimate the effect of social influence. Table 1 sums up the assumptions and the identification strategies.

**Table 1: Quantities of interest, potential outcomes, and identification strategies**

Quantity of interest	Potential outcomes <sub>Scohabitants</sub>	Potential outcomes <sub>SNon-cohabitants</sub>	PO <sub>Cohabitants</sub> – PO <sub>Non-cohabitants</sub>	Identification strategy
$\tau_{Individualturnout}(cohabitation)$	Turnout (X=1, C=1, S=1) X = Unobserved heterogeneity C = Context S = Social influence	Turnout (X=1, C=0, S=0)	Turnout (X=1, C=1, S=1) – Turnout (X=1, C=0, S=0) = Turnout (C=1, S=1) – Turnout (C=0, S=0)	Comparing turnout between voters in households formed before the election and after the election
$\tilde{\tau}_{Individualturnout}(social\ influence)$	Turnout <sub>Stayers</sub> (X=1, C=1, S=1) X = Unobserved heterogeneity C = Context S = Social influence	Turnout <sub>Stayers</sub> (X=1, C=1, S=0)	Turnout <sub>Stayers</sub> (X=1, C=1, S=1) – Turnout <sub>Stayers</sub> (X=1, C=1, S=0) = Turnout <sub>Stayers</sub> (S=1) – Turnout <sub>Stayers</sub> (S=0)	Comparing turnout between voters who did not change address and lived in households formed before the election and after the election

## **Context and data**

Our study relies on data from the Danish municipal elections held on November 19, 2013. Eligible citizens are automatically registered to vote and receive a polling card with the address of their polling station no later than five days ahead of Election Day. Danish municipalities are central in the governmental service provision, and the municipalities spend about one-fourth of the total Danish GDP. The areas of municipal responsibility include care for the elderly, childcare, and schools. Turnout for the 2013 municipal elections was 71.9 percent. Ninety-four percent of all votes are cast on Election Day by showing up at the assigned polling station; the remaining votes are cast by absentee ballot.

Our data on individual turnout are based on local government registers from all 98 municipalities. After the election, the official voter lists are usually destroyed within three weeks, but for this election, we were allowed to merge individual turnout with anonymized administrative data. We accessed 4,362,152 voters' validated turnout, which corresponds to approximately 99 percent of all eligible voters. The missing one percent is due to temporarily system failures at specific polling stations or mistakes by polling officers. Individual voting records from the 2009 municipal elections were also added to the dataset. These records are somewhat less complete compared to the 2013 municipality voting records since only a subset of the municipalities opted in to the study in 2009. While this is unfortunate, it is crucial to emphasize that there was no self-selection involved in participation in the study at the level of the individual voter and that the participating municipalities do not differ significantly from the nonparticipating municipalities in the 2009 study. We focus our attention on the 2013 elections and supplement with data from the 2009 elections at relevant points.

The administrative data include a long list of individual socio-demographic characteristics. Most important for this study, the data contain the official residential history for each individual. Law requires all individuals to report their new main address, including the date of

moving, up to four weeks before and no more than five days after a change of residence. Besides being required by law to report accurate addresses, individuals have strong incentives to do so because all official letters, including letters from banks, insurance companies, and private organizations of which the person is a member will be sent to the official address.<sup>7</sup> Further, many municipal services require residency at an address in the municipality. There are probably minor transgression to the exact date of moving, where people report moving a few days before or after their actual move. Furthermore, most moves occur around the beginning of the month, and as we describe below, we bin our observations in 30-days-intervals beginning around the Election Day, which was November 19. Minor transgressions are thus likely to be within the same 30-day bin.

The accurate information on residential history allows us to identify where an individual lived at any given time and with whom, exactly when individuals moved to a shared address, and when they left that address, either together or separately. Compared to existing studies, the information on the timing of residential change in a large administrative data set is extraordinarily detailed, as almost all existing studies are based on surveys and categorize residential change within one-year intervals (cf. Hansen 2016).

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<sup>7</sup> In practice, the address is automatically changed to the new address in letters from the government, banks, and so on. There are some cases where there are incentives to misreport addresses, as some income transfers can be dependent on the residential status (for instance, students receive a higher stipend if they do not live with their parents, and parents on public benefits receive higher amounts if they are separated and live apart). However, such cases are rare occasions and are considered fraud and punishable by jail.

## **Empirical Strategy**

The main quantity of interest is the effect of living together on individual turnout. As we discussed above, people sort into household based on a range of observable and unobservable factors, which are likely to predict turnout. Consequently, simple regression with, for example, a dummy variable for co-residence would likely provide biased estimates, even if we controlled for a large number of potential confounders (cf. Dunning 2012). If we wanted to know the causal effect of cohabitation, we would ideally observe the overall turnout of cohabiting two-voter households compared to turnout of the *same* two-voter households if they did not cohabit, which, for obvious reasons, we cannot. In a hypothetical experiment, we would randomly assign two-voter households to live either together or apart. This too is unfeasible.

To get closer to the elusive true effect of cohabitation, we compare turnout between voters in two-voter households formed before the election and after the election. Under the core assumption that it is unrelated to voters' underlying propensity to vote whether they move together just before or just after Election Day, the quantity that we identify is causal for those who move together around the Election Day. In that case, the only systematic difference between those moving in together just before the election and those moving in together just after the election is whether they live together on Election Day. We can use this strategy to isolate the effect of cohabitation, the bundle of joint social context and social influence, on the individual turnout for those who take up cohabitation around the election. Furthermore, some do not move themselves but only have another

eligible voter move in with them. We can compare these voters over the Election Day to estimate the effect of social influence on their turnout.<sup>8</sup>

As the core assumption for the causal interpretation of our design is that people do not sort with respect to the decision to move just before or after Election Day, the period that we perceive as “just before” and “just after” plays a crucial role. Even though we argue that people do not time their decision to move with the occurrence of an election, movers are not evenly distributed over the year or even within the month. More people tend to move in the first days of the month, and they might be different from those who move later in the month. The election in this paper took place in the middle of November. To even out the differential moving rates over the month, we bin our data in 30-day bins with Election Day included in the bin before the election, and the first day after the election in the next bin. Specifically, we look at two-voter households who live together up to a year and one month after the election (i.e., by December 31, 2014) and moved in together after January 1, 2007. This gives us 767,746 voters of which 43,754 moved in together, to form 21,877 households in the two months before or the two months after the election.<sup>9</sup>

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<sup>8</sup> It would also be tempting to compare the difference in effects of cohabitation between those who remain at the same address and those who move to a new address, and interpret the difference as the effect of contextual factors. Unfortunately, this quantity would not be causally identified. It is not random who changes address when two voters take up cohabitation. Thus, we cannot know if the effect of cohabitation is the same for those who change address and those who move, which makes it impossible to partition their effect into contributions from context and social influence.

<sup>9</sup> In the supporting information, we restrict our sample to households with two voters who are of opposite sex, are less than 19.2 years apart in age (7,000 days), and both eligible to vote. This yields about 630,000 individuals or 315,000 two-voter households. When we estimate the effects for this subset of voters, they are generally somewhat stronger.

Our baseline model is an OLS model where we use indicator variables for each bin of 30 days. For turnout, the unit of analysis is the individual voters, and we run a model of the form:

$$Turnout_i = a_{Month_{+1}} + b * Month_{i,-1} + \sum_k C_k * Month_{i,k} + e_i$$

where  $i$  is a subscript for individuals and the standard errors are clustered by household;  $k$  refers to the 30-day bins. For our second model, we adjust for covariate differences by expanding the model to include control variables for being of non-Danish origin, dummies for education, gender, dummies for marital status, a third degree polynomial for age, and turnout in 2009. As we only collected turnout for some municipalities in 2009, we include a dummy variable for those who have missing data on 2009 turnout.<sup>10</sup> Finally, we also include linear time trends in addition to the monthly dummies on both sides of the Election Day.

### **Dealing with key limitations to the research design**

One challenge to our approach is that close to the cutoff, there is a bundled treatment where voters both incur a cost of moving (both before and after they do so) and exposure to social influence (after they move). The cost of moving inarguably declines as we move away from the actual move, both before and after, whereas social influence persists. This means that closely around the cutoff our quantity of interest is confounded with the cost of moving. A second challenge pertains to measurement. Voters self-report when they move and they have a little discretion over this. This means that the exact date of moving should probably be taken with a margin of some days, which threatens identification in or closely around the cutoff (Davezies and Barbanchon 2017). To mitigate the potential bias from these two sources, we present two results. First, we compare voters who moved

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<sup>10</sup> In the supporting information, we estimate model 1-4 from Table 2 using only voters with validated turnout in 2009.

in together the month right before or right after the election. Second, we jettison those who moved one month before and after the election, and instead find the effect on voters who moved two months before or two months after the election.

When we jettison the month around the Election Day, we encounter another risk. We are moving away from the Election Day, which means that potential, unobservable confounders are a greater risk. As an alternative way to account for potentially asymmetric costs of moving, we can make the assumption that moving costs are comparable for two-voter households and singles. Then we can expand our model to also include single movers and use the difference in turnout for couples and singles moving before an election compared to the difference in turnout for singles and couples moving after the election to estimate the isolated effect of cohabitation.

It is probably too strong an assumption that the cost associated with moving is the same for singles and couples. A weaker assumption is just that any asymmetry in cost is in the same direction. Under this assumption, we can still use singles to determine the direction of the bias in the cohabitation estimate. Let us assume that costs are higher/lower for singles after a move than before. Then singles moving before the election, should vote at lower/higher rate than those moving after. If we are willing to accept the assumption that the effect of asymmetric costs for couples is in the same direction, then our baseline before and after comparison of couples will be biased in that direction. We would underestimate/overestimate the effect if the costs were higher after an election than before. To leverage the single movers, we expand our data to include singles, and we expand our model to:

$$Turnout_i = a_{Month_{+1}} + b_1 * Month_{i,-1} + b_2 * Couple_i + b_3 * Month_{i,-1} * Couple_i + \sum_k C_k * Month_{i,k} + e_i$$

This model is expanded with an indicator for being part of a couple and an interaction term between being a couple and moving right before the election. In the expanded model,  $b_1$  is the effect of moving

for singles,  $b_2$  is the difference between those who are in a couple and singles, and  $b_3$  is the effect of moving for couples if the effect of cost for couples and singles is the same. Everything else in the model is similar to the models above. We estimate this model both with and without the same battery of control variables as described above.

A final concern is that because we are comparing voters who move up to 60 days before the election to voters who move up to 60 days after the election, they could potentially differ in their underlying propensity to vote. In the supporting information, we inspect the balance for five background covariates around the Election Day. The variables are age, ethnicity, education, income, and gender, all of which are frequently used predictors of voting (Smets and van Ham 2013). There are no statistically significant differences between the households formed zero to 30 days before and after. In households that formed 31 to 60 days before or after the election, we see small differences.

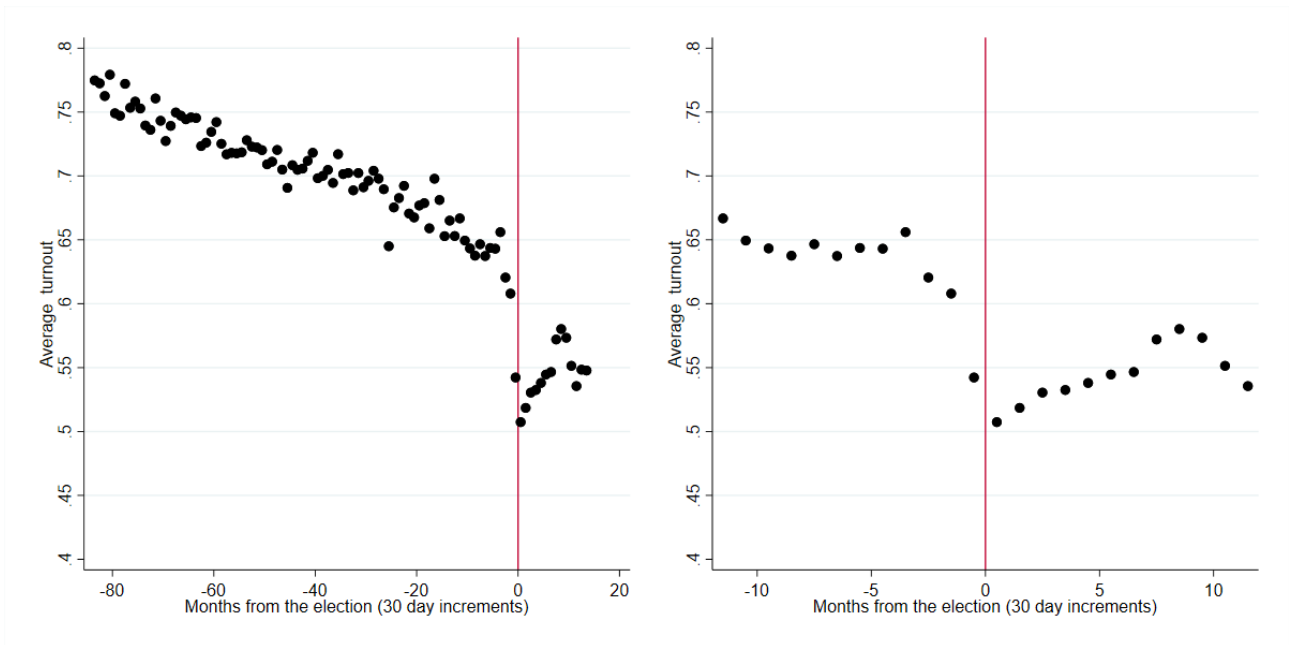
With this in mind, we feel confident about comparing voters who move up to 30 days before or after the election; although these are the voters for whom cost may potentially bias our results the most. For the voters moving 31 to 60 days before or after the election, we are less concerned about potential bias from the cost of moving, but slightly more concerned about biases from differences unrelated to the move. Ultimately, we have a tradeoff between two sources of bias, which we cannot fully overcome. To maximize transparency, we present both set of results with and without benchmarking against singles who move. Regardless of specification, we find a strong relationship between living together and voting.

### **Turnout is higher for cohabitants**

In Figure 1, we plot the average turnout of voters who joined households in each of the binned months. The vertical line at zero represents Election Day, and the points to the right of the line are households that were established after the election. The left panel includes months from 84 months before the election to 14 months after, while the right panel zooms in on voters who moved within 12 months

of the election. From the left panel, we learn that turnout gradually increases with household longevity.

**Figure 1: Turnout by month of cohabitation**

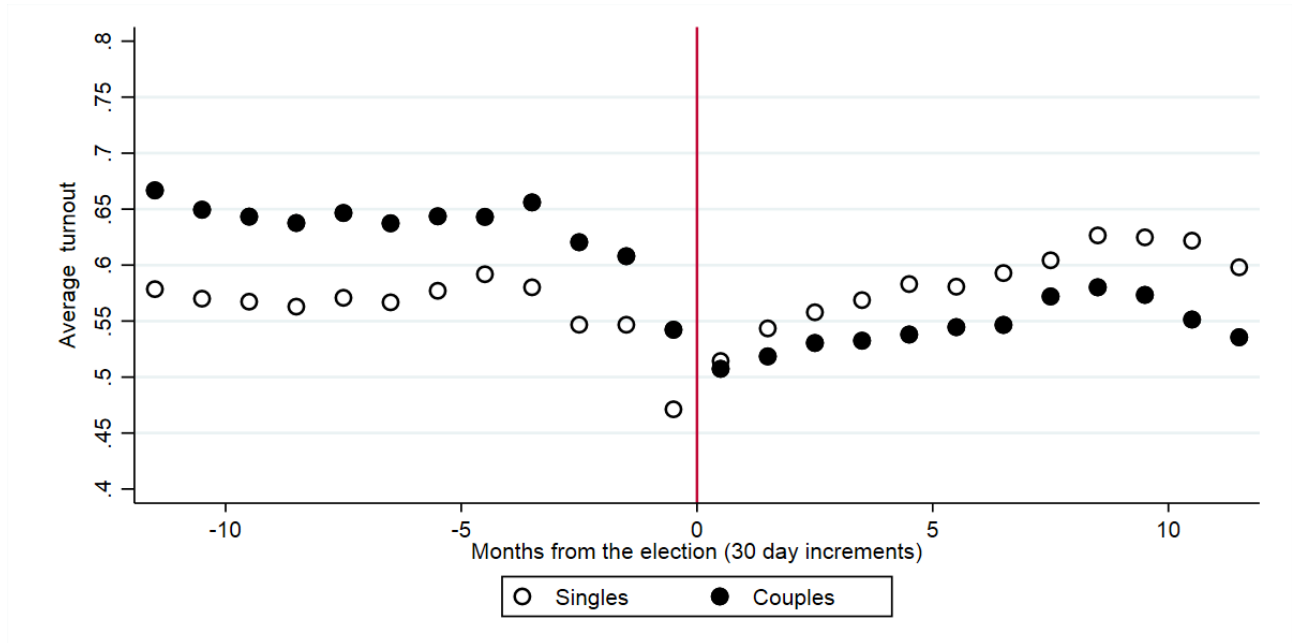


Note: The figure shows average turnout binned by 30-day windows around the Election Day represented by the vertical line. The households are placed in bins based on when they moved together. Households on the left side of the vertical line formed before the election and households on the right side formed after.

Figure 1 shows that turnout is lower for those who move right around the election, which could be attributable to the cost associated with voting. In Figure 2, we compare two-voter households to singles moving at the same time. As singles, we include those who move from single households to single households. The purpose of this exercise is to determine the direction of a potential bias from asymmetrical costs of moving around the Election Day. The singles also face a cost of moving, but they are not exposed to any cohabitation effect. As seen from Figure 2, singles who move before the election vote at a consistently lower rate than two-voter households who moved at the same time. For singles who move after the election, it is exactly the opposite: before they live together those who

will become two-voter households vote less than singles who are moving at the same time. After they live together, they vote more.

**Figure 2. Turnout by month of cohabitation singles and couples**



Note: The figure shows average turnout binned by 30-day windows around the Election Day, represented by the vertical line. The households are placed in bins based on when they moved together. Households on the left side of the vertical line formed before the election, households on the right side formed after. The black dots are two-voter households that form households, whereas the white dots are singles who move.

We cannot know if the cost of moving is the same for two-voter households as it is for singles. However, if we at least assume that it is also true for two-voter households that the cost of moving is greater *after* the move, it means that we err on the conservative side and underestimate how much higher turnout is for those who move before the election. Notably, the turnout rate for singles moving 31 to 60 days before or after election are comparable. They may differ on other variables, but if they do not, it suggests that the asymmetric cost of moving is mostly concentrated in the month around the election.

In Table 2, we present our four main models. The comparison of couples moving together before or after the election with and without control variables, and the difference-in-difference style comparison of differences between couples and single movers moving before and after the election with and without control variables. In model 1, we see that turnout is 3.5 percentage points higher, with a 95% confidence interval (CI) ranging from 1.9 to 5.1 percentage points for voters who moved in together the month before the election compared to those who moved in together the month after. When we instead compare those who moved 31 to 60 days after to those who moved 31 to 60 days before the election, the difference is even larger at 8.9 percentage points (95% CI [7.4; 10.5]). When we include control variables in model 2 to adjust for observable differences, the difference is even larger at 4.5 percentage points (95% CI [2.8; 6.1]) in the one-month window and 10.3 percentage points (95% CI [8.1; 12.6]) in the broader window with the month around the election jettisoned. However, both the new estimates fall within the 95% CIs from model 1.

**Table 2: The difference in turnout between households formed before and after the election**

	Model 1	Model 2	Model 3	Model 4
Month <sub>-1</sub> – Month <sub>+1</sub>	0.035	0.045	0.078	0.084
	[0.019,0.051]	[0.028,0.061]	[0.056,0.100]	[0.062,0.105]
Month <sub>-2</sub> – Month <sub>+2</sub>	0.089	0.103	0.086	0.083
	[0.074,0.105]	[0.081,0.126]	[0.064,0.109]	[0.062,0.105]
Turnout in Month <sub>+1</sub> (baseline)	0.507	0.507	0.507	0.507
<i>Min N<sub>Voters</sub> per month</i>	10,120	9,468	17,494	16,803
<i>Controls + time trend</i>	NO	YES	NO	YES
<i>DiD with single movers</i>	NO	NO	YES	YES

Note: The estimates in model 1 and 2 are difference-in-means for turnout. Month<sub>-1</sub> – Month<sub>+1</sub> is the difference in means between two-voter households moving in together up to 30 days before and up to 30 days after the election. Month<sub>-2</sub> – Month<sub>+2</sub> is the difference in means between two-voter households moving in together 31 to 60 days before and 31 to 60 days after the election. The estimates in model 3 and 4 are difference-in-difference estimates for the difference in turnout between couples and singles moving before and after the election. 95% confidence intervals in brackets. Standard errors are clustered by household.

In model 3 and 4, we try to account for the cost of moving, which will potentially confound the effect of cohabitation around the move. To do so we estimate the difference in the difference in turnout between couples and singles moving before and after the election. Generally, we see that the lower bound of the difference is substantially larger. In the month around the Election Day, it is 7.8

percentage points (95% CI [5.6; 10.0]) without additional control variables and 8.4 percentage points (95% CI [6.2; 10.5]) with control variables. These estimates fall outside the 95% CIs in model 1 and 2, suggesting that these models may underestimate the difference between those moving before and after the election by omitting the cost of moving. In the larger window, the differences in model 3 and 4 are 8.6 and 8.3 percentage points (95% CI [6.4; 10.9] and [6.2; 10.5]), which is slightly smaller than in the simpler models, but within the 95% CIs.

To give a sense of the scale of the differences, we can compare it to one of the most recognized predictors of turnout, education, and one of the most remarkable tools for mobilizing voters, social pressure mailings (Wolfinger and Rosenstone 1980; Blais 2000; Leighley and Nagler 2013; Persson 2015; Gerber et al. 2008). In our data, voters with a master's degree or equivalent had an 8.4 percentage point higher turnout rate than voters with a vocational education. In the seminal GOTV-experiment by Gerber et al. (2008), the most intensive form of social pressure had an effect size of 8.1 percentage points. Both figures compare well with the difference between those who live together and those who do not in most of our specifications.

### **Turnout is higher for cohabiting *Stayers***

In Table 3, we look at *Stayers*, defined as those who lived at the same address before and after another voter moved in with them. They are also more likely to vote if someone moved in with them before the election. The difference is 3.4 percentage points (95% CI [1.0; 5.8]) for the one month interval and 5.5 percentage points (95% CI [3.2; 7.8]) for those moving 31 to 60 days before or after the election. Both estimates are slightly larger when we control for covariates and time trends. Because *Stayers* are not well-defined for single movers, we cannot adjust these estimates for the cost of moving in a difference-in-difference analysis.

The estimates are only slightly smaller than among all voters who took up cohabitation. As we discussed above, the context for *Stayers* may also change when another voter moves in with them. However, aspects such as polling location, local candidates, and the municipality in which voters vote remain fixed, which means that the proportion of the mobilizing effect that we can ascribe to contextual changes should be smaller for *Stayers*. We cannot definitely determine that *Stayers* are only affected through social influence. What we can conclude, however, is that even for this group, which inarguably sees the smallest contextual change, we see a that those who had someone move in with them before the election are more likely to vote.

**Table 3: The difference in turnout between *Stayers* in households formed before and after the election**

	Model 1	Model 2
Month <sub>-1</sub> – Month <sub>+1</sub>	0.034	0.047
	[0.010, 0.058]	[0.015, 0.080]
Month <sub>-2</sub> – Month <sub>+2</sub>	0.055	0.063
	[0.032, 0.078]	[0.017, 0.110]
Turnout in Month <sub>+1</sub> (baseline)	0.570	0.570
<i>Min N<sub>Voters per month</sub></i>	3,019	2,935
<i>Controls + time trend</i>	NO	YES

## **Placebo tests and robustness specifications**

Ultimately, we did not randomly assign when households were established, and a skeptic might worry that households prone to vote more would, for some reason, sort around the Election Day. We therefore present a range of placebo and robustness checks. Generally, we run the robustness checks for the estimates in model 1. That model already seemed to err on the conservative side, so proving its robustness should be the most challenging.

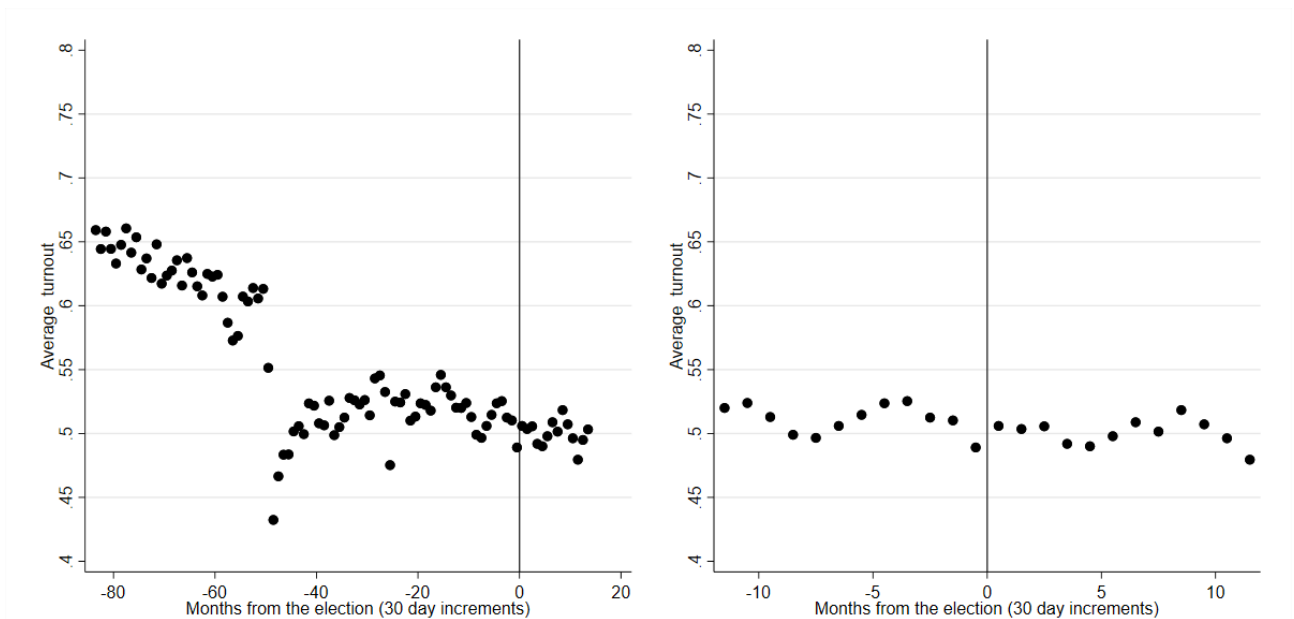
### **Estimating the difference for past turnout**

In the first test, we use turnout in the 2009 municipal elections as an outcome and keep the timing of forming households around the 2013 elections as the forcing variable. For roughly half the voters, we also know whether they voted in 2009.<sup>11</sup> If there were no sorting, we would expect no relationship between 2009 turnout and the timing of joining households four years later. In Figure 3, we look at the same timing of taking up cohabitation, but we study the difference in turnout in 2009 instead of 2013.

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<sup>11</sup> Not all municipalities opted to report turnout in 2009, so we are only working with a subset of the voters that we have for 2013. Further, some of those eligible to vote in 2013 were not eligible in 2009, primarily as they were not yet of voting age.

**Figure 3: Placebo effect on 2009 turnout**



Note: The figure shows average turnout in 2009 binned by 30-day windows around the Election Day in 2013, represented by the vertical line. Households on the left side of the vertical line formed before the election in 2013, households on the right side formed after.

In the right panel of Figure 3, we show binned turnout in 2009 for households that formed a year before or after the election in 2013. As is evident, there is no jump in 2009 turnout around the Election Day for the 2013 election. This is to be expected; we would be concerned if turnout in 2009 was correlated with the decision to take up cohabitation around the election in 2013. In the supporting information, we compare turnout for voters moving in together one month before and one month after the election and voters moving in together two months before and after the election. The differences are small: -1.7 and 0.7 percentage points, and the 95% CIs, [-3.4; 0.4] and [-1.4; 2.7], include zero.

In the left panel of Figure 3, we look at binned turnout over longer time. The bins around the Election Day are the same, so here we still do not see any selection. What stands out is the jump 48 months before the election. However, this is not a concern; it is consistent with our main findings.

The households that formed 48 months (i.e., four years) before the 2013 election formed right around the 2009 election. What the left panel shows is that there was also a jump in 2009 turnout for households that formed around the 2009 election. In other words, people who moved in together shortly before the election in 2009 were much more likely to vote in 2009 than voters who moved in together right after that election.

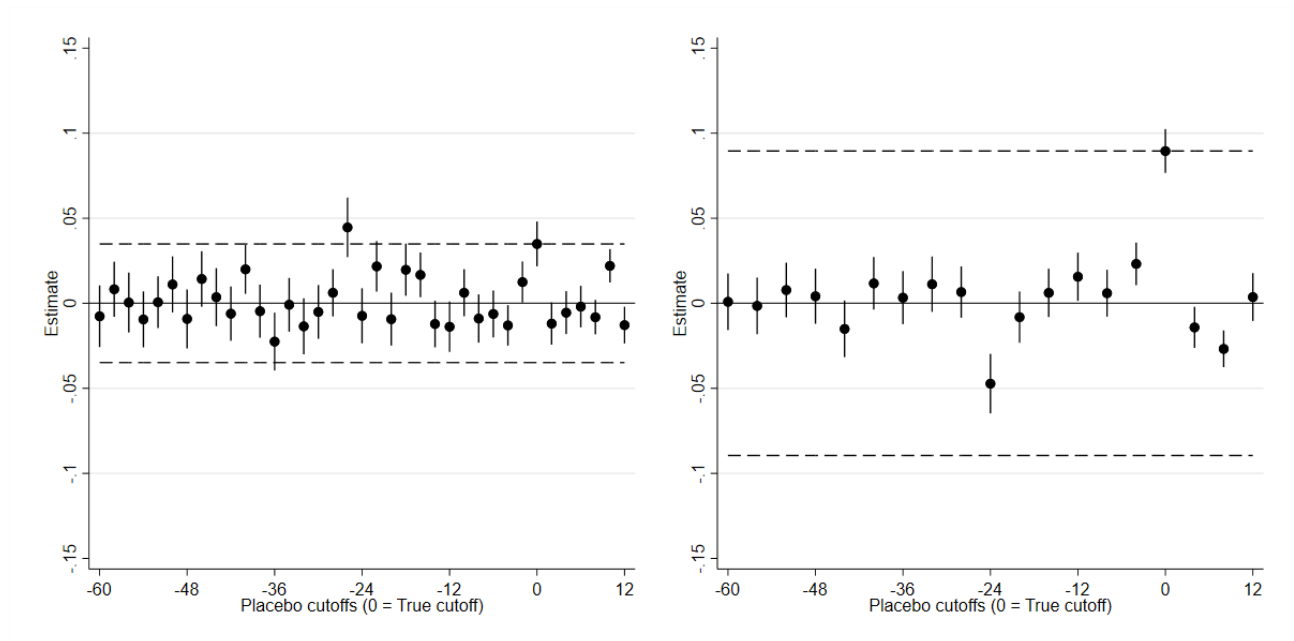
### **The differences in mock cutoffs**

As a second test, we chose several cutoffs unrelated to the election and estimate the difference in turnout in 30-day bins on either side of the artificial cutoffs. In Figure 4 below, we plot estimates and error bars at all the artificial cutoffs along with the Election Day from five years before to one year after the election. The dashed lines are the positive and negative values. The left panel shows mock estimates based on a  $\text{Month}_{-1} - \text{Month}_{+1}$  comparison. Most of the estimates are close to zero. In the right panel, we show  $\text{Month}_{-2} - \text{Month}_{+2}$  comparisons. In this plot, we jump four months at a time in order to compare only months that are outside of the estimation window of the neighboring cutoff. We see that the difference around the Election Day stands out by a considerable margin for the two-month comparison. For the one-month comparison there is only one random comparison, which is of similar magnitude.<sup>12</sup>

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<sup>12</sup> This comparison happens to be around the previous national election held on 16 September 2011, i.e., voters who formed households just before this election tend to vote more in the following local election in 2013; without going into more detail, this speaks to the habit-formation nature of voting as a consequence of cohabitation.

**Figure 4: Estimates in mock cutoffs**



Note: In the left panel we compare 2013 turnout in each of the 30-day windows to 2013 turnout in the neighboring window. At zero, we show the difference at the Election Day where we compare those who moved in together one month before the election to those who moved in together one month after. The horizontal, dashed lines show the positive and negative size of the difference around the Election Day. In the right panel, we compare turnout in 30-day windows with 60 days between them. The dashed, horizontal lines show the positive and negative value of the difference when we compare those who moved in together two months before the election to those who moved in together two months after.

### **Differences conditional on household members' past turnout**

For a subset of the households, we have turnout data from 2009 for both voters ( $n=138,463$  two-voter households). This allows us to compare voters with similar or different voting histories move in together. We can create four types of individual-level comparisons based on past participation. A voter could have voted in 2009 and moved in with a voter thereafter. A voter could have voted in 2009 and moved in with a nonvoter thereafter. A nonvoter in 2009 could have moved in with a voter thereafter. Finally, two nonvoters in 2009 could have moved in together after Election Day. In Table 4, we display, as in Table 2, both the comparisons between voters joining a new household one month

before or after the election and the same comparison for voters in households established two months before or after. We only present results for the simple comparisons without controlling for covariates.

**Table 4. The difference in turnout conditional on past household behavior**

	Voter moves in with voter	Voter moves in with non-voter	Non-voter moves in with voter	Non-voter moves in with non-voter
Month <sub>-1</sub> – Month <sub>+1</sub>	0.054 [0.014, 0.093]	0.014 [-0.035, 0.064]	0.077 [0.025, 0.129]	0.057 [0.009, 0.106]
Month <sub>-2</sub> – Month <sub>+2</sub>	0.074 [0.037, 0.111]	0.075 [0.030, 0.121]	0.172 [0.122, 0.223]	0.098 [0.048, 0.149]
Voted in 2009	YES	YES	NO	NO
Housemate voted in 2009	YES	NO	YES	NO
Turnout in Month <sub>+1</sub>	0.739	0.670	0.437	0.313
<i>Min N<sub>Voters</sub> per month</i>	1,146	682	682	992

Note: 95% confidence intervals in brackets. The estimates are from models that we ran separately for each type of household. For each type, the model is similar to the main model that we ran as specified earlier.

Beginning with households in which both voted in 2009, turnout is between 5.4 and 7.4 percentage points higher for household formed before the election, depending on how we estimate the difference.

Moving to past voters who start to live with nonvoters, we see that even they have higher turnout when they moved together before the election. For them, the difference is between 1.4 and 7.5 percentage points. There is a difference between 7.7 and 17.2 percentage points for past nonvoters who begin to live with past voters. Finally, even two individuals with a history of nonvoting joining household are more likely to vote. We estimate them to be between 5.7 and 9.8 percentage points more likely to vote when they move before an election.

### **Additional heterogeneity**

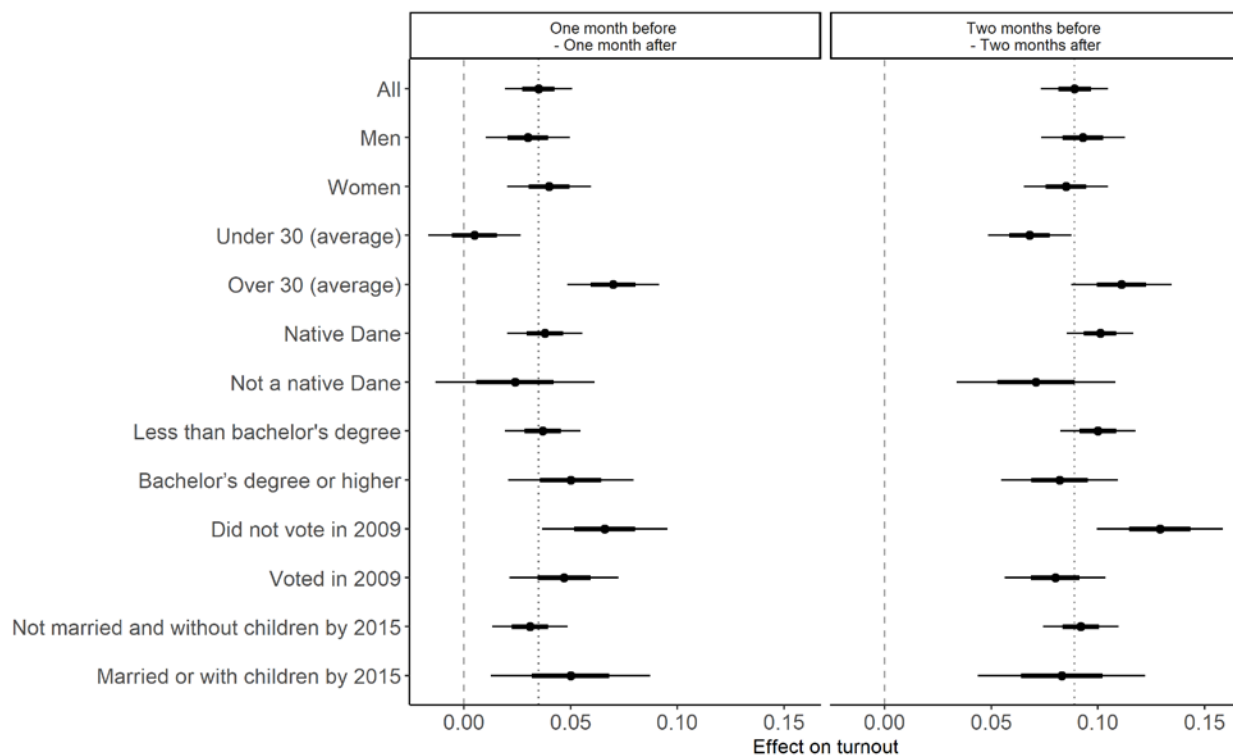
In Figure 5 below, we turn to the heterogeneous differences in turnout based on individual-level characteristics. The covariates are whether a voter voted in 2009, has a long education, is a native Dane, is more than 30 years old, and is female. We also explore variation conditional on two-voter households who had been married or become parents by 2015.<sup>13</sup>

There are a few notable results to take from Figure 5. First, we see that, on average, past nonvoters seem slightly more likely to vote after cohabitation at least when we look at the effect two months from the Election Day. Concerning native and non-natives, the point estimates are larger for native Danes compared to non-natives in both the one- and two-month windows. Likewise, it is strong among older voters compared to younger voters. Turnout is, in general, substantially higher for native Danes and older voters (Bhatti et al. 2019). In other words, in both cases, the difference is most pronounced in the group that already had the highest turnout rate.

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<sup>13</sup> Here we are conditioning on a posttreatment variable, which has known biases. However, given the assumption that becoming a parent or getting married is unrelated to whether people take up cohabitation before or after the election, the conditional effects are unbiased.

**Figure 5. The differences in turnout conditional on individual covariates**



Note: The figure shows heterogeneous differences in the one-month window in the left panel and in the two-month window in the right panel, with 95% and 83% error bars. The 83% error bars are included because 83% error bars are approximately non-overlapping when the difference is statistically significant (Payton et al. 2003). The estimates are from models that we ran separately for each type of household. For each type, the model is similar to the main model that we ran as specified above.

### Supplementary findings

In the supporting information, we also present two supplementary results. First, we show that the concordance in turnout between pairs is dramatically for households formed before the election. On average, 66.2% of the two-voter households who moved in together the month following the election did the same as one another on Election Day. That is, either both voted or both abstained. That figure was 10.5 and 15.5 percentage points higher for two-voter households who moved in together one or two months before the election, respectively. Second, we examined what happens to turnout when

two-voter households terminate their cohabitation. We find that an opposite pattern dominates: Two-voter households who separate before the election are considerably less likely to vote than two-voter households who separate after the election. We interpret this as additional evidence that it is the immediate social influence, which dominates. It is the act of living together, not mutual socialization, which go together with the increased turnout rate of two-voter households.

## **Discussion and conclusion**

In 1959, William Glaser concluded that “voting turnout tends to be a joint household activity, with the members either voting or staying home as a unit” (Glaser 1959: 569). The existing literature offers some indications as to whether this relationship is causal (Wolfinger and Rosenstone 1980; Stoker and Jennings 1995; Nickerson 2008; Sinclair et al. 2012; Foos and de Rooij 2016). Leveraging the timing of cohabitation, we find that turnout is significantly higher for people who move together in the months before Election Day compared to those moving together in the months after.

A key limitation in our design is that we suffer a tradeoff between difference sources of bias regardless of how we estimate the effect of cohabitation. Voters who move right around the Election Day could face a cost of moving, which could bias the results for them. We use two strategies, alone and in combination, to overcome this bias. One is to benchmark two-voter households against singles who move, the other is to jettison out people who move within 30 days before or after the Election Day and instead compare those who move together 31 to 60 before or after the election.

These strategies both have potential biases, which leaves us with the tradeoff between biases. Singles who move may face different costs than couples, so there is no guarantee that the amount of biased removed is right; and couples that move further away from the election may be different in their underlying propensity to vote. We try to overcome the latter problem by controlling

for observable characteristics including previous turnout. We also present a range of placebo and robustness checks. Based on estimates from our different specifications, we bracket the difference between 3.5 and 10.6 percentage points. For the reader willing to accept that any residual bias is smaller than our estimates or that we have bias towards zero, our results demonstrate a causal effect of cohabitation.

We also find that voters who do not move themselves, but only have someone move in with them, are equally more likely to vote. These voters, who we labeled *Stayers*, arguably see the smallest contextual change among all those who take up cohabitation. While we cannot rule out that they are also affected by contextual changes, we can conclude that even for the voters where most contextual factors are fixed, we see higher turnout for those who have a partner move in before rather than after an election.

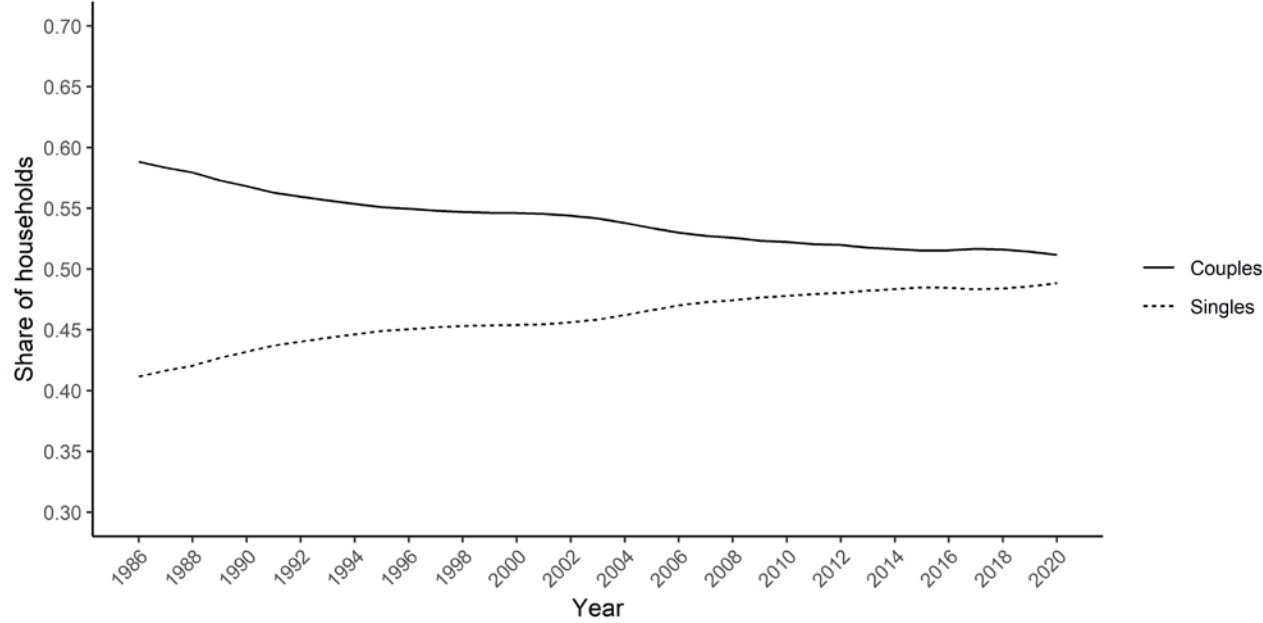
The differences are substantial and suggest that cohabitation is far from just another factor that explains some marginal proportion of turnout behavior. The difference for turnout compares with the most efficient example of voter mobilization efforts (Gerber et al. 2008). It is similar to the difference in turnout between voters with a master's degree and voters with a vocational education. Regardless of past behavior of either member of the household, voters taking up cohabitation before the election are more likely to vote. This is most pronounced for someone who did not vote in the past election and moves in with someone who did. Our estimate for them is that turnout is somewhere between 8 and 17 percentage points higher for those moving before the election.

When we look at two-voter households who have lived together for a long time, we see that their turnout remains heightened, which leads us to believe that the immediate increase endures. We could naïvely see whether there is a trend in which turnout increases with the longevity of a household. However, people who moved in together several years ago and still live together might differ in many respects compared to those who recently joined households. Consequently, we are

reluctant to provide estimates of these longer-term effects, but we do notice that our results do not suggest that additional long-term effects are as important as the immediate and lasting change associated with cohabitation.

On a final note, our results could have more broad perspectives for turnout on a societal level. Cohabitation patterns are changing in the Western world, with an increasing number of people living alone, increasing divorce rates, and fewer marriages (Lesthaeghe 2014; Carlson and Meyer 2014; Tach 2015). In Figure 6 below, we show how the portion of Danish citizens living as couples has dropped from almost 60 percent to just above 50 percent since the 1980s. Simultaneously, turnout is decreasing in many Western countries (Franklin 2004; International IDEA 2020). These trends could be completely unrelated, they could be reflections of the same underlying societal change, or they could be interrelated. Although our study cannot reach a conclusion regarding this question, what we find is consistent with the latter claim. At the household level, cohabitation is robustly associated with voter turnout. When we live with others, we could adapt to and comply with their norms, we could reduce our costs of voting, and having someone to go to the polling station with could make voting a more fulfilling personal experience. Perhaps the fact that fewer individuals experience these values of cohabitation is one of the explanations for decreasing turnout on a societal level.

**Figure 6: The trend in household types for Danish citizens**



Note: Data comes from Statistics Denmark’s StatBank Denmark, table FAM55N “Households 1 January by region, type of household, household size, and number of children in household.”

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## Online supporting information

Dahlgaard JO, Bhatti Y, Hansen JH and Hansen KM (2020) Living Together, Voting Together: Voters moving in together before an election have higher turnout. *British Journal of Political Science*. Online early access.

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### Contents

1. Demonstration of balance .....	39
2. Placebo estimates .....	42
3. Two-person households of opposite sex and limited age difference .....	43
4. Concordance in turnout binned by month of started cohabitation .....	45
5. The relationship between turnout and moving apart.....	48
6. Models with only voters with 2009 validated turnout .....	50

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## 1. Demonstration of balance

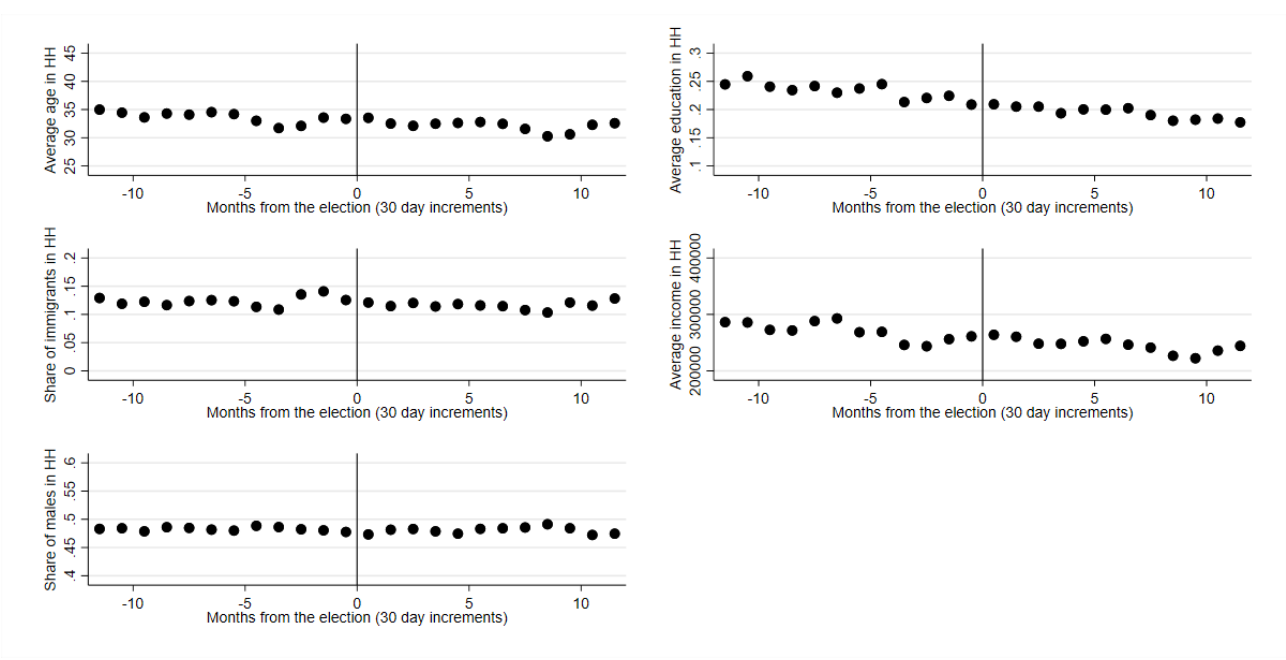
In this section, we provide demonstrations of the balance of covariates around the Election Day and robustness of results to alternative estimation strategy. In Table S1, we show means, standard deviations, N, and p-values from t-tests for Age, Education, Income, being of Danish origin, and Sex. In Figure S1, we show trends in covariate means around the Election Day. In the figure, we show means of the background covariates binned by months within a bandwidth of one year before and after the election. There is a trend in several of the covariates. If voters move into households at roughly the same age, voters in households that formed earlier will be older at one specific date than voters in households that are yet to form. Related to the development in age, a large share of the voters are young adults in the age range where one completes an education and starts earning a higher income, which explains the trends in both education and income. However, these trends are not a problem for the research design. What would have been a problem was if we saw distinct jumps around the Election Day, which as we show in the paper is not the case in the one month window. In the two months window, there are some imbalances.

**Table S1: Balance with respect to mean age, education, immigrant status, and income**

	Month-1	Month+1	P <sub>diff</sub>	Month-2	Month+2	P <sub>diff</sub>
Age	33.4 (11.9)	33.5 (12.1)	0.421	33.6 (12.2)	32.5 (11.7)	<0.001
N <sub>Age</sub>	5,663	5,055		5,670	5,747	
Education	0.21 (0.34)	0.21 (0.33)	0.931	0.22 (0.35)	0.21 (0.33)	0.004
N <sub>Education</sub>	5,226	4,734		5,079	5,137	
Immigrant or descendant	0.13 (0.30)	0.12 (0.29)	0.440	0.14 (0.31)	0.11 (0.28)	<0.001
N <sub>Immigrant/descendant</sub>	5,663	5,055		5,670	5,747	
Income (DKK)	261,197 (201,516)	263,985 (220,097)	0.496	256,299 (187,597)	260,541 (567,468)	0.598
N <sub>Income</sub>	5,663	5,060		5,671	5,783	
Male	0.48 0.22	0.47 0.23	0.319	0.48 0.22	0.48 0.24	0.813
N <sub>Male</sub>	5,663	5,055		5,670	5,747	

P-values are based on a regression of the pretreatment covariate of an indicator for moving before the election with only voters moving in the 60-day window around the election or only voters moving in a 120-day window around the election, with the 60-day window around the cutoff excluded. Standard errors are clustered by household. Table entries are means in households with standard deviations in parentheses. “Age” is age in years on Election Day; “Education” is the share of voters with a bachelor’s degree or higher; “Immigrant or descendants” is the share of immigrants or immigrant descendants; “Income” is the average annual income. The averages for all eligible voters are 48.9 for age, 0.24 for education, 0.10 for immigrant, and 308,962 for income. The lower age and income in our sample reflects that individuals typically form two-voter households relatively early in their lives.

**Figure S1: Means of background variables split by month of moving together**



Note: The figure shows averages binned by 30-day windows around the Election Day cutoff (the vertical line). The households are binned by the month they start living together.

## 2. Placebo estimates

In Table S2, we show the placebo estimates for the difference in turnout in 2009 conditional on the timing of taking up cohabitation in 2013. As we also demonstrate in the paper, there is no relationship between moving together right before or after the election in 2013 and whether one voted in 2009.

**Table S2: The effect on turnout in 2009 of cohabitation in 2013**

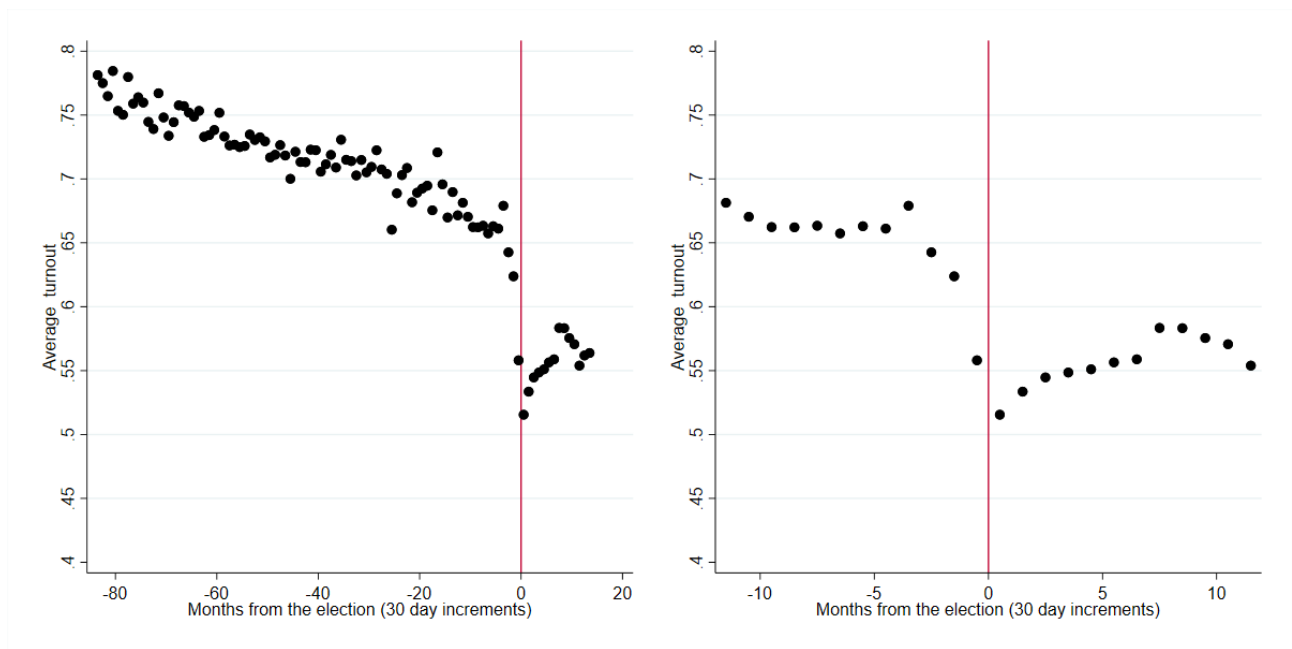
	2009 turnout
Month <sub>-1</sub> – Month <sub>+1</sub>	-0.017 [-0.038,0.004]
Month <sub>-2</sub> – Month <sub>+2</sub>	0.007 [-0.014,0.027]
Turnout in Month <sub>+1</sub> (baseline)	0.506
<i>Min N<sub>Voters per month</sub></i>	4,949

Note: 95% confidence intervals in brackets. Standard errors are clustered by household.

### 3. Two-person households of opposite sex and limited age difference

In the paper, we include all two-person households in the analysis. Here we focus instead of household pairs that are even more likely to be partners. Specifically, we focus on two-voter households of opposite sex with an age difference of no more than 7,000 days, approximately 19.2 years. This is far from a perfect measure of being a couple. We miss same sex couples and couples with a larger age difference, and we include opposite sex housemates who are not partners. Figure S2 shows turnout by month of cohabitation, and as in Figure 1 in the main analysis, we again see a distinct jump around the Election Day.

**Figure S2: Turnout by month of cohabitation**



Note: The figure shows average turnout binned by 30 day windows around the Election Day-cutoff represented by the vertical line. The households are placed in bins based on when they moved together. Household on the left side of the vertical line formed before the election, households on the right side formed after.

Table S3 summarizes the jump. As in model 1 in Table 2 in the main analysis, we compare the difference between those who move together one month before and after the election and those who

move two months before or after. The difference between movers within one month of the election is slightly larger at 4.3 percentage points. For those who moved between one and two months before or after the election, the difference is 9.0 percentage points, which is about the same as for the sample studied in the paper. In other words, it does not make an empirical difference if we study only this subset of housemates.

**Table S3: The effect on turnout of cohabitation**

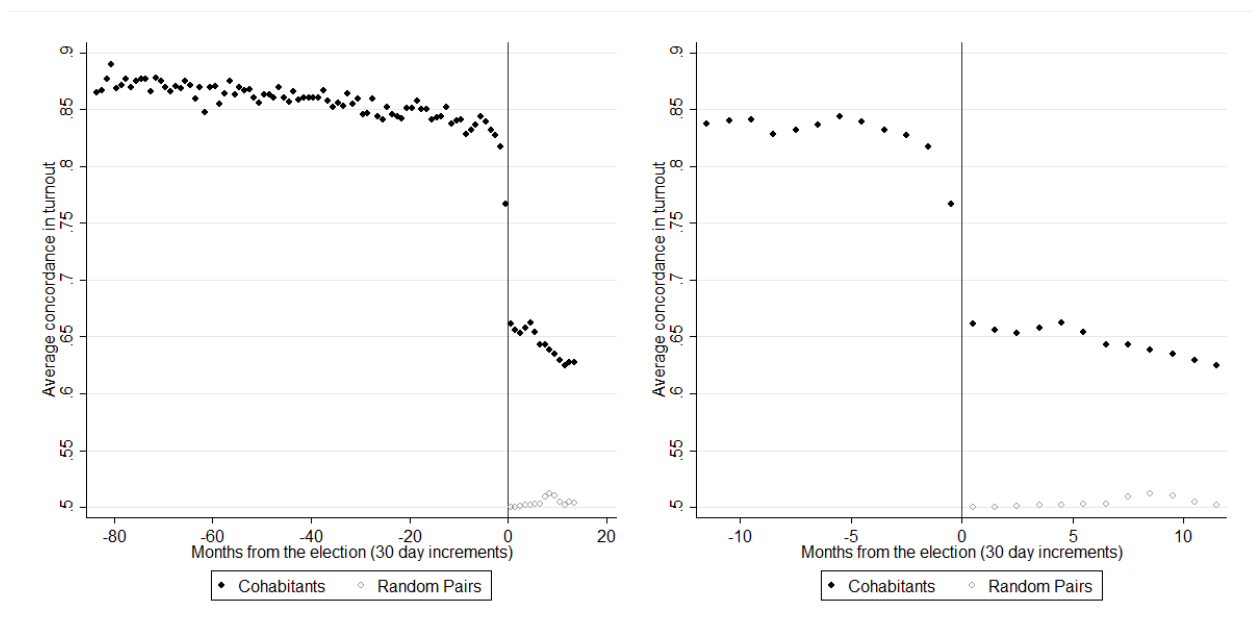
	Effect on turnout
Month <sub>-1</sub> – Month <sub>+1</sub>	0.043 [0.024,0.061]
Month <sub>-2</sub> – Month <sub>+2</sub>	0.090 [0.071,0.109]
Turnout in Month <sub>+1</sub> (baseline)	0.516
<i>Min N<sub>Voters</sub> per month</i>	7,266

95% confidence intervals in brackets. Standard errors are clustered by household.

#### 4. Concordance in turnout binned by month of started cohabitation

In the paper, we study the effect on individual turnout. We could also study an alternative outcome measured at the household level: the extent to which partners do the same thing on Election Day. We label this concordance and define it as a dichotomous variable, which equals one if both voted or both abstained and zero if one voted and the other abstained. In Figure S3, we show binned averages of concordance as black dots. Before the Election Day, we also include hollow dots to show how much concordance we would expect if we partnered people moving together in each bin with a random partner.

**Figure S3: Concordance in turnout binned by month of started cohabitation**



Note: Black dots show concordance in turnout behavior for cohabitating two-voter households, hollow dots show concordance if cohabitation within a month happened randomly. The households are placed in bins based on when they moved together

Two interesting patterns are revealed on the right side of the Election Day. First, future cohabitants are much more likely to behave according to one another than random pairs of voters even before they move in together. This is evident from the gap between the black and the hollow dots. The gap probably reflects at least two sources of selection. Voters might tend to move in with people similar to themselves, which drives voting up, and within the country there is much variation in turnout across different contexts. The random pairs are the expected concordance if we couple random pairs across the country. Voters are more likely to meet people from a context similar to their own, in which case their contextual concordance would be higher.

The second interesting pattern is that pairs moving in together closer to the Election Day are more concordant than households established a longer time after the election. One interpretation of the general tendency of increased concordance in the time leading up to the election is that pairs influence each other before moving in together. When we run a regression with indicator variables, we see that concordance is 10.5 percentage points higher in households that formed within thirty days before the election compared to households that formed within thirty days after with a 95 percent confidence interval of [8.8, 12.2]. However, this might be an underestimation of the full effect of cohabitation on concordance. Right after moving, people might still be settling in, which could depress the concordance, and, as we see in Figure S3, concordance increases sharply if we move out another month. If we just extend to compare with people who moved in one month earlier, concordance is 15.5 percentage points higher in households established two months before the election than in the month after with a 95 percent confidence interval of [13.9, 17.2]. After that, there is not much evidence of differences as the remaining point estimates fluctuate around similar values. In Table S4, we summarize these results and make other comparisons.

**Table S4: The effect of cohabitation on concordance**

	Effect on concordance
<i>Cohabitation before election</i>	

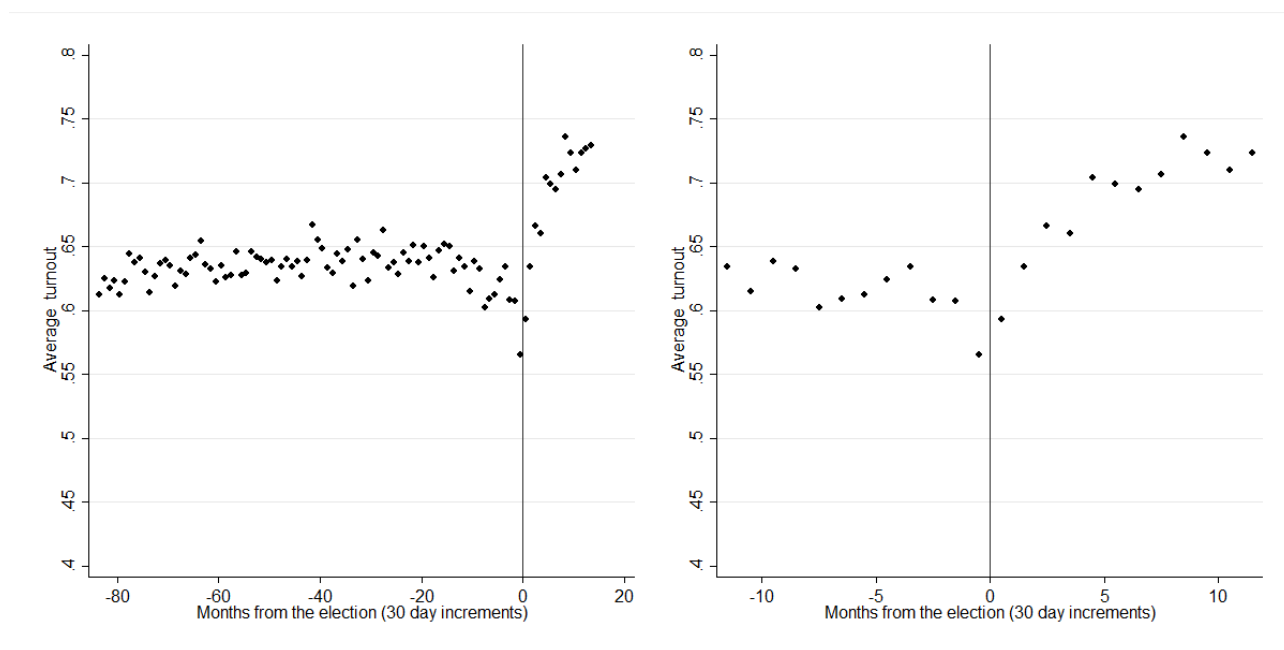
Month-1	0.105 [0.088,0.122]
Month-2	0.155 [0.139,0.172]
Month-3	0.165 [0.150,0.181]
Month-6	0.182 [0.165,0.198]
Month-12	0.175 [0.158,0.193]
<i>Cohabitation after the election</i>	
Month+2	-0.005 [-0.024,0.013]
Month+3	-0.009 [-0.026,0.008]
Month+4	-0.004 [-0.022,0.014]
Month+7	-0.018 [-0.036,-0.001]
Month+13	-0.034 [-0.051,-0.017]
Concordance in Month+1 (baseline)	0.662 [0.649,0.675]
$N_{household}$	159,571

Note: 95% confidence intervals in brackets. Each month is compared to households that formed in the month after the election.

## 5. The relationship between turnout and moving apart

What happens when two voters move apart? Do voters keep an elevated turnout level, or do we see an opposite effect where voters become less likely to vote as their household dissolves? To address this, we present a figure similar to Figure 1 of the paper; only this time we analyze turnout depending on the timing of moving apart. Voters in households on the right side of the Election Day still lived together on Election Day whereas voters on the left side had terminated their cohabitation.

**Figure S4: Turnout by month of terminating cohabitation**



Note: The figure shows average turnout binned by 30 day windows around the Election Day-cutoff represented by the vertical line. The households are placed in bins based on when they moved apart. Households on the left side of the vertical line dissolved before the election, households on the right side dissolved after.

Figure S4 shows that turnout decreases substantially around the Election Day, though the decrease is slightly less sharp around the cutoff compared to moving together. The difference is not immediate as for moving together, but especially when we consider the left panel where we have the longer timespan, we can see that turnout is dramatically higher in the time up to terminating cohabitation only to stabilize after cohabitation expires. In other words, the opposite pattern holds: voters who leave a joint household see a sharp decrease in turnout.



## 6. Models with only voters with 2009 validated turnout

In Table 2 of the main paper, we included all voters regardless of whether, we knew their turnout status in 2009. If 2009 turnout was missing, we simply added an additional dummy for this. In Table S5, we take a more restrictive approach and only include those for whom we know turnout in 2009. In every other regard, the models are the same. The conclusions are also the same. Among this more select subset, there is a strong mobilizing effect. In most cases, it is even slightly larger than for the entire population of couples moving together.

**Table S5: The effect on turnout of cohabitation for voters with 2009 validated turnout**

	Model 1	Model 2	Model 3	Model 4
Month <sub>-1</sub> – Month <sub>+1</sub>	0.051 [0.029,0.073]	0.058 [0.037,0.079]	0.097 [0.069,0.124]	0.081 [0.052,0.110]
Month <sub>-2</sub> – Month <sub>+2</sub>	0.106 [0.085,0.128]	0.104 [0.074,0.134]	0.107 [0.080,0.134]	0.075 [0.046,0.104]
Turnout in Month <sub>+1</sub> (baseline)	0.526	0.526	0.526	0.526
<i>Min N<sub>Voters per month</sub></i>	4,949	4,738	8,354	8,161
<i>Controls + time trend</i>	NO	YES	NO	YES
<i>DiD with single movers</i>	NO	NO	YES	YES

Note: The estimates in model 1 and 2 are difference-in-means for turnout. Month<sub>-1</sub> – Month<sub>+1</sub> is the difference in means between two-voter households moving in together up to 30 days before and up to 30 days after the election. Month<sub>-2</sub> – Month<sub>+2</sub> is the difference in means between two-voter households moving in together 31 to 60 days before and 31 to 60 days after the election. The estimates in model 3 and 4 are difference-in-difference estimates for the difference in turnout between couples and singles moving before and after the election. 95% confidence intervals in brackets. Standard errors are clustered by household.