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ARTICLE

Living Together, Voting Together: Voters Moving in Together Before an Election Have Higher Turnout

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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. This study examines a large administrative dataset containing detailed information about validated turnout and the timing of individuals moving in together, and finds evidence of a substantial and robust increase in turnout after cohabitation. The study exploits the fact that two-voter households moving in together right before an election are comparable to those moving in together right after the election. Depending on the 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 in the analysis. The results highlight the importance of social norms and the household's essential role as a proximate social network that increases turnout.

Keywords: turnout; social norms; cohabitation; participation; social influence

Scholars have for many years observed that eligible voters who share a residence tend to vote or abstain jointly (Anderson 1943; Glaser 1959; Stoker and Jennings 1995; Wolfinger and Rosenstone 1980). 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) focused on the time of transition – when voters get 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 long before getting married. Therefore, we focus on the 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 may be because inhabitants of a two-voter household affect each other's behavioral alignment: that is, either both vote or both abstain from voting (Fowler 2005; Klofstad 2010; Nickerson 2008; Zuckerman 2005). 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). However, the correlation in two-voter households' turnout could simply be due to sorting, where citizens with a taste for voting (or not voting) tend to live with those who share their preferences.

Most research to date has had to rely on data that assessed the concordance of attitudes between couples either before or after they got together (Alford et al. 2011; Huber and

Malhotra 2017; Klofstad, Mcdermott and Hatemi 2012; Stoker and Jennings 2005). As an alternative, some experiments have demonstrated how efforts to mobilize one household member to vote can spill over to other persons in a household (Foos and de Rooij 2016; Nickerson 2008; Sinclair, McConnell and Green 2012). We have learned much from both types of studies, but there has been a call for studies that use stronger designs to study 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 from Denmark to study how cohabitation affects turnout. Validated turnout data and voters' complete residential history are available. We use the latter to determine where a voter lived at any given time and with whom. We compare the turnout of voters who moved in together immediately before election day to that of voters who moved in together 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, which simply compares couples moving before and after, voters who move in together 0–30 days before or after an election are 3.5 percentage points more likely to vote, and those moving in together 31–60 days before or after an election are 8.9 percentage points more likely to vote. 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.* 2008; Wolfinger and Rosenstone 1980). We also find higher turnout after cohabitation among voters who had someone move in with them while remaining at the same address. Finally, both past voters and non-voters who took up cohabitation became more likely to vote regardless of whether the other person in the household had voted in the past or not.

For a causal interpretation of our results, we rely on the assumption that voters moving in together before or after the election are comparable. To test this assumption, we run a number of alternative specifications that control for covariates including time trends and benchmark against singles who move in together before or after election day. The relationship in the 0–30 day window is stronger in each of our alternative specifications, but is largely unchanged in the 31–60 day 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 bias, as we can only rule out one source by introducing another – that is, contextual bias vs. social influence (discussed further below). However, our results suggest that our most conservative estimates of the mobilizing effect of turnout underestimate 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 choose to 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 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 their 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 on couples and find strong concordance in attitudes. Iyengar, Konitzer and Tedin (2018) find that spousal correspondence in attitudes has increased over time for American couples and point to mate selection as the principal reason. Analyses based on data from dating sites also consistently find that political preferences correlate with mating preferences (Huber and Malhotra 2017; Klofstad, Mcdermott and Hatemi 2012).

Secondly, cohabitation might correlate with turnout because cohabitants are exposed to similar contextual factors (Bhatti 2012; Cho and Rudolph 2008; Enos 2016). In this article, we understand context as all external factors that affect an individual's decision about whether to vote, including influences from social networks, except for the social influence of one's cohabitant. When voters live together, they also start sharing an environment such as the intensity of local campaigning, social surroundings and distance from 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 the context changes because people start living together. They could start visiting restaurants or shops that they would not have frequented otherwise; they could alter their media diet; they could see changes in their social networks; and they could be targeted differently by political campaigns. Living 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).

Thirdly, social influence between the cohabitants could directly affect turnout. Following Sinclair (2012, 2), we can think of two ways in which individuals may be influenced by their social environment. First, information shared in their network could alter their behavior through persuasion or some other mechanism. Secondly, members of a network could simply conform to expected social norms. In this article, we constrain the social environment to include only the cohabitant, 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 (Bhatti et al. 2017; Blais 2000; Blais, Young and Lapp 2000; Sinclair 2012). Voters prioritize stable social relations, and learning that a housemate plans to vote could motivate one to avoid disapproval by breaking the norm (Coleman 1988). Indeed, experimental evidence demonstrates 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 with the norm of voting out into the open (Bhatti et al. 2017; Bhatti, Fieldhouse and Hansen 2020; Dahlgaard 2018; Fieldhouse and Cutts 2018). 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 voter in the household could persuade an otherwise reluctant voter to vote.

Is There a Social Influence on Turnout?

We compare the turnout of eligible voters in households that formed before and after the election in order to ascertain whether turnout increases after moving in with someone else. We argue that two-voter households that move in together right before an election are comparable to those that move in together immediately after. Theoretically, compliance with norms can affect turnout in either direction. In a household in which one normally votes and 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.

Comparing voters in households formed before the election to those in households formed after the election illustrates 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 take a focused look at a group for whom the context is arguably most stable: voters who 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, τ , 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 $\tilde{\tau}$ to emphasize that we are changing the subgroup for which the effect is identified. We understand the potential outcomes as functions of three factors:¹ 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* started 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 accept this assumption, the effect on *Stayers* will instead be a joint effect of social influence and contextual changes; the latter will arguably be 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 afterwards, 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 who move after the election than for those who move before. However, we would underestimate the true effect of cohabitation if the cost is higher after a move than before.

To gauge the potential bias associated with the cost of moving, we use singles who move as a benchmark to complement our analyses. These are voters who lived alone both 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 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 summarizes the assumptions and identification strategies.

Context and Data

Our study relies on data from the Danish municipal elections held on 19 November 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 before election day. Danish municipalities are central in governmental service provision, including care for the elderly, childcare and schools; they

¹Note that we implicitly assume that the factors are additively related to each other.

Table 1. Quantities of interest, potential outcomes, and identification strategies

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

spend about one-fourth of the total Danish GDP. Turnout for the 2013 municipal elections was 71.9 per cent; 94 per cent 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 ninety-eight 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 per cent of all eligible voters. The missing 1 per cent is due to temporary 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 than 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 study participation at the individual voter level, and that the participating municipalities do not differ significantly from nonparticipating municipalities in the 2009 study. We focus 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 importantly, each individual’s official residential history. All individuals are required by law to report their new main address, including their moving date, up to four weeks before and no more than five days after a change of residence. Individuals have strong incentives to report accurate addresses because all official letters, including those from banks, insurance companies and private organizations of which the person is a member, will be sent to the official address.² Further, many municipal services require residency at an address in the municipality. There are probably minor transgressions regarding the exact moving date, 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-day

²In practice, the address is automatically changed to the new address in letters from the government, banks and so on. There are some cases in which there are incentives to misreport addresses, as some income transfers can depend on the individual’s residential status (for instance, students receive a larger stipend if they do not live with their parents, and parents on public benefits receive more if they are separated and live apart). However, such cases are rare and are considered fraud and punishable by jail.

intervals beginning around election day, which was 19 November. 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 they moved to a shared address and when they left that address, either together or separately. The information on the timing of residential change in a large administrative data set is much more detailed than that used in prior studies, as almost all have been based on surveys and categorize residential change within one-year intervals (cf. Hansen 2016).

Empirical Strategy

The main quantity of interest is the effect of living together on individual turnout. As we discussed above, people sort into households based on a range of observable and unobservable factors, which are likely to predict turnout. Consequently, a 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 and after the election. Based on the core assumption that it is unrelated to voters' underlying propensity to vote whether they move in together just before or just after election day, the quantity that we identify is causal for those who move in together around election day. In that case, the only systematic difference between those moving in together just before the election vs. just after 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 individual turnout for those who began cohabiting around the time of the election. Furthermore, some do not move themselves, but only have another eligible voter move in with them. We can compare these voters on election day to estimate the effect of social influence on their turnout.³

Since the core assumption for the causal interpretation of our design is that people do not sort with respect to their 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 based on the occurrence of an election, movers are not evenly distributed during 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 article 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 (that is, by 31 December 2014) and moved in together after 1 January 2007. This gives us 767,746 voters, of whom 43,754 moved in together, to form 21,877 households in the two months before or after the election.⁴

³It would also be tempting to compare the difference in the effects of cohabitation between those who remain at the same address and those who move to a new address, and to 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 move in together. Thus we cannot know whether 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.

⁴In the Online Appendix, we restrict our sample to households with two voters who are of the opposite sex, are less than 19.2 years apart in age (7,000 days), and are 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 ordinary least squares model in which we use indicator variables for each 30-day bin. For turnout, the unit of analysis is individual voters, and we run a model of the form:

$$\text{Turnout}_i = a_{\text{Month}_{+1}} + b \times \text{Month}_{i,-1} + \sum_k C_k \times \text{Month}_{i,k} + e_i$$

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, 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 that have missing data for 2009 turnout.⁵ Finally, we include linear time trends and monthly dummies on both sides of election day.

Dealing with Key Limitations to the Research Design

One challenge associated with our approach is that close to the cutoff, there is a bundled treatment: voters incur the costs 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. Thus near 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 moving date may be a few days before or after it appears in the official data, 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 in together the month right before or right after the election. Secondly, we jettison those who moved one month before or after the election, and instead find the effect on voters who moved two months before or two months after the election.

Yet the analysis in this second step yields another risk. We are moving away from election day, which means that unobservable confounders may be a greater risk. As an alternative way to account for the potentially asymmetric costs of moving, we can assume that moving costs are comparable for two-voter households and singles. Then we can expand our model to 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 costs associated with moving are the same for singles and couples. A weaker assumption is simply that any asymmetry in costs 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 the costs are higher/lower for singles after a move than before. Then singles moving before the election should vote at a 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 comparing 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 expand our model to:

$$\begin{aligned} \text{Turnout}_i = & a_{\text{Month}_{+1}} + b_1 \times \text{Month}_{i,-1} + b_2 \times \text{Couple}_i \\ & + b_3 \times \text{Month}_{i,-1} \times \text{Couple}_i + \sum_k C_k \times \text{Month}_{i,k} + e_i \end{aligned}$$

⁵In the Online Appendix, we estimate Models 1–4 from Table 2 using only voters with validated turnout in 2009.

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 those who move up to 60 days after the election, they could potentially differ in their underlying propensity to vote. In the Online Appendix, we inspect the balance for five background covariates around election day – 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 0–30 days before and after. In households that formed 31–60 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 costs may bias our results the most. For voters moving 31–60 days before or after the election, we are less concerned about potential bias from the costs of moving, but slightly more concerned about biases associated with differences unrelated to the move. Ultimately, we have a trade-off between two sources of bias that we cannot fully overcome. To maximize transparency, we present both sets of results with and without benchmarking against singles who move. In all specifications, 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 voters who moved between 84 months before the election and 14 months after, while the right panel zooms in on voters who moved within 12 months of the election. The left panel shows that turnout gradually increases with household longevity.

Figure 1 shows that turnout is lower for those who move close to 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 (from single households to single households). The purpose of this exercise is to determine the direction of a potential bias from the asymmetrical costs of moving around election day. Singles also face moving costs, but they are not exposed to any cohabitation effect. Figure 2 illustrates that singles who moved 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.

We cannot know if the cost of moving is the same for two-voter households as it is for singles. However, if we assume that the cost of moving is also greater *after* the move for two-voter households, 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 31–60 days before or after the election is 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.

Table 2 presents our four main models. It compares couples moving in together before or after the election with and without control variables, and includes a difference-in-differences style comparison of the differences between couples and single movers moving before and after the election with and without control variables. Model 1 demonstrates that turnout is 3.5 percentage

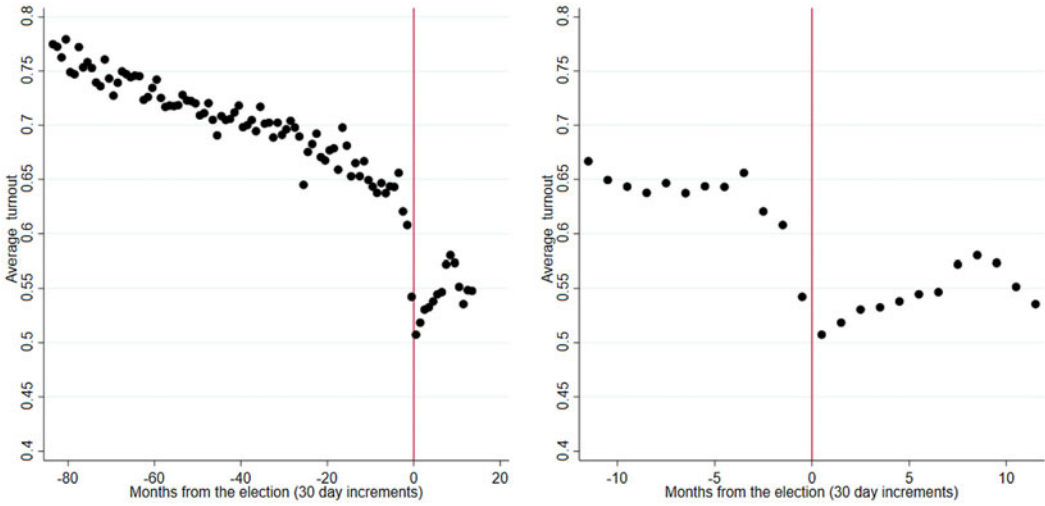


Figure 1. Turnout by month of cohabitation

Note: The figure shows average turnout binned by 30-day windows around 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.

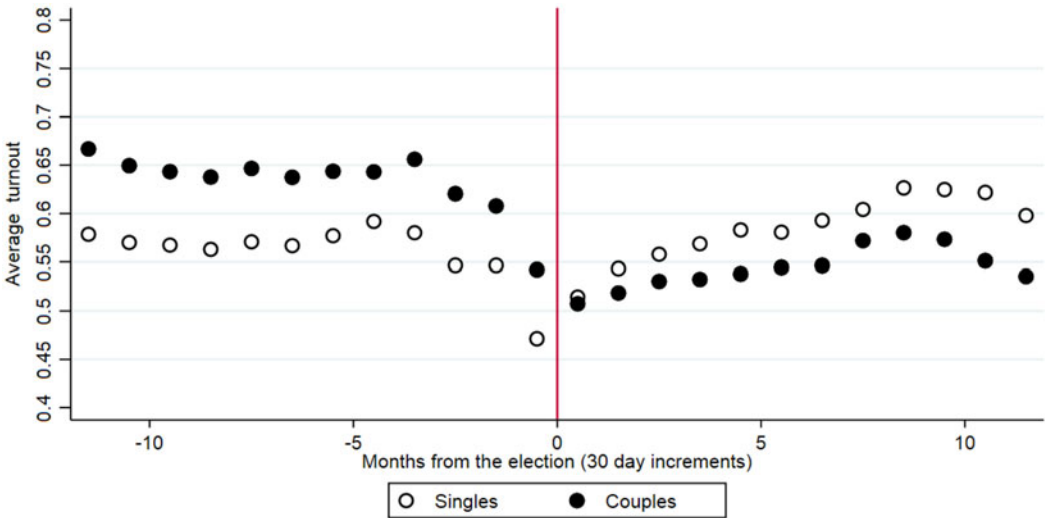


Figure 2. Turnout by month of cohabitation singles and couples

Note: the figure shows average turnout binned by 30-day windows around 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.

points higher, with a 95 per cent confidence interval (CI) of 1.9–5.1 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 31–60 days after to those who moved 31–60 days before the election, the difference is even larger at 8.9 percentage points (95 per cent 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 per cent CI [2.8; 6.1]) in

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

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

Note: the estimates in Models 1 and 2 are difference in means for turnout. Month₋₁ – Month₊₁ 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₋₂ – Month₊₂ 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 Models 3 and 4 are difference-in-differences estimates for the difference in turnout between couples and singles moving before and after the election. 95 per cent confidence intervals in brackets. Standard errors are clustered by household.

the one-month window and 10.3 percentage points (95 per cent CI [8.1; 12.6]) in the broader window with the month around the election jettisoned. However, both new estimates fall within the 95 per cent CIs from Model 1.

In Models 3 and 4, we try to account for the cost of moving, which could 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 election day, it is 7.8 percentage points (95 per cent CI [5.6; 10.0]) without additional control variables and 8.4 percentage points (95 per cent CI [6.2; 10.5]) with control variables. These estimates fall outside the 95 per cent CIs in Models 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 Models 3 and 4 are 8.6 and 8.3 percentage points (95 per cent CI [6.4; 10.9] and [6.2; 10.5]), respectively, which is slightly smaller than in the simpler models, but within the 95 per cent 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 effective tools for mobilizing voters (social pressure mailings) (Blais 2000; Gerber, Green and Larimer 2008; Leighley and Nagler 2013; Persson 2015; Wolfinger and Rosenstone 1980). In our data, voters with a master's degree or equivalent had an 8.4 percentage point higher turnout rate than those with a vocational education. In the seminal get-out-the-vote experiment by Gerber, Green and Larimer (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 per cent CI [1.0; 5.8]) for the one-month interval and 5.5 percentage points (95 per cent CI [3.2; 7.8]) for those moving 31–60 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-differences analysis.

The estimates are only slightly smaller than among all voters who moved in together. As 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

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

	Model 1	Model 2
Month ₋₁ – Month ₊₁	0.034 [0.010, 0.058]	0.047 [0.015, 0.080]
Month ₋₂ – Month ₊₂	0.055 [0.032, 0.078]	0.063 [0.017, 0.110]
Turnout in Month ₊₁ (baseline)	0.570	0.570
Min N_{voters} per month	3,019	2,935
Controls + time trend	NO	YES

contextual changes should be smaller for *Stayers*. We cannot definitely determine that *Stayers* are only affected through social influence. We can conclude, however, that even for this group, which inarguably sees the smallest contextual change, those who had someone move in with them before the election are more likely to vote.

Placebo Tests and Robustness Specifications

Since we could not randomly assign when households were established, a skeptic might worry that households that are more likely to vote would, for some reason, sort around election day. We therefore present a range of placebo and robustness checks using the estimates in Model 1. That model already erred 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 the outcome and keep the timing of household formation around the 2013 elections as the forcing variable. For roughly half of the voters, we also know whether they voted in 2009.⁶ 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 moving in together, but we study the difference in turnout in 2009 instead of 2013.

In the right panel of Figure 3, we show binned turnout in 2009 for households that formed a year before or after the 2013 election. It shows that there is no jump in 2009 turnout around election day in 2013. This is to be expected; we would be concerned if turnout in 2009 was correlated with the decision to begin cohabiting around the 2013 election. In the Online Appendix, we compare turnout for voters moving in together one month before and one month after the election with those moving in together two months before and after the election. The differences are small: -1.7 and 0.7 percentage points, respectively, and the 95 per cent 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 a longer period. The bins around 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 (that is, 4 years) before the 2013 election did so right around the 2009 election. The left panel shows 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 those who moved in together right after that election.

⁶Not 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 because they were not yet of voting age.

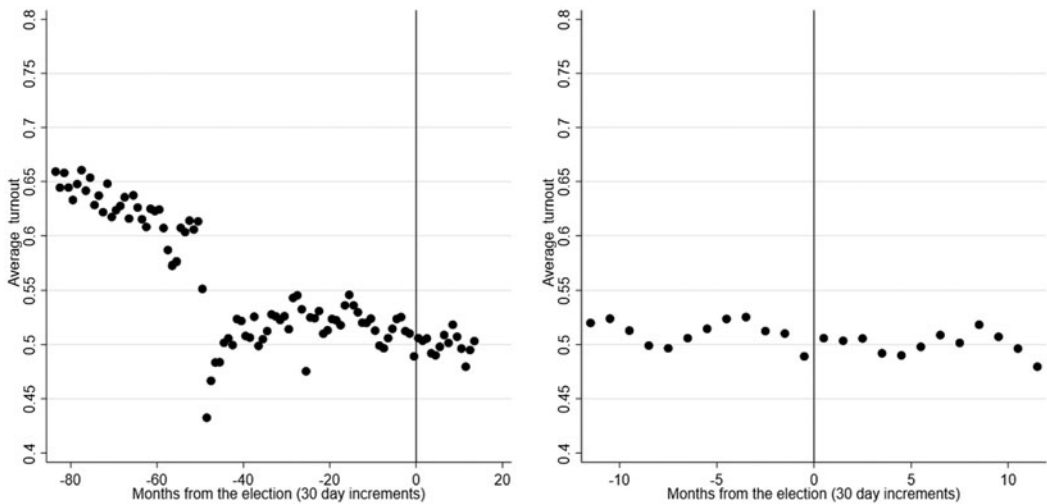


Figure 3. Placebo effect on 2009 turnout

Note: the figure shows average turnout in 2009 binned by 30-day windows around 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.

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. Figure 4 plots estimates and error bars at all the artificial cutoffs along with the election day from 5 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 the estimation window of the neighboring cutoff. We see that the difference around 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 a similar magnitude.⁷

Differences Conditional on Household Members' Past Turnout

For a subset of the households, we have turnout data from 2009 for both voters in each household ($n = 138,463$ two-voter households). This allows us to compare voters with similar or different voting histories who later move in together. We can create four types of individual-level comparisons based on past voting behavior. First, a voter could have voted in 2009 and moved in with another voter afterwards. Secondly, a voter could have voted in 2009 before moving in with a non-voter. Thirdly, a non-voter in 2009 could have later moved in with a voter. Finally, two non-voters in 2009 could have moved in together after election day. In Table 4 we display, as in Table 2, 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 the results for the simple comparisons without controlling for covariates.

Beginning with households in which both members voted in 2009, turnout is 5.4–7.4 percentage points higher for households formed before the election, depending on how we estimate the difference. Moving to past voters who move in with non-voters, we see that even they have higher

⁷This comparison happens to be around the previous national election held on 15 September 2011, i.e., voters who formed households just before this election were more likely to vote in the next local election in 2013, which speaks to the habit-formation nature of voting as a consequence of cohabitation.

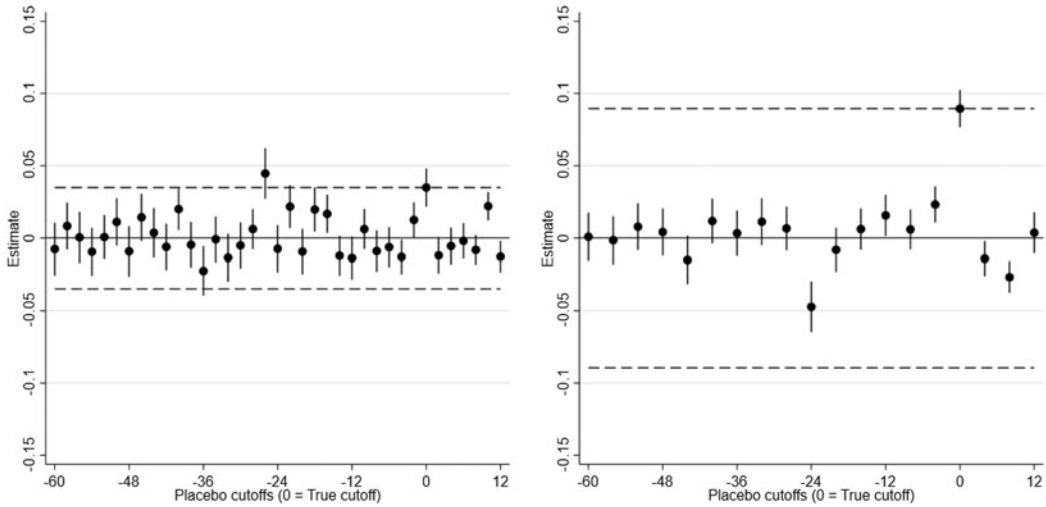


Figure 4. Estimates in mock cutoffs

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 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 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 values 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.

turnout when they moved in together before the election. For them, the difference is between 1.4 and 7.5 percentage points. There is a difference of 7.7–17.2 percentage points for past non-voters who begin to live with past voters. Finally, even two individuals with a history of non-voting who move in together are 5.7–9.8 percentage points more likely to vote when they move before an election.

Additional Heterogeneity

In *Figure 5*, we turn to the heterogeneous differences in turnout based on individual-level characteristics. The covariates are whether a voter voted in 2009, having completed higher education, is a native Dane, is more than 30 years old and is female. We also explore variation conditional on two-voter households with members who had been married or become parents by 2015.⁸

There are a few notable results to take from *Figure 5*. First, on average, past non-voters seem slightly more likely to vote after cohabitation, at least when we look at the effect two months from election day. The point estimates are also larger for native Danes compared to non-natives in both the one- and two-month windows. Likewise, it is stronger among older than younger voters. Turnout is generally 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.

Supplementary Findings

In the Online Appendix, we also present two supplementary results. First, we show that the concordance in turnout between pairs is dramatically different for households formed before

⁸Here we are conditioning on a post-treatment variable that has known biases. However, given the assumption that becoming a parent or getting married is unrelated to whether people move in together before or after an election, the conditional effects are unbiased.

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 ₋₁ – Month ₊₁	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 ₋₂ – Month ₊₂	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 ₊₁	0.739	0.670	0.437	0.313
Min $N_{\text{voters per month}}$	1,146	682	682	992

Note: 95 per cent 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.

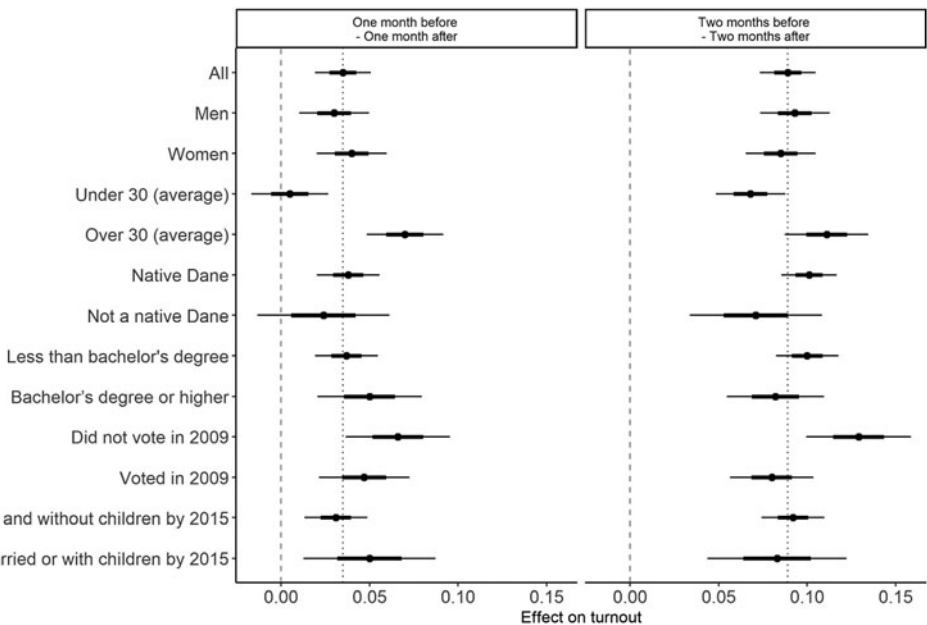


Figure 5. The differences in turnout conditional on individual covariates

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 per cent error bars. The 83 per cent error bars are included because they are approximately non-overlapping when the difference is statistically significant (Payton, Greenstone and Schenker 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.

compared to after the election. On average, 66.2 per cent 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 that moved in together one or two months before the election, respectively. Secondly, we examined what happens to turnout when two-voter households stop living together. We find that the opposite pattern dominates: two-voter households that separate before the election are considerably less likely to vote than those that split up after the election. We interpret this as additional evidence that it is the immediate social influence that dominates. It is the act of living together, rather than mutual socialization, that increases the turnout rate of two-voter households.

Discussion and Conclusion

William Glaser (1959, 569) concluded that ‘voting turnout tends to be a joint household activity, with the members either voting or staying home as a unit’. Prior studies offer some indications as to whether this relationship is causal (Foos and de Rooij 2016; Nickerson 2008; Sinclair, McConnell and Green 2012; Stoker and Jennings 1995; Wolfinger and Rosenstone 1980). We leverage the timing of cohabitation and find that turnout is significantly higher for people who move in together in the months before election day compared to those who do so afterwards.

A key limitation of our design is that we face a trade-off between different sources of bias regardless of how we estimate the effect of cohabitation. For voters who move near election day, the costs of moving could bias their results. We use two strategies, alone and in combination, to overcome this bias. The first is to benchmark two-voter households against singles who move, and the second is to exclude people who move within 30 days before or after election day and instead compare those who move in together 31–60 days before or after the election.

These strategies both have potential biases, which leaves us with the trade-off between biases. Singles who move may face different costs than couples, so there is no guarantee that the amount of bias we removed is right. Likewise, 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 as 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 (*Stayers*) are equally more likely to vote. These voters arguably experience the smallest contextual change of all those who start living together. While we cannot rule out the possibility that they are also affected by contextual changes, we can conclude that even for voters for whom most contextual factors are fixed, we observe a 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 in turnout compares with the most efficient example of voter mobilization efforts (Gerber, Green and Larimer 2008). It is also similar to the difference in turnout between voters with a master’s degree vs. a vocational education. Regardless of the past behavior of either household member, voters who move in together before the election are more likely to vote. This effect is most pronounced for those who did not vote in the past election and move in with someone who did. Our estimate for them is that turnout is 8–17 percentage points higher for those moving before the election.

When we look at two-voter households that 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 naively see whether there is a trend in which turnout increases with household longevity. 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 broader implications 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 (Carlson and Meyer 2014; Lesthaeghe 2014; Tach 2015). Figure 6 illustrates how the portion of Danish citizens living as couples has dropped from almost 60 per cent to just over 50 per cent since the 1980s. Turnout is also decreasing in many Western countries (Franklin 2004; International IDEA 2020). These trends could be

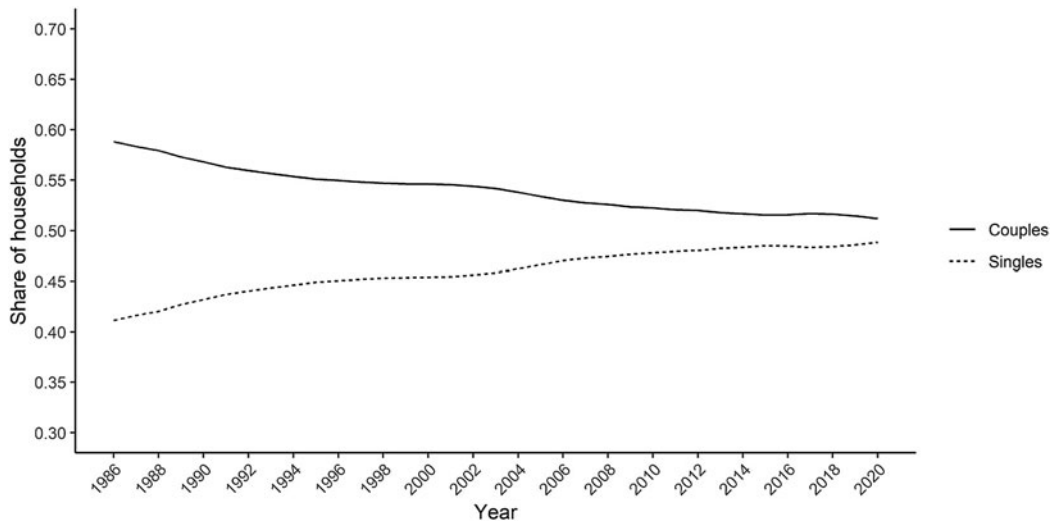


Figure 6. The trend in household types for Danish citizens

Note: the data come from Statistics Denmark's StatBank Denmark, table FAM55N 'Households 1 January by region, type of household, household size, and number of children in household.'

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, our findings are 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 explanation of decreasing turnout on a societal level.

Supplementary material. Online appendices are available at <https://doi.org/10.1017/S0007123420000691>.

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Data availability statement. Data replication files are available in Harvard Dataverse at: <https://doi.org/10.7910/DVN/DNPEI8>.

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