Peer Effects in Legislative Voting

Nikolaj Harmon, Raymond Fisman, and Emir Kamenica*
University of Copenhagen, Boston University, and University of Chicago

September 2018

Abstract

We exploit seating rules in the European Parliament to identify peer effects in legislative voting. Sitting adjacently leads to a 7 percent reduction in the overall likelihood that two Members of the European Parliament (MEPs) from the same party differ in their vote. Peer effects are markedly stronger among pairs of women, MEP pairs from the same country, and in close votes. Using variation in seating across the Parliament's two venues (Brussels and Strasbourg), we show that peer effects are persistent: MEPs who have sat together in the past disagree less even when they are not seated adjacently.

JEL classification: D72, D73, F53, P16
Keywords: seating, influence, European Parliament

*We thank the editor, David Deming, for helpful suggestions. We thank seminar participants at the University of Copenhagen, the 2014 DAEiNA meeting and the 2017 European meeting of the Econometric Society for helpful comments. We thank Sebastian Barfort and Session Services at the European Parliament for help with data collection. The University of Chicago Booth School of Business provided financial support.

1 Introduction

A primary function of politicians is to pass legislation. Political scientists and economists have thus long been interested in how legislation gains support. Lawmakers may be swayed by earmarks or other benefits to themselves or their electorate. Or they may be cajoled, convinced, or subjected to social pressure by their peers.

How big a role peer influence plays is an empirical matter, but one that is notoriously difficult to resolve (Manski, 1993). In this paper, we exploit alphabetical seating in the European Parliament to study peer effects among its members. Using flexible controls for name similarity, we instrument for whether two Members of the European Parliament (MEPs) sit next to each other based on their alphabetical adjacency within the party.[1] Our IV estimates indicate that sitting next to each other reduces the probability that two MEPs from the same party differ in their vote by 0.6 percentage points. This represents a 7 percent decline relative to the 8.55 percent average rate of within-party disagreement and is about one-tenth of the size of the impact of being from the same country.

To buttress our identification approach, we use placebo tests that exploit the fact that party leaders and members of four small parties do not sit alphabetically. Reassuringly, alphabetical adjacency has no impact on voting congruence among these MEPs. As an additional check, we also verify that once we condition on contemporaneous and past seating, future seat adjacency does not predict current voting outcomes.

We also explore the mechanisms behind the observed peer effects by examining the influence of present versus past adjacency, and by looking at heterogeneity in the magnitude of peer effects across MEP-pair attributes and the importance of the proposal under consideration. In our first set of analyses, we distinguish 'contemporaneous' peer effects — that operate while the pair is seated together — from 'persistent' peer effects that remain even when the peer is not present. The European Parliament convenes in two separate venues, in Brussels and Strasbourg, each with a distinct seating chart. Differences across venues in the seating layout makes it possible for a pair of alphabetically adjacent MEPs to sit together in one venue but not in the other. We find that having sat together in Strasbourg during the previous session affects current session voting in Brussels, even after accounting for (current) seating proximity in Brussels.[2] These findings show that peer effects contain a persistent component.

Turning to our analysis of heterogeneity in peer effects, we find that peer effects are much stronger when the two MEPs are from the same home country or are both women. For these

[1] By "party," we mean the European Political Group. We discuss the relevant institutional details in the next section.

[2] And analogously, of course, having sat together in Brussels during the previous session affects voting in Strasbourg today even after accounting for seating proximity in Strasbourg.

groups, we estimate that seat adjacency reduces disagreement by about 1.3 percentage points, corresponding to percentage declines in disagreement between 17 and 40 percent. These findings suggest that peer exposure serves as a complement to having other shared attributes, such as country of origin or gender — that facilitate social pressure or communication. Looking at subsamples of proposals that passed by narrow margins, we find that peer effects are also stronger in close, high-stakes votes. For example, in votes decided by a margin of 1 percent or less, seat adjacency reduces disagreement by nearly 2 percentage points.

Political economy scholars have been interested in how legislators' social connections affect the legislative process going back as far as Routt (1938) and Truman (1956). This attention has recently grown with increased interest in peer effects within political networks (see Lazer (2011) for an overview). Most recently, Canen and Trebbi (2016) examine how peer effects influence socialization and careers in the U.S. Congress, while Battaglini and Patacchini (2016) analyze how social networks of legislators impact political contributions.

Despite this increased attention, existing evidence on peer effects in the legislative process is limited. Masket (2008) shows that votes in the California Assembly are more similar for legislators seated in adjacent desks but acknowledges that seating might have been assigned on the basis of shared views.[3] Cohen and Malloy (2014) find that logrolling (i.e., trading votes) in the U.S. Senate is more widespread among members of the same alumni network. Two papers concurrent with our own, Jo and Lowe (2017) and Saia (2017), both examine peer influence among Icelandic legislators, also using exogenous variation in seating within the legislature. These papers focus on different outcomes however. While we focus on concordance in votes cast, both Jo and Lowe (2017) and Saia (2017) look at similarity of language in used in speeches given by parliamentarians. Saia (2017) also looks at voting but focuses primarily on vote participation (the high degree of party discipline in Iceland makes it difficult to examine vote concordance in this setting). Finally, Rogowski and Sinclair (2012) take advantage of the office lottery for new Members of Congress to generate causal estimates of office proximity on voting and bill co-sponsorship. While their point estimates are small and the authors report that they "find no evidence that office proximity affects patterns of legislative behavior," their confidence intervals allow for large peer effects. For example, the data does not rule out the possibility that having proximate offices has a bigger impact on cosponsoring legislation than being in the same party. In contrast, we present positive results with tighter standard errors and provide additional evidence on the mechanism behind the observed peer effects.

Two other features of our study warrant mention, relative to prior work. First, given the diverse and heterogeneous composition of the European Parliament, our setting may be particularly well-

[3]Ringe et al. (2013) also study social networks in the European Parliament, but using a methodology that cannot distinguish peer influence from correlated preferences.

suited to studying how commonalities – gender and place of origin in particular – interact with peer effects. Second, only Jo and Lowe (2017) shares with our work the analysis of the persistence of peer effects, which helps to distinguish between short-term mimicry or social pressure versus changed beliefs.

We also contribute to the broader literature on peer effects. A number of papers analyze political peer effects among citizens: Nickerson (2008) uses a canvassing experiment to show intrahousehold influence on turnout, while Perez-Truglia and Cruces (2016) and Perez-Truglia (2016) provide evidence of peer effects in campaign donations using a combination of experimental and observational data. DellaVigna et al. (2017) demonstrate the importance of social image concerns in voting. Campos et al. (2013) exploit random assignment of Brazilian freshmen to classrooms to study the impact of peers on political preferences and engagement. Holden et al. (2016) identify large peer effects among U.S. Supreme Court Justices. Moving beyond politics, much of the well-identified research on peer influence has focused on academic and workplace performance (e.g., Duflo et al. (2011), Guryan et al. (2009), Herbst and Mas (2015), Mas and Moretti (2009), Sacerdote (2011)).

2 Background and Data

2.1 The European Parliament

The European Parliament (EP) is the lower legislative chamber of the European Union (EU). Since 1979, Members of the European Parliament (MEPs) have been selected via elections held in each EU member country. While elections are thus conducted at the country level, once in the EP, MEPs join one of several cross-national European Political Groups (EPGs) according to their political leanings.[4] Each EPG consists of MEPs from different countries and national parties. During the day-to-day work of the EP, EPGs carry out many of the functions performed by parties in national legislatures. In particular, EPGs sit together during voting sessions in the parliament and also formulate a (non-binding) "party line" for many of the issues being voted on. Throughout, we will use the terms "party" and "EPG" interchangeably.

The work of the EP is centered around the plenary sessions held once or twice a month in either Strasbourg or Brussels. These sessions consist of several daily "sittings" of debate and voting. Importantly for the present paper, for about a third of these votes, individual voting is registered electronically, with MEPs casting ballots via electronic voting machines on their desks. To cast a ballot, an MEP inserts an ID card into the voting machine and presses the button corresponding to the desired choice.[5] MEPs are not permitted to cast votes for each other. We know of only a

[4] There is a small number of MEPs who are not affiliated to any EPG.

[5] Historically, votes involving individual registration were held as roll-call votes, with the EP president calling on

single alleged case of an MEP voting on behalf of an absent colleague, when a Marcel de Graaff was accused of casting ballots on behalf of fellow party member Marine Le Pen on October 28, 2015. The actions of Le Pen and her colleague were quickly uncovered and sanctioned.

For each proposal with individual registration between October 2006 and November 2010, we collected data on the vote cast by each MEP.[6] These data contain information from 3,123,419 votes cast by 1,261 distinct MEPs on 5,297 different proposals spread across 168 days of voting.

In both Strasbourg and Brussels, each MEP has an assigned seat within his or her EPG. We obtained official EP seating charts covering October 2006 to November 2010 for Brussels and November 2006 to November 2010 for Strasbourg. For days when no seating chart was available, we assume that seating was unchanged from the prior seating chart. To illustrate the nature of these data, Figure 1 shows sample seating charts for the two venues. Within each chart, we label the EPG (ALDE, VERTS/ALE, S&D, etc.) associated with each group of seats.[7] Each number corresponds to a seat and is mapped to a list of MEP names on a separate sheet.

(Figure 1 about here)

Finally, for each MEP, we have data on his or her age, tenure in the EP, and whether (s)he holds a degree from a "top 500" university.[8]

2.2 Alphabetical seat assignment in the EP

Seat assignment in the EP takes place according to rules laid down by the body's Conference of Presidents. As noted above, MEPs from the same EPG sit together. Furthermore, within each EPG, the party leadership sits in the first few rows in an otherwise unspecified manner. Importantly for our purposes, however, EP rules dictate that seats for non-leadership MEPs be "generally allocated

each member to announce his vote in turn. As a result, votes involving individual registration are commonly referred to as "roll-call" votes even today. However, since in practice voting is never sequential, we avoid using the term "roll-call" here.

[6]Data on votes was collected from the European Parliament website, http://www.europarl.europa.eu/activities/plenary/home.do?language=EN, between February 2007 and October 2010. There are a few MEPs for whom we cannot uniquely link their voting record to an MEP identifier on the EP website. These are excluded from the data.

[7]One small EPG in our data, "Identity, Tradition and Sovereignty," was dissolved in the middle of the 6th parliamentary term in November 2007. As most of its 25 members remained unaffiliated afterwards, we treat membership in "Identity, Tradition and Sovereignty" as non-affiliated throughout our sample period.

[8]This measure of education quality is more useful than education quantity in our sample because virtually every MEP holds a college degree. To construct our measure, we identified where MEPs attended college from their individual websites. We were able to obtain this information for nearly 90 percent of the sample. We merged these data with the 2010 Academic Ranking of World Universities, which provides a ranking of the top 500 universities in the world. Because the majority of MEPs (75 percent in our main sample) hold degrees from universities that have not made it into the list at all, we only use information on whether MEPs' hold any top 500 degree, rather than the precise rank of their degree. See Fisman et al. (2015) for additional details.

in alphabetical order" by last name, though the seating rules do allow for a member to "occasionally occupy another place for organizational or technical reasons."[9] In our analysis, we wish to exploit the quasi-random variation in seating generated by this alphabetical seating rule. Inspection of the seating charts clearly shows that four small EPGs, as well as the group of unaffiliated members, do not adhere to the alphabetical name assignment rule.[10] Among the remaining six larger EPGs (covering more than 80 percent of total MEPs in our data), the alphabetical assignment rule holds for most MEPs. In these "alphabetical parties," we observe a leadership section in the first few rows where seat assignment is unrelated to name, after which there is a non-leader section where seat assignment correlates strongly (but not perfectly) with the alphabetical ranking of last names.[11]

To illustrate the predictive power of alphabetical order on seat assignments, Figure 2 takes a seating chart and plots within-party alphabetical rank against within-party seat number for two different EPGs. In Panel A, we see that the "European United Left–Nordic Green Left" does not adhere to the alphabetical seating rule: there is no relationship between alphabetical rank and seat number for this group of MEPs. In Panel B, we plot the relationship for the "Greens–European Free Alliance." In the first handful of seats, alphabetical rank shows no relationship with seat number. This corresponds to the leadership section, where seat assignments are not name-based. Among the non-leaders from seat 9 onward, however, alphabetical rank is a very strong predictor of seat number, as indicated by the nearly monotone relationship between the two variables. Note, however, that compliance is not perfect, as the MEP in seat 42 sits out of alphabetical order. Across all days, the correlation between within-party seat number and alphabetical rank is 0.95 in the sample of non-leaders from alphabetically seated EPGs.

(Figure 2 about here)

The non-leadership sections of the six alphabetically seated EPGs form the main analysis sample. We use the leadership sections of these parties as well as the four non-alphabetically seated parties for a placebo test. Table 1 provides a summary and compares the main sample to the two placebo samples.

(Table 1 about here)

[9] See http://www.europarl.europa.eu/sed/hemicycle.do (last accessed August 2, 2018).

[10] The four EPGs that do not adhere to the alphabetical seating rule are the European United Left–Nordic Green Left, Independence-Democracy, Union for Europe of the Nations, and Identity, Tradition and Sovereignty.

[11] One additional exception to this description is the "Alliance of Liberals and Democrats for Europe," which seems to use alphabetical seating in a part of its leadership section. We thus exclude this party's leadership section from the placebo exercise in Table 5.

3 Empirical strategy and results

3.1 Empirical strategy

To explore whether MEPs that are placed next to each other tend to vote more similarly, we analyze the voting concordance of MEP pairs. For each proposal and each EPG in our data, we form all possible within-EPG pairs of MEPs in which both MEPs are present and participate in the vote.[12] Letting ij index MEP pairs and t index proposal, we construct the variable $Disagree_{ijt}$ as an indicator for whether MEPs i and j cast different votes on proposal t. We construct $SeatNeighbors_{ijt}$ to denote whether the MEP pair ij sat next to each other during proposal t.

A naive approach to estimating the treatment effect of sitting together on vote concordance would be to compare the votes of MEP pairs who sit next to each other (treated observations) to the votes of MEP pairs that do not (untreated observations) by simply regressing the outcome, $Disagree_{ijt}$, on the treatment indicator, $SeatNeighbors_{ijt}$:

$$Disagree_{ijt} = \beta_0 + \beta_1 SeatNeighbors_{ijt} + \nu_{ijt}. \tag{1}$$

There are two concerns with interpreting the estimate of β_1 as the causal impact of sitting together on vote concordance. First, when MEPs *choose* whether to sit together, those who vote alike may be more likely to sit together. Second, even among MEPs who follow the alphabetical seating rule, the rule itself might induce MEPs who are more likely to vote alike to sit together because, for example, individuals with more similar names tend to have more similar backgrounds.

To address the first issue, we restrict our attention to the non-leadership sections of the six alphabetically seated EPGs and we use the seat assignment rule as an instrument for seating adjacency. We define our main analysis sample to only include observations ijt where the pair of MEPs ij are from an alphabetically seated EPG and where both MEPs are non-leaders at the time when voting on proposal t took place. For this sample, we define $NameAdjacent_{ijt}$ to be an indicator variable which denotes whether MEP pair ij's last names are adjacent in the alphabetical ordering of names among the non-leaders in their EPG on the day when the vote on proposal t took place. Table 2 provides summary statistics for this main analysis sample and all main variables.

(Table 2about here)

With these sample and variable definitions, we can obtain an intent-to-treat (ITT) estimate, $\hat{\gamma_1}$, from the equation:

[12] MEPs can actively vote to abstain, although such votes are rare (less than four percent of votes cast). We treat abstentions as non-participation.

$$Disagree_{ijt} = \gamma_0 + \gamma_1 NameAdjacent_{ijt} + \varepsilon_{ijt}. \quad (2)$$

In addition, we can use $NameAdjacent_{ijt}$ as an instrument for $SeatNeighbors_{ijt}$ in Equation 1 to get a Local Average Treatment Effect (LATE) estimate, $\beta_1^{\hat{I}V}$.

In order for these ITT and LATE estimates to reflect true peer effects, the assignment indicator, $NameAdjacent_{ijt}$, should not be systematically related to unobservables that cause MEP pairs to vote more similarly.[13] This raises two potential concerns. First, $NameAdjacent_{ijt}$ is mechanically correlated with the pair being in a smaller party because a larger fraction of MEP pairs are name-adjacent in small parties.[14] We will address this concern by adding party-by-parliamentary term fixed effects as controls. Second, individuals with similar last names may have more similar backgrounds. We will therefore introduce a flexible set of controls for name similarity.

We begin by exploring whether MEPs with alphabetically adjacent last names are more similar in terms of predetermined characteristics. Akin to the standard approach of testing for covariate balance in randomized trials, we can replace the left-hand side of the regression in Equation (2) with various predetermined characteristics of the MEP pair that we observe in our data. Doing so with and without name similarity controls allows us to assess whether MEPs with alphabetically adjacent last names are more similar in terms of predetermined characteristics both before and after conditioning on our controls. The six predetermined characteristics we examine are whether the members of an MEP pair are from the same country, whether they have educations of similar quality (as measured by an indicator for having a degree from a "top 500" university), whether they are either both freshmen or both non-freshmen, whether they are of the same gender, their age difference in years, and their difference in EP tenure in years.

Table 3 shows the extent to which alphabetical order predicts similarity in these predetermined attributes. In Panel A, we provide results without controls, while Panel B includes party-by-parliamentary term (EPG-by-EP) fixed effects and our baseline set of name similarity controls,

[13] An additional, separate concern with our empirical approach is the possibility of common shocks. Angrist (2014) points out that even when there is exogenous variation in peer assignment, estimates of peer effects that come from examining correlations in outcomes across peers may reflect common shocks to peer groups. In the educational peer effects literature, a simple illustration would be, say, randomly assigned college roommates whose grades comove not because of a peer effect but because the academic performance of both students is affected by whether or not their neighbors play loud music late at night. Since peer groups in our setting are simply defined by where MEPs sit during plenary sessions, common shocks would imply that sitting in a certain location directly influences how you vote (for example, because of the angle from which you see the speaker). Correlated shocks seem unlikely to occur in our setting. Further, they would need to be highly localized because, as we show in Appendix D, our estimated peer effect dissipates very rapidly with seating distance.

[14] This mechanical correlation occurs because adding additional MEPs to a party increases the total number of within-party pairs more than the number of pairs that are name adjacent. For example, increasing the number of MEPs in a party from 3 to 4 will double the number of within-party pairs, from $\binom{3}{2} = 3$ to $\binom{4}{2} = 6$. The number of pairs that have alphabetically adjacent names only increases by 1, however (from 2 to 3).

which include an indicator for whether the MEPs have the same last name and a flexible measure of the distance between the MEPs' last names in the alphabetical ranking of all MEPs in our data. Because observations in our data pertain to behavior by pairs of MEPs (dyads) and because seating peer effects imply that behavior can be correlated within clusters of MEPs sitting close to each other, we use dyadic-cluster robust standard errors throughout our analysis (Cameron and Miller (2014) and Aronow et al. (2015)). We cluster at the level of the row-by-EP-by-EPG, thus allowing for arbitrary correlation over time in behavior and outcomes within each row of each EPG during the two parliamentary terms we analyze. As noted in the table, there are 76 such clusters in the main analysis sample. See Appendix A for additional details on the computation of standard errors.

(Table 3 about here)

Before we turn to covariate balance, in Column (1) we present the first stage of our IV approach, showing that alphabetical adjacency is strongly predictive of being seated together. The coefficient on $NameAdjacent_{ijt}$ is above 0.8 and is precisely estimated, with a standard error below 0.03 in both panels. We next turn to the relationship between alphabetical adjacency and pre-determined attributes. In Column (2), we see that without name similarity controls, alphabetical adjacency is predictive of whether the pair comes from the same country, but this relationship disappears when we condition on their party and name similarity in Panel B. As the remaining columns show, alphabetical adjacency is not predictive of any other similarity measure — age gap, tenure gap, freshman status, gender, and whether both went to prestigious schools — regardless of whether we control for name similarity. Overall, we take this as evidence that the most obvious confounds of alphabetical adjacency do not appear to be correlated with our instrument once we condition on party and name similarity.

3.2 Estimated peer effects

We present our first set of results on peer effects in Table 4. We begin with our intent-to-treat analysis. In Column (1), we report a specification that only includes time fixed effects. The point estimate is -0.0116 and highly significant. To deal with the obvious concern that name adjacency is correlated with party identity or simply picks up name similarity, in Column (2) we include EPG-by-EP fixed effects and our baseline set of name similarity controls. Unsurprisingly (given the results from Table 3), the magnitude of the coefficient drops substantially, to -0.0048.

(Table 4 about here)

The next three columns assess the robustness of this estimate by adding further controls. In Column (3), we control for the similarity of each MEP pair in terms of observable predetermined characteristics. In Columns (4) and (5), we add additional measures of name similarity to probe whether the baseline set eliminates most of the omitted variable bias. Specifically, in Column (4) we add other standard measures of name similarity, including cubic polynomials in the Bigram-Jaccard and Levensthein measures of similarity as well as an indicator for whether the MEPs' names sound alike according to the SoundEx algorithm.[15] In Column (5), we enrich the set of controls based on the overall name rank gap by including indicators for every possible bin of ten values (21-30, 41-50, etc.). Across all of these specifications, the coefficient on $NameAdjacent_{ijt}$ remains stable around -0.0047.

Finally, in Columns (6) and (7) we present our LATE estimates, using the result from Table 3, Column (1) to instrument for $SeatNeighbors_{ijt}$ with $NameAdjacent_{ijt}$. In our preferred specification with just the baseline set of name similarity controls (Column (6)), the impact of being $SeatNeighbors_{ijt}$ on $Disagree_{ijt}$ is -0.0060. This implies that sitting together reduces the chance that two MEPs from the same party differ in their vote by 0.6 percentage points. Following the literature on persuasion (DellaVigna and Gentzkow (2010)), we can convert this estimate into a persuasion rate, which captures the fraction of MEP pairs that were induced to agree as a result of seating proximity, and would not have done so otherwise. In Table 4 as well as those that follow, we list the implied persuasion rates at the bottom of each table to more easily compare persuasion rates across models. We note that the computation of persuasion rates requires that we assume monotonicity, i.e., closer proximity (weakly) increases agreement. This is a non-trivial assumption, since proximity could plausibly intensify extant disagreements as well. Since the baseline disagreement rate among same-party MEP pairs is 8.55 percent, a 0.6 percentage point decrease in disagreement implies a persuasion rate of 7 percent.[16]

As an alternative approach to thinking about the magnitude of the seat-adjacency effect, we can compare it to the effects of other pair characteristics. Shared nationality is by far the strongest

[15] For the Bigram-Jaccard similarity measure, we create a list of all the possible pairs (bigrams) of two consecutive characters contained in each name (e.g., the name "Joly" contains the pairs "jo", "ol", and "ly"). For each pair of names, we then count the number of such character pairs that the names have in common and divide by the number of unique pairs that are present in at least one of the names (this is referred to as a Jaccard index). The Levenshtein distance between two names is the smallest number of characters that needs to be changed (including removing or adding extra characters) to turn one name into the other. We convert this distance to a similarity measure by taking the length of the longer name in the pair, subtracting the Levenshtein distance, and dividing by the length of the longer name. This transformation implies that both Levensthein similarity and Bigram-Jaccard similiarity range from zero to one and are equal to one only when the two names are identical.

[16] Formally, the persuasion rate is defined as the estimated "treatment" effect divided by the average number of pairs disagreeing in the "control" group of pairs who are not seat-adjacent. However, since only a very small fraction of MEP pairs sit adjacently in our sample (less than 2 percent), the disagreement rate among non-adjacent pairs is indistinguishable from the overall disagreement rate.

predictor of vote concordance in our analysis, consistent with national interests serving as an important determinant of MEPs' voting behavior (see for example Hix (2002)). Since the coefficient on being from the same country is -0.0505, the overall effect of seating adjacency is approximately a tenth of the effect of shared nationality.

In Column (7), we consider a specification with the full set of controls and the coefficient is unchanged. In Appendix D, we consider whether peer effects operate at greater physical distances than immediate neighbors by comparing whether MEPs two, three, or four seats apart, or MEPs in the same row, vote more similarly than MEPs seated further apart. We find no evidence that seating peer effects are present beyond pairs of MEPs that are immediate neighbors. This further helps to reinforce our interpretation that the patterns we document are peer effects based on seating proximity, rather than some function of name similarity.[17]

As a further check on whether unobserved differences might be driving our results, we consider MEP pairs from the four parties that do not sit alphabetically and the non-alphabetically seated leaders of the otherwise alphabetically seated parties as placebo tests.[18] If the ITT and LATE estimates in our main analysis sample only reflect causal peer effects of seat proximity, we should not see a relationship between voting similarity and alphabetical adjacency in these alternative samples where seating is unrelated to surname.

For the sample of proposals and MEP pairs in which both MEPs are in the leadership section of alphabetically seated parties, we define $NameAdjacent_{ijt}$ as an indicator for whether the two MEPs were adjacent in the ranking of surnames within their leadership section.[19] In Table 5, Panel A, we examine how alphabetical adjacency affects seating and voting among leaders. In the first two columns, we show that, consistent with the non-alphabetization of leaders (and our casual empirics in Figure 2), alphabetical adjacency does not predict whether leaders are seat neighbors. In the next five columns, we repeat the intent-to-treat analysis from Table 4 in this alternative sample. The point estimates on $NameAdjacent_{ijt}$ are very close to zero (and in fact positive). In

[17] The layout of the European Parliament is such that front and back neighbors are distinctly further away than side-to-side neighbors. In addition to the incremental distance, the seating across rows is tiered and separated by sizeable desks, making physical interaction awkward. We therefore do not expect peer effects to lead to a correlation with the votes of MEPs in the front and back of an MEP. Additionally, we have no instrument for proximity of front-and-back MEPs.

[18] Data from non-alphabetical parties also illustrates the importance of our instrumental variable approach and the pitfalls of a naive specification that simply looks at the relationship between sitting together and disagreement without accounting for potential selection. In Table A.10 in Appendix H, we find that in parties that do not impose alphabetical seating, sitting together reduces agreement by 3 percentage points, an estimate that is 5 times greater than the selection-proof LATE estimate in from out main specification.

[19] This definition of $NameAdjacent_{ijt}$ exactly mirrors the one used in the main analysis sample in the sense that it focuses on alphabetical adjacency within the given group of MEPs. Because there are fewer leaders than non-leaders — in alphabetically seated parties, 32 percent of MEPs are leaders on average — one might be concerned that name adjacency in the group of leaders is a weaker correlate of name similarity. In Appendix E, we therefore redo the placebo test for the leaders while measuring name adjacency across the full EPG. This leads to similar results.

each specification, we can reject the null that having alphabetically adjacent last names reduces disagreement by more than 0.3 percentage points. In Panel B, we provide the same set of analyses for the sample of MEP pairs from non-alphabetical parties. Again, we see little evidence that alphabetical adjacency predicts voting disagreement (although we do note that estimates in this sample are sufficiently imprecise as to allow for sizeable peer effects). The lack of any correlation between alphabetical adjacency and voting behavior throughout Table 5 further mitigates concerns that our results are driven by unobservable MEP pair characteristics that correlate with name adjacency, such as socioeconomic backgrounds or regions of origin within countries.[20]

(Table 5 about here)

3.3 Contemporaneous versus persistent peer effects

Thus far, our treatment of peer effects has been entirely static in the sense that we have only considered an effect of sitting together during a particular vote. In other words, we have focused on *contemporaneous* peer effects whereby an individual is influenced by her immediate neighbor during the actual vote. But peer effects could also be *persistent* if they operate through altering peers' deeper allegiances or beliefs and thus influence future votes when the peers are no longer sitting next to one another. The persistent effects we document in this section also help to rule out some of the more mechanical forms of peer effects.[21]

We distinguish between these two types of peer effects by exploiting the two-venue nature of the EP. In each venue, an EPG's members are spread across several rows. Thus, even if there were perfect compliance with the alphabetical seating rule, there would still be some MEP pairs with adjacent names who do not sit next to each other because the first MEP is assigned the last seat in one row, while the second MEP is assigned the first seat of the next one. Moreover, row endings occur at different places in the two venues (cf: Figure 1), so some alphabetically adjacent MEPs sit next to each other in Brussels but not in Strasbourg, and vice versa. Figure 3 provides an example of this by zooming in on rows occupied by the Greens–European Free Alliance group, that are contained in the dashed rectangles of Figure 1. As this figure highlights, MEPs Jadot, Joly, and Keller are alphabetically consecutive MEPs from the group during the September-October 2009 sessions. Jadot and Joly are adjacent in both the Brussels and Strasbourg sessions, whereas Joly and Keller sit together in Strasbourg but not in Brussels, owing to a row end that separates them.

[20] One potential concern with the placebo tests conducted here is that name adjacency is a weaker correlate of name similarity when we look only at the small leadership section. We address this issue in Appendix E by conducting our placebo tests using a different measure of name adjacency and find similar results.

[21] This also helps to rule out the concern that the contemporaneous correlation in votes are a result of peers who dislike one another choosing not to attend the parliament.

We will exploit this variation in peer exposure over time to conduct a simple test for the existence of persistent peer effects.

(Figure 3 about here)

Define $SeatNeighborsPreviousVenue_{ijt}$ as an indicator for whether, during proposal t taking place in some venue, the MEP pair ij sat next to each other during the most recent proposal that did *not* take place in that venue. Furthermore define $SeatNeighborsBothVenues_{ijt}$ as an indicator for whether the MEP pair ij is seated adjacently in both the current venue and during the most recent proposal that did *not* take place in that venue. That is, $SeatNeighborsBothVenues_{ijt}$ is the interaction between $SeatNeighbors_{ijt}$ and $SeatNeighborsPreviousVenue_{ijt}$. To test for the existence of persistent peer effects, we then consider the following two specifications:

$$Disagree_{ijt} = \delta_0 + \delta_1 SeatNeighbors_{ijt} + \delta_2 SeatNeighborsPreviousVenue_{ijt} + \xi_{ijt} \tag{3}$$

$$\begin{aligned} Disagree_{ijt} = \eta_0 + \eta_1 SeatNeighbors_{ijt} + \eta_2 SeatNeighborsPreviousVenue_{ijt} \\ + \eta_3 SeatNeighborsBothVenues_{ijt} + \upsilon_{ijt}. \end{aligned} \tag{4}$$

In Equation 3, δ_1 captures the effect of an MEP pair sitting next to each other during current voting, while δ_2 captures the effect of having sat together in the past. To check for persistence in the observed peer effects, we can test whether past seating adjacency matters for current votes, i.e., whether $\delta_2 = 0$. As written, Equation 3 imposes that the effects of current and past seating are additively separable. Equation 4 additionally allows for an interaction between current and past seating. The hypothesis that past seating does not matter for current votes corresponds in this latter expression to having a zero coefficient on both the past seating variable and its interaction with current seating, i.e., $\eta_2 = \eta_3 = 0$.[22]

As before, because of possible sorting by like-minded MEPs into adjacent seats, we do not estimate Equation 3 directly using OLS but use the variation in current and past seating that is generated by the interaction between the alphabetical seating rule and the changing seat layouts. Using the layout of seats allocated to each EPG during each meeting in each of the venues, we therefore

[22] One potential concern with the specifications is that being seat neighbors in the previous venue is correlated with current proximity of seats even conditional on not being immediate seat neighbors. However, as we discuss in Appendix D, contemporaneous peer effects do not extend beyond the immediate neighbor, so this is unlikely to be an important confound.

compute the predicted seat and row for each of the MEPs in our main analysis sample, assuming perfect compliance with the alphabetical seating rules. From these predicted seating configurations, we construct self-explanatory variables $SeatNeighborsPredicted_{ijt}$, $SeatNeighborsPreviousVenuePredicted_{ijt}$, and $SeatNeighborsBothVenuesPredicted_{ijt}$. If the layout of seats in the two venues were the same, these variables would be almost perfectly collinear and only differ on dates when MEPs join or leave the non-leadership groups. Because of the differences in layouts across the two venues, however, there is substantial independent variation in these variables so they can serve as instruments in Equation (3).[23]

Table 6 presents the results that capture peer effects across venues. Throughout the table, we focus on our preferred specification that includes time fixed effects, EPG-by-EP fixed effects, and the baseline name similarity controls. We first present results from the specification that includes only $SeatNeighbors_{ijt}$ and $SeatNeighborsPreviousVenue_{ijt}$ (Equation 3). Column (1) presents reduced form estimates from regressing $Disagree_{ijt}$ directly on the two instruments (akin to the ITT estimates in previous tables), while Column (2) presents 2SLS estimates in which we instrument for $SeatNeighbor_{ijt}$ and $SeatNeighborsPreviousVenue_{ijt}$ (akin to the LATE estimates in previous tables). We find clear evidence of persistent peer effects in both sets of results. Focusing on the 2SLS estimates, the coefficient on $SeatNeighborsPreviousVenue$ is -0.0055 and is significant at the 5 percent level ($p = 0.014$), allowing us to reject the hypothesis that only current seating matters. The coefficient on $SeatNeighbor_{ijt}$ is much smaller (-0.0011) and is not statistically significant. At the same time, however, standard errors are large enough that we cannot rule out substantial effects of current seating or that the coefficients on $SeatNeighbor_{ijt}$ and $SeatNeighborsPreviousVenue_{ijt}$ are the same. In Columns (3) and (4), we move to the richer specification that allows current and past seating to interact. The conclusions from this analysis are similar. Focusing again on the 2SLS estimates, we find a coefficient of -0.004 on both $SeatNeighborsPreviousVenue$ and the interaction term; these are jointly significant, implying that past seating matters for current voting. The coefficient on $SeatNeighbor_{ijt}$ is again small and even slightly positive in this specification. However, the standard errors once again do not allow us to rule out that current seating has important independent effects or that the coefficients on all three seating variables are the same. Thus, we find support for the view that peer effects are persistent, so that past seating proximity matters for current voting irrespective of current seating. Unfortunately, our data does not allow

[23]In Appendix G, we show the first stages for these instruments. All instruments are highly significant in all the first-stage specifications. However, each of the predicted seating variables is a particularly strong predictor of its non-predicted counterpart (i.e., $SeatNeighborsPredicted_{ijt}$ is a particularly strong predictor of $SeatNeighbors_{ijt}$, while $SeatNeighborsPreviousVenuePredicted_{ijt}$ is a particularly strong predictor of $SeatNeighborsPreviousVenue_{ijt}$). This ensures that we have enough independent predictive power in the three instruments to identify the separate effects of all endogenous variables. Accordingly, the Sanderson and Windmeijer (2016) conditional first stage F-statistic measures of instrument strength presented are high for all of the endogenous variables.

us to make precise statements about the relative importance of current versus past seating.

(Table 6 about here)

The two-venue nature of the EP also provides us with additional placebo tests that examine whether being neighbors in a *future* venue reduces current disagreement. Specifically, in Table 7, we augment the specifications in Equations 3 and 4 by including the variables $SeatNeighborsFutureVenue_{ijt}$ (in the OLS specification) and $SeatNeighborsFutureVenuePredicted_{ijt}$ (in the 2SLS specification). The first of these variables is an indicator denoting whether members of the MEP pair will be seating neighbors during the first upcoming meeting that will take place in a different venue than the current one, and the second variable is constructed using the predicted seating indicators for actual seating adjcency in the future venue. Reassuringly, we find no significant impact of being (predicted) neighbors in the next venue on current levels of disagreement.

(Table 7 about here)

3.4 Heterogeneity in peer effects

While the results on the persistence of peer effects in the preceding section shed some light on the underlying mechanisms (e.g., the peer effects must involve something more than simple "parroting" behavior), we may further enrich our understanding of mechanisms by exploring the heterogeneity in their strength across MEP-pair and vote characteristics. We begin by examining whether seat neighbors that share salient social characteristics (namely gender and country of origin) influence each other more.[24] Shared social characteristics might strengthen peer effects either because of the greater deference that individuals show toward the ideas and interests of in-group members (Tajfel (1970)) or because social connection leads to more communication and thus greater influence.[25]

A common country of origin is a natural source of shared culture, language, and social ties. Moroever, existing work documents gender-specific influence in the context of job referrals (Bayer et al. (2008)) and information flows among stock analysts (Fang and Huang (2016)). Recent news coverage also supports the idea that women might have particularly strong influence on each other in the male-dominanated context of parliamentary politics. Two female members of the U.S. Senate, Barbara Murkowski and Susan Collins, sit in adjacent desks and have consistently voted together on key legislation, in opposition to their party's position. Further emphasizing the role of seat

[24]Table A.3 in Appendix B examines heterogeneous peer effects across the various other MEP characteristics available to us in our data; we find no significant heterogeneity along any other dimension.

[25]See, in particular, Garlick (2018), which finds that peer influence in academic among South African university students assigned to live in the same dormitory is stronger for students of the same race or enrolled in the same program.

proximity in accentuating their mutual influence, a *New York Times* column on their defiance of the party line reported that, "[Ms. Collins and Ms. Murkowski] discussed the possibility that the leadership might want to change their seating arrangement to keep them from being bad influences on each other."[26]

In Table 8 we split the sample by the MEP pairs' gender mix and by whether MEPs are from the same country, and repeat our primary specification.[27] The table shows that peer effects are indeed much greater if the MEP-pair are both women or if they come from the same country. In those cases, sitting together reduces disagreement by 1.2-1.4 percentage points, implying a persuasion rate between 17 and 40 percent. It is also worth noting that the two sources of social proximity do not have additive effects: the impact of sitting together on two female MEPs is a 1.3-1.4 percentage point reduction in disagreement regardless of whether the MEPs are from the same country or not. These results might suggest that for peer effects to operate, the MEP-pair needs to clear some threshold of pre-existing social affinity.[28]

(Table 8 about here)

We next explore whether peer effects are more pronounced in high-stakes situations, by focusing on votes for proposals that were either passed or defeated by a small margin. Table 9 repeats our preferred specifications for samples of proposals with different win margins. Columns (1) and (2) show ITT and LATE estimates for the sample of proposals that passed or were defeated comfortably, as defined by a win margin of more than 10 percent. Ninety percent of our sample is comprised of such "comfortable" votes, and thus it is unsurprising that the estimated peer effects for this subsample are very similar to those reported in our main results. The LATE estimate in Column (2) implies that adjacent seating lowers disagreement by 0.55 percentage points. Columns (3) and (4) estimate peer effects in proposals that passed with a small margin of victory, defined by a win margin of less than 10 percent. The estimated peer effects in these close votes are about twice those found for "comfortable" votes, a difference which is marginally significant ($p = 0.09$ for both ITT and LATE estimates). The next four columns show corresponding estimates for narrower win margins of 5 and 1 percentage points respectively. These yield similar or even slightly larger estimated peer effects. Overall, these results suggest that peer effects are stronger when vote outcomes are close.[29]

[26] See "Lisa Murkowski, a Swing Vote on Health Care, Isn't Swayed," *New York Times*, page A1, July 26, 2017.

[27] For completeness, Tables A.1 and A.2 in Appendix B show results where the sample is instead split only by gender mix or shared country of origin.

[28] In Appendix F, we examine whether peer effects operate across parties, a setting in which pre-existing social distance between MEPs is likely to be high. We do so by examining the votes of MEPs seated at row ends within each party. We find no evidence that peer effects operate across party lines; however, confidence intervals do allow for non-trivial peer effects across parties.

[29] It is possible to explore heterogeneity based on other characteristics of the proposal up for vote. In Appendix C we examine whether the strength of peer effects differs as a function of whether MEPs' countries are aligned in

(Table 9 about here)

4 Conclusion

We exploit alphabetical seating assignments of Members of the European Parliament to identify peer influence among politicians. Through a combination of placebo tests and detailed name similarity controls, we bolster the interpretation that the patterns we observe are the result of peer influence rather than unobserved commonalities among MEP pairs with alphabetically adjacent names.

We further document important heterogeneity in these effects. Peer effects are stronger for MEP pairs that are from the same county or that are both women. These results suggest that similarity along other dimensions can serve to reinforce peer influence from direct exposure.

One useful aspect of the European Parliament as a setting for studying peer effects is its migration between two venues, Brussels and Strasbourg, with distinct seating arrangements. This allows us to explore both contemporaneous and persistent peer effects. While limited statistical power prevents us from drawing sharp conclusions regarding their relative magnitude, we confirm that peer effects do have a persistent component. This suggests that peer influence extends beyond mere parroting to impact beliefs or alliances.

how they vote. We find that the estimated peer effect is similar, regardless of the concordance in voting between the MEPs from the pair's home countries. Of course, the baseline rate of disagreement for MEP pairs is lower when the MEPs' countries are well-aligned, which implies that the persuasion rate is much higher for pairs for which ex ante alignment is stronger.

References

Angrist, J. D. (2014). The perils of peer effects. *Labour Economics 30*, 98–108.

Aronow, P. M., C. Samii, and V. A. Assenova (2015). Cluster–Robust Variance Estimation for Dyadic Data. *Political Analysis*.

Battaglini, M. and E. Patacchini (2016). Influencing Connected Legislators. *NBER Working Paper*.

Bayer, P., S. L. Ross, and G. Topa (2008). Place of work and place of residence: Informal hiring networks and labor market outcomes. *Journal of Political Economy 116*(6), 1150–1196.

Cameron, C. A. and D. L. Miller (2014). Robust Inference for Dyadic Data. *Working Paper*.

Campos, C., S. H. Heap, and F. de Leon (2013). The political influence of peer groups: Experimental evidence in the classroom.

Canen, N. and F. Trebbi (2016). Endogenous Network Formation in Congress. *NBER Working Paper*.

Cohen, L. and C. J. Malloy (2014). Friends in high places. *American Economic Journal: Economic Policy 6*(3), 63–91.

DellaVigna, S. and M. Gentzkow (2010). Persuasion: Empirical Evidence. *Annual Review of Economics 2*(1), 643–669.

DellaVigna, S., J. A. List, U. Malmendier, and G. Rao (2017). Voting to tell others. *The Review of Economic Studies 84*(1), 143–181.

Duflo, E., P. Dupas, and M. Kremer (2011). Peer Effects, Teacher Incentives, and the Impact of Tracking: Evidence from a Randomized Evaluation in Kenya. *American Economic Review 101*(5), 1739–1774.

Fang, L. H. and S. Huang (2016). Gender and connections among wall street analysts.

Fisman, R., N. A. Harmon, E. Kamenica, and I. Munk (2015). Labor supply of politicians. *Journal of the European Economic Association 13*(5), 871–905.

Garlick, R. (2018). Academic peer effects with different group assignment policies: Residential tracking versus random assignment. *American Economic Journal: Applied Economics 10*(3), 345–69.

Guryan, J., K. Kroft, and M. J. Notowidigdo (2009). Peer effects in the workplace: Evidence from random groupings in professional golf tournaments. *American Economic Journal: Applied Economics 1*(4), 34–68.

Herbst, D. and A. Mas (2015). Peer effects on worker output in the laboratory generalize to the field. *Science 350*(6260), 545–549.

Hix, S. (2002). Parliamentary Behavior with Two Principals: Preferences, Parties, and Voting in the European Parliament. *American Journal of Political Science 46*(3), 688.

Holden, R., M. Keane, and M. Lilley (2016). Peer effects on the united states supreme court.

Jo, D. and M. Lowe (2017). Social Learning of Political Elites: A Natural Experiment in Iceland. *Working Paper*.

Lazer, D. (2011). Networks in political science: Back to the future. *PS: Political Science & Politics*.

Manski, C. F. (1993). Identification of endogenous social effects: The reflection problem. *The review of economic studies 60*(3), 531–542.

Mas, A. and E. Moretti (2009). Peers at work. *The American Economic Review 99*(1), 112–145.

Masket, S. (2008). Where you sit is where you stand: The impact of seating proximity on legislative cue-taking. *Quarterly Journal of Political Science 3*, 301–311.

Nickerson, D. W. (2008). Is voting contagious? evidence from two field experiments. *American Political Science Review 102*(01), 49–57.

Perez-Truglia, R. (2016). Political conformity: Event-study evidence from the united states.

Perez-Truglia, R. and G. Cruces (2016). Partisan interactions: Evidence from a field experiment in the united states.

Ringe, N., J. N. Victor, and J. H. Gross (2013). Keeping your friends close and your enemies closer? information networks in legislative politics. *British Journal of Political Science 43*(3), 601.

Rogowski, J. C. and B. Sinclair (2012). Estimating the causal effects of social interaction with endogenous networks. *Political Analysis*.

Routt, G. C. (1938). Interpersonal relationships and the legislative process. *Annals of the American Academy of Political and Social Science 195*(1), 129–136.

Sacerdote, B. (2011). Peer effects in education: How might they work, how big are they and how much do we know thus far? *Handbook of the Economics of Education 3*, 249–277.

Saia, A. (2017). Random Interactions in the Chamber: Legislators' Behavior and Political Distance. *Working Paper*.

Sanderson, E. and F. Windmeijer (2016). A weak instrument F-test in linear IV models with multiple endogenous variables. *Journal of Econometrics 190*(2), 212–221.

Stock, J. and M. Yogo (2005). *Testing for Weak Instruments in Linear IV Regression*, pp. 80–108. New York: Cambridge University Press.

Tajfel, H. (1970). Experiments in intergroup discrimination. *Scientific American 223*(5), 96–103.

Truman, D. B. (1956). The state delegations and the structure of party voting in the united states house of representatives. *American Political Science Review 50*(04), 1023–1045.

Tables and Figures

Table 1: Overview of samples used in the analysis

	Main analysis sample: Non-leaders, EPGs using alphabetical seating	Placebo sample 1: EPGs not using alphabetical seating	Placebo sample 2: Leaders, EPGs using alphabetical seating
Frequencies:			
Number of EPGs	6	4	5
Number of MEPs	857	165	320
Number of cast votes	1,820,233	357,579	680,915
MEP characteristics:			
Mean age	51.07	53.41	53.67
Mean tenure in EP	3.56	3.26	6.51
Share top ranked education	0.25	0.21	0.31
Share women	0.39	0.21	0.32

The table shows counts for the various subsamples in the data, as well as means and shares of MEP characteristics for the different subsamples. An MEP is coded as belonging to one of the three groups if he or she is ever observed in that group. As a result, the groups overlap and table numbers do not add up to the full sample totals. MEPs' characteristics are measured at the time of their first observed vote in the data. Means and shares are computed over individual MEPs.

Table 2: Summary statistics

	N	mean	sd	min	max
MEP Pair characteristics:					
Same country	107,325,010	0.0782	0.2685	0	1
Same quality education	107,325,010	0.6085	0.4881	0	1
Same freshman status	107,325,010	0.5218	0.4995	0	1
Age difference (years)	107,325,010	1.1691	0.8724	0	5.5953
Tenure difference (years)	107,325,010	0.5258	0.5632	0	2.9660
Same gender	107,325,010	0.5474	0.4977	0	1
Number of women	107,325,010	0.7347	0.6907	0	2
Voting and seating:					
Disagree	107,325,010	0.0855	0.2796	0	1
Name adjacent	107,325,010	0.0137	0.1163	0	1
Seat neighbors	107,325,010	0.0127	0.1119	0	1
Seat neighbors, predicted	107,325,010	0.0126	0.1117	0	1
Seat neighbors, previous venue	101,126,434	0.0124	0.1107	0	1
Seat neighbors, previous venue, predicted	101,126,434	0.0124	0.1106	0	1
Seat neighbors, both venues	101,126,434	0.0108	0.1033	0	1
Seat neighbors, both venues, predicted	101,126,434	0.0114	0.1059	0	1
Name similarity measures:					
Same name	107,325,010	0.0001	0.0076	0	1
Overall name rank gap	107,325,010	416	293	1	1,258
Names sound alike	107,325,010	0.0005	0.0231	0	1
Levenshtein name similarity	107,325,010	0.1223	0.1003	0	1
Bigram-Jaccard name similarity	107,325,010	0.0379	0.0801	0	1

The table provides summary statistics for the main analysis sample. Observations are at the level of the proposal-by-MEP-pair. *Same quality education* is an indicator variable denoting that both MEPs have the same quality of education (i.e., either both or neither have high quality), as measured by a degree from a top 500 university. *Same freshman status* is an indicator for whether both MEPs have the same freshman status. *Name adjacent* is an indicator for whether the pair of MEPs is immediately adjacent in the alphabetical ordering of surnames within its seating section (the non-leadership section of their EPG) at the time of the proposal's vote. *Seat neighbors* is an indicator for whether members of the MEP pair are seating neighbors. *Seat neighbors, previous venue* is an indicator for whether members of the MEP pair were seating neighbors during the most recent meeting that took place in a different venue than the current one. *Seat neighbors, both venues* is an indicator for whether members of the MEP pair are currently seating neighbors and were also seating neighbors during the most recent meeting that took place in a different venue than the current one. *Names sound alike* is an indicator for whether the MEPs' surnames sound alike according to the SoundEx-algorithm. *Overall name rank gap* is the distance between the MEPs' last names in the alphabetical ranking of all MEP last names in the data. All other variables are self-explanatory. Variables with the suffix "predicted" were constructed from counterfactual seating charts that take as given the layout of seats within the non-leadership section of each EPG on each day but assume perfect compliance with the alphabetical seating rules within the section. For variables involving information about the previous venue, the number of observations is lower because these variables are undefined for the first few meetings in the data.

Table 3: Covariate balance for alphabetically adjacent versus non-adjacent MEP pairs

Panel A - no controls

	(1) Seat neighbors	(2) Same country	(3) Same educ. quality	(4) Same freshman status	(5) Same gender	(6) Age difference	(7) Tenure difference
Name adjacent	0.8404***	0.0236**	-0.0183	-0.0047	-0.0230	-0.0138	-0.0044
	(0.0201)	(0.0100)	(0.0141)	(0.0180)	(0.0204)	(0.0249)	(0.0104)
Constant	0.0012***	0.0779***	0.6088***	0.5218***	0.5477***	1.1693***	0.5258***
	(0.0003)	(0.0031)	(0.0255)	(0.0067)	(0.0202)	(0.0279)	(0.0206)
Observations	107,325,010	107,325,010	107,325,010	107,325,010	107,325,010	107,325,010	107,325,010
Clusters	76	76	76	76	76	76	76

Panel B - baseline name similarity controls

	(1) Seat neighbors	(2) Same country	(3) Same educ. quality	(4) Same freshman status	(5) Same gender	(6) Age difference	(7) Tenure difference
Name adjacent	0.7959***	0.0020	-0.0233	-0.0183	0.0034	-0.0282	-0.0121
	(0.0257)	(0.0133)	(0.0229)	(0.0225)	(0.0242)	(0.0381)	(0.0173)
Same name	-0.0324	0.5470***	0.1868	0.1783	0.3620*	-0.6824***	-0.4548***
	(0.0618)	(0.1760)	(0.1594)	(0.1855)	(0.2022)	(0.2571)	(0.0786)
Overall name rank gap is 1	0.0845**	0.0342	-0.0214	-0.0399	-0.0888	0.1245	0.0683
	(0.0348)	(0.0356)	(0.0464)	(0.0513)	(0.0561)	(0.1075)	(0.0500)
Overall name rank gap is 2-5	0.0686***	0.0165	0.0073	0.0257	-0.0325	0.0338	0.0473
	(0.0161)	(0.0206)	(0.0308)	(0.0303)	(0.0227)	(0.0523)	(0.0351)
Overall name rank gap is 6-10	0.0327***	0.0211	0.0034	-0.0247	-0.0030	-0.0291	0.0327
	(0.0086)	(0.0153)	(0.0197)	(0.0176)	(0.0280)	(0.0503)	(0.0244)
Overall name rank gap is 11-20	0.0282***	-0.0174**	-0.0083	-0.0042	-0.0375***	0.0717***	0.0299*
	(0.0085)	(0.0082)	(0.0132)	(0.0147)	(0.0116)	(0.0252)	(0.0160)
Overall name rank gap is 21-40	0.0048**	0.0054	-0.0169	-0.0075	-0.0076	0.0145	0.0203
	(0.0019)	(0.0060)	(0.0145)	(0.0131)	(0.0103)	(0.0224)	(0.0146)
Overall name rank gap is 41-80	0.0005	-0.0007	-0.0143**	-0.0224***	-0.0038	0.0185	0.0419***
	(0.0008)	(0.0061)	(0.0056)	(0.0064)	(0.0163)	(0.0162)	(0.0082)
Overall name rank gap is 81-160	-0.0001	0.0012	-0.0044	-0.0199***	-0.0176	0.0127	0.0289**
	(0.0003)	(0.0048)	(0.0100)	(0.0047)	(0.0111)	(0.0121)	(0.0132)
Overall name rank gap is 161-320	0.0001	0.0016	0.0005	-0.0225***	-0.0191	0.0123	0.0409**
	(0.0002)	(0.0062)	(0.0089)	(0.0083)	(0.0160)	(0.0192)	(0.0162)
Overall name rank gap is 321-640	0.0002	-0.0000	-0.0052	-0.0177**	-0.0198	0.0190	0.0319***
	(0.0003)	(0.0047)	(0.0048)	(0.0087)	(0.0160)	(0.0245)	(0.0091)
EP-by-EPG fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	107,325,010	107,325,010	107,325,010	107,325,010	107,325,010	107,325,010	107,325,010
Clusters	76	76	76	76	76	76	76

Each panel of the table presents OLS regression results. Observations are proposals-by-MEP-pairs in the main analysis sample. The outcome variables in both panels are: an indicator for the pair sitting next to each other (Column (1)); an indicator for whether the two MEPs are from the same country (Column (2)); an indicator for whether the two MEPs have the same education quality, i.e., either both MEPs have or neither has a degree from a top 500 university (Column (3)); an indicator for same freshman status, i.e., either both MEPs or neither is a freshman (Column (4)); an indicator for same gender (Column (5)); the age difference between the two MEPs (Column (6)); and the difference in EP tenure between the two MEPs (Column (7)). In Panel A, the only regressor is an indicator for whether members of the MEP pair are immediately adjacent in the alphabetical ordering of surnames within their seating section (the non-leadership section of their EPG). Panel B also includes EP-by-EPG fixed effects, an indicator for whether the MEP pair has the same surname and a flexible set of indicators to capture the distance between the MEPs' last names in the alphabetical ranking of all MEP last names in our data. Standard errors in parentheses are dyadic cluster-robust, clustered at the level of row-by-EPG-by-parliamentary-term. The number of such clusters is listed in the bottom row of the table. *** p<0.01, ** p<0.05, * p<0.1

Table 4: Peer effects - main analysis

	(1) Disagree OLS	(2) Disagree OLS	(3) Disagree OLS	(4) Disagree OLS	(5) Disagree OLS	(6) Disagree 2SLS	(7) Disagree 2SLS
Name adjacent	-0.0116***	-0.0048**	-0.0047*	-0.0046*	-0.0047**		
	(0.0019)	(0.0024)	(0.0024)	(0.0023)	(0.0024)		
Seat neighbors						-0.0060**	-0.0060*
						(0.0030)	(0.0030)
Same quality education			-0.0029**	-0.0029**	-0.0029**		-0.0029**
			(0.0012)	(0.0012)	(0.0012)		(0.0012)
Same freshman status			-0.0001	-0.0001	-0.0001		-0.0001
			(0.0019)	(0.0018)	(0.0019)		(0.0019)
Same country			-0.0505***	-0.0507***	-0.0507***		-0.0507***
			(0.0042)	(0.0042)	(0.0043)		(0.0043)
Age difference			0.0012	0.0012	0.0012		0.0012
			(0.0012)	(0.0012)	(0.0012)		(0.0012)
Tenure difference			0.0004	0.0004	0.0005		0.0005
			(0.0030)	(0.0030)	(0.0030)		(0.0030)
Same gender			0.0022	0.0021	0.0021		0.0021
			(0.0015)	(0.0015)	(0.0015)		(0.0015)
Day-level fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline name controls	No	Yes	Yes	Yes	Yes	Yes	Yes
EP-by-EPG fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes
Additional name similarity controls	No	No	No	Yes	Yes	No	Yes
Additional name rank gap controls	No	No	No	No	Yes	No	Yes
Observations	107,325,010	107,325,010	107,325,010	107,325,010	107,325,010	107,325,010	107,325,010
Clusters	76	76	76	76	76	76	76
Implied persuasion rate	0.1357	0.0561	0.0550	0.0538	0.0550	0.0702	0.0702
F-stat						958.3	960.3

Observations in the reported regression results are proposals-by-MEP-pairs in the main analysis sample of non-leader MEPs from alphabetical parties. The outcome variable is an indicator variable denoting whether the MEP pair cast different votes on the proposal. *Name adjacent* is an indicator for whether members of the MEP pair is immediately adjacent in the alphabetical ordering of surnames within their seating section (the non-leadership section of their EPG). *Seat neighbors* is an indicator for whether the MEP pair are seated adjacently. The remaining variables are self-explanatory (see Table 2 notes for detailed definitions). "Baseline name controls" are comprised of an indicator for whether the MEPs have the same last name and a flexible set of indicators to capture the distance between the MEPs' last names in the alphabetical ranking of all MEP last names in our data. "Additional name similarity controls" include cubic polynomials in Bigram-Jaccard and Levensthein name similarity as well as an indicator variable for whether the names sound alike under the SoundEx algorithm. "Additional name rank gap controls" are indicators for every 10-seat bin in the "overall name rank gap" variable (as described in the main text). Estimates in Columns (1)–(5) were obtained via OLS. Estimates in Columns (6) and (7) were obtained using 2SLS, using the indicator for whether members of the MEP pair is immediately adjacent in the alphabetical ordering of surnames within their seating section to instrument for whether the MEP pair is seated adjacently (the same sets of name similarity controls are employed in the first-stage estimation). Standard errors in parentheses are dyadic cluster-robust, clustered at the level of row-by-EPG-by-parliamentary term. The number of such clusters is listed in the bottom section of the table. The persuasion rate implied by the estimated peer effect is based on DellaVigna and Gentzkow (2010). The listed F-statistics in the 2SLS column correspond to the first- stage F-statistic measure of instrument strength proposed by Stock and Yogo (2005). *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 5: Placebo test - leaders and non-alphabetical parties

Panel A - leadership sections of alphabetical parties

	(1) Seat neighbors	(2) Seat neighbors	(3) Disagree	(4) Disagree	(5) Disagree	(6) Disagree	(7) Disagree
Name adjacent	0.0170	-0.0111	0.0005	0.0022	0.0029	0.0026	0.0024
	(0.0106)	(0.0119)	(0.0014)	(0.0018)	(0.0020)	(0.0019)	(0.0016)
Observations	18,772,462	18,772,462	18,772,462	18,772,462	18,772,462	18,772,462	18,772,462
Clusters	69	69	69	69	69	69	69

Panel B - non-alphabetical parties

	(1) Seat neighbors	(2) Seat neighbors	(3) Disagree	(4) Disagree	(5) Disagree	(6) Disagree	(7) Disagree
Name adjacent	-0.0073	0.0046	0.0104	-0.0048	-0.0046	-0.0043	-0.0026
	(0.0132)	(0.0189)	(0.0109)	(0.0054)	(0.0052)	(0.0053)	(0.0054)
Observations	4,917,460	4,917,460	4,917,460	4,917,460	4,917,460	4,917,460	4,917,460
Clusters	49	49	49	49	49	49	49
Day-level fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
EP-by-EPG fixed effects	No	Yes	No	Yes	Yes	Yes	Yes
Baseline name controls	No	Yes	No	Yes	Yes	Yes	Yes
Observable pair characteristics	No	Yes	No	Yes	No	No	Yes
Additional name similarity controls	No	Yes	No	No	No	Yes	Yes
Additional name rank gap controls	No	Yes	No	No	No	No	Yes

Observations in the presented regression results are proposals-by-MEP-pairs. Panel A includes all observations in which both MEPs are leaders of the same alphabetically seated EPG. Panel B includes all observations in which both MEPs are from the same non-alphabetically seated EPG. The outcome variable in Columns (1) and (2) is an indicator for whether the MEP pair is seated adjacently, and in Columns (3)–(7) it is an indicator for whether the MEP pair cast different votes on the proposal. The control variables listed at the bottom are included in the analyses of both Panels A and B. *Name adjacent* is an indicator for whether members of the MEP pair are immediately adjacent in the alphabetical ordering of surnames within their seating section (the non-leadership section of their EPG). *Seat neighbors* is an indicator for whether the MEP pair is seated adjacently. The remaining variables are self-explanatory (see Table 2 notes for detailed definitions). "Baseline name controls" are comprised of an indicator for whether the MEPs have the same last name and a flexible set of indicators to capture the distance between the MEPs' last names in the alphabetical ranking of all MEP last names in our data. "Additional name similarity controls" include cubic polynomials in Bigram-Jaccard and Levensthein name similarity as well as an indicator variable for whether the names sound alike under the SoundEx algorithm. "Additional name rank gap controls" are indicators for every 10-seat bin in the "overall name rank gap" variable (as described in the main text). Standard errors in parentheses are dyadic cluster-robust, at the level of row-by-EPG-by-parliamentary-term. The number of such clusters is listed in the bottom row of each panel. *** p<0.01, ** p<0.05, * p<0.1

Table 6: Measuring contemporaneous versus persistent peer effects using venue variation

	(1) Disagree OLS	(2) Disagree 2SLS	(3) Disagree OLS	(4) Disagree 2SLS
Seat neighbors, predicted	-0.0011		-0.0005	
	(0.0023)		(0.0042)	
Seat neighbors, previous venue, predicted	-0.0043***		-0.0036	
	(0.0016)		(0.0036)	
Seat neighbors, both venues, predicted			-0.0015	
			(0.0059)	
Seat neighbors		-0.0009		0.0007
		(0.0033)		(0.0061)
Seat neighbors, previous venue		-0.0055**		-0.0036
		(0.0025)		(0.0053)
Seat neighbors, both venues				-0.0037
				(0.0093)
Day-level fixed effects	Yes	Yes	Yes	Yes
EP-by-EPG fixed effects	Yes	Yes	Yes	Yes
Baseline name controls	Yes	Yes	Yes	Yes
Observations	101,126,434	101,126,434	101,126,434	101,126,434
Clusters	76	76	76	76
F-stat: Seat neighbors		221.5		104.7
F-stat: Seat neighbors, previous venue		125.3		69.16
F-stat: Seat neighbors, both venues				60.91
p-value: past seating does not matter			0.028**	0.099*
p-value: coefficients are equal	0.329	0.391	0.609	0.711

Observations for the presented regression results are proposals-by-MEP pairs in the main analysis sample, excluding the dates prior to the first observed venue change, in which there is no information about previous venue. In all columns, the outcome variable is an indicator variable denoting whether the MEP pair cast different votes on the proposal. *Name adjacent* is an indicator for whether the pair of MEPs is immediately adjacent in the alphabetical ordering of surnames within its seating section (the non-leadership section of their EPG) at the time of the proposal's vote. *Seat neighbors* is an indicator for whether members of the MEP pair are seating neighbors. *Seat neighbors, previous venue* is an indicator for whether members of the MEP pair were seating neighbors during the most recent meeting that took place in a different venue than the current one. *Seat neighbors, both venues* is an indicator for whether members of the MEP pair are currently seating neighbors and were also seating neighbors during the most recent meeting that took place in a different venue than the current one. In addition to day-level and EP-by-EPG fixed effects, all columns include the following "Baseline name similarity controls": an indicator for whether the MEPs have the same last name and a flexible set of indicators to capture the distance between the MEPs' last names in the alphabetical ranking of all MEP last names in our data. Estimates in Columns (1) and (3) were obtained via OLS. Estimates in Columns (2) and (4) were obtained using 2SLS, using the the predicted seating indicators as instruments for the actual seating variables. Standard errors in parentheses are dyadic cluster-robust, clustered at the level of row-by-EPG-by-parliamentary-term. The number of such clusters is listed in the bottom section of the table. The listed F-statistics in the 2SLS column correspond to Sanderson and Windmeijer (2016)'s "conditional first stage F-statistic" measures of instrument strength under multiple endogenous variables. For Columns (3) and (4) the table shows p-values from a Wald test of the hypothesis that the coefficients on *Seat neighbors, previous venue* and *Seat neighbors, both venues* or their predicted versions are both zero ("past seating does not matter"). For all columns the table shows p-values from a Wald test of the hypothesis that all listed coefficients are the same ("coefficients are equal"). *** p<0.01, ** p<0.05, * p<0.1

Table 7: Placebo test - the effect of future seating

	(1) Disagree OLS	(2) Disagree 2SLS	(3) Disagree OLS	(4) Disagree 2SLS
Seat neighbors, future venue, predicted	0.0030		0.0027	
	(0.0024)		(0.0023)	
Seat neighbors, future venue		0.0045		0.0040
		(0.0037)		(0.0035)
Seat neighbors, predicted	-0.0004		0.0008	
	(0.0021)		(0.0044)	
Seat neighbors, previous venue, predicted	-0.0078***		-0.0064	
	(0.0026)		(0.0040)	
Seat neighbors, both venues, predicted			-0.0024	
			(0.0061)	
Seat neighbors		0.0001		0.0027
		(0.0032)		(0.0064)
Seat neighbors, previous venue		-0.0108**		-0.0077
		(0.0042)		(0.0060)
Seat neighbors, both venues				-0.0054
				(0.0096)
Day-level fixed effects	Yes	Yes	Yes	Yes
EP-by-EPG fixed effects	Yes	Yes	Yes	Yes
Baseline name controls	Yes	Yes	Yes	Yes
Observations	86,517,817	86,517,817	86,517,817	86,517,817
Clusters	76	76	76	76
F-stat: Seat neighbors, future venue		82.48		82.71
F-stat: Seat neighbors		88.96		60.35
F-stat: Seat neighbors, previous venue		75.82		115.7
F-stat: Seat neighbors, both venues				43.17
p-value: same effect of future/previous venue	0.0213**	0.0381**	0.0998*	0.158

Observations for the presented regression results are proposals-by-MEP pairs in the main analysis sample, excluding the dates prior to the first observed venue change, in which there is no information about previous venue, and also excluding the dates after the last venue change in which there is no information about future venues. In all columns, the outcome variable is an indicator variable denoting whether the MEP pair cast different votes on the proposal. *Name adjacent* is an indicator for whether the pair of MEPs is immediately adjacent in the alphabetical ordering of surnames within its seating section (the non-leadership section of their EPG) at the time of the proposal's vote. *Seat neighbors* is an indicator for whether members of the MEP pair are seat neighbors. *Seat neighbors, previous venue* is an indicator for whether members of the MEP pair were seat neighbors during the most recent meeting that took place in a different venue than the current one. *Seat neighbors, future venue* is an indicator for whether members of the MEP pair will be seat neighbors during the first upcoming meeting that will take place in a different venue than the current one. *Seat neighbors, both venues* is an indicator for whether members of the MEP pair are currently seat neighbors and were also seat neighbors during the most recent meeting that took place in a different venue than the current one. In addition to day-level and EP-by-EPG fixed effects, all columns include the following "Baseline name similarity controls": an indicator for whether the MEPs have the same last name and a flexible set of indicators to capture the distance between the MEPs' last names in the alphabetical ranking of all MEP last names in our data. Estimates in Columns (1) and (3) were obtained via OLS. Estimates in Columns (2) and (4) were obtained using 2SLS, using the predicted seating indicators as instruments for the actual seating variables. Standard errors in parentheses are dyadic cluster-robust, clustered at the level of row-by-EPG-by-parliamentary-term. The number of such clusters is listed in the bottom section of the table. The listed F-statistics in the 2SLS column correspond to Sanderson and Windmeijer (2016)'s "conditional first stage F-statistic" measures of instrument strength under multiple endogenous variables. For all columns the table shows p-values from a Wald test of the hypothesis that the coefficients on *Seat neighbors, previous venue* and *Seat neighbors, future venue* or their predicted versions are the same ("same effect of future/previous venue"). *** p<0.01, ** p<0.05, * p<0.1

Table 8: Peer effects by gender mix and same country of origin

Panel A - MEPs in pair from different countries						
SUBSAMPLE:	Two women		One woman, one man		Two men	
	(1) Disagree OLS	(2) Disagree 2SLS	(3) Disagree OLS	(4) Disagree 2SLS	(5) Disagree OLS	(6) Disagree 2SLS
Name adjacent	-0.0113***		-0.0017		-0.0028	
	(0.0038)		(0.0024)		(0.0048)	
Seat neighbors		-0.0142***		-0.0022		-0.0035
		(0.0045)		(0.0031)		(0.0060)
Observations	13,838,010	13,838,010	45,058,692	45,058,692	40,032,900	40,032,900
Clusters	63	63	76	76	71	71
Disagree mean	0.0796	0.0796	0.0866	0.0866	0.0963	0.0963
Implied persuasion rate	0.1420	0.1784	0.0196	0.0254	0.0291	0.0363
F-stat		224		617.6		744.3
Panel B - MEPs in pair from same country						
SUBSAMPLE:	Two women		One woman, one man		Two men	
	(1) Disagree OLS	(2) Disagree 2SLS	(3) Disagree OLS	(4) Disagree 2SLS	(5) Disagree OLS	(6) Disagree 2SLS
Name adjacent	-0.0113***		-0.0107***		-0.0099	
	(0.0040)		(0.0035)		(0.0078)	
Seat neighbors		-0.0131***		-0.0120***		-0.0129
		(0.0046)		(0.0038)		(0.0103)
Day-level fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
EP-by-EPG fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Baseline name controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,301,556	1,301,556	3,516,470	3,516,470	3,577,382	3,577,382
Clusters	66	66	75	75	70	70
Disagree mean	0.0332	0.0332	0.0362	0.0362	0.0410	0.0410
Implied persuasion rate	0.3404	0.3946	0.2956	0.3315	0.2415	0.3146
F-stat		378.3		826.8		205.3

Observations for the presented regression results are proposals-by-MEP pairs from the main analysis sample. The two panels and the different columns correspond to different subsamples of MEP pairs. In Panel A, the sample only includes MEP pairs from different countries. In Panel B, the sample only include MEP pairs from the same country. In Columns (1) and (2), the sample only includes all-women MEP pairs. In Columns (3) and (4), the sample only includes mixed-gender MEP pairs. In Columns (5) and (6), the sample only includes all-male MEP pairs. In all columns, the outcome variable is an indicator variable denoting whether the MEP pair cast different votes on the proposal. *Name adjacent* is an indicator for whether members of the MEP pair are immediately adjacent in the alphabetical ordering of surnames within their seating section (the non-leadership section of their EPG). *Seat neighbors* is an indicator for whether the MEP pair is seated adjacently. In addition to day-level and EP-by-EPG fixed effects, all columns include the following "Baseline name similarity controls": an indicator for whether the MEPs have the same last name and a flexible set of indicators to capture the distance between the MEPs' last names in the alphabetical ranking of all MEP last names in our data. Estimates in Columns (1), (3), and (5) were obtained via OLS. Estimates in Columns (2), (4), and (6) were obtained using 2SLS, using the indicator for whether members of the MEP pair are immediately adjacent in the alphabetical ordering of surnames within their seating section to instrument for whether the MEP pair is seated adjacently (the same sets of name similarity controls are employed in the first-stage estimation). Standard errors in parentheses are dyadic cluster-robust, clustered at the level of row-by-EPG-by-parliamentary-term. The number of such clusters is listed in the bottom section of the table. The persuasion rate implied by the estimated peer effect is based on DellaVigna and Gentzkow (2010). The listed F-statistics in the 2SLS columns correspond to the first-stage F-statistic measure of instrument strength proposed by Stock and Yogo (2005). *** p<0.01, ** p<0.05, * p<0.11

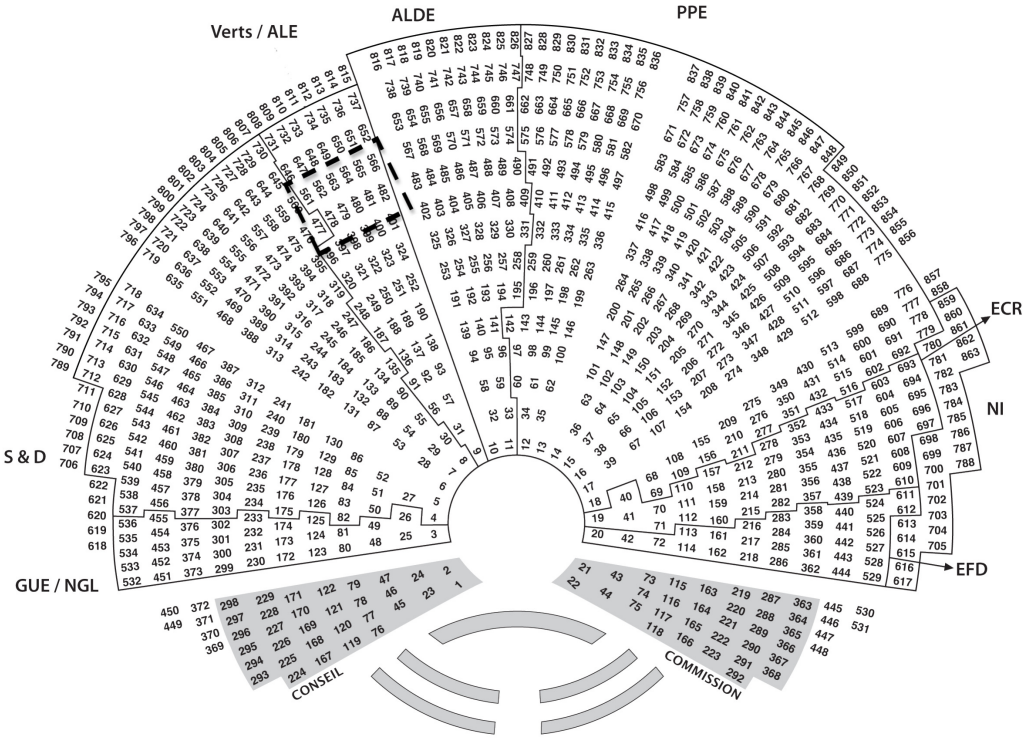
Table 9: Peer effects in proposals with small margins of victory

SAMPLE:	Win margin > 10 percent		Win margin ≤ 10 percent		Win margin ≤ 5 percent		Win margin ≤ 1 percent	
	(1) Disagree OLS	(2) Disagree 2SLS	(3) Disagree OLS	(4) Disagree 2SLS	(5) Disagree OLS	(6) Disagree 2SLS	(7) Disagree OLS	(8) Disagree 2SLS
Name adjacent	-0.0044*		-0.0085***		-0.0086***		-0.0158***	
	(0.0024)		(0.0031)		(0.0032)		(0.0047)	
Seat neighbors		-0.0055*		-0.0107***		-0.0108***		-0.0197***
		(0.0030)		(0.0038)		(0.0040)		(0.0057)
Day-level fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
EP-by-EPG fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline name controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	97,082,264	97,082,264	10,242,746	10,242,746	5,233,431	5,233,431	863,769	863,769
Clusters	76	76	73	73	71	71	70	70
Disagree mean	0.0835	0.0835	0.104	0.104	0.100	0.100	0.109	0.109
Implied persuasion rate	0.0527	0.0659	0.0816	0.1027	0.0860	0.1080	0.1444	0.1801
F-stat		948.7		1032		1027		1080

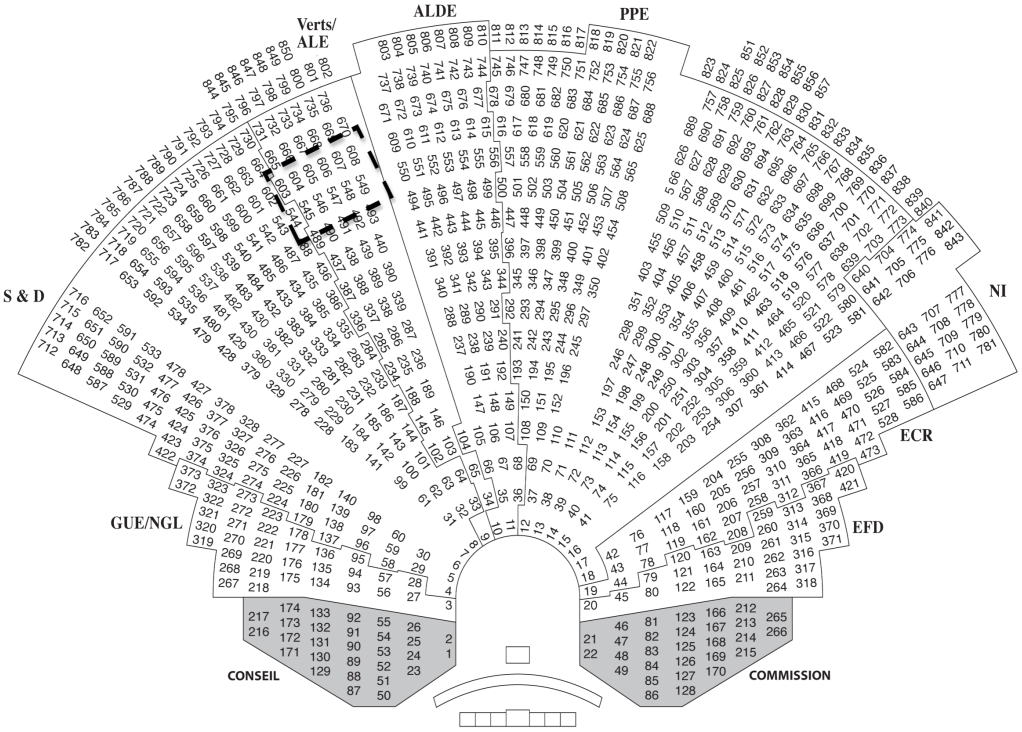
Observations for the presented regression results are proposals-by-MEP pairs from the main analysis sample. Different columns correspond to different subsamples that only include proposals with win margins above or below a certain cutoff. In Columns (1) and (2), the sample only proposals with a win margin above 10 percent. In Columns (3) and (4), the sample only includes proposals with a win margin below 10 percent. In Columns (5) and (6), the sample only includes proposals with a win margin below 5 percent. In Columns (7) and (8) the sample only includes proposals with a win margin below 1 percent. In all columns, the outcome variable is an indicator variable denoting whether the MEP pair cast different votes on the proposal. *Name adjacent* is an indicator for whether members of the MEP pair are immediately adjacent in the alphabetical ordering of surnames within their seating section (the non-leadership section of their EPG). *Seat neighbors* is an indicator for whether the MEP pair is seated adjacently. In addition to day-level and EP-by-EPG fixed effects, all columns include the following "Baseline name similarity controls": an indicator for whether the MEPs have the same last name and a flexible set of indicators to capture the distance between the MEPs' last names in the alphabetical ranking of all MEP last names in our data. Estimates in Columns (1), (3), (5), and (7) were obtained via OLS. Estimates in Columns (2), (4), (6), and (8) were obtained using 2SLS, using the indicator for whether members of the MEP pair are immediately adjacent in the alphabetical ordering of surnames within their seating section to instrument for whether the MEP pair is seated adjacently (the same sets of name similarity controls are employed in the first-stage estimation). Standard errors in parentheses are dyadic cluster-robust, clustered at the level of row-by-EPG-by-parliamentary-term. The number of such clusters is listed in the bottom section of the table. The persuasion rate implied by the estimated peer effect is based on DellaVigna and Gentzkow (2010). The listed F-statistics in the 2SLS columns correspond to the first-stage F-statistic measure of instrument strength proposed by Stock and Yogo (2005). *** p<0.01, ** p<0.05, * p<0.11

Figure 1: Sample seating charts

Panel A - Strasbourg, September 14, 2009:



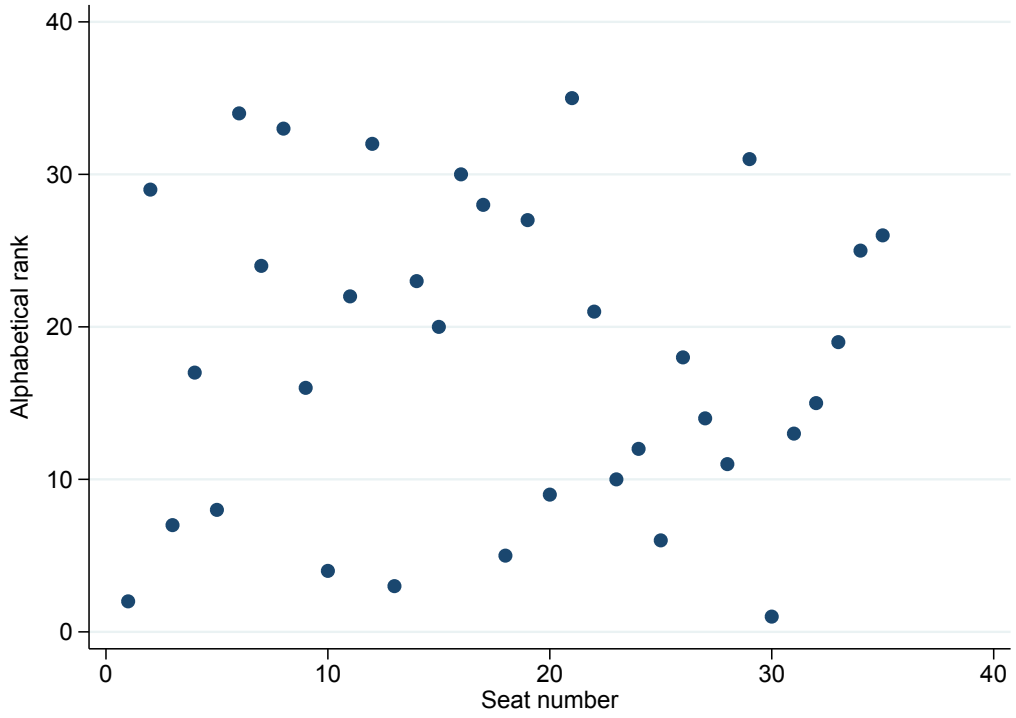
Panel B - Brussels, October 7, 2009:



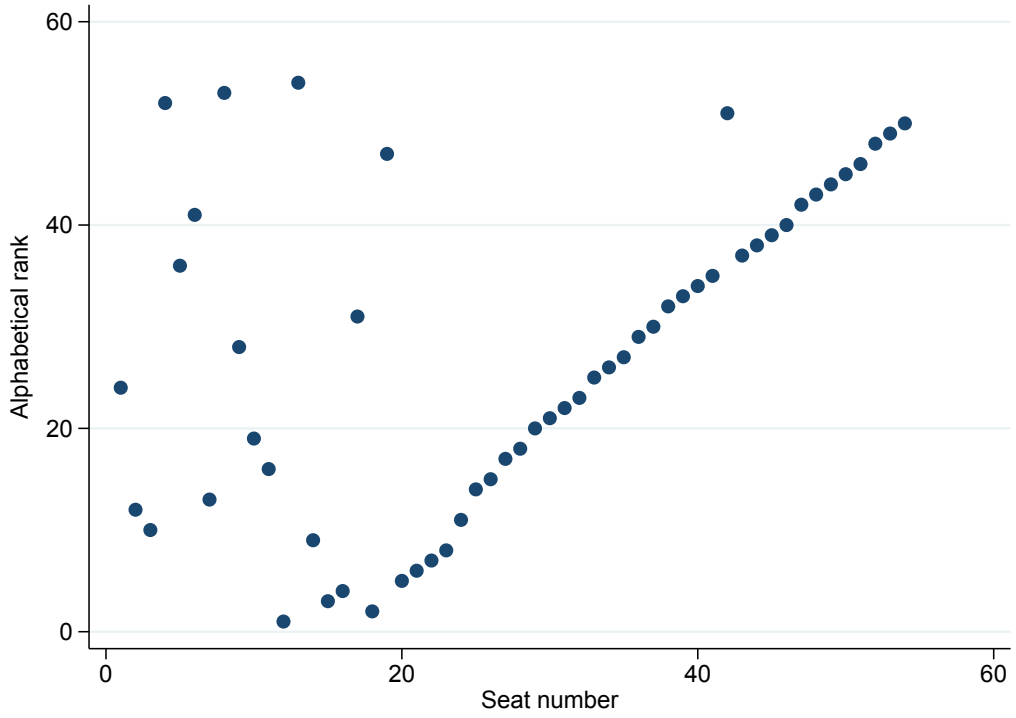
Panel A shows a sample seating chart for the European Parliament in Strasbourg and Panel B shows a chart for Brussels. Each number corresponds to an individual MEP, with their associated party listed around the outside of the chart. The seats in the dashed rectangle in each chart are magnified in Figure 3.

Figure 2: Seating and alphabetical rank

Panel A - European United Left–Nordic Green Left (a non-alphabetical party):



Panel B - Greens–European Free Alliance group (alphabetical seating for non-leaders):



In each panel, the horizontal axis is the seat ordering of MEPs within a political party. The vertical axis is the alphabetical rank of MEPs' surnames. The data are plotted for a sitting held on July 7, 2010.

Figure 3: Variation in seating across venues

Panel A - Strasbourg, September 14, 2009, Rows 9-12, Greens–European Free Alliance group (VERTS/ALE)

	S & D:	*VERTS/ALE:*						*ALDE:*
Row 12:		⋮	⋮	⋮	⋮	⋮	⋮	
Row 11:	...	Seat 561: **JADOT**	Seat 562: **JOLY**	Seat 563: **KELLER**	Seat 564: KIIL-NIELSEN	Seat 565: LAMBERT	Seat 566: LAMBERTS	...
Row 10:	...		Seat 478: GREZE	Seat 479: HAFNER	Seat 480: HASSI	Seat 481: HAUSLING	Seat 482: HAUTALA	...
Row 9:			⋮	⋮	⋮	⋮	⋮	

Panel B - Brussels, October 7, 2009, Rows 12-15, Greens–European Free Alliance group (VERTS/ALE)

	S & D:	*VERTS/ALE:*						*ALDE:*
Row 15:		⋮	⋮	⋮	⋮	⋮	⋮	
Row 14:	...	Seat 603: **KELLER**	Seat 604: KIIL-NIELSEN	Seat 605: LAMBERT	Seat 606: LAMBERTS	Seat 607: LOCHBIHLER	Seat 608: LOVIN	...
Row 13:	...		Seat 545: HASSI	Seat 546: HAUSLING	Seat 547: HAUTALA	Seat 548: **JADOT**	Seat 549: **JOLY**	...
Row 12:			⋮	⋮	⋮	⋮	⋮	

The panels show close-ups of seating arrangements in consecutive sittings of the European Parliament that took place in different venues. The highlighted and underlined names illustrate how the layouts of the two venues induce different seat adjacencies for alphabetically adjacent MEPs.