Peer Effects in Legislative Voting

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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, but peer effects are markedly stronger among women, among MEP pairs from the same country, and in close votes. Using variation in seating across the two venues of the Parliament (Brussels and Strasbourg), we also show that peer effects are persistent: MEPs who have sat together in the past are less likely to disagree even when they are not seated adjacently.

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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.

We also explore heterogeneity in peer effects, depending on MEP-pair characteristics and the importance of the proposal being voted on. Peer effects are much stronger when the two MEPs are from the same home country or are both women. For these groups, we estimate that seat adjacency reduces disagreement by about 1.5 and 1.6 percentage points, respectively, corresponding to percentage declines in disagreement of 39 and 21 percent. Using the national position of each MEP's home country as a proxy for their likely leaning on a given proposal, we also find that peer effects are stronger when MEPs are ex ante more closely aligned. These findings suggest that peer exposure serves as a complement to having other shared attributes — country of origin or gender — that facilitate social pressure. 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.

Finally, we analyze the mechanism behind the observed peer effects. We distinguish 'contemporaneous' peer effects — when an individual merely copies her immediate neighbor — 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

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

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.

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. Saia (2016), a paper concurrent with our own, documents peer effects among Icelandic politicians, also using exogenous variation in seating within the legislature. In contrast to our work, he primarily analyzes voting participation. (The outcome of interest in Saia (2016) is whether a politician does something different than her party leader on a given proposal. In the Icelandic Parliament, these differences mostly arise when either the politician or her party leader is absent.) 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.

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

[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.

[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.

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., Sacerdote (2011), Mas and Moretti (2009), Guryan et al. (2009), Duflo et al. (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 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

[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 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.

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.[7] 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.[8] Each number corresponds to a seat and is mapped to a list of MEP names on a separate sheet.[9]

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.[10]

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 assigned alphabetically by last name, though the seating rules do allow for a member to "occasionally occupy another place for organizational or technical reasons."[11] 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.[12] 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

[7] This introduces some measurement error, as a small number of MEPs move around due to changes in party leadership and MEP arrivals and departures.

[8] 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.

[9] A dashed rectangle has been superimposed on each chart and we "zoom in" on these areas in Figure 3. We discuss the importance of Figure 3 further below.

[10] 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.

[11] See http://www.europarl.europa.eu/sed/hemicycle.do (last accessed October 21, 2016).

[12] 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.

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.[13]

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.

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.

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.[14] 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

[13] 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.

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

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.

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

$$Disagree_{ijt} = \gamma_0 + \gamma_1 NameAdjacent_{ijt} + \varepsilon_{ijt}. \tag{2}$$

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, $\hat{\beta_1^{IV}}$.

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.[15] 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

[15] 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 C, our estimated peer effect dissipates very rapidly with seating distance.

name-adjacent in small parties.[16] We will address this concern by adding party-by-parliamentary session 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 session (EPG-by-EP) fixed effects and our baseline set of name similarity controls, 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.

Before we turn to the balance of the covariates, 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 predetermined attributes. In Column (2), we see that without name similarity controls, alphabetical

[16] 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).

adjacency is predictive of whether the pair comes from the same country, but this relationship disappears when we condition 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.[17] 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 noticeably to -0.0048.

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 further controls for 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.[18] 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 -0.047.

Finally, in Columns (6) and (7) we present our LATE estimates, using the result from Table

[17] The overall tendency for MEPs to disagree changes noticeably over time so we use time fixed effects throughout all our specifications.

[18] 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.

3, Column (1) to instrument for $SeatNeighbors_{ijt}$ with $NameAdjacent_{ijt}$.[19] 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. 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.[20]

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 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 a little more than 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.[21]

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. 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.[22] In Table 5, Panel

[19] Of course, in both cases we include the same set of controls in both the first and second stage estimation.

[20] 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.

[21] In Appendix C, 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.

[22] 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 D, we therefore redo the placebo test for the leaders while measuring name adjacency across the full EPG. This leads to similar results.

A, we examine how alphabetical adjacency affects seating and voting among leaders.[23] 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.[24] The point estimates on $NameAdjacent_{ijt}$ are very close to zero (and in fact positive). In 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 find small — though imprecise — estimates of alphabetical adjacency on voting disagreement. 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.[25]

3.3 Differences in peer effects by MEP characteristics

Having documented the existence and overall magnitude of peer effects in the previous section, we now turn to examine whether these peer effects are stronger for particular types of politician pairs. We focus here on two characteristics that one would expect might play an important role in strengthening peer effects. Broadly speaking, we are motivated by the literature on the distinct attitudes toward in-group versus out-group members. Research dating back at least to Tajfel (1970) emphasizes the deference that individuals show toward the ideas and interests of those from one's own group, even in cases in which the group's formation is entirely artificial.

A natural starting point for exploring how group affiliation affects the strength of peer effects is shared country of origin — a potential source of shared culture, language, and social ties. Table 6 estimates peer effects separately for the MEP pairs that are from the same country and those that are from different countries. Columns (1) and (2) show ITT and LATE estimates respectively for pairs of MEPs from different countries. Throughout, we use the controls from our preferred specification (Column (2) of Table 4). In both columns, the estimated peer effect is slightly smaller than in the full sample. The LATE estimate in Column (2) implies that among MEPs not from the same country, seating adjacency lowers disagreement by 0.46 percentage points. Relative to a

[23] One might worry that this is an imperfect placebo test since the voting behavior of leaders might systematically differ from that of non-leaders. This concern is somewhat mitigated by the fact that leaders tend to disagree in their votes roughly as much as non-leaders do (8.15 percent vs 8.55 percent).

[24] Note that $SameName_{ijt}$ drops out of our placebo analyses because no MEP pairs have identical surnames in the placebo samples.

[25] 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 D by conducting our placebo tests using a different measure of name adjacency and find similar results.

baseline disagreement rate of 0.09, this corresponds to a persuasion rate of 5.1 percent. The smaller estimated effects and slightly larger standard errors imply that we can not rule out a zero peer effect for these pairs. In Columns (3) and (4), we instead examine MEP pairs who are from the same country. We estimate a markedly larger seating peer effect for these pairs. The LATE estimate in Column (4) implies that sitting next to each other reduces disagreement by 1.5 percentage points. Relative to a baseline disagreement rate of 3.8 percent, this implies a large persuasion rate of 39 percent. Peer effects are thus much stronger among politician pairs from the same country. This difference is statistically significant; $p = 0.03$ for both ITT (Column (1) vs. Column (3)) and LATE (Column (2) vs. Column (4)) estimates.

We turn next to examining the strength of peer effects by gender. That networks and influence may be in part gender specific finds support in work, for example, on job referrals (Bayer et al. (2008)) and information flows among stock analysts (Fang and Huang (2016)). For politicians, the notion that peer influence may be particularly strong among women is supported at least on an anecdotal level. Most prominently — and entirely in the spirit of our empirical exercise — 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 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]

Table 6 estimates peer effects separately for female-only, mixed gender, and male-only MEP pairs. In Columns (1) and (2), which show ITT and LATE estimates for women-only MEP pairs, the measured peer effects are markedly larger than in the overall sample. The LATE estimate in Column (2) implies that seating adjacency reduces the likelihood of disagreement between two women by 1.6 percentage points. Relative to a baseline disagreement rate of 7.6 percent this corresponds to a persuasion rate of 21 percent. Turning to the samples of mixed gender and male-only pairs in Columns (3)-(4) and (5)-(6) respectively, we observe much smaller estimated peer effects. The LATE estimates are -0.0031 and -0.0043 respectively, and none of the estimates are significantly different from zero. We can reject that the estimated effects for women-only pairs are the same as the estimated effects for male-only and mixed-gender pairs at the 10 percent level ($p = 0.08$ for both the ITT and LATE estimates).

Appendix B presents some additional results on heterogeneous effects. Table A.2 examines peer effects simultaneously by gender mix and whether MEPs are from the same country. For completeness, Table A.1 examines heterogeneous peer effects across the various other MEP characteristics

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

available to us in our data, but shows no significant differences beyond the gender and same-country results discussed above.

3.4 Differences in peer effects by vote characteristics

Next we examine whether peer effects differ by the characteristics of the vote taking place. The intensity of peer influence may depend on the importance of the proposal or the external pressures MEPs face to vote a certain way. If peer effects in voting simply reflect a moderate desire for social conformity, we would expect them to be most prevalent in low-stakes situations in which there is little cost to succumbing to pressure to change one's vote. Conversely, if peer effects reflect more profound changes in peers' allegiances or beliefs, we would expect them to also be present in higher-stakes situations in which an MEP incurs a real cost for changing her vote.

To examine whether peer effects are more or less pronounced in high-stakes situations, we look at proposals that were either passed or defeated by a small margin. Table 8 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.

Finally, we ask whether peer effects differ depending on whether politicians are ex ante more or less aligned on a given proposal. On the one hand, peers who are far apart on an issue might exert a stronger influence on each other. Alternatively, if the ex ante disagreement is too large, it might prevent peers from influencing one another at all. To construct a measure of the ex ante position of MEPs on a given proposal, we proxy individual MEPs' positions by the overall position of their home country since, as noted in Section 3.2, national interests and pressure from national parties are an important determinant of MEPs' voting behavior.[27] To construct a proxy for national positions

[27] Party affiliation is much more predictive of vote concordance than home country or any other MEP characteristic, as evidenced by the high rate of agreement among our (within-party) MEP pairs. Because our analysis focuses on within-party concordance, it is natural to focus on MEPs' home country interests as a source of variation in a vote's

on each proposal, we rely on the voting behavior of MEPs excluded from the main analysis sample (i.e., leaders and MEPs from non-alphabetical parties).[28] For each country and each proposal, we define a country to be *in favor* of a given proposal if at least 90 percent of its MEPs voted in favor of the proposal among MEPs not in the main analysis sample. Similarly, we define a country to be *against* a given proposal if less than 40 percent of its MEPs voted in favor. Otherwise, we say that a country is *neutral* on a given proposal. These cutoffs were chosen in order to split the sample roughly evenly. Under these definitions, 32 percent of country-proposals are coded as in favor, 29 percent are against, and 39 percent are neutral.[29] To reduce mismeasurement, we drop all country-proposal pairs in which the country has less than 10 participating MEPs among the leaders and non-alphabetically seated parties.

Focusing on the subsample of MEP pairs that are from different countries, Table 9 repeats our main analysis depending on the country positions of the MEPs' home countries. Since we employ a subsample — we drop same-country MEP pairs and country-proposal pairs with too few MEPs participating — we first show in Columns (1) and (2) that the estimated peer effects from Table 4 do not change substantially in this subsample.

The next six columns show ITT and LATE estimates of peer effects for proposals where the MEPs' home countries are aligned (Columns (3) and (4)), where one country is neutral but the other is not (Columns (5) and (6)) and where the two countries are opposed (Columns (7) and (8)). Looking across the columns the estimated peer effects are very similar, although they are only statistically significant when the countries are aligned. When it comes to the absolute peer effect, therefore, there is little evidence that the strength of the seating peer effects varies with home country positions.

If we focus on relative effects or persuasion rates, however, we see a different picture. Unsurprisingly, for proposals in which the MEPs' home countries are aligned, MEPs are much more likely to agree regardless of seating. The baseline disagreement rate for these proposals is only 5.3 percent, whereas it is much higher for other proposals. As a result, our LATE estimates imply that seating peer effects have a persuasion rate (or relative effect) of 12 percent for proposals in which home countries are aligned, whereas the persuasion rate is only 3.2 percent when one country is neutral and 0.1 percent when countries are not aligned.[30] To the extent that we care more about persuasion

importance to an MEP.

[28]We obtain similar results if we use the full set of MEPs to generate the proxy for national interest.

[29]Reassuringly, the resultant measure of country interest is a strong predictor of voting behavior in our main analysis sample. Unconditionally, MEPs are 30.2 percentage points more likely to vote yes on a proposal if their home country is *in favor* relative to if they are *neutral* and are 54.3 percentage points less likely to vote yes if their home country is *against* rather than neutral. After controlling for party-by-proposal fixed effects, MEPs are 9.5 percentage points more likely to vote yes if their home country is *in favor* and 19.1 percentage points less likely to vote yes if their home country is *against*.

[30]Converted to relative effects, the confidence intervals from Columns (6) and (8) allow us to rule out persuasion

rates, the results thus indicate that peer effects are stronger when politicians are ex ante not in too strong disagreement with each other.[31]

3.5 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.

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

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

rates higher than 9.5 percent when one country is neutral and 4.9 percent when countries are opposed (note that the large number of votes in the samples implies that the uncertainty on the baseline disagreement rate is negligible).

[31] In Appendix E, we examine whether peer effects operate across parties, a divide that also reflects sharper differences in policy preferences. We do so by examining the votes of MEPs seated at row ends. We find no evidence that peer effects operate across party lines, however, confidence intervals do allow for non-trivial peer effects across parties.

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., $\eta_2 = \eta_3 = 0$.[32]

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 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 where MEPs join or leave the non-leadership groups.[33] 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

[32]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 C, contemporaneous peer effects do not extend beyond the immediate neighbor, so this is unlikely to be an important confound.

[33]Such changes do occur in the data both because of MEPs dropping out of the EP and because of changes in the size and composition of the non-leadership group over time. They are quite limited, however. The correlation in $SeatNeighborsPredicted_{ijt}$ across two subsequent meetings in the same venue is 0.995.

Equation (3).[34]

Table 10 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.

In summary, 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 us to make precise statements about the relative importance of current versus past seating.[35]

[34] In Appendix F, 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.

[35] In unreported results, we have examined other specifications aimed at identifying the dynamics of peer effects, including separating votes by the time since the start of the parliamentary session or assuming that the full path of past seating is exogenous. In no instance do we have power to produce useful bounds on how peer effects evolve over time.

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. There are also indications that peer effects are also stronger when there is relatively mild ex ante disagreement among MEPs. Overall, these results suggest that peer exposure serves as a complement to similarity along other dimensions. Finally, we find that peer effects are stronger in close votes.

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.

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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	225	320
Number of cast votes	1,820,233	456,247	680,915
MEP characteristics:			
Mean age	51.07	52.97	53.67
Mean tenure in EP	3.56	3.21	6.51
Share top ranked education	0.25	0.22	0.31
Share women	0.39	0.22	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
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 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 tests

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	0.0095	0.0104	-0.0049	-0.0047	-0.0044	-0.0027
	(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: Peer effects by same country of origin

SAMPLE:	From different countries		From same country	
	(1) Disagree OLS	(2) Disagree 2SLS	(3) Disagree OLS	(4) Disagree 2SLS
Name adjacent	-0.0037 (0.0024)		-0.0126*** (0.0040)	
Seat neighbors		-0.0046 (0.0031)		-0.0150*** (0.0049)
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	10,242,746	10,242,746	5,233,431	5,233,431
Clusters	76	76	76	76
Disagree mean	0.0896	0.0896	0.0378	0.0378
F-stat		860.4		1053

Observations for the presented regression results are proposals-by-MEP pairs from the main analysis sample. Different columns correspond to different subsamples of MEP pairs. In Columns (1) and (2), the sample only includes MEP pairs from different countries. In Columns (3) and (4), the sample only includes MEP pairs from the same country. 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) and (3) were obtained via OLS. Estimates in Columns (2) and (4) 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 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 7: Peer effects by gender mix

SAMPLE:	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	-0.0130*** (0.0037)		-0.0025 (0.0025)		-0.0034 (0.0047)	
Seat neighbors		-0.0162*** (0.0043)		-0.0031 (0.0031)		-0.0043 (0.0059)
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	15,139,566	15,139,566	48,575,162	48,575,162	43,610,282	43,610,282
Clusters	68	68	76	76	71	71
Disagree mean	0.0756	0.0756	0.0830	0.0830	0.0918	0.0918
F-stat		249.5		722.5		755

Observations for the presented regression results are proposals-by-MEP pairs from the main analysis sample. Different columns correspond to different subsamples of MEP pairs. 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 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 8: 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
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 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 by home country positions

SAMPLE:	Full subsample		Countries aligned		One country neutral		Countries opposed	
	(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	-0.0041		-0.0050***		-0.0037		-0.0063	
	(0.0026)		(0.0018)		(0.0038)		(0.0101)	
Seat neighbors		-0.0053		-0.0064***		-0.0048		-0.0080
		(0.0033)		(0.0023)		(0.0049)		(0.0131)
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	71,870,700	71,870,700	47,621,572	47,621,572	23,422,403	23,422,403	826,725	826,725
Clusters	74	74	74	74	74	74	70	70
Disagree mean	0.0919	0.0919	0.0526	0.0526	0.151	0.151	0.686	0.686
F-stat		648.8		601.9		709.8		443.2

Observations for the presented regression results are proposals-by-MEP pairs from the main analysis sample, excluding MEP pairs that are from the same country as well as country-by-proposal pairs where the country has too few participating MEPs to reliably determine the national position (fewer than 10 MEPs participating among the group of leaders and alphabetically seated parties). Columns (1) and (2) includes all such observations, while the remaining columns correspond to different subsamples based on the configuration of home country positions. Columns (3) and (4) only include observations where the home countries' positions are the same. Columns (5) and (6) only include observations where one home country is neutral but the other is not. Columns (7) and (8) only include observations where one home country is against and the other is in favor. 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 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<0.1

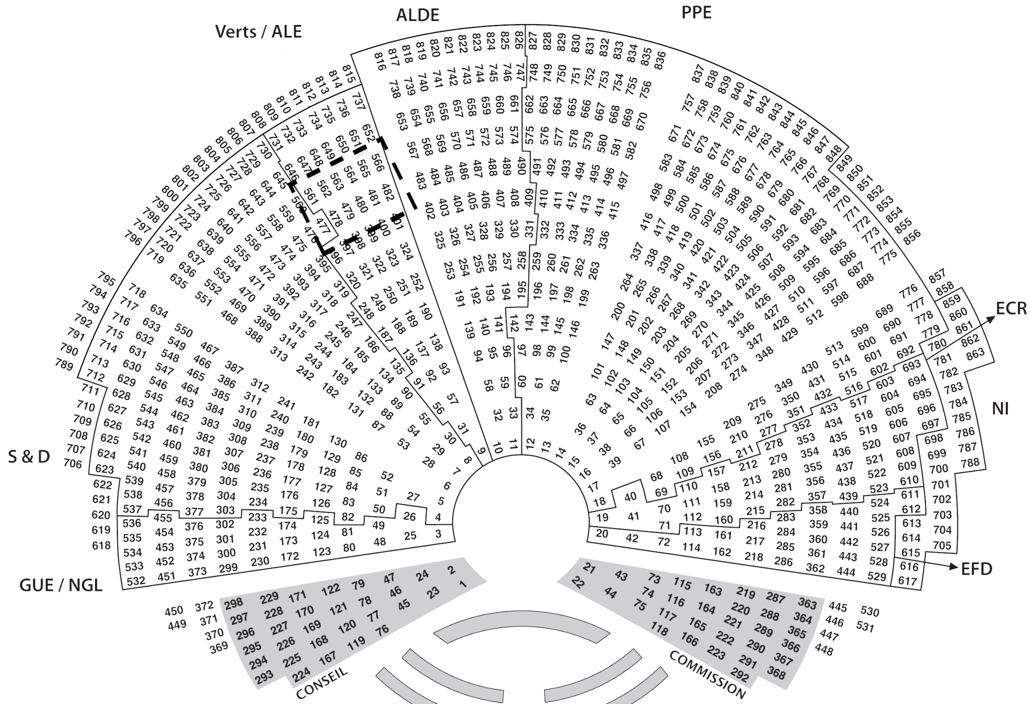
Table 10: Measuring contemporaneous versus persistent peer effects using venue variation

	(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 matters			0.028**	0.099*
p-value: coefficients are equal	0.329	0.391	0.609	0.711

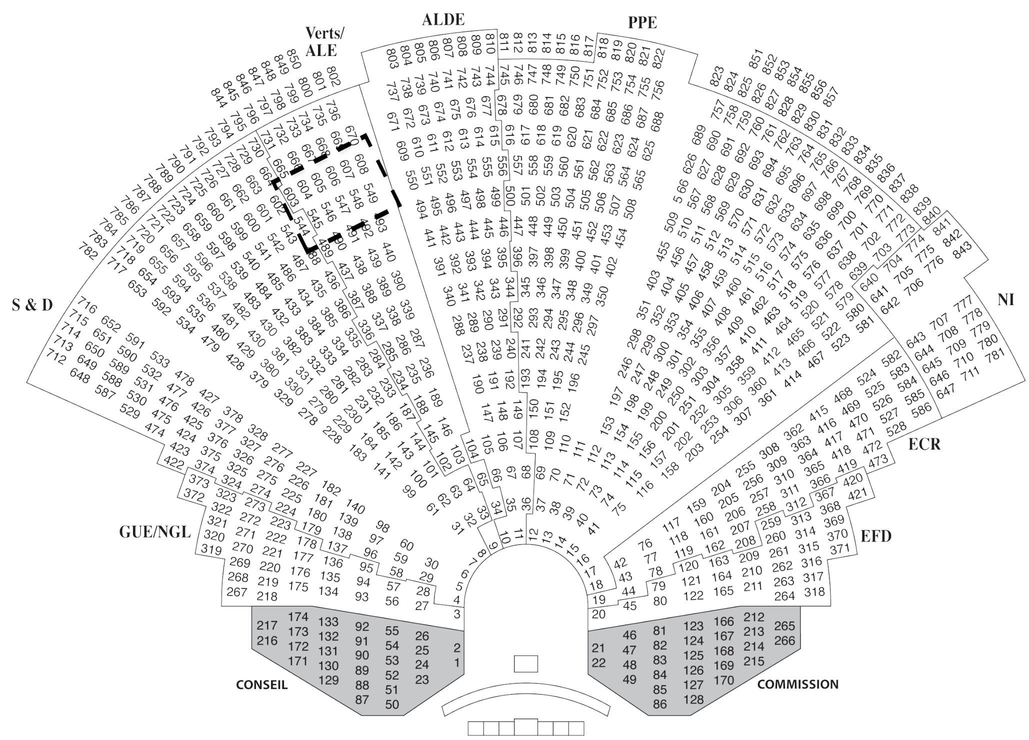
Observations for the presented regression results are proposals-by-MEP pairs in the main analysis sample, excluding the dates prior to the first observed venue change, in which there is no information about previous venue. In all columns, the outcome variable is an indicator variable denoting whether 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. *** p<0.01, ** p<0.05, * p<0.1

Figure 1: Sample seating charts

Panel A - Strasbourg, September 14, 2009:



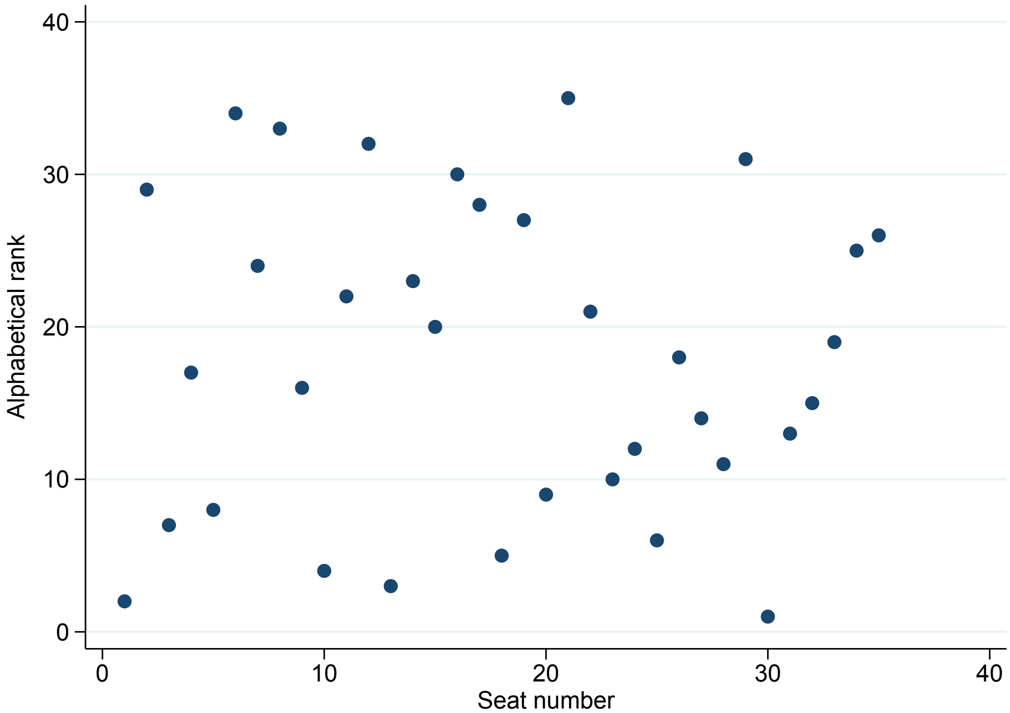
Panel B - Brussels, October 7, 2009:



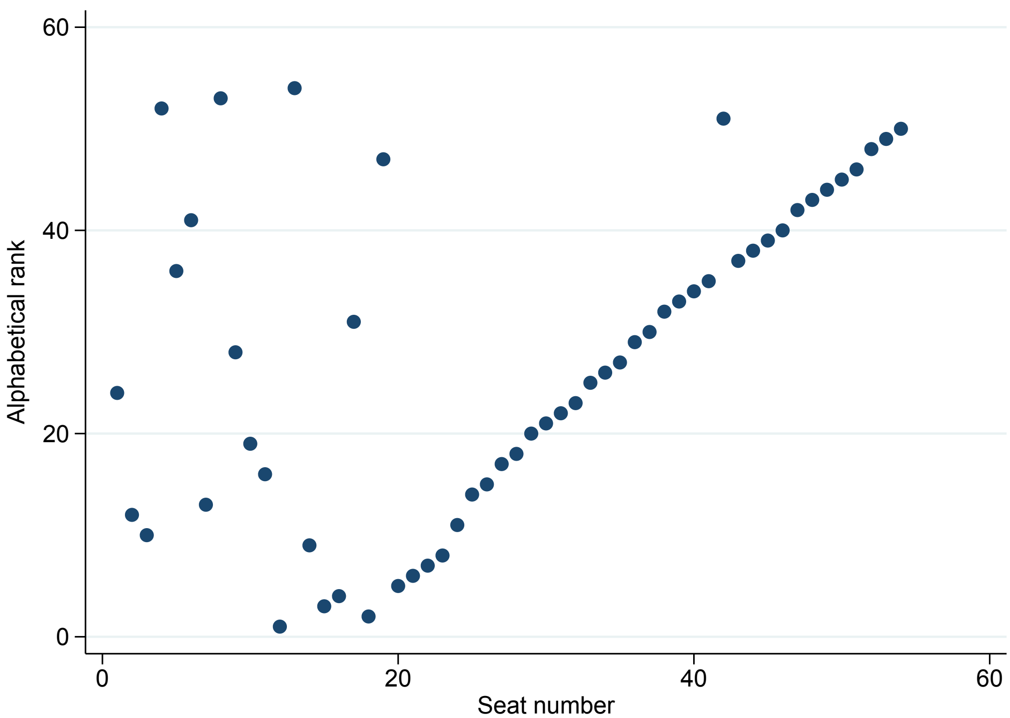
Panel A shows a sample seating chart for the European Parliament in Strasbourg and Panel B shows a chart for Brussels. Each number corresponds to an individual MEP, with their associated party listed around the outside of the chart. The seats in the dashed rectangle in each chart are magnified in Figure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

APPENDIX

A Standard error calculations

There are two main issues we confront in the correlation structure of our data. First, the existence of seating-based peer effects (and/or effects of having similar last names) imply that behavior may be correlated across MEPs sitting next to each other. If we define clusters based on groupings of closely seated MEPs (such as rows), this becomes a standard clustering problem, in which voting behavior is correlated across MEPs within each cluster. Second, our data is dyadic, so that an observation in our data reflects the behavior of *a pair* of individuals during voting on a given proposal rather than a single individual. On its own this implies a mechanical correlation across all pairs in our data which have a member in common. Combined with the clustering issue, this further implies that there may be a correlation across two pairs in our data as soon as one member from each of the pairs are seated close together and belong to the same cluster.

To deal with these issues we use dyadic cluster-robust standard errors throughout (Cameron and Miller (2014) and Aronow et al. (2015)). In standard regression notation, let x_{ijt} be the vector of (exogenous) regressors (and instruments), let $\hat{e}_{ijt}$ be the regression residuals and let $\rho(i,t)$ denote the cluster to which MEP i belongs during a vote on proposal t. Let $\mathbf{1}[\cdot]$ denote the standard indicator function. We then estimate the covariance matrix of our OLS or 2SLS regressions by replacing the inner part of the standard (Huber-Eicker-White) sandwich estimator by:

$$\sum_{i,j,t}\sum_{i',j',t'} I(i,j,t;i',j',t')e_{ijt}e_{i'j't'}x_{ijt}x'_{i'j't'}.$$

Here $I(i,j,t;i',j',t')$ is an indicator for whether any of the MEPs in the observation ijt and the observation $i'j't'$ belong to the same cluster, formally:

$$I(i,j,t;i',j',t') \equiv \mathbf{1}\Big[\rho(i,t)=\rho(i',t') \vee \rho(j,t)=\rho(j',t') \vee \rho(i,t)=\rho(j',t') \vee \rho(i',t')=\rho(j,t)\Big].$$

Our implementation of the estimator closely follows the recommendations in Cameron and Miller (2014). In particular, we apply a degree-of-freedom correction $\frac{G}{G-1}\frac{N-1}{N-k}$, where G is the number of clusters, N is the total number of observations, and k is the number of estimated regression coefficients, and use eigenvector decomposition to deal with non-positive semi-definite variance matrices in finite samples.[36]

[36] One practical issue with the dyadic-cluster robust variance estimator is that it is not guaranteed to be positive semi-definite in finite samples. In our analysis, there is only one specification in which the relevant part of the

In implementing our analysis, we define a cluster to be a row-by-EP-by-EPG. One example of a cluster in our data is thus "the 9th row of the Greens-European Free Alliance Group during the 6th parliamentary term". By choosing this level of clustering, we allow for arbitrary correlation within each row of each EPG in each of the two parliamentary terms we analyze.[37]

B Peer effects by MEP characteristics, additional results

This section presents some additional results regarding heterogeneous peer effects. First, we examine whether peer effects are stronger among seating neighbors who are similar in terms of observable characteristics beyond those emphasized in the main text. For brevity, we focus only on reduced form ITT estimates here. For a given pair characteristic C_{ij} we obtain heterogeneous ITT estimates from the interaction term in the following equation:

$$Disagree_{ijt} = \kappa_0 + \kappa_1 NameAdjacent_{ijt} + \kappa_2 NameAdjacent \times C_{ij} + \kappa_3 C_{ij} + \nu_{ijt}. \quad (5)$$

The characteristics we focus on are: whether members of the MEP pair have an education of similar quality (as measured by an indicator for having a degree from a "top 500" university), their age difference in years, their difference in EP tenure in years, and whether one or both members of the pair are freshmen. Table A.1 shows the corresponding results. Throughout the table, the coefficients on the interaction terms are small and never statistically significant. We see no evidence that peer effects vary by any of these characteristics.

Next, we examine the interaction between gender and country of origin. Table A.2 estimates peer effects separately by the gender mix (Columns (1)-(2) vs. (3)-(4) vs. (5)-(6)) of the MEPs while focusing either on MEPs from different countries (Panel A) or on MEPs from the same country (Panel B). The estimated peer effects in Panel A closely mirror the results in the main text: peer effects are markedly stronger (and only statistically significant) among all-women pairs. Turning to the results for MEPs from the same country in Panel B, however, the estimated coefficients look very similar regardless of gender mix. Overall, peer effects appear to be stronger for same-country MEPs regardless of the gender mix, while gender mix is important among MEP pairs from different

estimated covariance matrix is not positive semi-definite (Column (6) in Panel B of Table 3). Following Cameron and Miller (2014), we simply correct this by setting negative eigenvalues to zero in the eigenvalue decomposition of the estimated matrix.

[37] A practical complication arises due to differences in layouts across the two venues where the EP meets. Because the Brussels layout spreads the MEPs out over more rows than the Strasbourg layout, many MEPs do not sit on the same row number in both of the venues. Rather than having these MEPs switch clusters every time the EP changes venue, we always assign all MEPs to their Strasbourg row number when calculating standard errors. More than 80 percent of the voting in our data takes place in Strasbourg. Moreover, since the rows in Strasbourg are wider, this approach is conservative in the sense that it implies a coarser level of clustering.

countries.

Table A.1: Heterogeneous peer effects in other dimensions

	(1) Disagree	(2) Disagree	(3) Disagree	(4) Disagree
Name adjacent	-0.0054*	-0.0051**	-0.0045*	-0.0043
	(0.0030)	(0.0024)	(0.0026)	(0.0059)
Same quality education	-0.0037***			
	(0.0013)			
Name adj. X Same quality education	0.0009			
	(0.0027)			
Age difference		0.0016		
		(0.0012)		
Name adj. X Age difference		0.0004		
		(0.0014)		
Tenure difference			0.0009	
			(0.0023)	
Name adj. X Tenure difference			-0.0006	
			(0.0019)	
One freshman				0.0002
				(0.0016)
Two freshmen				-0.0019
				(0.0035)
Name adj. X One freshman				-0.0014
				(0.0066)
Name adj. X Two freshmen				0.0006
				(0.0049)
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	107,325,010	107,325,010	107,325,010	107,325,010
Clusters	76	76	76	76
p-value, significance of interaction terms				0.972

Observations for the presented regression results are proposals-by-MEP pairs in the main analysis sample. 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). The following variables and their interactions with *Name adjacent* are added in various columns: an indicator for whether the two MEPs have the same education quality as measured by whether both or neither MEPs have a degree from a top 500 university, the age difference between the two MEPs, the difference in EP tenure between the two MEPs, and indicators for whether one or both of the MEPs in the pair is a freshman. 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. 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 shown in the table. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A.2: 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*** (0.0038)		-0.0017 (0.0024)		-0.0028 (0.0048)	
Seat neighbors		-0.0142*** (0.0045)		-0.0022 (0.0031)		-0.0035 (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
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*** (0.0040)		-0.0107*** (0.0035)		-0.0099 (0.0078)	
Seat neighbors		-0.0131*** (0.0046)		-0.0120*** (0.0038)		-0.0129 (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
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 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

C Peer effects at longer distances

Throughout the main text, we estimate seating peer effects only between immediate seat neighbors. However, peer effects could, in principle, also operate between MEPs sitting further apart. We can define indicator variables to denote whether two MEPs are sitting 2, 3 or 4 seats apart ($Seated2Apart_{ijt}$, $Seated3Apart_{ijt}$, $Seated4Apart_{ijt}$). To test for the existence of more distant peer effects, we consider regressions of the following form:

$$
\begin{aligned}
Disagree_{ijt} \quad = \quad & \pi_0 + \pi_1 SeatNeighbors_{ijt} + \pi_2 Seated2Apart_{ijt} + \pi_3 Seated3Apart_{ijt} \\
& + \pi_4 Seated4Apart_{ijt} + \varsigma_{ijt}. \qquad (6)
\end{aligned}
$$

As in our main analysis, we use an IV strategy to deal with the sorting of MEPs and use as instruments a set of indicators for whether the MEPs are 2, 3, or 4 apart in the alphabetical ranking of last names within their section ($Names2Apart_{ijt}$, $Names3Apart_{ijt}$, $Names4Apart_{ijt}$).

Table A.3 shows the estimated peer effects when we include indicators for sitting 1, 2, 3, or 4 seats apart. Columns (1)–(4) present reduced form OLS estimates in which we simply replace the endogenous variables by the relevant instruments, while Columns (5)–(8) present 2SLS estimates. In both sets of columns the estimated effect of being immediate neighbors is virtually unchanged from the specification in the main text, while the estimated effects of being 2, 3, or 4 apart are very close to zero and in fact positive. Because standard errors increase when we include the additional variables, however, none of the estimated effects are statistically significant on their own when we include more than one seating variable. As shown at the bottom of the table, however, the seating variables are jointly significant in all specifications, and we can always reject that being immediate neighbors has the same effect as being more distant neighbors.[38] Overall, we conclude that peer effects operate only among immediate neighbors.

[38]Intuitively, the reason that the coefficient on being immediate neighbors is not significant on its own is that we cannot rule out the (somewhat pathological) case where there are no peer effects among immediate neighbors but are large peer effects among more distant neighbors.

Table A.3: Peer effects at longer distances

	(1) Disagree OLS	(2) Disagree OLS	(3) Disagree OLS	(4) Disagree OLS	(5) Disagree 2SLS	(6) Disagree 2SLS	(7) Disagree 2SLS	(8) Disagree 2SLS
Name adjacent	-0.0048**	-0.0046	-0.0045	-0.0043				
	(0.0024)	(0.0031)	(0.0036)	(0.0040)				
Names 2 apart		0.0005	0.0005	0.0007				
		(0.0018)	(0.0023)	(0.0027)				
Names 3 apart			0.0002	0.0004				
			(0.0016)	(0.0019)				
Names 4 apart				0.0005				
				(0.0015)				
Seat neighbors					-0.0060**	-0.0057	-0.0056	-0.0054
					(0.0030)	(0.0038)	(0.0044)	(0.0050)
Seated 2 apart						0.0009	0.0009	0.0012
						(0.0025)	(0.0030)	(0.0035)
Seated 3 apart							0.0003	0.0004
							(0.0023)	(0.0027)
Seated 4 apart								0.0009
								(0.0026)
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	107,325,010	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	76
p-value, coefficients zero		0.001***	0.003***	0.002***		0.002***	0.004***	0.003***
p-value, coefficients are equal		0.003***	0.004***	0.012**		0.002***	0.007***	0.014**
F-stat: Seat neighbors					958.3	1001	540	393.4
F-stat: Seated 2 apart						661	372.2	629.5
F-stat: Seated 3 apart							228.9	179.6
F-stat: Seated 4 apart								135.5

Observations for the presented regression results are proposals-by-MEP pairs in the main analysis sample. In all columns, the outcome variable is an indicator variable denoting whether the MEP pair cast different votes on the proposal. The listed regressors are indicators for whether the MEP pair is 1, 2, 3, and 4 seats apart in the alphabetical ordering of MEPs within their seating section (the non-leadership section of their EPG), as well as indicator for whether the MEP pair is seated 1, 2, 3, and 4 seats apart. 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)–(4) were obtained via OLS. Estimates in Column (5)–(8) were obtained using 2SLS, using the namel adjacency variables as instruments for the 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 shown in 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, except in Column (5), where it correspond to the first-stage F-statistic measure of instrument strength proposed by Stock and Yogo (2005) for the case of a single endogenous variable. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

D Alternative placebo test

In conducting our placebo test on the sample of EPG leaders in Panel A of Table 5 we defined name adjacency in terms of whether a pair of leadership MEPs were adjacent in the alphabetical ranking of names within their group of leaders. 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 group of MEPs who are sitting together (leaders or non-leaders). Because there are markedly fewer leaders than non-leaders, however, one might be concerned that name adjacency in the group of leaders is a weaker correlate of name similarity. To see why this is so, consider the extreme case in which there are only two MEPs in the leadership group of some EPG. These two MEPs will be name adjacent regardless how similar their surnames actually are. Conversely, if the size of a group of MEPs tends to infinity, a pair of MEPs will be name adjacent within that group only if their surnames are virtually identical.

To check whether such differences in group size affect the conclusions of our placebo test, Table A.4 repeats the placebo test from Panel A of Table A.4 using a different measure of name adjacency. Instead of defining a pair of leaders as being name adjacent if they are next to each other in the alphabetical ordering of leaders, we define them as being name adjacent only if they are next to each other in the alphabetical ordering of *all* MEPs within their EPG. Results are very similar to those presented in the main text. The alternative measure of name adjacency does not predict seating and also does not correlate with voting similarity. In all specifications, we can reject that name similarity reduces disagreement by more than 0.6 percentage points.

Table A.4: Leadership placebo test, alternative name adjacency definition

	(1) Seat neighbors	(2) Seat neighbors	(3) Disagree	(4) Disagree	(5) Disagree	(6) Disagree	(7) Disagree
Name adjacent, full party	0.0393	0.0013	0.0005	0.0044	0.0042	0.0038	0.0036
	(0.0279)	(0.0322)	(0.0030)	(0.0036)	(0.0033)	(0.0042)	(0.0047)
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	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

Observations in the presented regression results are proposals-by-MEP pairs. The regressions include all observations in which both MEPs are leaders of the same 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. *Name adjacent, full party* denotes whether the MEP pair is immediately adjacent in the alphabetical ordering of surnames within their EPG. "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. "Observable pair characteristics" include controls for whether the two MEPs are from the same country, have the same education quality, have the same freshman status, their age difference, and their difference in EP tenure. "Additional name similarity controls" include the cubic polynomials in Bigram-Jaccard and Levensthein name similarity, as well as the 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 parenthesis are dyadic cluster-robust, clustered at the level of row-by-EPG-by-parliamentary term. The number of such clusters is shown in the table. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

E Peer effects across parties

In the main text, we estimate peer effects among MEPs from the same party. Peer effects could also operate across party lines, however. Although our empirical strategy leverages random assignment of seats within EPGs, we can adapt our data and specification to examine whether we see evidence of seating peer effects across EPGs as well. In particular, there are MEPs in our data who are assigned to seats at the edge of their EPGs' sections and hence will be seated next to MEPs from other parties.

To examine cross-party peer effects, we take the sample of non-leadership MEPs from alphabetically seated EPGs and form all possible pairs of MEPs and proposals in which both MEPs are present and participate in the vote, and where the two MEPs are from *different* EPGs. Letting ij index MEP pairs and t index proposal, we define the variables $Disagree_{ijt}$ and $SeatNeighbors_{ijt}$ as in the main analysis and consider the following regression:

$$Disagree_{ijt} = \psi_0 + \psi_1 SeatNeighbors_{ijt} + \iota_{ijt}. \tag{7}$$

The coefficient ψ_1 measures whether MEPs from different parties who sit next to each other are less likely to disagree, relative to MEP pairs from different parties who do not sit adjacently. To deal with systematic sorting into seats, we instrument $SeatNeighbors_{ijt}$ by predicted adjacency based only on the alphabetical name rankings and seat layouts for each EPG and each day. In addition, to address possible differences in typical agreement across EPGs we include party-pair-by-EP-session fixed effects, as well as our baseline set of name controls. Table A.5 shows the results. Column (1) shows the first stage regression for the predicted seating instrument, Columns (2) shows the reduced form (ITT) estimate, and Column (3) shows LATE estimates using 2SLS. The estimates show little indication that peer effects operate across parties, as both the ITT and LATE estimates are in fact positive. Because standard errors are also quite large, however, our results do not rule out that cross-party peer effects may be non-trivial. For the LATE estimate in Column (3), the 95 percent confidence interval does not allow us to reject the possibility that seat adjacency reduces disagreement by 1.1 percentage points, corresponding to a persuasion rate of 3.6 percent. This lack of precision reflects the relative rarity of MEPs that sit on the edge of their parties' sections (0.02 percent of the sample) and the relatively low predictability of alphabetical seating on cross-party adjacency (since a single out-of-order MEP affects the ordering of all row-end MEPs).

Table A.5: Peer effects across parties

	(1) Seat neighbors OLS	(2) Disagree OLS	(3) Disagree 2SLS
Seat neighbors, predicted	0.2796*** (0.0455)	0.0023 (0.0028)	
Seat neighbors			0.0082 (0.0098)
Day-level fixed effects	Yes	Yes	Yes
EP-by-EPG-pair fixed effects	Yes	Yes	Yes
Baseline name controls	Yes	Yes	Yes
Observations	212,057,965	212,057,965	212,057,965
Clusters	76	76	76
Disagree mean		0.307	0.307
F-stat			104.7

Observations for the presented regression results are proposals-by-MEP pairs. The sample only includes MEP pairs who are from two different alphabetically seated parties and where both MEPs are non-leaders. The outcome variable in Columns (1) is an indicator for whether the MEP pair is seated adjacently. In Columns (3) and (4), the outcome variable is an indicator denoting whether the MEP pair cast different votes on the proposal. The listed regressors are indicators for whether the MEP pair is seat neighbors and for whether the MEP pair is *predicted* to be seat neighbors. *Predicted* seating refers to what would have occurred if there had been perfect compliance with the alphabetical seating rule. In addition to day-level and EP-by-EPG-pair 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 (2) were obtained via OLS. Estimates in Column (3) were obtained using 2SLS, using the the predicted seat neighbor indicator as instruments for the actual seat neighbor variable. Standard errors in parenthesis 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. *** p<0.01, ** p<0.05, * p<0.1

F First stage regressions when exploiting venue variation

The table below shows the first-stage regressions corresponding to Table 10. We note that all of the predicted seating variables have predictive power in all of the first-stage specifications. At the same time, 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 variation in the instruments to estimate the effect of each of the three seating variables. Accordingly, the Sanderson and Windmeijer (2016) conditional first stage F-statistic measures of instrument strength shown at the bottom of Table 10 are high for all of the endogenous variables.

Table A.6: First stage regressions for Table 10

	(1) Seat neighbors	(2) Seat neighbors previous venue	(3) Seat neighbors	(4) Seat neighbors previous venue	(5) Seat neighbors both venues
Seat neighbors, predicted	0.7229***	0.0867***	0.7841***	0.1420***	0.1398***
	(0.0448)	(0.0241)	(0.0460)	(0.0370)	(0.0346)
Seat neighbors, previous venue, predicted	0.1474***	0.7679***	0.2209***	0.8342***	0.2012***
	(0.0296)	(0.0365)	(0.0510)	(0.0404)	(0.0484)
Seat neighbors, both venues, predicted			-0.1384**	-0.1251**	0.4869***
			(0.0611)	(0.0503)	(0.0916)
Day-level fixed effects	Yes	Yes	Yes	Yes	Yes
EP-by-EPG fixed effects	Yes	Yes	Yes	Yes	Yes
Baseline name controls	Yes	Yes	Yes	Yes	Yes
Observations	101,126,434	101,126,434	101,126,434	101,126,434	101,126,434
Clusters	76	76	76	76	76

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 Column (1) and (3) the outcome variable is an indicator for whether the MEP pair are seating neighbors during the current meeting. In Columns (2) and (4) the outcome variable is an indicator for whether the MEP pair were seating neighbors during the most recent meeting that took place in a different venue than the current one. In Column (5) the outcome variable is an indicator for whether 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. The listed regressors are indicators for whether the MEP pair is *predicted* to be seat neighbors currently, whether the MEP pair is *predicted* to have been seat neighbors during the most recent previous proposal that was voted on in a different venue than the current one, and whether the MEP pair is *predicted* to both be seat neighbors currently and to have been seat neighbors during the most recent previous proposal that was voted on in a different venue than the current one. *Predicted* seating refers to what would have occurred if there had been perfect compliance with the alphabetical seating rule. 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. Standard errors in parenthesis 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. *** $p<0.01$, ** $p<0.05$, * $p<0.1$