Social interactions of lawmakers and the partisan divide: A natural experiment in Iceland

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Abstract

How does bipartisan seating integration in a Parliament affect bipartisanship? To tackle this question, we use annual randomized seating lotteries in the Icelandic Parliament as a case study. We find a 0.5 to 1 percentage point increase in pair-level similarities of roll-call votes when two MPs from different parties sit together. This effect is larger when one MP in the pair is randomly assigned a corner seat, with no escape from their other-party neighbor. Despite these individual similarity increases, we find no effect of other-party neighbors on general bipartisan voting, as measured by the likelihood of an MP's deviation from the party leader's vote. Furthermore, the pair-level similarity effect is itself only temporary, disappearing the following year when the MPs are no longer seated together. These results support legislative cue-taking and social pressure as likely mechanisms for the effect of other-party proximity on voting decisions. On the other hand, we find suggestive evidence that social interactions may lead to enduring social connections, through co-sponsorship, despite a lack of convergence in revealed political preferences.

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Lawmakers are humans, and they engage in social interactions with fellow lawmakers following their very human nature. As such, friendship and respect between lawmakers have long been widely studied (Patterson, 1972; Caldeira and Patterson, 1988; Caldeira et al., 1993) and were found to be associated with a variety of political behaviors (Wahlke et al., 1962; Ringe et al., 2013). More recently, there has been speculation that disappearing social interactions across party lines may have contributed to the deepening partisan divide among politicians (Haidt, 2012). Social interactions between lawmakers may affect legislative behaviors through many causal pathways, including (i) cognitive mechanisms such as information transmission and persuasion, (ii) affective mechanisms such as increased partisan tolerance through contact, (iii) political cue-taking, and (iv) direct social pressure and monitoring.

Identifying causal relationships and mechanisms between lawmakers' social interactions and their political behaviors is a nontrivial task. Not only are there numerous factors that contribute both to social connections between politicians and the similarities of their political behaviors (Fowler et al., 2011), additional challenges with respect to interference, or spillover effects between individuals are also involved (Manski, 1993). Extant literature has shown progress in identifying the causal effect of various social interactions on economic and political outcomes—such as racial and gender stereotypes (Paluck et al., 2019), macroeconomic knowledge (Carlson, 2019), and policy views (Klar, 2014)—predominantly in controlled laboratory environments. However, it is far more difficult to conduct similar research with lawmakers in the real world where the stakes are much greater, despite the fact that such high stakes are the very reason why their behaviors are important to study.

The Icelandic Parliament (the *Althingi*) provides a unique opportunity to study the effect of institutionally-induced social interactions on the behaviors of lawmakers. Unlike the US, members of Parliament (MPs) in the *Althingi* do not sit together with their party members. Instead, MPs' assigned seats are determined by an annual lottery. This randomized experiment gives exogenous variation in the seating neighbors of each MP every year. Our aim is not to learn about Icelandic politicians in particular. Rather, it is to provide a cleanly-identified case study on how politicians adjust their behaviors during and after sitting next to randomly assigned peers in a high stakes setting, and to shed light on mechanisms.

Using data from 1991 to 2018, we find evidence of an *individual* effect of social interaction—two MPs from different parties vote 0.5 to 1 percentage point more similarly when they are randomly assigned to sit next to each other, compared with two other-party MPs who are not sitting together. However, this effect disappears the next year when the two MPs no longer sit next to each other. We do not find any evidence of a *general* effect on bipartisan voting—MPs are no less likely to adhere to their own partisan voting norm during and after sitting next to MPs from different parties. These results suggest that the causal mechanism on voting outcomes operates only through temporary channels, such as political cue-taking or social pressure, and more fundamental cognitive and affective changes are unlikely to be prevalent.

On the other hand, we find suggestive evidence of a long-term effect of other-party proximity on cross-party social connections proxied by bipartisan co-sponsorship links, an often used indicator of social ties between participating lawmakers (Kessler and Krehbiel, 1996; Fowler, 2006; Zhang et al., 2008; Ringe et al., 2017). There were 0.29 more co-sponsorship links between pairs seating together at the corner and 10 (19%) more bipartisan co-sponsorship links for an MP who sat next to an MP from another party, all measured the next year when the MPs are no longer sitting together. Taken together, the results suggest that seating integration results in persistent increases in bipartisan social connections without any bipartisan convergence in political preferences—i.e., without any general effects on roll-call voting. Of course, even in the absence of convergence in preferences, improved social connectedness can open the possibility of mutually beneficial compromises and avoidance of legislative gridlock, perhaps at political stages preceding roll-call votes.

Delicate empirical analyses are necessary to navigate and verify complex causal pathways. To study individual and general effects separately, we conduct analyses with two different regression specifications—dyad-session level and MP-session level regressions. We also exploit rich longitudinal data and study both contemporaneous and longer-term effects. Using our near-complete knowledge of the randomization assignment mechanism, we conduct Fisherian exact inference in addition to large sample approaches (Gerber and Green, 2012; Imbens and Rubin, 2015).

1 Social interactions and lawmaker behaviors

In this section, we introduce four mechanisms through which social interactions between lawmakers can alter their behaviors. For each mechanism, we first introduce the concept using related literature and then discuss its implications by asking the following questions. (i) Is this mechanism changing behaviors with respect to a general group (general effect) or a particular person (individual effect)? (ii) Do the behavioral changes last after the interaction period ends?

1.1 Cognitive mechanism

Social interactions with fellow lawmakers may involve informal deliberations about political issues. Through the process of deliberation, lawmakers may reflect on their own opinions and reasoning, become aware of the reasoning behind others' points of view, and/or be persuaded to change their beliefs (Habermas, 1991; Mutz, 2002).

What are the empirical implications of cognitive changes via social interactions? (i) Both individual and general effects are plausible as a lawmaker's newly acquired knowledge or updated opinions may induce their political decisions to resemble those of a fellow lawmaker or their group. (ii) Cognitive changes are not entirely situational and, therefore, can have a lasting impact even after the social interactions are over.

1.2 Affective mechanism

Mutz (2002) argues that cross-partisan contacts can lead to greater partisan tolerance via an affective mechanism—through contact, one could realize that "those different from one's self are not necessarily bad people." Similarly, cross-partisan contacts are suggested as one of the potential remedies of affective polarization (Iyengar et al., 2019).

This line of thought relates to the literature on the "contact hypothesis"—the idea that interpersonal contact with outgroups can reduce prejudice under certain conditions (Allport, 1954; Scacco and Warren, 2018; Paluck et al., 2018; Lowe, 2020; Mousa, 2020).

Empirical implications of the affective mechanism are similar to the cognitive mechanism. (i) Renewed feelings may apply to a particular individual or to a group, and (ii) they can have lasting impact on lawmaker's behaviors long after the regular social interaction has ended.

1.3 Legislative cue-taking

Lawmakers may not be fully informed about all issues, so they may mimic nearby lawmakers' actions. This is often called legislative cue-taking (Matthews and Stimson, 1975). In many legislative settings, including the *Althingi*, seat neighbors' roll-call votes are often observed or verbally communicated, enabling cue-taking. Zelizer (2019) provides experimental evidence of legislative cue-taking through information-provision treatments conducted in a state legislature.

Masket (2008) studies relationships between seating proximity and roll-call voting similarity in the 1941-1973 California Assembly. The author finds a strong association between proximity and voting similarity and interprets this as evidence of legislative cue-taking. However, seating in the California Assembly is endogenously selected by the lawmakers themselves. Moreover, Masket's article does not attempt to distinguish cue-taking from other potential mechanisms of voting similarity.

The effect of social interaction through legislative cue-taking is expected to have different empirical implications than it has through cognitive and affective mechanisms. (i) The cue-taking effect is likely to be individual, as a lawmaker only observes the actions of their neighbor, not their neighbor's entire party. (ii) The impact of such mimicking behavior is unlikely to last long as cue-taking will discontinue when regular social interactions are no longer sustained, given that the voting of the peer is no longer immediately observed.

1.4 Social pressure and monitoring

The final mechanism is social pressure and monitoring. Since a lawmaker's political actions can be observed by their seating neighbor, they may take actions that conform to the neighbor's views to signal that they share an agreement or that they listen to the neighbor, perhaps ultimately to avoid stigma or unnecessary conflict and for the hedonic value of having a good relationship with neighbors ("a warm glow"; Andreoni, 1990). This possibility has not been discussed widely in legislative contexts in the extant literature, but it appears in other contexts, such as voter turnout and charitable giving (Gerber et al., 2008; DellaVigna et al., 2012, 2017).

Social pressure from a neighbor has similar empirical implications as it does from cue-taking. (i) Social pressure steers behaviors primarily toward the direction of the person giving the pressure (individual effect), not to the group overall. (ii) It is unlikely to have an impact when social interactions are no longer happening.

In principle the mechanisms of social pressure and cue-taking can be distinguished by noting that in the case of the former, individuals would like to *avoid* influence (Andreoni et al., 2017), whereas with cue-taking, the influence is valuable. In this setting we cannot measure avoidance behaviors, and so we cannot easily distinguish the two mechanisms.

1.5 Hypothesis

We analyze two political behaviors in this study—roll-call votes and co-sponsorship. We treat roll-call votes as the main measure of lawmakers' revealed preferences. Roll-call votes have the most significant direct consequences among political behaviors, and not surprisingly they have traditionally been used to construct benchmark measures of ideology (Clinton et al., 2004; Poole and Rosenthal, 2011). Therefore, roll-call votes are used predominantly to distinguish mechanisms outlined above. On the other hand, co-sponsorship has been regarded as "a rich source of information about the social network between legislators" (Fowler, 2006), and here we follow that tradition and use the measure in a supplementary analysis focusing on social connectedness.

Political Preferences. We hypothesize that social interactions prompted by random seating arrangements in the Parliament are unlikely to induce significant bipartisan convergence in roll-call voting patterns. Regarding voting—lawmakers' most consequential activity—lawmakers are likely to have a set agenda originating from their values and self-interest, or from the demands of their constituency and party. If anything, the effect of a random seating arrangement on political behavior will be through a temporary mimicking of a fellow politician's behaviors when a lawmaker does not yet have full knowledge about the agenda at hand (cue-taking) or through social pressure from a year-long desk-mate. To answer questions about mechanisms with data-driven methods, we exploit that cue-taking and social pressure mechanisms are expected to have distinct empirical implications from cognitive and affective mechanisms, as discussed above and hypothesized below.

H1. (Individual effect on voting). Two seating neighbors will vote more similarly than a comparable pair. However, this will not generate a statistically significant *general effect* of MPs deviating from their own party's agenda.

H2. (Temporary effect on voting). The individual effect described above will be temporary, lasting only when the two are seated next to each other and disappearing the following year when they are no longer seated together.

To test for individual and general effects separately (H1), we use two specifications. First, we estimate pair-level effects (individual effects) by comparing the similarity of voting behavior of MP pairs seated together versus otherwise-similar MP pairs seated apart. These pair-level effects capture direct influences from neighbors. Second, we estimate MP-level general effects—i.e., not specific to neighbors—of integration on bipartisanship by comparing the behavior of MPs seated next to more versus fewer other-party MPs. We conduct both levels of analyses on this year's outcomes; we also conduct these analyses on next year's outcomes to test for longer-term effects (H2).

In this paper, we emphasize cross-partisan interactions and their impacts on the partisan divide. To this end, the main outcome variable we use for MP-level regressions is whether MP's roll-call votes are deviating from their own party's norm when the MP is sitting next to more (rather than fewer) other-party MPs. Similarly, for pair-level analyses, we separately estimate the effect of seating adjacency on same-party pairs and other-party pairs. This analysis complements Harmon et al. (2019), who exploit alphabetical seating in the EU Parliament to test for pair-level effects in legislative voting. They find that sitting next to a politician from the *same* party increases the probability that both politicians will vote the same way. Harmon et al. (2019) can only estimate imprecise effects on pairs from different parties as members of the EU Parliament sit in party groups.[1]

Social Connections. Social connections can be made and endure even without any changes in political preferences. To supplement the analyses above, we also investigate whether there are more bipartisan social connections when and after two MPs from different parties are assigned to adjacent seats. We test this by estimating effects on co-sponsorship links. We conduct this supplementary analysis because even in the absence of convergence in preferences, improved social connectedness can open the possibility of mutually beneficial bipartisan cooperation at political stages preceding roll-call votes.

H3-1. (Effect on co-sponsorship). Bipartisan seating results in increased bipartisan social connections measured by co-sponsorship links between other-party MPs.
H3-2. (No effect on co-sponsorship). Bipartisan seating arrangement does not facilitate creation of bipartisan social connections.

For pair-level co-sponsorship results, we use the number of co-sponsorship links between two lawmakers. As in voting, we focus on the effect of seating adjacency on co-sponsorship links between other-party pairs. For the MP-level general effect estimation, we use the total number of co-sponsorship links with other-party MPs.

This paper contributes to the literature by (i) cleanly detecting causal effects of bipartisan integration among lawmakers in a legislature and (ii) discerning the mechanisms of how social interactions between lawmakers affect their behaviors. Rogowski and Sinclair (2012) find no effect of office location proximity in the US House on roll-call votes and co-sponsorship behaviors. In a paper written contemporaneously to ours, Saia (2018) conducts MP-level analysis using the same randomized experiment in the Icelandic Parliament but does not distinguish between the mechanisms that we have outlined above. Furthermore, we provide analyses from multiple specifications (MP-session and dyad-session levels) on multiple outcome variables (voting and co-sponsorship) and study persistent effects.[2]

[1] See Appendix F of Harmon et al. (2019). Only 0.02% of the pairs comprise MPs from different parties.

[2] Saia (2018) finds that those sat next to all other-party legislators are 30 to 50 percentage points more likely to go against their party leader's vote than those sat next to no other-party legislators. We find that some of the large

2 Icelandic politics and the annual seating lottery

Parliament. Iceland has a unicameral parliamentary democracy with a multi-party system. The head of the executive branch is the prime minister who is the leader of a multi-party coalition with a majority—no single political party has won a majority of seats since Iceland became a republic in 1944. Parliamentary elections are held every four years; accordingly, the length of an MP's term is four years. A total of 63 MPs are elected by proportional representation each term. Similar to other democratic nations, the Parliament of Iceland is the legislative branch of the government—it represents the constituency, enacts laws, and oversees the government.

Seating. The seats in Parliament are allocated to MPs annually by lottery, a practice established in 1916 (Magnusson, 2014). At the beginning of each annual session, each MP draws a ball from a box (Figure 1); that ball indicates the designated seat of the MP for the parliamentary year.[3] Some MPs are exempt from the random draw: the prime minister, speaker, ministers,[4] and chairs of parliamentary groups (caucuses) have their own designated seats.[5] MPs with physical disabilities are also exempt from the lottery—they are assigned corner seats for easier wheelchair access. Figure 2 shows the seating assignment at the end of the 2014-15 session. Ministers sit at special desks on the right side of the figure, whereas other MPs are assigned to the main seats on the left. Our analysis includes all those who participate in the seating lottery, as well as those pre-assigned to main seats on the left—although their seats are not randomly chosen, their neighbors are randomly assigned. This means that the prime minister and ministers are excluded from our analysis.

On rare occasions, the seating assignment can change during an annual session. The most typical case is when a chair of a parliamentary group is appointed as a new minister. On average, 95% of MPs maintain their initial seating assignment until the end of the session. Nevertheless, in our empirics we always present intent-to-treat (ITT) estimates using the initial assignment of

general effects on bipartisanship in his paper can be attributed to a regression misspecification. See Appendix C for more discussion about the results of Saia (2018).

[3]A video of the lottery for the 2014-15 session can be downloaded here.

[4]Some ministers are MPs themselves, whereas others are appointed from outside of Parliament.

[5]Chairs of parliamentary groups are assigned to aisle seats, for easier access to the podium. Though this custom has been present throughout the duration of our analysis period, this pre-assignment was only formalized and recorded since the 2004-05 session. Prior to that, there is ambiguity as to whether a chair of a parliamentary group in an aisle seat was pre-assigned that seat, or assigned it by lottery. We address this issue by assuming that any chairs of parliamentary groups in aisle seats were pre-assigned to that seat. In practice this choice is unlikely to affect our results given that less than 10% of MPs in each session are parliamentary chairs, and that this ambiguity does not apply to seating assignments after 2004. Consistent with this, our frequent placebo tests—using the previous year's outcomes on the left hand side—give evidence against any resultant selection concerns.

seating—our estimates are not confounded by the small amount of endogenous movement during the session.

Treatment intensity. MPs assigned to neighboring seats spend a significant amount of time sitting next to each other. The average total length of an annual parliamentary session (across 1992-93 to 2017-18, excluding special and short sessions) is 670 hours, excluding committee meetings where MPs are not expected to sit at their designated seats. During each regular parliamentary session, members should not move to empty seats or sit in other unoccupied seats.[6] That said, MPs tend to be present in the chamber only for votes and their own speeches. In practice, MPs may spend one to two hours in their assigned seats on a typical voting day, and otherwise only 20 to 30 minutes in their assigned seats on any given day in the session.[7] Nevertheless, over the course of a parliamentary session, this adds up to many hours of contact.

The Icelandic seating lottery provides a rare opportunity to study social influence among political elites. Iceland is one of only a few countries where political representatives do not sit in party groups—other exceptions being Norway and Sweden, where seating is grouped by constituency. The Icelandic Parliament is also the only national parliament in the world with a randomized seating arrangement.[8]

3 Data and specification

3.1 Data description

We compiled data on initial seating assignments, voting, and co-sponsorship for all sessions from 1991-92 to 2017-18.[9] We describe the main features of the data in this section, with further details on data sources in Appendix B.

Seating. We collected data on annual initial seating assignments from the parliamentary records ("*Althingi* journals"). For sessions from 1995-96 to 2017-18, we web-scraped parliamentary records available on the *Althingi* website. For sessions prior to 1995-96, we digitized scanned copies of parliamentary records, also available on the *Althingi* website.

[6] According to Hildur Gróa Gunnarsdóttir, an Information Officer of the *Althingi*.

[7] According to personal correspondence with Gylfi Magnússon, Icelandic economist and Minister for Economic Affairs from 2009 to 2010.

[8] Similar examples exist in history (Magnusson, 2014): from 1845 to 1913 US House of Representatives members held a lottery for seating, and the Philippine Assembly had random seating arrangements in the lower chamber from 1907 to 1988. Unlike Iceland, in both of those cases the random drawing would determine not the exact seat, but only the priority order in which seats were chosen. This gave opportunities for party-sorting.

[9] Seating arrangements are available starting in 1991, which is when the Parliament became unicameral.

MP demographics. The *Althingi* website also posts biographical information about MPs, from which we collected basic information such as party, constituency, gender, and tenure. We combined this data with the seating assignment data to link each seat with the MP's characteristics.

Voting. We web-scraped roll-call voting data from the *Althingi* website, and used this data to construct two MP-session-level voting outcomes. *Leader Non-Compliance* is the proportion of times the MP cast a vote that was different from their party leader in a given session, weighted by bill.[10] A vote can be in one of four categories: yes, absent, abstain, or no.[11] The MP is non-compliant when the vote chosen from among these four categories is different from that chosen by their party leader. We consider *Leader Non-Compliance* to be a measure of general bipartisanship.

Our second voting outcome, *Leader Difference*, accounts for the fact that when the party leader votes yes, an MP that votes no is more rebellious than an MP who is absent. Put another way, non-compliant votes differ in their degree of non-compliance. To capture this variation, we code the degree of vote difference on a zero to three scale. We consider the categories of votes to be ordered by their strength of support: yes being the most supportive, followed by absent, then abstention, then no. If an MP votes identically to their party leader, they score zero on the scale of vote difference. If their votes differ, but are adjacent in their order of strength (i.e., yes-absent, absent-abstain, or abstain-no), they score one. If their votes differ by two (yes-abstain or absent-no), they score two. If their votes differ by three (yes-no), they score three. We average these difference scores across all votes in a session, again weighting equally by bill, to construct the measure *Leader Difference*.

We also construct two corresponding but reverse-coded voting outcomes at the *MP-pair*-session-level. We reverse-code these outcomes so that in all specifications a more positive outcome is reflective of more bipartisanship. Our two pair-level measures are: *Pair Compliance*, which is the proportion of times the two MPs in a pair vote the same way, and *Pair Similarity*, which is three minus the average vote difference (as previously explained) at the pair-level.

Co-sponsorship. We web-scraped co-sponsorship data from the *Althingi* website, covering the sponsor and co-sponsors of each bill, resolution, and report. We used this data to construct two MP-session-level co-sponsorship outcomes. *Raw Number of Co-sponsorship Links* is the total number of links an MP has with other-party members through sponsorship or co-sponsorship

[10] In other words, two bills will be weighted equally even if there were more votes on one bill than the other.

[11] Absent means the MP is not present during the vote procedure, whereas abstain means an MP who is on the Parliamentary floor does not cast a vote.

during that session. To reduce the influence of outliers and give the coefficients an elasticity interpretation, we took the *Inverse Hyperbolic Sine* of this measure as our second co-sponsorship outcome. Our two measures at the MP-pair-session-level are similar, but at the pair-level. The raw number of links is then the number of bills, resolutions, or reports containing the names of both MPs in a pair, either as sponsor or co-sponsor. The second measure is the inverse hyperbolic sine of the first.

3.2 Empirical specification

We use two main empirical specifications. To estimate pair-specific effects of proximity, we use an MP-pair-session-level specification. To estimate general effects of cross-party proximity, we use an MP-session-level specification.

Pair-session-level specification. To estimate pair-specific effects of cross-party proximity, we use the following MP-pair-session-level specification:

$$\begin{aligned} y_{ab\{t-1,t,t+1\}} = \alpha_{p(a)p(b)st} &+ \gamma_1 \left(\text{Neighbor}_{abt} \times \text{Same Party}_{abt}\right) \\ &+ \gamma_2 \left(\text{Neighbor}_{abt} \times \text{Different Party}_{abt}\right) + u_{abt} \end{aligned} \tag{1}$$

More specifically, since each session gives a new experiment (given the new randomized seating plan), this specification stacks different cross-sections (one per session) to pool the experiments to estimate the effect of cross-party proximity. An observation within a session is at the MP-pair-level. With N MPs represented in a given session, this implies a total of $\frac{N(N-1)}{2}$ observations for that session, reflecting all possible combinations of MP pairs, given that an MP cannot be paired with themself.

This specification estimates the effect of MPs a and b, who are seating neighbors, separately by whether a and b belong to the same, or different, political parties. y_{abt} is one of our measures of similarity between MPs a and b during session t, while $\alpha_{p(a)p(b)st}$ are session-by-strata-by-party pair fixed effects. We require only session-by-strata-by-Same Party fixed effects for identification, but we use this richer set of fixed effects to increase precision.

Strata fixed effects are necessary for identification. For each session, there are three strata, depending on whether MP a and/or b were pre-assigned seats in session t or not. The first strata equals one when both MPs in the pair were pre-assigned. For these pairs it is always the case that $\text{Neighbor}_{abt} = 0$. The second strata equals one when either one, but not both, of them was pre-assigned. The third equals one when neither were pre-assigned. Two types of MPs are pre-assigned seats and included in our analysis: MPs with disabilities and chairs of parliamentary groups. We include MPs with disabilities and chairs of parliamentary groups in the analysis since, from their perspective, the MP assigned to sit next to them was chosen randomly. In contrast, the prime minister and the ministers also do not participate in the lottery but are excluded from the analysis. This is because they sit in a section that is separate from the other MPs, with no possibility of sitting next to an MP who was assigned a seat by lottery. This sample restriction leaves 53 MPs in our analysis for the median session.

Neighbor_{abt} is a dummy variable equal to one if MPs a and b are assigned to sit next to each other (on the left or right) during session t. MPs have either one or two neighbors in total (Figure 2). Same Party_{abt} is a dummy variable equal to one if MPs a and b both belong to the same party during session t, and $\text{Different Party}_{abt} = 1 - \text{Same Party}_{abt}$. It follows that γ_1 is the causal effect of two same-party MPs being assigned to sit next to each other. Similarly, γ_2 is the causal effect of proximity for different-party MPs. γ_2 is our primary parameter of interest, given its relation to the question of the effects of integration on bipartisanship. With Iceland's fragmented party system, 77.1% of our observations in this specification are different-party MP pairs. In this setting, we thus have more statistical power to detect cross-party proximity effects than same-party proximity effects. That said, we still estimate both γ_1 and γ_2, and test for $\gamma_1 = \gamma_2$ to understand whether the effects of proximity depend on pre-existing similarity.

To test for persistent treatment effects, we replace the left-hand-side variable with $y_{ab,t+1}$, the outcome for MP-pair ab during the subsequent session, after the seating plan has been re-randomized. As a placebo check, we replace the left-hand-side variable with $y_{ab,t-1}$, the outcome for MP pair ab during the previous session.[12]

We take two approaches to inference. First, we report dyadic-robust standard errors and p-values (Cameron and Miller, 2014), which allow for residuals to be correlated between any two MP-pair-session observations with an MP in common—allowing for both cross-sectional correlation (e.g., MPs who co-sponsor frequently with some MPs may also tend to co-sponsor frequently with others in the same session) and across time (e.g., MPs who co-sponsor frequently with others at time t may also tend to co-sponsor frequently with others at $t+1$). Second, we use randomization inference to calculate Fisher's exact p-values. For this randomization

[12]We exclude special and short sessions from the analysis. In addition, for the lead and lag specifications, we drop any sessions where the lead/lag would be a special or short session, or a session in a different parliamentary term. We do the latter to avoid selection problems that might arise if the seating arrangements also somehow affect parliamentary turnover. For example, MPs may be more likely to run for re-election if they spent the last session sitting next to friends from their own party than otherwise.

inference, we simulate placebo seating assignments by following the *Althingi*'s exact procedure for assigning seating (in particular, we hold the seating position of those MPs pre-assigned to seating constant). The method for estimating dyadic standard errors assumes that the number of clusters goes to infinity. The advantage of randomization inference is that it does not rely on asymptotics, giving an exact test against the sharp null hypothesis of no treatment effects (Young, 2015; Imbens and Rubin, 2015).

When we use randomization inference to test for $\gamma_1 = \gamma_2$, we follow Gerber and Green (2012) and employ the sharp null hypothesis that $\gamma_{1i} = \gamma_{2i} = \hat{\gamma}$ where $\hat{\gamma}$ is the point estimate on Neighbor_{abt} from the pooled specification:

$$y_{ab\{t-1,t,t+1\}} = \alpha_{p(a)p(b)st} + \gamma \text{Neighbor}_{abt} + e_{abt} \tag{2}$$

MP-session-level specification. To estimate general effects of cross-party proximity, we use the following MP-session-level specification:

$$y_{i\{t-1,t,t+1\}} = \alpha_{pst} + \beta \text{Proportion Other Party Neighbor}_{it} + \varepsilon_{it} \tag{3}$$

Similar to the pair-session-level specification, this specification stacks one cross-section per session. The specification differs in that an observation within a session is at the MP-level.

y_{it} is a co-sponsorship or voting outcome for MP i during session t, while α_{pst} are session-by-party-by-strata fixed effects. Party fixed effects are not necessary for the internal validity but, nonetheless, are added to reduce noise and thus improve statistical power.

The strata fixed effect is necessary for internal validity. This fixed effect is a dummy variable for whether MP i was pre-assigned—either chairs of parliamentary groups or MPs with disabilities—a seat during session t as opposed to having participated in the seating lottery.

$\text{Proportion Other Party Neighbor}_{it} \in \left\{0, \frac{1}{2}, 1\right\}$ is the fraction of left-right seating neighbors of MP i during session t who belong to a different political party. β is our parameter of interest, capturing the general effect of having all versus no other-party left-right neighbors on co-sponsorship and voting outcomes.

For inference, we report standard errors clustered at the MP-level and corresponding p-values, as well as p-values from randomization inference. Cluster standard errors at the MP-level account for the fact that a given MP will regularly appear in multiple cross-sections since MPs usually serve more than one year in Parliament.

As with the first specification, to test for persistent effects we replace the outcome with $y_{i,t+1}$, for the placebo check we use $y_{i,t-1}$, and we follow the same session-dropping rules.

4 Results

4.1 Pair-specific effects on voting

We first study pair-specific effects of proximity using specification (1). We use this specification to compare pair-level outcomes between MP pairs who are seating neighbors with MP pairs who sit apart. We make these comparisons separately by whether the MP pair belong to the same party or to different parties. By doing so, we estimate both the outgroup (for different-party pairs) and ingroup (for same-party pairs) effect of proximity. Since the main inquiry of this paper is the effect of integration on bipartisanship, we focus particularly on outgroup effect, which is also more precisely estimated due to the greater number of different-party pairs.

MPs from different parties vote 0.5 percentage points (RI p-value = 0.07) more similarly when they are randomly seated next to each other (Column 1, Table 1), and their mean voting similarity is 0.04 standard deviations (RI p-value = 0.005) higher (Column 2). We find the outgroup proximity effect on next-session voting to be close to zero (Columns 3-4). MPs from different parties who sit next to each other for one session vote no more similarly than other MP pairs in the subsequent session. Placebo coefficients are statistically insignificant (Columns 5-6, Table 1), ruling out concerns of chance imbalances. Note that dyadic-robust standard errors are in parentheses, corresponding p-values are in square brackets, and p-values from randomization inference are in curly brackets.

It is possible that we are attenuating our estimates by including many lopsided votes where most MPs vote the same way. For these votes, there may be barely any systematic differences in the voting patterns of different parties, reducing the scope for cross-party influence. To address this, we recreate the two voting outcomes in Table 2 using only data from the more contested votes. Specifically, for each vote we identify the modal vote (whether yes, absent, abstain, or no) and the share of MPs who vote in the same way as the modal vote. We then recreate the two voting outcomes using (i) only the votes in which the share of modal vote MPs is less than the median; and (ii) only the votes in which the share of modal vote MPs is less than the twenty-fifth percentile. Consistent with intuition, we find qualitatively similar yet stronger outgroup effect. For these votes, different-party pairs of MPs are roughly one percentage point more likely to vote similarly (Panel A), and the different-party proximity p-values are all below 0.015. Note that even when we focus on contested votes, no persistent bipartisanship effects are found (Panel

B), consistent with H2.

Another potential attenuating factor is divided attention—with seating neighbors on the left and right for most MPs, the attention of each MP is potentially divided. Furthermore, this attention may not be directed equally to the MP on the left and the MP on the right—if an MP sits next to one same-party member and one other-party member, the MP naturally might direct most of their attention to the same-party member. To address this, we use the random assignment of MPs to corner seats versus seats in the middle of rows in Table 3. MPs in corner seats have only one seating neighbor—their attention is undivided. For brevity, we restrict our sample only to different-party MP pairs. In addition we keep only the MP pairs who were both part of the seating lottery.[13]

Consistent with our hypothesis, proximity effects on voting for corner-seat MPs are three to five times larger than for middle-seat MPs, though given a lack of power we cannot quite reject that the effects are equivalent at the 10% level (Columns 1 and 2, Table 3). As before, proximity effects are not persistent (Columns 3-4).

Finally, our pair-level results on voting similarity are comparable if we consider pairs to only be voting the same way if they both vote yes or both vote no (Table A1). This suggests that the small positive effect of proximity for different-party pairs is not driven by seating neighbors abstaining together or coordinating their absences.

To summarize, we find evidence of a temporary and individual effect of bipartisan integration on roll-call votes. The results are stronger when we focus on contested votes and corner seats where attenuating factors are expected to be removed and are robust to different measures of vote similarity.

4.2 General effects on voting

We present general effects of cross-party proximity using empirical specification (3). We use this specification to compare the voting outcomes between MPs seated next to a different number of politicians from a different party.

Cross-party proximity has small and statistically insignificant effects on rebellious voting (Columns 1-2, Table 4). The point estimate in Column 1 suggests that sitting next to all other-party neighbors versus none decreases the fraction of votes different to the party leader's by 0.46 percentage points. This point estimate is, however, very small relative to the mean of voting non-compliance (32 percentage points), and statistically insignificant using either inference approach.

[13] In other words, we drop any MPs pre-assigned to seats, since these pre-assigned seats are corner seats, but these MPs do not contribute any random variation in assignment to corner seats.

General effects of proximity on voting are larger in magnitude one year later (Columns 3-4, Table 4), although still statistically insignificant. Overall, the evidence in Table 4 suggests that cross-party proximity does little to reduce party-line voting, either contemporaneously or one year later. We reach a similar conclusion when we separately estimate the effects of having half versus all left-right seating neighbors from a different party (Table A2). In particular, there is no consistent evidence that having all neighbors from a different party increases bipartisan behavior. These results are consistent with H1 and rule out mechanisms that involve fundamental cognitive or affective shifts.[14]

Finally, we conduct a placebo check to test for internal validity. MPs due to sit next to more other-party MPs in the next period are no more or less likely to rebel in their voting with other party members (Columns 5-6 in Table 4). This placebo check supports the claim that the seating plan is randomized, and in particular, that Proportion Other Party Neighbor$_{it}$ is exogenous after controlling for session-by-party-by-strata fixed effects.

The results in Sections 4.1 and 4.2 support temporary individual effects (H1 and H2) of integration. As noted in Section 1, these empirical patterns are more likely when the main mechanism is through legislative cue-taking or social pressure and not through cognitive or affective mechanisms.

4.3 Effects on co-sponsorship

In this section, we focus on another legislative activity—co-sponsorship. As discussed in Section 1, we treat co-sponsorship outcomes as one of the closest proxies to social connectedness between lawmakers. As with voting, we analyze (i) individual effects with dyad-session level regressions and (ii) general effects with MP-session-level specification.

Table 5 reports pair-level effects of cross-party proximity on co-sponsorship links. Bipartisan proximity does not lead to increased co-sponsorship links for different-party pairs in any time period that we consider.[15] In Table 6 we compare the treatment effects of other-party pairs who sat at corners of the parliamentary floor to investigate whether undivided attention between neighbors can strengthen the treatment effect on co-sponsorship. We find 0.29 more co-sponsorship

[14] We focus on contested votes only in Table A3 and find similar results as Table 4, with all estimates statistically insignificant for both contemporaneous and persistent effects.

[15] Although not the main focus of this paper, there is some evidence of a negative effect of in-group proximity, reducing co-sponsorship links at the pair-level by ~9% (RI p-value = 0.05). We observe placebo estimates with the same sign and similar magnitudes (Columns 5-6), despite not being significant. In this case, the negative effect potentially comes from a chance failure of baseline balance (i.e., that same-party pair seating neighbors already happened to be co-sponsoring together less frequently than same-party pairs seated apart).

links between pairs who sat at corners, and this is statistically significantly larger than the effect on the pairs who sat in the middle. This provides suggestive evidence that year-long deskmates may forge an enduring social connection when the attention of one deskmate is undivided.

Table 7 reports general effects on bipartisan co-sponsorship links. Having a larger proportion of other-party neighbors does not affect the number of contemporaneous bipartisan co-sponsorship links (Columns 1-2). In Column 3 we observe a long-term general effect of cross-party proximity on bipartisan co-sponsorship links. The effect size is moderate (10 additional links or 19%), though it becomes marginally insignificant when we use the inverse hyperbolic sine transformation instead of raw numbers. We also observe sensibly that the persistent impact on bipartisan links is larger for those with two other-party neighbors than those with only one other-party neighbor (Column 3, Table A4).

Although more suggestive, these enduring impacts on cross-party co-sponsorship links offer some hope that bipartisan seating arrangements may create bipartisan social connections. That said, any such connections do not appear to translate into position convergence, given the lack of persistent effects on roll-call votes.

5 Conclusion

Politicians in many countries are segregated at the workplace. MPs in the UK sit with the government on one side of a 3.96 meter aisle, and the opposition facing them on the other side. This adversarial arrangement is reflected in the history of the aisle width: 3.96 meters is roughly equivalent to the length of two swords. The arrangement need not be this way. In Norway and Sweden, MPs sit together in constituencies. In Iceland, MPs are randomly assigned to seats every year. Does integrating politicians have any impact on partisanship? And if so, through what mechanisms?

Using the Icelandic Parliament as a case study and exploiting its random seating assignments, we estimate the effects of bipartisan integration on roll-call votes and co-sponsorship outcomes. We find a statistically significant pair-level out-group effect on voting similarity. However, these effects are short-lived, and do not translate into general bipartisan voting outcomes. These empirical patterns are more consistent with legislative cue-taking and social pressure mechanisms than cognitive and affective changes.

On the other hand, we find a suggestive positive effect of bipartisan seating proximity on cross-party social connections, as proxied by the number of bipartisan co-sponsorship links. This positive effect appears not to translate into long-term effects on the revealed bipartisan preferences of politicians, given the lack of a general or persistent effect on voting outcomes.

Different mechanisms point to different policy implications of social integration in Parliament, which makes distinguishing mechanisms more than just a theoretical endeavor. If the results are likely to be driven by cognitive and affective mechanisms, facilitating social interactions can be an effective tool for reducing the partisan divide. On the other hand, if the results are mostly driven by legislative cue-taking and social pressure, they are likely to be short-lived and require the nearby physical presence of other-party peers, limiting the scope of bipartisan interaction effects. This paper demonstrates a method to distinguish between these mechanisms.

Republicans and Democrats in the US Congress play an annual charity baseball game together.[16] Heroically extrapolating the results of this paper, bipartisan behavior demonstrated on the baseball field is unlikely to spill over into lawmakers' bipartisan roll-call voting, though may be more likely to create new bipartisan social connections. Of course, more studies in a variety of contexts should be conducted to test whether the results found in this Icelandic study can be generalizable, which we leave to future research.

[16]See https://en.wikipedia.org/wiki/Congressional_Baseball_Game.

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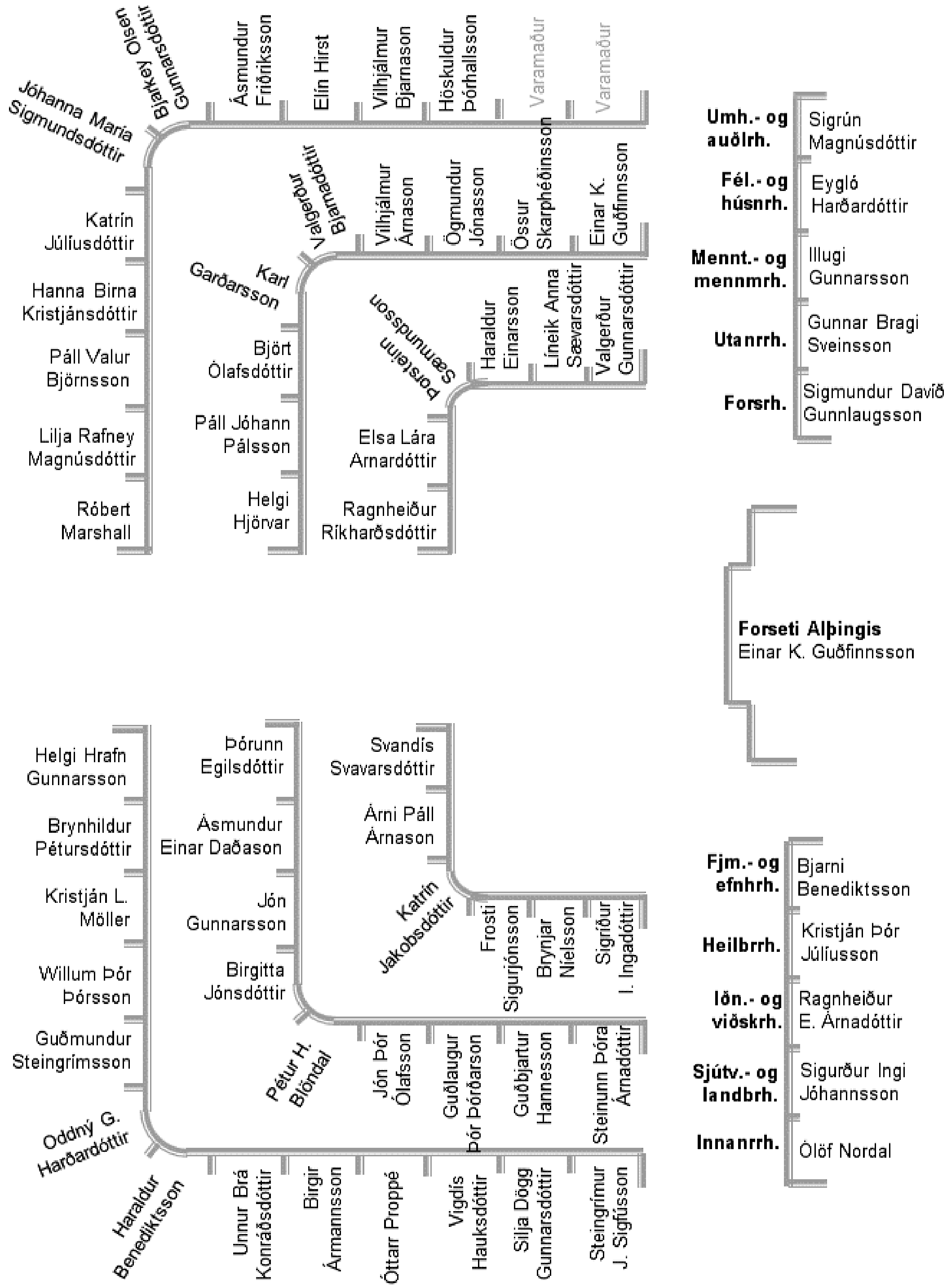
Figures

Figure 1: An MP is drawing her seat number for session 2013-2014

Figure 2: Seating assignment for 2014-2015



Source: http://www.althingi.is/ [Link]

Tables

Table 1: Pair-level Effects on Voting

	Contemporaneous Effect (t)		One Year Later (t+1)		Previous Year (Placebo) (t-1)	
	Pair Compliance (1)	Pair Similarity (2)	Pair Compliance (3)	Pair Similarity (4)	Pair Compliance (5)	Pair Similarity (6)
Neighbor × Different Party	.0051	.0071	.0008	.000057	.0013	.0017
(proximity effect on bipartisanship)	[.057]*	[.0047]***	[.86]	[.99]	[.68]	[.59]
	{.07}*	{.005}***	{.81}	{.98}	{.71}	{.63}
Neighbor × Same Party	.0036	.0037	.011	.0099	.0044	.0027
	[.57]	[.57]	[.19]	[.28]	[.59]	[.75]
	{.6}	{.58}	{.15}	{.17}	{.6}	{.76}
Same = Different	[.82]	[.61]	[.32]	[.35]	[.74]	[.92]
	{.84}	{.65}	{.29}	{.29}	{.76}	{.94}
Session × Party Pair × Strata FE	Y	Y	Y	Y	Y	Y
Observations	35259	35259	21589	21589	21638	21638
Outcome Mean	.57	2.5	.57	2.5	.57	2.5
Outcome S.d.	.13	.17	.13	.17	.13	.17

Notes: Each column in this table originates from a separate linear regression. Pair Compliance is the proportion of times the two MPs in a pair vote the same way in a given session. Pair Similarity is the average vote similarity between the two MPs in a pair. Neighbor is a dummy variable equal to one if the MPs in the pair are randomly assigned to sit next to each other during that session. Same Party is equal to one if both MPs in the pair are in the same party for that session. Dyadic-robust p-values are in square brackets. Randomization inference p-values (200 draws) are in curly brackets. Special sessions and a short session (147) are excluded. For lead and lag specifications, sessions are also dropped where lead/lag would be a special/short session or a session in a different parliamentary term. All regressions include Session × Party Pair × Strata FE. Strata FE are dummy variables for whether both MPs in a pair were pre-assigned seats, one MP in a pair was pre-assigned a seat, or neither MP in a pair was pre-assigned a seat. Outcome Mean and S.D. in all columns are based on the full sample (the sample included in the contemporaneous regressions in Columns 1-2). *** p<0.01, ** p<0.05, * p<0.1.

Table 2: Pair-level Effects: Voting on Contested Votes

	Below 50th Votes		Below 25th Votes	
	Pair Compliance (1)	Pair Similarity (2)	Pair Compliance (3)	Pair Similarity (4)
	Panel A: Contemporaneous Effect (t)			
Neighbor × Different Party	.0092	.012	.01	.016
(proximity effect on bipartisanship)	[.00038]***	[.00002]***	[.0021]***	[.037]**
	{.015}**	{.005}***	{0}***	{.005}***
Neighbor × Same Party	.0083	.0094	.009	.015
	[.21]	[.27]	[.3]	[.28]
	{.23}	{.2}	{.22}	{.11}
Observations	35205	35205	35102	35102
	Panel B: One Year Later (t+1)			
Neighbor × Different Party	-.0021	-.0039	-.0041	-.011
(proximity effect on bipartisanship)	[.61]	[.38]	[.42]	[.21]
	{.6}	{.41}	{.33}	{.14}
Neighbor × Same Party	.014	.011	-.00062	-.0064
	[.18]	[.38]	[.95]	[.73]
	{.085}*	{.18}	{.96}	{.58}
Observations	21589	21589	21540	21540
	Panel C: Previous Year (Placebo) (t-1)			
Neighbor × Different Party	.00055	.0012	-.0018	-.0017
(proximity effect on bipartisanship)	[.88]	[.79]	[.68]	[.85]
	{.91}	{.8}	{.63}	{.83}
Neighbor × Same Party	.0058	.0044	-.0013	-.0041
	[.55]	[.69]	[.91]	[.8]
	{.5}	{.68}	{.86}	{.78}
Observations	21638	21638	21638	21638
Session × Strata × Party Pair FE	Y	Y	Y	Y
Outcome Mean	.46	2.3	.37	2
Outcome S.d.	.15	.32	.23	.55

Notes: Each panel shows the estimates from four regressions. Below 50th/25th votes are votes in which the share of MPs voting the modal vote is less than the median/25th percentile among all votes. Pair Compliance is the proportion of times the two MPs in a pair vote the same way in a given session. Pair Similarity is the average vote similarity between the two MPs in a pair. Dyadic-robust p-values are in square brackets. Randomization inference p-values (200 draws) are in curly brackets. Special sessions and a short session (147) are excluded. For lead and lag specifications, sessions are also dropped where lead/lag would be a special/short session or a session in a different parliamentary term. All regressions include Session × Strata × Party Pair FE. Strata FE are dummy variables for whether both MPs in a pair were pre-assigned seats, one MP in a pair was pre-assigned a seat, or neither MP in a pair was pre-assigned a seat. Same Party is equal to one if both MPs in the pair are in the same party for that session. Outcome Mean and S.D. in all columns are based on the full sample (the sample included in the Panel A regressions). *** p<0.01, ** p<0.05, * p<0.1.

Table 3: Pair-level Effects on Bipartisan Voting: Effects with Undivided Attention

	Contemporaneous Effect (t)		One Year Later (t+1)		Previous Year (Placebo) (t-1)	
	Pair Compliance (1)	Pair Similarity (2)	Pair Compliance (3)	Pair Similarity (4)	Pair Compliance (5)	Pair Similarity (6)
Neighbor × Middle	.0024	.0047	.0012	.0014	.0045	.0043
	[.42]	[.084]*	[.81]	[.79]	[.19]	[.24]
	{.46}	{.17}	{.76}	{.77}	{.26}	{.29}
Neighbor × Corner	.013	.013	.0043	.0013	-.012	-.012
	[.046]**	[.041]**	[.68]	[.9]	[.19]	[.16]
	{.1}	{.08}*	{.57}	{.89}	{.14}	{.14}
Middle = Corner	[.14]	[.24]	[.79]	[.99]	[.07]*	[.06]*
	{.21}	{.32}	{.72}	{.99}	{.095}*	{.085}*
Session × Corner FE	Y	Y	Y	Y	Y	Y
Session × Party Pair FE	Y	Y	Y	Y	Y	Y
Observations	22652	22652	14140	14140	13863	13863
Outcome Mean	.56	2.5	.56	2.5	.56	2.5
Outcome S.d.	.13	.16	.13	.16	.13	.16

Notes: Each column in this table originates from a separate linear regression. Pair Compliance is the proportion of times the two MPs in a pair vote the same way in a given session. Pair Similarity is the average vote similarity between the two MPs in a pair. Neighbor is a dummy variable equal to one if the MPs in the pair are randomly assigned to sit next to each other during that session. Corner is equal to one if at least one MP in a pair has only one seating neighbor. Middle is equal to one minus Corner. Regressions include different-party dyads only, with neither MP pre-assigned seats. Dyadic-robust p-values are in square brackets. Randomization inference p-values (200 draws) are in curly brackets. Special sessions and a short session (147) are excluded. For lead and lag specifications, sessions are also dropped where lead/lag would be a special/short session or a session in a different parliamentary term. All regressions include Session × Corner FE. Outcome Mean and S.D. in all columns are based on the full sample (the sample included in the contemporaneous regressions in Columns 1-2). *** p<0.01, ** p<0.05, * p<0.1.

Table 4: General Effects on Bipartisan Voting

	Contemporaneous Effect (t)		One Year Later (t+1)		Previous Year (Placebo) (t-1)	
	Leader Non-Compliance	Leader Difference	Leader Non-Compliance	Leader Difference	Leader Non-Compliance	Leader Difference
	(1)	(2)	(3)	(4)	(5)	(6)
Proportion Other-Party Neighbor	-.0046	-.0052	-.021	-.021	-.01	-.013
	(.013)	(.013)	(.017)	(.018)	(.012)	(.013)
	[.72]	[.69]	[.23]	[.24]	[.38]	[.34]
	{.76}	{.71}	{.23}	{.23}	{.47}	{.4}
Session × Party × Strata FE	Y	Y	Y	Y	Y	Y
Observations	1364	1364	906	906	908	908
Outcome Mean	.32	.33	.32	.33	.32	.33
Outcome S.d.	.17	.18	.17	.18	.17	.18

Notes: Each column in this table originates from a separate linear regression. Leader Non-Compliance is the proportion of times the MP votes differently from their party leader in a given session. Leader Difference is the average vote difference score between the MP and their party leader. MP-clustered standard errors are in parentheses and p-values are in square brackets. Randomization inference p-values (200 draws) are in curly brackets. Special sessions and a short session (147) are excluded. For lead and lag specifications, sessions are also dropped where lead/lag would be a special/short session or a session in a different parliamentary term. Proportion Other-Party Neighbor is the proportion of left-right seating neighbors from a different party. All regressions include Session × Party × Strata FE. Strata FE is a dummy variable for whether MP was pre-assigned a seat. Outcome Mean and S.D. in all columns are based on the full sample (the sample included in the contemporaneous regressions in Columns 1-2). *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Pair-level Effects on Co-Sponsorship Links

	Contemporaneous Effect (t)		One Year Later (t+1)		Previous Year (Placebo) (t-1)	
	Raw Number	Inverse Hyperbolic Sine	Raw Number	Inverse Hyperbolic Sine	Raw Number	Inverse Hyperbolic Sine
	(1)	(2)	(3)	(4)	(5)	(6)
Neighbor × Different Party	-.037	-.013	.07	.023	-.025	.016
(proximity effect on bipartisanship)	[.65]	[.6]	[.5]	[.52]	[.76]	[.59]
	{.56}	{.55}	{.39}	{.4}	{.74}	{.6}
Neighbor × Same Party	-.24	-.022	-.37	-.093	-.43	-.055
	[.22]	[.52]	[.1]	[.011]**	[.1]	[.22]
	{.18}	{.57}	{.15}	{.05}*	{.1}	{.32}
Same = Different	[.34]	[.83]	[.088]*	[.019]**	[.15]	[.23]
	{.32}	{.9}	{.12}	{.08}*	{.14}	{.29}
Session × Party Pair × Strata FE	Y	Y	Y	Y	Y	Y
Observations	35314	35314	23265	23265	23472	23472
Outcome Mean	3.3	1.3	3.3	1.3	3.3	1.3
Outcome S.d.	4.8	1.1	4.8	1.1	4.8	1.1

Notes: Each column in this table originates from a separate linear regression. Raw Number is the total number of co-sponsorship links between the two MPs in a pair in a given session. Inverse Hyperbolic Sine is the inverse hyperbolic sine transformation of Raw Number. Neighbor is a dummy variable equal to one if the MPs in the pair are randomly assigned to sit next to each other during that session. Same Party is equal to one if both MPs in the pair are in the same party for that session. Dyadic-robust p-values are in square brackets. Randomization inference p-values (200 draws) are in curly brackets. Special sessions and a short session (147) are excluded. For lead and lag specifications, sessions are also dropped where lead/lag would be a special/short session or a session in a different parliamentary term. All regressions include Session × Party Pair × Strata FE. Strata FE are dummy variables for whether both MPs in a pair were pre-assigned seats, one MP in a pair was pre-assigned a seat, or neither MP in a pair was pre-assigned a seat. Outcome Mean and S.D. in all columns are based on the full sample (the sample included in the contemporaneous regressions in Columns 1-2). *** p<0.01, ** p<0.05, * p<0.1.

Table 6: Pair-level Effects on Co-Sponsorship Links with Undivided Attention

	Contemporaneous Effect (t)		One Year Later (t+1)		Previous Year (Placebo) (t-1)	
	Raw Number	Inverse Hyperbolic Sine	Raw Number	Inverse Hyperbolic Sine	Raw Number	Inverse Hyperbolic Sine
	(1)	(2)	(3)	(4)	(5)	(6)
Neighbor × Middle	-.0085	-.0071	.043	.011	-.0051	.0055
	[.92]	[.8]	[.71]	[.78]	[.95]	[.89]
	{.91}	{.77}	{.72}	{.73}	{.94}	{.89}
Neighbor × Corner	-.07	.0015	.29	.089	-.14	.038
	[.61]	[.98]	[.075]*	[.12]	[.48]	[.6]
	{.69}	{.97}	{.12}	{.13}	{.45}	{.59}
Middle = Corner	[.62]	[.87]	[.071]*	[.16]	[.55]	[.7]
	{.71}	{.88}	{.28}	{.25}	{.52}	{.72}
Session × Corner FE	Y	Y	Y	Y	Y	Y
Session × Party Pair FE	Y	Y	Y	Y	Y	Y
Observations	22687	22687	15172	15172	15130	15130
Outcome Mean	1.9	.98	1.9	.98	1.9	.98
Outcome S.d.	2.7	.94	2.7	.94	2.7	.94

Notes: Each column in this table originates from a separate linear regression. Raw Number is the total number of co-sponsorship links between the two MPs in a pair in a given session. Inverse Hyperbolic Sine is the inverse hyperbolic sine transformation of Raw Number. Neighbor is a dummy variable equal to one if the MPs in the pair are randomly assigned to sit next to each other during that session. Corner is equal to one if at least one MP in a pair has only one seating neighbor. Middle is equal to one minus Corner. Regressions include different-party dyads only, with neither MP pre-assigned seats. Dyadic-robust p-values are in square brackets. Randomization inference p-values (200 draws) are in curly brackets. Special sessions and a short session (147) are excluded. For lead and lag specifications, sessions are also dropped where lead/lag would be a special/short session or a session in a different parliamentary term. All regressions include Session × Corner FE. Outcome Mean and S.D. in all columns are based on the full sample (the sample included in the contemporaneous regressions in Columns 1-2). *** p<0.01, ** p<0.05, * p<0.1.

Table 7: General Effects on Bipartisan Co-Sponsorship Links

	Contemporaneous Effect (t)		One Year Later (t+1)		Previous Year (Placebo) (t-1)	
	Raw Number	Inverse Hyperbolic Sine	Raw Number	Inverse Hyperbolic Sine	Raw Number	Inverse Hyperbolic Sine
	(1)	(2)	(3)	(4)	(5)	(6)
Proportion Other-Party Neighbor	1.4	.055	10	.19	4.5	.11
	(3.6)	(.068)	(4.7)	(.12)	(3.8)	(.086)
	[.69]	[.42]	[.035]**	[.12]	[.24]	[.21]
	{.74}	{.48}	{.05}*	{.12}	{.3}	{.28}
Session × Party × Strata FE	Y	Y	Y	Y	Y	Y
Observations	1420	1420	941	941	946	946
Outcome Mean	82	4.7	82	4.7	82	4.7
Outcome S.d.	76	1.1	76	1.1	76	1.1

Notes: Each column in this table originates from a separate linear regression. Raw Number is the total number of co-sponsorship links between the MP and any other-party MP in a given session. Inverse Hyperbolic Sine is the inverse hyperbolic sine transformation of Raw Number. Proportion Other-Party Neighbor is the proportion of left-right seating neighbors from a different party. Neighbor is a dummy variable equal to one if the MPs in the pair are randomly assigned to sit next to each other during that session. Same Party is equal to one if both MPs in the pair are in the same party for that session. MP-clustered standard errors are in parentheses and p-values are in square brackets. Randomization inference p-values (200 draws) are in curly brackets. Special sessions and a short session (147) are excluded. For lead and lag specifications, sessions are also dropped where lead/lag would be a special/short session or a session in a different parliamentary term. Proportion Other-Party Neighbor is the proportion of left-right seating neighbors from a different party. All regressions include Session × Party × Strata FE. Strata FE is a dummy variable for whether MP was pre-assigned a seat. Outcome Mean and S.D. in all columns are based on the full sample (the sample included in the contemporaneous regressions in Columns 1-2). *** p<0.01, ** p<0.05, * p<0.1.

A Online Appendix: Tables

Table A1: Pair-level Effects: Voting Similarity without Absenteeism and Abstention

	Pair Compliance	Pair Yes-Yes/No-No		
	All	All	Below 50th	Below 25th
	(1)	(2)	(3)	(4)
	Panel A: Contemporaneous Effect (t)			
Neighbor × Different Party	.0051	.0033	.0056	.0058
(proximity effect on bipartisanship)	[.057]*	[.26]	[.044]**	[.034]**
	{.07}*	{.17}	{.05}*	{.015}**
Neighbor × Same Party	.0036	.0012	-6.2e-06	.0068
	[.57]	[.88]	[1]	[.3]
	{.6}	{.86}	{1}	{.32}
Observations	35259	35259	35205	35102
	Panel B: One Year Later (t+1)			
Neighbor × Different Party	.0008	-.00012	-.004	-.0028
(proximity effect on bipartisanship)	[.86]	[.98]	[.45]	[.54]
	{.81}	{.99}	{.22}	{.34}
Neighbor × Same Party	.011	.0032	.01	-.0048
	[.19]	[.74]	[.35]	[.56]
	{.15}	{.69}	{.23}	{.59}
Observations	21589	21589	21589	21540
	Panel C: Previous Year (Placebo) (t-1)			
Neighbor × Different Party	.0013	.000077	-.00016	-.0021
(proximity effect on bipartisanship)	[.68]	[.98]	[.96]	[.51]
	{.71}	{.99}	{.94}	{.4}
Neighbor × Same Party	.0044	.0033	.0045	.0069
	[.59]	[.73]	[.68]	[.42]
	{.6}	{.7}	{.64}	{.47}
Observations	21638	21638	21638	21638
Session × Strata × Party Pair FE	Y	Y	Y	Y
Outcome Mean	.57	.48	.31	.22
Outcome S.d.	.13	.17	.17	.22

Notes: Each panel shows the estimates from four regressions. Pair Compliance is the proportion of times the two MPs in a pair vote the same way in a given session. Pair Yes-Yes/No-No is the proportion of times the two MPs in a pair both vote yes or both vote no in a given session. Below 50th/25th votes are votes in which the share of MPs voting the modal vote is less than the median/25th percentile among all votes. Neighbor is a dummy variable equal to one if the MPs in the pair are randomly assigned to sit next to each other during that session. Same Party is equal to one if both MPs in the pair are in the same party for that session. Dyadic-robust p-values are in square brackets. Randomization inference p-values (200 draws) are in curly brackets. Special sessions and a short session (147) are excluded. For lead and lag specifications, sessions are also dropped where lead/lag would be a special/short session or a session in a different parliamentary term. All regressions include Session×Strata×Party Pair FE. Strata FE are dummy variables for whether both MPs in a pair were pre-assigned seats, one MP in a pair was pre-assigned a seat, or neither MP in a pair was pre-assigned a seat. Outcome Mean and S.D. in all columns are based on the full sample (the sample included in the Panel A regressions). *** p<0.01, ** p<0.05, * p<0.1.

Table A2: General Effects on Bipartisan Voting by Intensity of Contact

	Contemporaneous Effect (t)		One Year Later (t+1)		Previous Year (Placebo) (t-1)	
	Leader Non-Compliance (1)	Leader Difference (2)	Leader Non-Compliance (3)	Leader Difference (4)	Leader Non-Compliance (5)	Leader Difference (6)
Proportion Other-Party Neighbor = 1/2	-.014	-.013	-.01	-.012	-.02	-.02
	[.32]	[.39]	[.62]	[.55]	[.21]	[.24]
	{.29}	{.35}	{.55}	{.53}	{.24}	{.25}
Proportion Other-Party Neighbor = 1	-.0099	-.0097	-.021	-.022	-.017	-.019
	[.48]	[.49]	[.27]	[.27]	[.23]	[.21]
	{.52}	{.55}	{.27}	{.27}	{.3}	{.3}
Session $\times$ Party $\times$ Strata FE	Y	Y	Y	Y	Y	Y
Observations	1364	1364	906	906	908	908
Outcome Mean	.32	.33	.32	.33	.32	.33
Outcome S.d.	.17	.18	.17	.18	.17	.18

Notes: Each column in this table originates from a separate linear regression. Leader Non-Compliance is the proportion of times the MP votes differently from their party leader in a given session. Leader Difference is the average vote difference score between the MP and their party leader. MP-clustered p-values are in square brackets. Randomization inference p-values (200 draws) are in curly brackets. Special sessions and a short session (147) are excluded. For lead and lag specifications, sessions are also dropped where lead/lag would be a special/short session or a session in a different parliamentary term. Proportion Other-Party Neighbor is the proportion of left-right seating neighbors from a different party. All regressions include Session $\times$ Party $\times$ Strata FE. Strata FE is a dummy variable for whether MP was pre-assigned a seat. Outcome Mean and S.D. in all columns are based on the full sample (the sample included in the contemporaneous regressions in Columns 1-2). *** p<0.01, ** p<0.05, * p<0.1.

Table A3: General Effects on Bipartisan Voting on Contested Votes

	Below 50th Votes		Below 25th Votes	
	Leader Non-Compliance (1)	Leader Difference (2)	Leader Non-Compliance (3)	Leader Difference (4)
	Panel A: Contemporaneous Effect (t)			
Proportion Other-Party Neighbor	-.02	-.022	-.008	-.012
	(.014)	(.015)	(.015)	(.019)
	[.16]	[.15]	[.59]	[.52]
	{.21}	{.21}	{.57}	{.53}
Observations	1362	1362	1359	1359
	Panel B: One Year Later (t+1)			
Proportion Other-Party Neighbor	-.012	-.011	-.019	-.021
	(.021)	(.022)	(.021)	(.026)
	[.58]	[.61]	[.38]	[.41]
	{.55}	{.6}	{.37}	{.41}
Observations	906	906	905	905
	Panel C: Previous Year (Placebo) (t-1)			
Proportion Other-Party Neighbor	-.013	-.017	-.011	-.019
	(.014)	(.015)	(.015)	(.019)
	[.37]	[.25]	[.44]	[.32]
	{.43}	{.32}	{.56}	{.43}
Observations	908	908	908	908
Session×Party×Strata FE	Y	Y	Y	Y
Outcome Mean	.38	.41	.34	.41
Outcome S.d.	.19	.2	.2	.25

Notes: Each panel shows the estimates from four regressions. Below 50th/25th votes are votes in which the share of MPs voting the modal vote is less than the median/25th percentile among all votes. Leader Non-Compliance is the proportion of times the MP votes differently from their party leader in a given session. Leader Difference is the average vote difference score between the MP and their party leader. MP-clustered standard errors are in parentheses and p-values are in square brackets. Randomization inference p-values (200 draws) are in curly brackets. Special sessions and a short session (147) are excluded. For lead and lag specifications, sessions are also dropped where lead/lag would be a special/short session or a session in a different parliamentary term. Proportion Other-Party Neighbor is the proportion of left-right seating neighbors from a different party. All regressions include Session×Party×Strata FE. Strata FE is a dummy variable for whether MP was pre-assigned a seat. Outcome Mean and S.D. in all columns are based on the full sample (the sample included in the Panel A regressions). *** p<0.01, ** p<0.05, * p<0.1.

Table A4: General Effects on Bipartisan Co-Sponsorship Links by Intensity of Contact

	Contemporaneous Effect (t)		One Year Later (t+1)		Previous Year (Placebo) (t-1)	
	Raw Number	Inverse Hyperbolic Sine	Raw Number	Inverse Hyperbolic Sine	Raw Number	Inverse Hyperbolic Sine
	(1)	(2)	(3)	(4)	(5)	(6)
Proportion Other-Party Neighbor = 1/2	5.3	.12	7.5	.041	4.9	-.037
	[.16]	[.16]	[.083]*	[.75]	[.21]	[.66]
	{.14}	{.11}	{.17}	{.78}	{.36}	{.77}
Proportion Other-Party Neighbor = 1	3.5	.095	11	.17	5.7	.068
	[.36]	[.22]	[.013]**	[.19]	[.12]	[.41]
	{.42}	{.23}	{.03}**	{.2}	{.28}	{.59}
Session $\times$ Party $\times$ Strata FE	Y	Y	Y	Y	Y	Y
Observations	1420	1420	941	941	946	946
Outcome Mean	82	4.7	82	4.7	82	4.7
Outcome S.d.	76	1.1	76	1.1	76	1.1

Notes: Each column in this table originates from a separate linear regression. Raw Number is the total number of co-sponsorship links between the MP and any other-party MP in a given session. Inverse Hyperbolic Sine is the inverse hyperbolic sine transformation of Raw Number. MP-clustered p-values are in square brackets. Randomization inference p-values (200 draws) are in curly brackets. Special sessions and a short session (147) are excluded. For lead and lag specifications, sessions are also dropped where lead/lag would be a special/short session or a session in a different parliamentary term. Proportion Other-Party Neighbor is the proportion of left-right seating neighbors from a different party. All regressions include Session $\times$ Party $\times$ Strata FE. Strata FE is a dummy variable for whether MP was pre-assigned a seat. Outcome Mean and S.D. in all columns are based on the full sample (the sample included in the contemporaneous regressions in Columns 1-2). *** p<0.01, ** p<0.05, * p<0.1.

B Data Appendix

In this section, we give further detail about our data sources and construction. Links are to the *Althingi* website unless stated otherwise.

- The link between session numbers and years can be found here.
- MP biographies are scraped from the *Althingi* website's pages, with one example here for Andrés Ingi Jónsson. The data comprises each MP's party, constituency, gender, whether and when the MP was the chair of a parliamentary group, and the MP's ID. We use this biographical data to link with the co-sponsorship and speech data. Where party affiliation is unclear, we supplement this data with bill co-sponsorship data, which can be used to identify an MP's party at a particular point in time. We obtain this data from here.
- Initial seating assignment data for sessions from 1995-96 to 2017-18 is scraped from pages like this. This page shows the assignments for session 2015-2016. For sessions 1991-92 to 1994-95, we collected data from scanned copies of the congressional records, available here. The data contain seat number and MP name. We establish the mapping from seat number to seat location by comparing this data with the images of the end of session seating assignments. We link this seating data with biographical data by matching on MP name.
- Seating at the end of each session can be found here. The images contain each seat's physical location and the name of the MP in each seat. We do not use this for analysis except for comparison to the initial seating assignments.
- Roll-call voting data can be found here. For each vote we have: MP name, MP vote (yes, no, absent, abstention), vote date, and associated bill ID.
- Co-sponsorship data can be found here. For each bill we have: bill name, sponsor ID, name, and party, and co-sponsor IDs, names, and parties.

C Discussion on Saia (2018)

Saia (2018) and this project were conducted independently, but both papers use the same natural experiment, which warrants some discussion. The objective of this Appendix section is twofold. First, although the two papers' aims are different, there is one result that is inconsistent between the two. We provide evidence that the inconsistency is due in part to a misspecification in Saia (2018). Note that this is not to reject all findings in Saia (2018)—the paper has other interesting findings including some data-driven discussions about the US Congress. Second, we demonstrate that randomization inference is a useful tool to verify complex regression specifications. This adds credibility to the regression results discussed in the main sections of this paper.

Saia (2018) finds that when an MP's other-party neighbor votes differently from the MP's party leader's vote, this MP is 30 to 50 percentage points more likely to also vote differently from the party leader's vote. This can be interpreted as evidence of the bipartisan proximity effect on general bipartisan voting. We provide evidence that the main table for this claim in Saia (2018) (Table 4) is misspecified, and the result he finds is driven by a mechanical correlation.

Saia (2018) begins with the following MP-vote-level specification:

$$Non\text{-}compliance_{iv} = \alpha + \beta_1 Divergent\,Peers_{iv} + \varepsilon_{iv}$$

where $Non\text{-}compliance_{iv}$ is a dummy variable that takes the value one when the vote of the focal legislator i in voting procedure v is different from her own party line. Votes and party lines can be: Yes (67% of party lines), Absence (17% of party lines), Abstained (11% of party lines), or No (5% of party lines). $Divergent\,Peers_{iv}$ is the fraction of legislators seated nearby with voting decisions different from the party line of legislator i observed in procedure v. Standard errors are clustered at the MP-session-level. Saia (2018) then instruments for the behavior of peers by using the party lines of peers: i.e., $Divergent\,Peers_{iv}$ is instrumented for using $Divergent\ peers'\ party\,lines_{iv}$—the fraction of peers whose party lines observed in voting procedure v are different from the party line of legislator i. In addition, Saia (2018) shows the key 2SLS coefficient (on $Divergent\,Peers_{iv}$) to be robust to including various sets of fixed effects: MP, Seat, Voting Procedure, Party-by-Session, Peers' Parties $\neq$MP i's party, and MP-by-Topic (see his Table 4).

Our claim is that $Divergent\,Peers_{iv}$ (and indeed the IV $Divergent\ peers'\ party\,lines_{iv}$) is mechanically positively correlated with the dependent variable, $Non\text{-}compliance_{iv}$, and that this will be the case even in the absence of any causal peer effect, and even conditional on the fixed effects and other controls that Saia (2018) includes. To see this, consider a simplified setting. Suppose there are only two possible votes: yes and no, and that no votes are much rarer—10% of votes are nos and 90% of votes are yeses. Suppose that everyone votes randomly (implying that there are no peer effects). In this setting, when i's party leader votes no, 90% of MPs are "divergent," and 90% of each MP's peers (on average), whether seated next to that MP or not, are "divergent." When the party leader instead votes yes, 10% of MPs are "divergent," and 10% of each MP's peers are "divergent." In this simplified setting, the more divergent i's neighbors are, the more likely it is that i's party leader voted no. The more likely it is that i's party leader voted no, the more likely it is that i herself is divergent. It follows that the more divergent i's neighbors are, the more likely it is that i herself is divergent. This correlation is mechanical—working through the effect of having divergent peers on the type of vote of i's party leader.

We demonstrate that this claim is true by showing results from a series of regressions. In Column 1 of Table C1, we first replicate Column 3 of Table 4 in Saia (2018) with the party line data kindly provided by Saia.[17] We get a near-identical result, where the slight difference is likely due to differences in data collection methods and data cleaning procedures.

As evidence of a mechanical correlation, we show in Columns 2-5 of Table C1 that $Divergent\ peers_{iv}$ is predictive of the type of vote of MP i's party leader even conditional on the fixed effects and with the instrument. Furthermore, as shown in Column 6, the estimated 2SLS coefficient on $Divergent\ Peers_{iv}$ becomes less positive after controlling for party leader vote fixed effects (i.e., four dummy variables for whether the party leader votes Yes, No, Absence, Abstain). Finally, the estimated 2SLS coefficient on $Divergent\ Peers_{iv}$ becomes statistically indistinguishable from zero after controlling appropriately for Voting Procedure-by-Party fixed effects (as opposed to just Voting Procedure fixed effects)—since within each Voting Procedure-by-Party cell, there is no longer any variation in the type of vote by the party leader, eliminating the mechanical correlation (though there remains variation in $Divergent\ peers'\ party\ lines_{iv}$).

[17] We choose this column here because it has the highest number of observations and the largest set of fixed effects that we could include. All other columns suffer from the same source of mechanical correlation—Figure C1 gives results of randomization inference for Columns 3 and 6. Note that we do not have the topics of voting procedures in our data, which makes us unable to replicate his Column 4 and 7. This does not affect identification. We follow the same sample selection procedure as in Saia (2018).

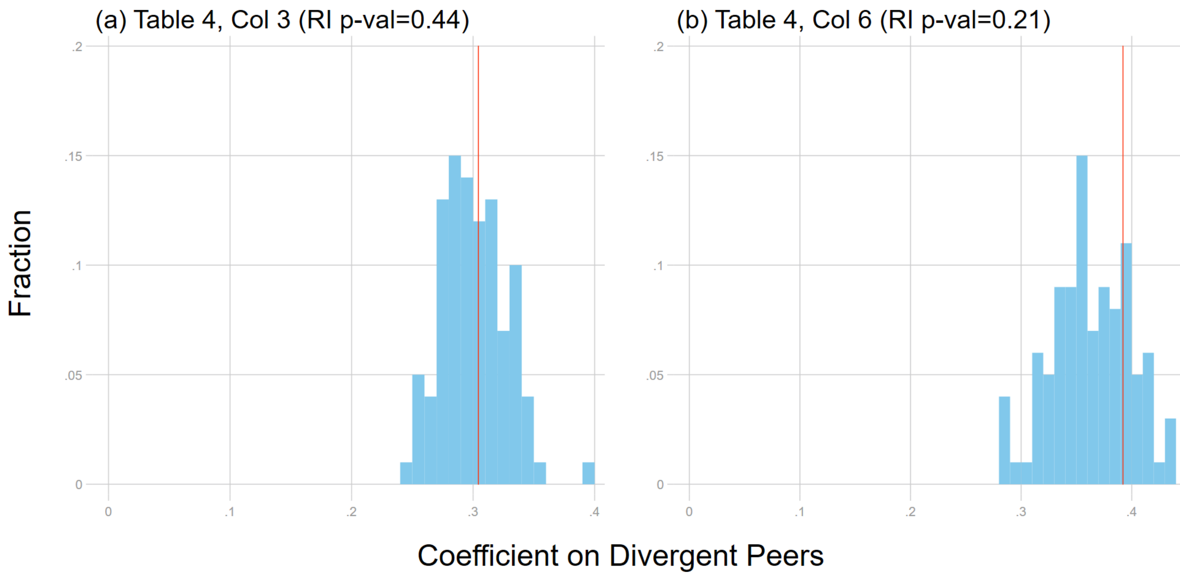
Table C1: Replication of Table 4 in Saia (2018) and raising concerns

	Col.3 replication	Party leader vote				Col.3 with appropriate FEs	
	Non-compl-iance	No	Yes	Abstain	Absent	Non-compl-iance	Non-compl-iance
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Divergent Peers	0.30***	-0.02**	-0.88***	0.30***	0.60***	0.07**	0.05
	(0.03)	(0.01)	(0.04)	(0.02)	(0.03)	(0.03)	(0.04)
MP FEs	Y	Y	Y	Y	Y	Y	Y
Seat FEs	Y	Y	Y	Y	Y	Y	Y
Peers' Parties $\neq$ MP i's party	Y	Y	Y	Y	Y	Y	Y
Voting Procedure FEs	Y	Y	Y	Y	Y	Y	(implicit)
Party $\times$ Session FEs	Y	Y	Y	Y	Y	Y	(implicit)
Party Leader Vote FEs	N	N	N	N	N	YES	(implicit)
Party $\times$ Voting Procedure FEs	N	N	N	N	N	N	YES
MP $\times$ Topic FEs	N	N	N	N	N	N	N
Observations	1064563	1064563	1064563	1064563	1064563	1064563	1053203

Notes: Each column in this table originates from a separate 2SLS regression. Non-compliance is a dummy variable indicating that the MP voted differently from their party leader for the particular voting procedure. In Columns 2-5, the dependent variable is a dummy variable indicating the vote of the MP's party leader. Standard errors are clustered at MP-session-level. *** p<0.01, ** p<0.05, * p<0.1.

In Figure C1, we show that the estimated 2SLS coefficients on $\textit{Divergent Peers}_{iv}$ remain positive in a placebo specification in which we calculate the right-hand-side variables using a counterfactual random draw (100 times) of the seating arrangement. We run specifications equivalent to Columns 3 and 6 of Table 4 in Saia (2018) for each random draw. The histogram of the coefficients on $\textit{Divergent Peers}_{iv}$ from 100 placebo 2SLS regressions are shown in the Figure. Despite the fact that the seating arrangement is artificial and thus we should not get positive results, we get a positive and statistically significant coefficient on $\textit{Divergent Peers}_{iv}$ for both specifications in all 100 draws, confirming the intuition on mechanical correlation. From the randomization inference point of view, the results in Table 4 of Saia (2018) are no longer statistically significant—the p-values from the randomization inference are 0.44 and 0.21.

Figure C1: Randomization inference of Table 4 in Saia (2018) using counterfactual seating



Notes: Histograms report coefficients on *Divergent Peers$_{iv}$* of 2SLS specifications from Columns 3 and 6 of Table 4 in Saia (2018) with counterfactual seating arrangements (100 re-randomizations). Red lines indicate corresponding coefficients on Divergent Peers from the specification using the actual seating arrangement.

This result demonstrates the usefulness of randomization inference as a misspecification check. Throughout the main sections of this paper, we provide p-values from both large-sample inference and randomization inference.