Legislature Integration and Bipartisanship: A Natural Experiment in Iceland

October 8, 2023

Abstract

Nearly all legislatures segregate politicians by party. We use seating lotteries in the Icelandic Parliament to estimate the effects of seating integration on bipartisanship. When two MPs from different parties are randomly assigned to sit together, they are roughly 0.5 to 1 percentage point more likely to vote alike. This limited peer influence is only robust in the case of voting on contested bills. In a survey of past and present MPs, most respondents doubt the possibility of peer influence. Exploring dynamics, we find that neighbor influence is temporary, disappearing the following year. These results support cue-taking and social pressure as likely mechanisms for the small effect of other-party proximity on voting. Finally, we find suggestive evidence that seating proximity builds weak ties, through co-sponsorship, despite the lack of persistent effects on voting.

Politicians in almost all countries are segregated at the workplace. Members of Parliament (MPs) in the UK are seated 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 Iceland, Sweden, and Norway, MPs from different parties sit next to each other. Such seating arrangements may spawn bipartisan friendships, build respect (Caldeira and Patterson 1988; Caldeira et al. 1993), and even change political behaviors (Wahlke et al. 1962; Ringe et al. 2013). The decline of such cross-party social interactions may even lie behind the deepening partisan divide in the US (Haidt 2012; Ripley 2023). Does the integration of politicians increase bipartisanship?

A broad body of work argues that political behaviors are socially determined, and shaped by homophilous interactions in networks (Beck et al. 2002; Bond et al. 2012; Bjarnegård 2013). Recent quantitative work confirms that even legislators are influenced by one another, but almost invariably the evidence is of influence between trusted peers, embedded in homophilic networks (Zelizer 2019; Harmon et al. 2019; Fong 2020). A pressing question then is whether integration can create *cross*-party links between legislators, and in turn, catalyze bipartisanship. This question is challenging to answer: political networks are endogenously formed, making it impossible to credibly estimate the effects of network changes without a source of randomness in who is connected with whom (Fowler et al. 2011; Rogowski and Sinclair 2012). We circumvent this challenge by studying a natural experiment in the Icelandic Parliament (the *Althingi*). The assigned seats of Icelandic MPs are determined by a lottery held each session. This arrangement gives exogenous variation in the party affiliation of the seating neighbors of each MP. We use this variation to provide a cleanly-identified case study on how politicians' voting and co-sponsorship behaviors change during and after sitting next to randomly assigned peers.

Social interactions between lawmakers may affect legislative behaviors through many mechanisms, including (i) cognitive channels such as information transmission and persuasion, (ii) affective changes such as increased partisan tolerance through contact, (iii) legislative cue-taking, and

(iv) social pressure and monitoring. These mechanisms have different implications for whether effects are transitory or persistent. We use this logic to map our results to mechanisms, going beyond a simple description of the causal effects of seating proximity.

In our analysis, we use both MP-pair-session level and MP-session level regression specifications. The use of both specifications is important for two reasons. First, we show theoretically that the results from the two specifications need not coincide – in particular, while cue-taking from cross-party neighbors increases vote similarity at the pair-level, it can reduce or increase party-line voting at the MP-level. Second, we show using simulations that, for a given amount of cue-taking, the pair-level specification has much greater statistical power to reject the null of no peer influence than the MP-level specification. For both specifications, we use our near-complete knowledge of the randomization mechanism to conduct Fisherian exact inference, in addition to large sample approaches (Gerber and Green 2012).

Using data from 1991 to 2018, we find evidence of a small pair-level effect of seating proximity—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 cross-party MPs sat apart. The proximity effect is not driven by low-stakes votes – effects are similar or larger when considering only voting on draft bills, or only votes related to economic management and foreign policy. However, the proximity effect is only robust to multiple hypothesis testing correction and alternative definitions of voting similarity when considering voting on contested bills. In this sense, we find evidence of highly limited influence, and only for a subset of bills.

Considering dynamics, the proximity effect disappears the next year when the two MPs no longer sit next to each other. This result suggests that the causal mechanism on voting outcomes operates only through temporary channels, such as cue-taking or social pressure, and not through more enduring cognitive and affective channels. Providing further evidence on channels, in a survey taken by 14 sitting and past MPs, most doubt the possibility of any peer influence – suggesting that the effects we find are either too small to be detectable and remembered by MPs, or that MPs are not consciously aware of how neighbors affect their voting choices.

We do not find evidence of an effect of cross-party neighbors on bipartisan voting in our MP-session level specification. Our simulations suggest that this null effect is due to a lack of statistical power, rather than the theoretical argument that pair- and individual-level effects need not coincide. On the other hand, we find suggestive evidence of a long-term effect of outparty exposure on bipartisan co-sponsorship links, an indicator of weak social ties and interest overlap between legislators (Kessler and Krehbiel 1996; Fowler 2006; Ringe et al. 2017). There were 10 (19%) more bipartisan co-sponsorship links for MPs who sat next to other-party MPs, measured the next year when the MPs are no longer sitting together. This result should however be taken with caution – it does not survive a multiple hypothesis testing correction (sharpened q-values are 0.16 and 0.21), and thus we consider it more exploratory.

Overall, seating integration has highly limited effects: it has small transitory effects on voting similarity and suggestive enduring effects on co-sponsorship ties. Of course, even in the absence of enduring effects on voting, a more bipartisan co-sponsorship network might open the possibility of mutually beneficial compromises and avoidance of legislative gridlock, perhaps at political stages preceding roll-call votes. Our exploratory evidence of effects on co-sponsorship ties then merits future research to establish whether our estimated effects are real or merely false-positives.

Our paper contributes primarily to work on legislative cue-taking. First, rather than take an ex ante stance on cue-taking, we use the dynamics of effects to distinguish between different channels of social influence, and we use theory and simulations to demonstrate the importance of considering effects at both the pair- and the individual-level. Second, pushing boundary conditions, we estimate effects in a parliamentary setting with strong parties, while almost all existing work is in presidential settings. Harmon et al. (2019) provide one exception. Using the quasi-random variation in proximity from alphabetical seating, they find that same-party Members of the European Parliament (MEPs) that sit together are 0.6 percentage points more likely to vote alike.[1] Third, we estimate influence between random *cross*-party peers. Without this feature, we would learn nothing about the relationship between integrative policies and bipartisanship. In studying cross-

[1] Since MEPs sit in party groups, only 0.02% of the pairs comprise MPs from different parties. Given this, Harmon et al. (2019) can only estimate very imprecise effects on cross-party pairs.

party influence, we build on Fong (2020), who finds cross-party cue-taking between already-linked legislators, and in studying exogenous networks, we build on Rogowski and Sinclair (2012), who find null effects of office location proximity in the US House of Representatives. Fourth, we use a short survey of past and present MPs to provide qualitative evidence on the possibility and type of peer influence. Finally, in a paper written contemporaneously with ours, Saia (2018) conducts MP-level analysis using the same Icelandic experiment but does not study persistence, effects on co-sponsorship, survey MPs, or distinguish between the mechanisms that we have outlined above.[2]

1 Icelandic Politics and the Seating Lottery

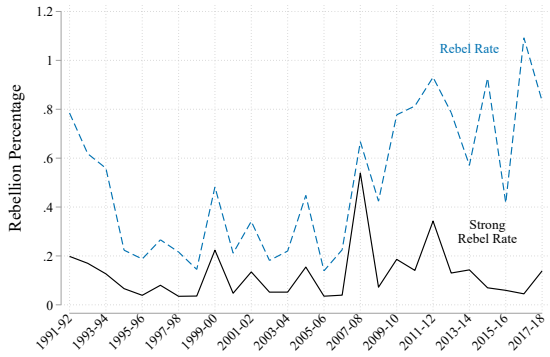
Iceland's Political System. Like the other Nordic countries, Iceland has a unicameral parliamentary democracy with a multi-party system. A total of 63 MPs are elected by proportional representation every four years. The head of state is the president, a position with only limited powers.[3] The head of the executive branch is the prime minister. Like Finland, but unlike the other Nordic countries, majority (and sometimes ideologically diverse) coalitions dominate Icelandic politics (Hansen 2017). These majority coalitions have been argued to be a consequence of Iceland's clientelistic practices (Indridason 2005), and of the president's cabinet-appointing powers, with the resultant threat of the appointment of a non-partisan government (Kristjánsson and Indridason 2011).

Legislating follows the spirit of majoritarian democracies more so than that of the other Nordic (more consensual) democracies (Jónsson 2014). Cabinets pass legislation by disciplining the coalition's parliamentary parties, rather than through reaching compromises with opposition parties (Kristjánsson and Indridason 2011). Party cohesion in the *Althingi* is high (Jensen 2000; Kristinsson 2011), with MPs dissenting from the vote of their party leader as rarely as in other Northern

[2]Different to us, 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 these large MP-level effects on bipartisanship can be attributed to a regression misspecification. See Appendix D for a full discussion.

[3]Finland and Iceland both have semi-presidential systems, whereas the other Nordic countries (Denmark, Norway, and Sweden) are constitutional monarchies. The role of the president in Iceland has been debated in more recent years, particularly after the first ever use of the presidential veto in 2004. Nevertheless, Kristjánsson and Indridason (2011) write that, despite Iceland's semi-presidential constitution, "the system functions in practice much like a parliamentary system characterized by a high degree of "partyness"."

Figure 1: *Althingi* MPs Rarely Dissent From the Party Line



Notes: Rebel Rate is the percentage of votes for which an MP votes yes or abstain when their party leader votes no, or for which an MP votes no or abstain when their party leader votes yes. *Strong Rebel Rate* is the percentage of votes for which an MP votes yes when their party leader votes no, or for which an MP votes no when their party leader votes yes. The figure shows the average of each measure for each regular session from 1991-92 to 2017-18, excluding party leaders, ministers, the speaker, and any MP-session observations where the MP's party does not have a party leader.

European parliaments (Kristinsson 2011; Figure 1). Furthermore, legislative productivity has been relatively stable since 1991-92 (Figure A1), though with some gridlock during periods of weak coalitions.[4] Finally, parties are substantially more polarized along the left-right dimension than those of the UK and the US, while slightly more polarized when compared with the other Nordic countries (Bengtsson et al. 2013, p. 30). Collectively, Iceland's parliamentary system provides a relatively demanding setting for a test of cross-party social influence.

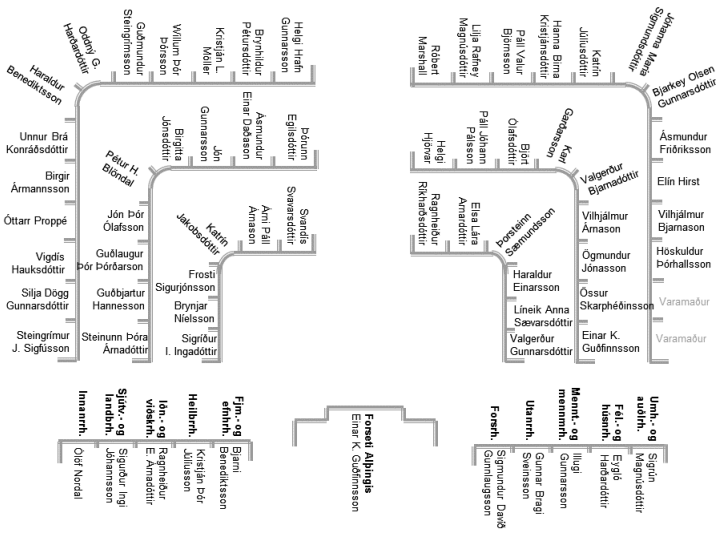
Seating. Iceland is the only national parliament with seats assigned by lottery. This custom was established in 1916 when parties were weak, but has been kept and broadly supported since, despite today's strong parties (Magnússon 2014).[5] At the beginning of each session, each MP draws a ball from a box (Figure A2). The ball indicates the designated seat of the MP for the session.[6] Some MPs are exempt from the random draw: the prime minister, speaker, ministers, and chairs

[4]Most notably, the four-year period following the 2008 economic crisis in which the Left-Green-Social Democrat coalition held power. With a radical agenda, the coalition struggled to legislate, and became essentially a minority government.

[5]Similar examples exist in history: from 1845 to 1913 the US House of Representatives held a lottery for seating, and the Philippine Assembly had random seating arrangements in the lower chamber from 1907 to 1988 (Magnússon 2014). 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.

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

Figure 2: Seating Assignment for 2014-15



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

of parliamentary groups have their own designated seats.[7] 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.

On rare occasions, the seating assignment can change during a session. A typical case is when a chair of a parliamentary group becomes a 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 seating.

[7]Chairs of parliamentary groups are assigned aisle seats, for easier access to the podium. Though this custom has been present throughout our analysis period, it was only formalized and recorded since 2004-05. 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 those seats. 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 since 2004-05. In addition, our balance and placebo checks, described below, give evidence against selection concerns.

Treatment Intensity. The average total length of a regular parliamentary session (1992-93 to 2017-18) is 670 hours, excluding committee meetings where MPs are not expected to sit at their designated seats. 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.[8] While contact on a given day is low, over the course of a parliamentary session, this adds up to many hours of contact.

Expert Survey. For qualitative descriptions of interactions between seating neighbors, we contacted 64 sitting MPs and 36 ex-MPs with a three-question survey. 12 sitting MPs and 2 ex-MPs gave answers (full details and complete anonymized answers in Appendix B). The first of the three questions asked about the nature of social interactions between seating neighbors. Several themes emerge in the responses. First, several respondents note that interactions with seating neighbors are limited (MPs 7, 8, ex-MP 14); MPs interact with neighbors on voting days, though more time is spent in committee meetings than voting, and few MPs sit in the assembly for discussions. Second, interactions that do occur tend to be positive, even when across party lines: whether jokes and small talk (MPs 2, 4, 6, 9, 10, 12, ex-MP 13), positive feedback after a seating neighbor gives a speech or asks a good question (MP 6), or practical help to keep up with the voting procedures (MP 12 and ex-MP 14). MP 10 states explicitly that "I feel little difference whether my neighbors are from 'friendly' parties or not," and only one respondent mentions "political trash-talk" (ex-MP 14). Third, respondents give a range of opinions on whether seating neighbors become friends. For some, the seating arrangement is not a basis for friendship (MPs 1 and 11), with friendships more commonly built from communications outside the chamber (MP 1). While for others, seating proximity helps an MP get to know someone they otherwise would not know, like cross-party MPs (MP 2). Supporting this, MP 6 describes the consequences of such contact: "last year, I did have one coalition member sitting next to me and since we also shared a committee we got to know each other much better – and that then also led to us working closer on finding common grounds on

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

some bills being discussed in the committee." MP 6 goes on to say "I am sure some people become good friends, but I think it is more common that people at least become a bit closer – which then enables them to do better discussions with each other outside of the chamber."

Summarizing the views of the experts, interaction between seating neighbors is concentrated on voting days, tends to be positive, even for cross-party MPs, and sometimes creates and strengthens friendships between MPs.

2 Mechanisms of Influence and Lawmaker Behaviors

2.1 Mechanisms

In this section we introduce four mechanisms through which social interactions between lawmakers can shape behavior, and distinguish between mechanisms that predict persistent effects and those that do not. We pay special attention to legislative cue-taking, the most prominent mechanism in work on social influence among legislators.

Cognitive. Social interactions with fellow lawmakers may involve informal deliberations about political issues. Through the process of deliberation, lawmakers may reflect on their own opinions, become aware of the reasoning behind the opinions of others, and be persuaded to change their beliefs (Habermas 1991; Mutz 2002). These cognitive changes are not entirely situational and, therefore, can have a lasting impact even after social interaction has ended.

Affective. 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 work on the "contact hypothesis"—the idea that interpersonal contact with outgroups can reduce prejudice under certain conditions (Allport 1954; Paluck et al. 2018; Lowe 2021). Like cognitive mechanisms, affective mechanisms imply lasting impacts on a lawmaker's behaviors.

Cue-Taking. Lawmakers are not fully informed about all issues, and so they may take cues from other lawmakers (Matthews and Stimson 1975). Such informational shortcuts are most helpful when legislators are overloaded with decisions. Fitting this description, *Althingi* MPs cast an average of 1,347 votes per session from 1991-92 to 2017-18, with 58% of these votes taken on days with at least 50 votes (Figure A3).

Two main approaches exist for the empirical identification of legislative cue-taking. First, we can take as given a pre-existing social network, and observe whether the vote or co-sponsorship decision of a legislator changes in response to an exogenous shock to the information or expertise of a different connected legislator. The two legislators might be linked through sharing the same office (Zelizer 2019) or through co-sponsorship of each other's bills (Fong 2020). Zelizer (2019) finds compelling evidence of cue-taking using this approach. In his setting, co-sponsorship decisions respond to a randomized technical policy briefing, and respond nearly as strongly when a legislator's officemate receives the briefing.

The second approach exploits exogenous shocks to the social networks of politicians, and explores whether the decision-making of two legislators becomes more similar after they become connected, perhaps through random assignment to nearby offices (Rogowski and Sinclair 2012) or through assignment to nearby seats in the legislative chamber (Masket 2008; Saia 2018; Harmon et al. 2019). Our paper takes this second approach, which differs in at least two important ways from the first. First, it is not as obvious that legislators would take cues from *random* peers, rather than those they intentionally chose to be networked with. The second approach then entails a more demanding test of cue-taking. Second, if existing networks are homophilic, the first approach cannot answer the question of whether cue-taking can be used as a means to reduce polarization—for this we need a means of testing for social influence between groups that are not currently in the same network.

With the second approach, we might reasonably ask: would we even expect cue-taking between random cross-party peers? Arguments can be made in both directions. Legislators are less trusting

of and less ideologically aligned with peers from other parties. The lack of trust should reduce the likelihood of cue-taking, while the lack of ideological alignment could even lead to negative cue-taking (Ringe et al. 2013). But legislators are likely to observe cues from copartisan legislators whether or not they sit next to them—information diffuses more easily within than across networks. Given this, the effect of seating proximity on similarity in decision-making may actually be *larger* for pairs of legislators that would not otherwise interact,[9] provided some minimum level of cross-party consensus exists. We find support for this idea below by estimating effects separately for periods before and after a disruption to cross-party consensus—the Icelandic economic crisis. Mapping to predictions, we expect legislative cue-taking to only have transitory effects, unlike cognitive and affective mechanisms.

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 to avoid stigma or conflict, and for the hedonic value of having a good relationship with neighbors. This possibility of social pressure has not been discussed widely in legislative contexts, but appears in other contexts, such as voter turnout (Gerber et al. 2008), and in polling, where social pressure can explain interviewer effects (West and Blom 2017). Social pressure from a neighbor and cue-taking have similar empirical implications: both have effects only when social interactions are happening, and not once they have ended.

2.2 Measures of Legislator Behavior

We analyze two formal political behaviors—roll-call votes and co-sponsorship.[10] We treat voting as the main measure of lawmakers' revealed preferences, as in a large body of existing work (Clin-

[9] A similar story might explain the finding of Fong (2020) of more cue-taking between cross-party than same-party pairs of legislators.

[10] One weakness of this focus is that we cannot speak to social influence over more informal practices – like the insider favoritism central to Iceland's bank privatization of 2001-3 and resultant economic crisis of 2008 (SIC 2010; Viken 2011; Wade and Sigurgeirsdottir 2011). Nevertheless, our approach has an important advantage: by focussing on behaviors that are pervasive across democracies, our findings can be more easily compared to existing studies, and are more generalizable to contexts as yet unstudied.

ton et al. 2004; Poole and Rosenthal 2011). Therefore, voting is used to distinguish between the mechanisms outlined above.

While co-sponsorship is considered a proxy for social connectedness in American politics (Fowler 2006), we know of no scholarship that explores whether co-sponsorship ties in Iceland imply social connection. To make progress, we included a question on the nature of co-sponsorship in our survey of past and present MPs (full anonymized responses in Appendix B). The responses establish that co-sponsors only rarely work closely together on the bill in question, with the sponsoring MP more commonly emailing other MPs (sometimes all MPs), or talking in the halls to ask them to join the bill (MP 10, ex-MP 13). In these cases, the co-sponsoring MP will read the legislation, and sometimes suggest changes (MP 8). However, several MPs note exceptions. MP 6 notes that like-minded MPs will sometimes work together on bills related to topics they are passionate about. Ex-MP 14 notes that co-sponsors will work closely on bills which are likely to be highly debated. The same respondent also notes that co-sponsorships provide "some indications on who is friends with whom in Parliament," given that there is weaker party discipline with co-sponsorship than with voting. In light of the experts' opinion, we consider co-sponsorship a measure of only weak social ties (i.e. through emails and informal conversations), and a measure of similarity in interests, less constrained by party discipline than voting.

3 Data and Specification

3.1 Data Description

We compiled data on initial seating assignments, voting, and co-sponsorship for all regular sessions from 1991-92 to 2017-18.[11] We describe the main features of the data in this section, with further details on data sources in Appendix C.

Seating and MP Demographics. We collected data on annual initial seating assignments from the parliamentary records ("*Althingi* journals"). For sessions from 1995-96 to 2017-18, we web-

[11] 1991-92 is the first regular session for which the seating assignment is available.

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. 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 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.[12] A vote can be in one of four categories: yes, absent, abstain, or no.[13] 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.

A limitation of our *Leader Non-Compliance* measure is that absence from a vote might not reflect position-taking—legitimate reasons exist for absence, and we cannot systematically distinguish between legitimate and position-taking absences (Kam 2009, p. 95). We address this concern with our second voting outcome, *Rebel Rate*, which is the proportion of times the MP voted yes or abstain when the party leader voted no, or voted no or abstain when the party leader voted yes, again weighted by bill. This type of dissent is not a function of absence, and happens only infrequently (recall Figure 1). Both MP-session-level outcomes are set to missing for the party leaders themselves and for those without party leaders (e.g. Independents).

We also construct two 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 first pair-level measure is *Compliance*, which is the proportion of times the two MPs in a pair vote the same way, mirroring *Leader Non-Compliance.* Our second pair-level measure is *Similarity*,

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

[13] 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. Two types of absence are recorded: "fjarvist", meaning that the absence was reported to the secretary in advance, and "fjarverandi", meaning that the absence was not reported. We group these two types of absences since, given that legislative calendars are known in advance, both types of absences can reflect the same type of position on an issue—i.e. not wanting to go on record as either a supporter or opposer.

and aims to capture the idea that pairs of MPs that vote yes-abstain or yes-absent are more similar than pairs of MPs that vote yes-no. 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 two MPs in a pair vote identically (i.e. yes-yes, absent-absent, abstention-abstention, or no-no), they score three, while if one votes yes and the other votes no, they score zero, with other combinations in between. To again address the concern that absence might not reflect positions, we consider alternative versions of *Compliance* which do not count both MPs being absent as the two voting the same way.

Co-Sponsorship. We web-scraped co-sponsorship data from the *Althingi* website, covering bills, resolutions, and reports. 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 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

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

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

This specification stacks one cross-section per session, pooling all session-level experiments. 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. y_{abt} is one of our measures of similarity between MPs a and b during session t. 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.[14] 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 Different Party$_{abt} = 1 -$ Same Party$_{abt}$.[15] $\alpha_{p(a)p(b)st}$ 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.

For each session, there are three strata. The first strata equals one when both MPs in the pair were pre-assigned seats. For these pairs it is always the case that 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. We include pre-assigned MPs since, from their perspective, the MP assigned to sit next to them was chosen randomly. Together with the MPs subject to the lottery, we are left with 53 analysis sample MPs for the median session.

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 effectively compares an outcome at the pair-level (e.g. voting similarity) between a pair of different-party MPs that are seated together with a pair of different-party MPs that are sat apart. If seating neighbors influence one another's behaviors (with influence potentially going in both directions), their behavior converges, leading to $\gamma_2 > 0$.[16]

[14] We take a particular stance on the relevant network for spillovers—we assume they exist only between left-right seating neighbors. Given the seating map (Figure 2), we find this assumption plausible. Nevertheless, we also test for and reject the possibility of the most obvious alternative spillover—between front-back seating neighbors—in Section 4.1.

[15] Note that the non-interacted variable Same Party$_{abt}$ is not shown as a separate control because it is fully absorbed by the session-by-strata-by-party pair fixed effects.

[16] To build intuition with a simplified example, suppose that different-party MPs a_1 (from party a) and b_1 (from party b) are sat together, while MP b_2 is sat apart from a_1. a_1 and b_1 have voting similarity p_{11} while a_1 and b_2 have voting similarity p_{12}. If a_1 influences b_1 towards their vote, p_{11} increases, while p_{12} does not, creating a force for $\gamma_2 > 0$. If b_1 influences a_1, p_{11} again increases, but in this case, p_{12} potentially increases too – if a_1's behavior converges

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. Given Iceland's coalitional politics, we also estimate heterogeneity by coalition, replacing Same Party$_{abt}$ with Same Coalition$_{abt}$, a dummy variable equal to one if MPs a and b belong to the same "coalition"—either both in government, or both in opposition.

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.[17]

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 inference, we simulate placebo seating assignments by following the *Althingi*'s exact procedure for assigning seating. 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).

towards b_1's, their behavior may somewhat converge toward b_2's as well. If b_1 and b_2 behave identically (e.g. because of strong party discipline), p_{11} and p_{12} would increase the same amount, leading to $\gamma_2 = 0$. In practice, however, the behavior of b_1 and b_2 is not perfectly aligned, particularly when considering abstentions and absences in voting, or when considering any co-sponsorship outcome. Given this, when b_1 influences a_1, we still have a force pushing toward $\gamma_2 > 0$. For a formal description of these points, see the model in Appendix E.

[17] 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.

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:

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

To account for multiple hypothesis testing, we use our dyadic-robust p-values to calculate sharpened q-values (Anderson 2008). By using the q-values for hypothesis testing we can control the false discovery rate, which is the expected proportion of rejections that are type I errors. We report q-values for all non-placebo tests of key coefficients in our main tables.

MP-Session-Level Specification. To estimate effects of cross-party proximity on party discipline, we use the following 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 rebellion outcome for MP i during session t, while 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. To estimate cross-coalition effects, we estimate some specifications with Proportion Other Coalition Neighbor$_{it}$ instead as the key right-hand-side variable.

α_{pst} are session-by-party-by-strata fixed effects. Party fixed effects increase precision and are necessary for identification—since the likelihood of being exposed to other-party seating neighbors depends on how many other members of your own party are also being assigned seats. The strata fixed effect is also necessary for identification. This fixed effect is a dummy variable for whether MP i was pre-assigned a seat during session t as opposed to having participated in the seating lottery. The estimates then only come from within-strata variation—i.e. we do not make comparisons

between the voting of regular MPs and the voting of chairs of parliamentary groups.

β is our parameter of interest, capturing the effect of having all versus no other-party neighbors on MP-level 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. MP-clustered standard errors account for the fact that a given MP will regularly appear in multiple cross-sections since MPs usually serve for more than one session. As described above, we also report sharpened q-values in our main tables.

To test for persistent effects we again 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.

Balance. As a check on the randomization, we test for covariate balance by running specifications 1 and 3 above with pre-determined variables on the left-hand-side, including those related to gender, experience, constituency, and previous exposure to other-party seating neighbors. With both approaches to inference, two of 27 coefficients are statistically significant at the 10% level (Tables A1, A2), consistent with our specifications correctly isolating the random variation created by the lottery. Balance checks are also similar for the cross-coalition specifications (Tables A3, A4).

Linking Pair-Level and MP-Level Effects. Intuitively, we might think that cross-party influence that increases pair-level voting similarity ($\hat{\gamma}_2 > 0$) must also increase voting rebellion ($\hat{\beta} > 0$). This is not the case for two reasons. First, theoretically, a positive pair-level effect can coincide with effects on party-line voting of either sign (full model and proofs in Appendix E). In particular, suppose a simple case with two parties with different party lines, two vote options (yes and no), one neighbor for each MP, and a probability of defying the party line (in the absence of peer influence) equal to r. When an MP and their different-party neighbor are planning to vote alike, there is no scope for peer influence. When each is planning to vote the party line, each MP can influence the other to switch their vote with probability p_l. When each is planning to rebel, each can influence the other to switch their vote with probability p_r.

In this stylized model, the pair-level effect of proximity γ_2 is weakly positive, and increasing in both p_l and p_r (Proposition 2, Appendix E). In contrast, the effect of having a different-party

neighbor on defiance of the party-line is $\beta = p_l(1-r)^2 - p_r r^2$ (Proposition 1, Appendix E) – this individual-level effect can be of either sign, with p_l and p_r now having opposite effects on rebellion. If cross-party neighbors are only persuasive when they are defying the party-line ($p_r > 0 = p_l$), cross-party exposure *reduces* rebellion. If cross-party neighbors are only persuasive when they follow the party-line ($p_l > 0 = p_r$), exposure *increases* rebellion. In principle, we may even intuit that $p_r > p_l$, since there is more information value in a cross-party neighbor's rebelling vote than in their obedient vote.[18] Somewhat counterintuitively, this shows that there is a force by which cross-party cue-taking can actually facilitate party discipline.[19] However, the role of p_r and p_l is mediated by the frequency of rebellion – even if $p_r > p_l$, outparty exposure will tend to increase rebellion when $p_l > 0$ and the rebellion rate r is low. This reflects the Icelandic case, where party discipline is high (Figure 1).

A second reason for a disconnect between the estimated pair-level and individual-level effects is statistical: using simulations, we show that for a given level of peer influence, we have far more statistical power to reject the null hypothesis of no effect with the pair-level specification than with the individual-level specification (Appendix E). As we elaborate further below, our null effects on rebellious voting at the individual-level are likely due to this limitation of power.

4 Results

4.1 Pair-Specific Effects on Voting

MPs from different parties vote 0.5 percentage points (RI p-value = 0.06) 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 = 0.009) higher (Column 2). The former effect is sensitive to correcting for multiple hypothesis testing ($q = 0.26$), while the latter is more robust ($q = 0.038$).

[18] Empirical evidence from Chiang and Knight (2011) supports this idea. They find that endorsements of Democratic candidates are more influential when coming from neutral or right-leaning newspapers than when coming from left-leaning newspapers (and similar, but reversed, for Republican candidates).

[19] We thank an anonymous referee for suggesting this line of reasoning.

Table 1: Pair-Level Effects on Voting

	Contemporaneous Effect (t)		One Year Later (t+1)		Previous Year (Placebo) (t-1)	
	Compliance (1)	Similarity (2)	Compliance (3)	Similarity (4)	Compliance (5)	Similarity (6)
Neighbor × Different Party	.0051	.0071	.0008	.000057	.0013	.0017
(proximity effect on bipartisanship)	[.057]*	[.0047]***	[.86]	[.99]	[.68]	[.59]
	{.057}*	{.009}***	{.81}	{.99}	{.73}	{.64}
Neighbor × Same Party	.0036	.0037	.011	.0099	.0044	.0027
	[.57]	[.57]	[.19]	[.28]	[.59]	[.75]
	{.58}	{.57}	{.13}	{.19}	{.57}	{.71}
Same = Different	[.82]	[.61]	[.32]	[.35]	[.74]	[.92]
	{.84}	{.65}	{.26}	{.28}	{.74}	{.91}
Observations	35259	35259	21589	21589	21638	21638
Same Party Dummy	Y	Y	Y	Y	Y	Y
Session × Party Pair × Strata FE	Y	Y	Y	Y	Y	Y
Outcome Mean	.57	2.5	.55	2.5	.57	2.5
Outcome S.d.	.13	.17	.12	.16	.12	.16

Notes: Compliance is the proportion of times the two MPs in a pair vote the same way in a given session. 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. Dyadic-robust p-values are in square brackets. Randomization inference p-values (1000 draws) are in curly brackets. Special sessions and a short session (2017) 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. 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. *** p<0.01, ** p<0.05, * p<0.1.

Our focus on left-right spillovers appears reasonable—front-back seating neighbors are no more likely to vote alike, nor does allowing for front-back spillovers affect our left-right estimates (Table A5).

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 (Columns 3-4, Table 1). Placebo coefficients are statistically insignificant (Columns 5-6), ruling out concerns of chance imbalances.

For certain votes, different parties vote similarly, reducing the scope for cross-party influence. To address this, we recreate the two voting outcomes used in Table 1 using only data from the more contested votes. Specifically, for each vote we identify the modal vote choice 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. Proximity effects are stronger for these contested votes (Table 2), with different-party pairs roughly one percentage point more likely to vote similarly (Panel A), different-party proximity p-values all weakly below 0.01, and three of four different-party effects robust to our multiple hypothesis testing correction. Again, these effects are temporary (Panel B).

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 the 12 corner seats (Figure 2) versus seats in the middle of rows. MPs in corner seats have only one left-right seating neighbor—their attention is undivided. For brevity, we restrict our sample only to different-party MP pairs, where we find stronger neighbor effects. In addition, we keep only the MP pairs who were both part of the seating lottery. We do so to avoid confounding the "undivided attention" channel with the fact that MPs pre-assigned to corner seats are different to other MPs (for example, they are more likely to be chairs of parliamentary groups, and thus may be more

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

	Below 50th Votes		Below 25th Votes	
	Compliance (1)	Similarity (2)	Compliance (3)	Similarity (4)
	Panel A: Contemporaneous Effect (t)			
Neighbor × Different Party	.0096	.013	.0051	.0071
(proximity effect on bipartisanship)	[<0.001]***	[<0.001]***	[.057]*	[.0047]***
	{.006}***	{.001}***	{.057}*	{.009}***
	<.003>***	<.001>***	<.23>	<.023>**
Neighbor × Same Party	.0085	.0096	.0036	.0037
	[.12]	[.12]	[.57]	[.57]
	{.2}	{.17}	{.58}	{.57}
	<.33>	<.33>	<.64>	<.64>
Observations	35205	35205	35259	35259
	Panel B: One Year Later (t+1)			
Neighbor × Different Party	-.0023	-.0042	.0008	.000057
(proximity effect on bipartisanship)	[.61]	[.36]	[.86]	[.99]
	{.57}	{.35}	{.81}	{.99}
	<.64>	<.5>	<.8>	<.8>
Neighbor × Same Party	.014	.012	.011	.0099
	[.15]	[.26]	[.19]	[.28]
	{.072}*	{.15}	{.13}	{.19}
	<.33>	<.5>	<.41>	<.5>
Observations	21589	21589	21589	21589
	Panel C: Previous Year (Placebo) (t-1)			
Neighbor × Different Party	.00052	.0012	.0013	.0017
(proximity effect on bipartisanship)	[.88]	[.77]	[.68]	[.59]
	{.9}	{.81}	{.73}	{.64}
Neighbor × Same Party	.0061	.0046	.0044	.0027
	[.49]	[.61]	[.59]	[.75]
	{.44}	{.61}	{.57}	{.71}
Observations	21638	21638	21638	21638
Session × Party Pair × Strata FE	Y	Y	Y	Y
Outcome Mean	.46	2.3	.57	2.5
Outcome S.d.	.15	.32	.13	.17

Notes: Each panel shows the estimates from four linear 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. Compliance is the proportion of times the two MPs in a pair vote the same way in a given session. 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 (1000 draws) are in curly brackets. Sharpened q-values (Anderson 2008) for non-placebo tests are in <>. Special sessions and a short session (2017) 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. 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 Standard Deviation are for the sample included in the Panel A regressions. *** p<0.01, ** p<0.05, * p<0.1.

influential).

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 A6). Nevertheless, these proximity effects still do not persist (Columns 3-4).

Considering robustness, our estimates of individual effects fall by roughly 40% if we consider pairs to only be voting the same way if they vote yes-yes, no-no, or abstain-abstain (Table A7), or if they vote yes-yes or no-no (Table A8), suggesting that some of our pair-level effect is driven by co-absenteeism, which may not reflect convergence in position-taking. With these dependent variables, we only estimate statistically significant neighbor effects for contested votes, highlighting the limited peer influence we observe overall. Our estimates are however similar if we code absenteeism as equivalent to abstention, or closer to a no vote than abstention (Tables A9, A10). Our estimated coefficients are also similar, although less precisely estimated, when reweighting the regressions so that different strata are weighted equally (Tables A11 and A12, following Gerber and Green (2012)).

Awareness of Influence. In our survey of past and present MPs, we asked "If you had to guess, how do you think an MP might influence the voting (if only a little bit) of another MP that sits next to them?" We did not tell respondents the results of our paper. Respondents expect little or no peer influence. We code five respondents as saying there is no influence at all (MPs 4, 7, 8, 10, ex-MP 13); five respondents as saying any influence is very unlikely (MPs 1, 2, 3, 5, 6); and the remaining four respondents as saying there is not much influence (full anonymized responses in Appendix B). Broadly, this suggests that most MPs are not aware of the small peer influence we detect, consistent with evidence in other contexts (Cialdini 2005). This may be because the influence is too small to be detected or remembered, or because peer influence is subconscious. When MPs note the possibility of peer influence, they suggest that neighbors might point out when a neighbor has forgotten to vote or made a mistake by pressing the wrong button (MPs 1, 11, 12). Like cue-taking and social pressure, the mistake correction channel would lead to only temporary pair-level effects. That said,

somewhat against our findings, one would expect this channel to imply larger same-party neighbor effects than different-party neighbor effects, since same-party neighbors have stronger incentives to correct mistakes.

Summary and Discussion. We find evidence of a small, temporary effect of bipartisan integration on roll-call votes, suggesting that exposure works through channels like cue-taking and social pressure, rather than cognitive and affective mechanisms. Our estimated proximity effect of roughly one percentage point is consistent with the two closest random-network studies of cue-taking: for the US House of Representatives, Rogowski and Sinclair (2012) find statistically insignificant effects of proximity, but given large standard errors, they cannot reject our point estimates. Interestingly, their OLS specifications deliver more precisely estimated coefficients that are in fact very similar to ours. For the European Parliament, Harmon et al. (2019) estimate a 0.6 percentage point effect of sitting together on voting similarity. Our results go beyond these two papers by showing that similar influence exists even for cross-party pairs.

Using our simulations of a model of peer influence in Appendix E, we can see that an estimated pair-level effect of one percentage point is consistent with an underlying probability of cross-party peer influence of 2% – since influence is only possible in cases where neighbors would otherwise vote differently, our estimated effect understates the probability of influence.

While cross-party cue-taking has been observed between those linked through co-sponsorship (Fong 2020), it is not immediately clear why such influence would exist between *randomly* selected cross-party pairs assigned to adjacent seating. One possibility is that the cross-party influence we observe comes from other parties that nevertheless belong to the same political coalition. We do not find evidence for this—cross-coalition effects are similar in magnitude, and similarly transitory (Tables A13, A14).

A second possibility is that cross-party influence only exists for the least important votes, or perhaps only for amendments—with cue-taking more likely here given their greater technicality (Box-Steffensmeier et al. 2015). We do not find evidence for this either—proximity effects remain substantial when considering voting only on draft bills, and stronger than those for amendments

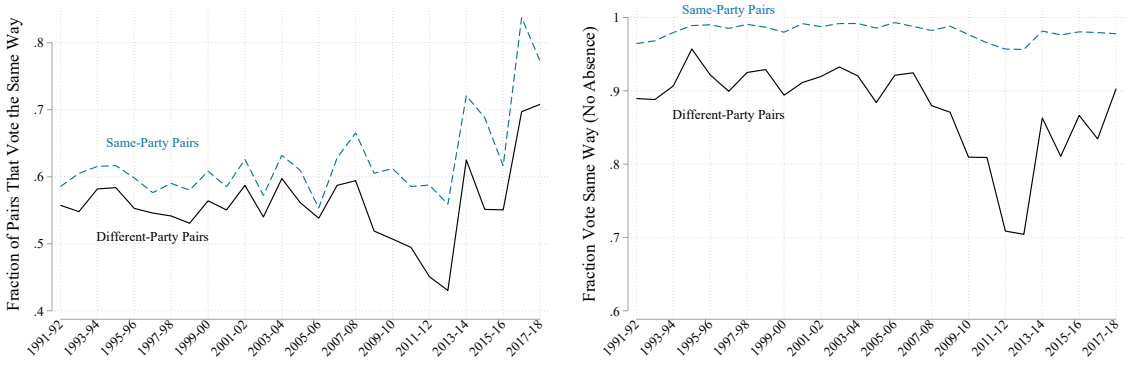
(Table A15). Using data on bill topic available for session 2001-02 onwards, we also find that proximity effects for contested votes are similar across bill topics, and large even for the most obviously substantive categories, like Economic Management and Foreign Relations (Table A16).

A third possibility is that, with limited attention, MPs do not fully understand what they are voting on, and take cues in these cases. Relatedly, surveyed MPs suggest that some MPs may point out and correct the mistakes of their neighbors. The effects of limited attention should be magnified on days with many votes (recall Figure A3). However, peer influence is similar for votes on busy voting days and votes on quieter days (Table A17).

A fourth possibility is that seating proximity to copartisans is less important because information would diffuse between copartisans whether or not they sit together. Consistent with this, proximity effects are stronger for different-gender than same-gender pairs of MPs (Table A18), and for pairs of MPs from more ideologically distant parties (Tables A19 and A20 using data on party positions from Döring et al. (2022)), which is what we would expect if gender and party homophily facilitates information diffusion between same-gender and similar-ideology MPs regardless of where they sit.

A final explanation is that cross-party influence is facilitated by cross-party consensus, providing enough trust in even random cross-party seating neighbors. To explore this, we make use of the breakdown in cross-party voting agreement that occurred following the 2009 snap election prompted by the Icelandic economic crisis (Figure 3). Cross-party neighbor effects are much stronger, and only statistically significant, prior to the 2009/10 session (Table A21), while same-party neighbor effects show the opposite pattern. Though more suggestive, these results support the hypothesis that cross-party influence is possible, though perhaps only during periods of cross-party consensus.

Figure 3: Cross-Party Consensus Fell After the 2008 Economic Crisis



Notes: The left-panel outcome is the average fraction of MP pairs that voted the same way (both yes, both no, both abstain, or both absent) for a given session. The right-panel outcome is the same, but calculated only for votes in which both MPs in the pair were not absent. In both panels, the prime minister, ministers, and speaker are excluded.

4.2 Effects on Party Rebellion

Moving to our MP-session-level specification, cross-party proximity has neither consistent nor detectable effects on rebellious voting, whether contemporaneously (Columns 1-2, Table 3), or one year later (Columns 3-4). Placebo tests again rule out chance imbalances (Columns 5-6), and results are similar when reweighting by the block-level inverse probability of treatment assignment (Table A22).

Since experienced MPs are more likely to be cue-*givers* than cue-takers, we might expect these null effects to mask heterogeneity, with the less-experienced MPs more affected by peers. However, if anything, we find the opposite (Table A23), and pair-level effects on voting similarity are similar for MP pairs that differ a lot in political experience and those that differ little (Table A24).

We find similar null effects when we estimate effects on alternative measures of rebellion (Table A25), effects for contested votes (Table A26), effects of cross-coalition exposure (Table A27), and when we separately estimate the effects of having half versus all seating neighbors from a different party or coalition (Tables A28, A29). The one exception is an increase in dissent for those assigned to one other-coalition neighbor relative to none (Column 2, Table A29, RI p-value = 0.089). While this collection of null effects suggests that outparty exposure does not increase rebellious voting, our simulations suggest that these effects may be due to a lack of statistical power – in particular,

Table 3: Effects of Other-Party Neighbors on Rebellious Voting

	Contemporaneous Effect (t)		One Year Later (t+1)		Previous Year (Placebo) (t-1)	
	Leader Non-Compliance (1)	Rebel Rate (2)	Leader Non-Compliance (3)	Rebel Rate (4)	Leader Non-Compliance (5)	Rebel Rate (6)
Proportion Other-Party Neighbor	.0028	-.00061	.0014	.00017	.012	-.00049
	(.0076)	(.00057)	(.0098)	(.00051)	(.0097)	(.00054)
	[.71]	[.29]	[.89]	[.73]	[.2]	[.37]
	{.71}	{.31}	{.89}	{.77}	{.19}	{.37}
	<1>	<1>	<1>	<1>		
Observations	1294	1294	826	826	835	835
Session × Party × Strata FE	Y	Y	Y	Y	Y	Y
Outcome Mean	.42	.005	.44	.0044	.43	.005
Outcome S.d.	.13	.011	.11	.01	.11	.0073

Notes: Leader Non-Compliance is the proportion of times the MP votes differently from their party leader in a given session. Rebel Rate is the proportion of times the MP voted yes/abstain (no/abstain) when their party leader voted no (yes) in a given session. Proportion Other-Party Neighbor is the proportion of left-right seating neighbors from a different party. MP-clustered standard errors are in parentheses and p-values are in square brackets. Randomization inference p-values (1000 draws) are in curly brackets. Sharpened q-values (Anderson 2008) for non-placebo tests are in <>. Special sessions and a short session (2017) 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. Strata FE is a dummy variable for whether MP was pre-assigned a seat. *** p<0.01, ** p<0.05, * p<0.1.

for the 2% peer influence parameter implied by our pair-level estimates, we have 90% power to reject the null of no pair-level effect, but only 28% power to reject the null of no MP-level effect (Table C2). This illuminates an advantage of our approach: by estimating pair-level specifications, we uncover evidence of cross-party cue-taking which is undetectable with MP-level specifications.

Other than shifting votes away from the party-line, we might expect other-party neighbors to decrease an MP's confidence in their votes, as a result of the conflicted cueing the MP receives. If this were the case, we might expect outparty exposure to increase absences and abstentions, as MPs become less confident in taking positions. However, we estimate null effects on contemporaneous absences and abstentions (Columns 1-2, Table A30), and *negative* effects on abstentions in the

following year. Taking the negative effect at face value, it would appear that outparty exposure actually increases an MP's future confidence in position-taking. However, it is hard to think of mechanisms that would create this effect without also creating a contemporaneous effect. Given this, we consider this result mostly as evidence against outparty exposure decreasing confidence in position-taking.

4.3 Effects on Co-Sponsorship

Bipartisan proximity does not lead to increased co-sponsorship links for different-party pairs in any time period that we consider (Table 4).[20] In Table A31 we compare the treatment effects of different-party pairs who sat at corners of rows to investigate whether undivided attention between neighbors can strengthen the treatment effect on co-sponsorship. We find 0.29 more co-sponsorship links (RI p-value = 0.15) between pairs who sat at corners, and this is larger than the effect on the pairs who sat in the middle. However, the effect does not survive our correction for multiple hypothesis testing ($q = 0.92$). This result should be considered only as suggestive evidence that year-long neighbors may forge enduring weak ties, or interest overlap, when the attention of one MP is undivided.

Table 5 reports MP-level effects on bipartisan co-sponsorship links. Having a larger proportion of other-party neighbors does not affect the number of contemporaneous links (Columns 1-2), but does increase future links (Column 3). 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 the raw number, or when correcting for multiple hypothesis testing ($q = 0.16$ and $q = 0.21$ in Columns 3 and 4). Encouragingly, the persistent impact on bipartisan links is similar when reweighting (Table A32), it is larger for those with two other-party neighbors than those with only one (Column 3, Table A33), and the persistent impact is similar when considering cross-coalition exposure (Table A34). Although more suggestive, these enduring impacts on cross-party

[20] Although not our main focus, there is some evidence of a negative effect of proximity for same-party pairs, reducing co-sponsorship links at the pair-level by ~9% (RI p-value = 0.065). Placebo estimates have 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.

co-sponsorship links offer some hope that bipartisan seating can create weak social ties and lead to interest overlap.

Table 4: Pair-Level Effects on Co-Sponsorship Links

	Contemporaneous Effect (t)		One Year Later (t+1)		Previous Year (Placebo) (t-1)	
	Number (1)	IHS (2)	Number (3)	IHS (4)	Number (5)	IHS (6)
Neighbor × Different Party	-.037	-.013	.07	.023	-.025	.016
(proximity effect on bipartisanship)	[.65]	[.6]	[.5]	[.52]	[.76]	[.59]
	{.56}	{.56}	{.41}	{.39}	{.75}	{.54}
	<.95>	<.95>	<.95>	<.95>		
Neighbor × Same Party	-.24	-.022	-.37	-.093	-.43	-.055
	[.22]	[.52]	[.1]	[.011]**	[.1]	[.22]
	{.21}	{.55}	{.14}	{.065}*	{.11}	{.3}
	<.79>	<.95>	<.54>	<.097>*		
Same = Different	[.34]	[.83]	[.088]*	[.019]**	[.15]	[.23]
	{.32}	{.84}	{.12}	{.067}*	{.14}	{.25}
Observations	35314	35314	23265	23265	23472	23472
Session × Party Pair × Strata FE	Y	Y	Y	Y	Y	Y
Outcome Mean	3.3	1.3	3.4	1.3	3.2	1.2
Outcome S.d.	4.8	1.1	5	1.2	4.8	1.1

Notes: Number is the total number of co-sponsorship links between the two MPs in a pair in a given session. IHS is the inverse hyperbolic sine transformation of 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 (1000 draws) are in curly brackets. Sharpened q-values (Anderson 2008) for non-placebo tests are in <>. Special sessions and a short session (2017) 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. 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. *** p<0.01, ** p<0.05, * p<0.1.

5 Conclusion

Icelandic legislators randomly assigned to sit next to each other are 0.5 to 1 percentage point more likely to vote alike. Most surveyed legislators are not aware of this small peer influence. Never-

Table 5: Effects on Bipartisan Co-Sponsorship Links

	Contemporaneous Effect (t)		One Year Later (t+1)		Previous Year (Placebo) (t-1)	
	Number (1)	IHS (2)	Number (3)	IHS (4)	Number (5)	IHS (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]
	{.69}	{.48}	{.037}**	{.095}*	{.31}	{.32}
	<.53>	<.38>	<.16>	<.21>		
Observations	1420	1420	941	941	946	946
Session × Party × Strata FE	Y	Y	Y	Y	Y	Y
Outcome Mean	82	4.7	83	4.5	76	4.5
Outcome S.d.	76	1.1	82	1.3	73	1.2

Notes: Number is the total number of co-sponsorship links between the MP and any other-party MP in a given session. IHS is the inverse hyperbolic sine transformation of Number. Proportion Other-Party Neighbor is the proportion of left-right seating neighbors from a different party. MP-clustered standard errors are in parentheses and p-values are in square brackets. Randomization inference p-values (1000 draws) are in curly brackets. Special sessions and a short session (2017) 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. Strata FE is a dummy variable for whether MP was pre-assigned a seat. *** p<0.01, ** p<0.05, * p<0.1.

theless, proximity effects are short-lived, and thus more consistent with legislative cue-taking and social pressure mechanisms than cognitive and affective changes. Otherwise, we find a suggestive positive effect of seating proximity on cross-party weak ties and interest overlap, as proxied by co-sponsorship links. Overall, our main takeaway is that physical integration has limited power to durably increase bipartisanship in a setting with strong parties.

Mechanisms aside, the *Althingi* is a small parliament with a unique seating arrangement—how generalizable are our findings? Our own view is that Iceland provides a relatively demanding test for cross-party influence, given its strong parties and Westminster-style adversarial politics. The existence of neighbor effects in the *Althingi* then suggest that peer effects in legislatures may also be present in other parliamentary settings, though perhaps only those with a reasonable amount of cross-party consensus, given the fall in cross-party influence after the Icelandic economic cri-

sis. Indeed, the one existing study in a parliamentary setting finds very similar pair-level effects (Harmon et al. 2019). Going beyond our work, the external validity of our findings can be tested directly with a regression discontinuity design in two other Nordic parliamentary settings—the within-constituency seating order in the Norwegian Storting is ordered by the Sainte-Laguë vote score, while in the Swedish Riksdag MPs are seated in order of tenure, and then age. Each system delivers quasi-random variation in the party of seating neighbors whenever two different-party neighbors have very similar vote scores or ages. Outside of the handful of legislatures with integrated seating, social and sporting events may be an alternative source of partisan integration—for example, Republicans and Democrats in the US Congress play an annual charity baseball game together (Lawless et al. 2018).

Do seating arrangements exist that can generate stronger effects on bipartisanship? One hypothesis would be that legislators need to sit next to other-party colleagues for more than one session for enough trust to build to catalyze bipartisan behaviors. With the caveat of lower statistical power, we find suggestive support for this hypothesis—the effects of other-coalition exposure are more positive for MPs who experienced more other-coalition exposure in the previous session (Table A35).

Finally, we note an important limitation of our analysis: we estimate the effects of having more versus fewer other-party seating neighbors in the context of an already integrated chamber. We cannot estimate the overall effects of a chamber moving from party-grouped to integrated. The latter might have additional effects: for example, in personal correspondence a sitting MP speculated that the seating arrangement as a whole reduces party cohesion by making it more difficult for parties to notice individuals voting out of line. In his words: "I believe that if we were seated by party, the cohesion would increase dramatically, as not only would it stick out on the voting board if someone voted differently than everyone else, but also one's group members would be more likely to verbally intervene in some way, even if only to ask a question or joke about it."

References

Allport, Gordon, *The nature of prejudice*, Addison-Wesley, Reading, MA, 1954.

Anderson, Michael L, "Multiple inference and gender differences in the effects of early intervention: A reevaluation of the Abecedarian, Perry Preschool, and Early Training Projects," *Journal of the American statistical Association*, 2008, *103* (484), 1481–1495.

Beck, Paul Allen, Russell J Dalton, Steven Greene, and Robert Huckfeldt, "The social calculus of voting: Interpersonal, media, and organizational influences on presidential choices," *American political science review*, 2002, pp. 57–73.

Bengtsson, Åsa, Kasper Hansen, Ólafur Þ Harõarson, Hanne Marthe Narud, and Henrik Oscarsson, *The Nordic voter: myths of exceptionalism*, Ecpr Press, 2013.

Bjarnegård, Elin, *Gender, informal institutions and political recruitment: Explaining male dominance in parliamentary representation*, Springer, 2013.

Bond, Robert M, Christopher J Fariss, Jason J Jones, Adam DI Kramer, Cameron Marlow, Jaime E Settle, and James H Fowler, "A 61-million-person experiment in social influence and political mobilization," *Nature*, 2012, *489* (7415), 295–298.

Box-Steffensmeier, Janet, Josh M Ryan, and Anand Edward Sokhey, "Examining Legislative Cue-Taking in the US Senate," *Legislative Studies Quarterly*, 2015, *40* (1), 13–53.

Caldeira, Gregory A and Samuel C Patterson, "Contours of Friendship and Respect in the Legislature," *American Politics Quarterly*, 1988, *16* (4), 466–485.

_ , John A Clark, and Samuel C Patterson, "Political respect in the legislature," *Legislative Studies Quarterly*, 1993, pp. 3–28.

Cameron, A. Colin and Douglas L. Miller, "Robust inference for dyadic data," *Unpublished manuscript, University of California-Davis*, 2014.

Chiang, Chun-Fang and Brian Knight, "Media bias and influence: Evidence from newspaper endorsements," *The Review of Economic Studies*, 2011, p. rdq037.

Cialdini, Robert B, "Basic social influence is underestimated," *Psychological inquiry*, 2005, *16* (4), 158–161.

Clinton, Joshua, Simon Jackman, and Douglas Rivers, "The statistical analysis of roll call data," *American Political Science Review*, 2004, pp. 355–370.

Döring, Holger, Constantin Huber, and Philip Manow, "Parliaments and governments database (ParlGov): Information on parties, elections and cabinets in established democracies," Technical Report 2022.

Fong, Christian, "Expertise, Networks, and Interpersonal Influence in Congress," *The Journal of Politics*, 2020, *82* (1), 269–284.

Fowler, James H, "Connecting the Congress: A study of cosponsorship networks," *Political Analysis*, 2006, *14* (4), 456–487.

_ , Michael T Heaney, David W Nickerson, John F Padgett, and Betsy Sinclair, "Causality in political networks," *American Politics Research*, 2011, *39* (2), 437–480.

Gerber, Alan S and Donald P Green, *Field experiments: Design, analysis, and interpretation*, WW Norton, 2012.

_ , _ , and Christopher W Larimer, "Social pressure and voter turnout: Evidence from a large-scale field experiment," *American political Science review*, 2008, *102* (1), 33–48.

Habermas, Jürgen, *The structural transformation of the public sphere: An inquiry into a category of bourgeois society*, MIT press, 1991.

Haidt, Jonathan, *The righteous mind: Why good people are divided by politics and religion*, Vintage, 2012.

Hansen, Martin Ejnar, "Cabinets and ministerial turnover in the Scandinavian countries," in "The Routledge handbook of Scandinavian politics," Routledge, 2017, pp. 92–102.

Harmon, Nikolaj, Raymond Fisman, and Emir Kamenica, "Peer effects in legislative voting," *American Economic Journal: Applied Economics*, 2019, *11* (4), 156–80.

Imbens, Guido W and Donald B Rubin, *Causal inference in statistics, social, and biomedical sciences*, Cambridge University Press, 2015.

Indridason, Indridi H, "A theory of coalitions and clientelism: Coalition politics in Iceland, 1945–2000," *European Journal of Political Research*, 2005, *44* (3), 439–464.

Iyengar, Shanto, Yphtach Lelkes, Matthew Levendusky, Neil Malhotra, and Sean J Westwood, "The origins and consequences of affective polarization in the United States," *Annual Review of Political Science*, 2019, *22*, 129–146.

Jensen, Torben K, "Party cohesion," in "Beyond Westminster and Congress: The Nordic Experience" Ohio State University Press Columbus, OH 2000, pp. 210–236.

Jónsson, Guðmundur, "Iceland and the Nordic model of consensus democracy," *Scandinavian Journal of History*, 2014, *39* (4), 510–528.

Kam, Christopher J, *Party discipline and parliamentary politics*, Cambridge University Press, 2009.

Kessler, Daniel and Keith Krehbiel, "Dynamics of cosponsorship," *American Political Science Review*, 1996, *90* (3), 555–566.

Kristinsson, Gunnar Helgi, "Party cohesion in the Icelandic Althingi," *Stjrnml og stjrnssla Icelandic*, 2011.

Kristjánsson, Svanur and Indridi H Indridason, "Iceland: Dramatic Shifts," *The Madisonian Turn: Political Parties and Parliamentary Democracy in Nordic Europe*, 2011, p. 158.

Lawless, Jennifer L, Sean M Theriault, and Samantha Guthrie, "Nice girls? Sex, collegiality, and bipartisan cooperation in the US congress," *The Journal of Politics*, 2018, *80* (4), 1268–1282.

Lowe, Matt, "Types of contact: A field experiment on collaborative and adversarial caste integration," *American Economic Review*, 2021, *111* (6), 1807–44.

Magnússon, Thorsteinn, "Seating Arrangement in Althingi," *Icelandic Review of Politics and Administration*, 2014, *10* (2), 217–248.

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

Matthews, Donald Rowe and James A Stimson, *Yeas and nays: Normal decision-making in the US House of Representatives*, Wiley-Interscience, 1975.

Mutz, Diana C, "Cross-cutting social networks: Testing democratic theory in practice," *American Political Science Review*, 2002, *96* (1), 111–126.

Paluck, Elizabeth L., Seth A. Green, and Donald P. Green, "The Contact Hypothesis Re-Evaluated," *Behavioural Public Policy*, 2018, pp. 1–30.

Poole, Keith T and Howard L Rosenthal, *Ideology and congress*, Vol. 1, Transaction Publishers, 2011.

Ringe, Nils, Jennifer Nicoll Victor, and Justin H Gross, "Keeping your friends close and your enemies closer? Information networks in legislative politics," *British Journal of Political Science*, 2013, pp. 601–628.

_ , _ , and Wendy Tam Cho, "Legislative networks," *The Oxford Handbook of Political Networks*, 2017, p. 471.

Ripley, Amanda, "These Radically Simple Changes Helped Lawmakers Actually Get Things Done," *The Washington Post*, 2023.

Rogowski, Jon C and Betsy Sinclair, "Estimating the causal effects of social interaction with endogenous networks," *Political Analysis*, 2012, pp. 316–328.

Saia, Alessandro, "Random interactions in the Chamber: Legislators' behavior and political distance," *Journal of Public Economics*, 2018, *164*, 225–240.

SIC, "Report of the Special Investigation Commission (SIC)," report delivered to Althingi, April 12 2010.

Viken, Bård Skaar, "The birth of a system born to collapse: laissez-faire the Icelandic way," *European Political Science*, 2011, *10* (3), 312–323.

Wade, Robert H and Silla Sigurgeirsdottir, "Iceland's meltdown: the rise and fall of international banking in the North Atlantic," *Brazilian Journal of Political Economy*, 2011, *31* (5), 684–697.

Wahlke, John C, Heinz Eulau, William Buchanan et al., *The legislative system: Explorations in legislative behavior*, New York: Wiley, 1962.

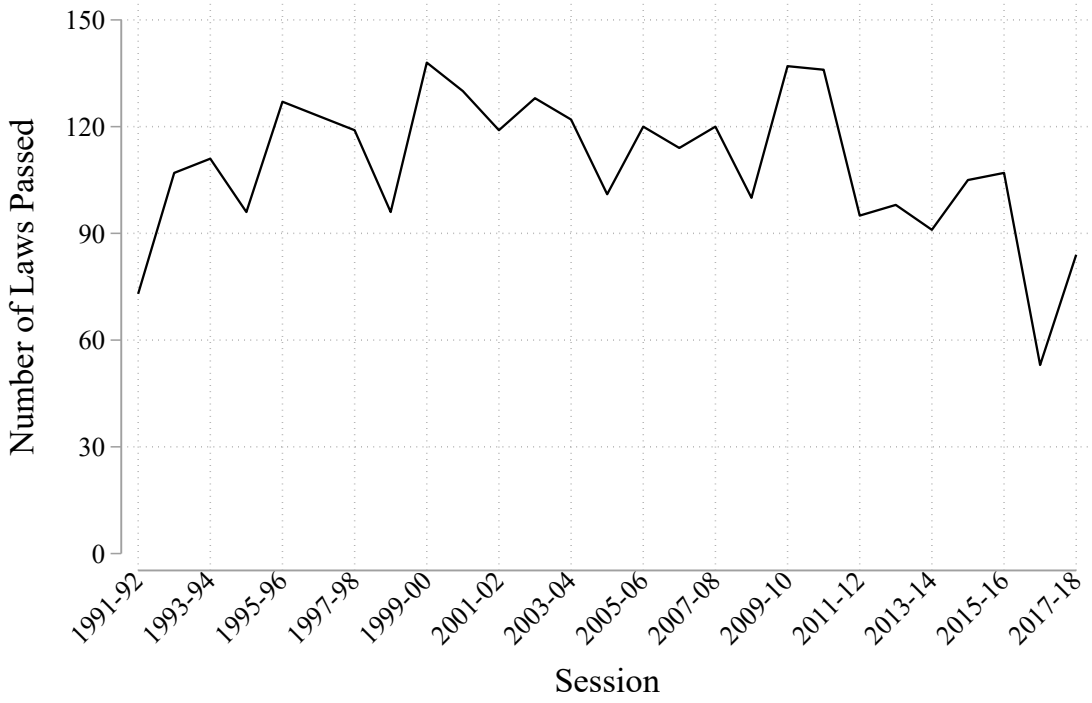
West, Brady T and Annelies G Blom, "Explaining interviewer effects: A research synthesis," *Journal of survey statistics and methodology*, 2017, *5* (2), 175–211.

Young, Alwyn, "Channeling fisher: Randomization tests and the statistical insignificance of seemingly significant experimental results," 2015.

Zelizer, Adam, "Is position-taking contagious? evidence of cue-taking from two field experiments in a state legislature," *American Political Science Review*, 2019, *113* (2), 340–352.

A Appendix Tables and Figures

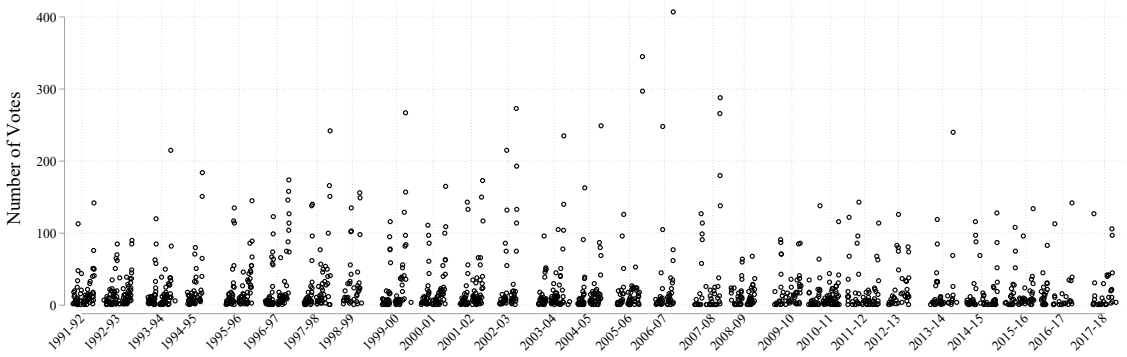
Figure A1: Legislative Productivity in the *Althingi*



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

Figure A2: An MP Draws Her Seat Number for 2013-14

Figure A3: Information Overload With Voting in the *Althingi*



Notes: This figure visualizes the number of votes taken per day on all days with at least one vote for each regular session from 1991-92 to 2017-18. The gaps reflect special and short sessions and periods when the *Althingi* was not in session. 36,366 votes were taken during the period shown.

Table A1: Pair-Level Balance Table

		Same...				Difference in...			
	Neighbor (t-1)	Gender	Ever Minister	Committee (t-1)	Constit.	Age	Sessions Experience	Wages (t-1)	Expenses (t-1)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Neighbor × Different Party	.0069	-.0098	-.0066	-.0023	-.0025	-.0099	.16	-376288	-44906
(proximity effect on bipartisanship)	[.56]	[.55]	[.51]	[.87]	[.79]	[.96]	[.43]	[.071]*	[.62]
	{.39}	{.54}	{.52}	{.86}	{.86}	{.96}	{.39}	{.027}**	{.49}
Neighbor × Same Party	-.017	.024	.0076	.012	.011	.57	.11	133811	-262
	[.15]	[.2]	[.66]	[.66]	[.53]	[.22]	[.81]	[.69]	[1]
	{.24}	{.42}	{.73}	{.64}	{.58}	{.2}	{.79}	{.72}	{1}
Same = Different	[.14]	[.13]	[.54]	[.63]	[.49]	[.27]	[.93]	[.2]	[.68]
	{.13}	{.34}	{.59}	{.6}	{.58}	{.26}	{.91}	{.2}	{.82}
Observations	21954	35314	35314	35314	35314	35314	35314	13579	13579
Session × Party Pair × Strata FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Outcome Mean	.034	.54	.73	.6	.15	10	7.6	3512167	1511681

Notes: The outcome in column (1) is a dummy variable equal to one if the two MPs in a pair were seating neighbors in the previous session (only for non-short/special sessions in same parliamentary term). Each outcome in columns (2) to (5) is a dummy variable equal to one if the two MPs in a pair share the same value for the following variables: (2) dummy variable equal to one if MP is male, (3) dummy variable equal to one if ever held a ministerial position prior to this session, (4) dummy variable equal to one if chaired a committee at any point during the previous session, and (5) constituency. The outcomes in columns (6) to (9) are the absolute difference between the two MPs in a pair for the following variables: (6) age in years as of the start of the current session, (7) number of sessions since first session as Althingi member, (8) wages received in Althingi during the calendar year prior to the current session, and (9) expenses claimed in Althingi during the calendar year prior to the current session. The waves and expenses data are only available from session 136 (2008/9) onwards. 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 (1000 draws) are in curly brackets. Special sessions and a short session (2017) are excluded. 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. *** p<0.01, ** p<0.05, * p<0.1.

Table A2: MP-level Balance Table

	Proportion Oth-Party Neighbor (t-1) (1)	Male (2)	Age (3)	Reykjavik Constit. (4)	Ever Minister (5)	Committee (t-1) (6)	Sessions Experience (7)	Wages (t-1) (8)	Expenses (t-1) (9)
Proportion Other-Party Neighbor	-.0037	-.0042	.51	-.012	.0066	-.012	-.49	219539	23761
	(.037)	(.046)	(.93)	(.048)	(.036)	(.035)	(.79)	(683569)	(250630)
	[.92]	[.93]	[.58]	[.81]	[.85]	[.74]	[.53]	[.75]	[.92]
	{.92}	{.93}	{.53}	{.79}	{.84}	{.77}	{.48}	{.69}	{.92}
Observations	924	1420	1420	1420	1420	1420	1420	536	536
Session × Party × Strata FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Outcome Mean	.76	.64	49	.31	.17	.27	8.6	2968661	1119860

Notes: Outcome variables are: (1) proportion other-party neighbor in previous session (only for non-short/special sessions in same parliamentary term), (2) dummy variable equal to one if MP is male, (3) age in years as of the start of the current session, (4) dummy variable equal to one if elected from Reykjavik constituency (North or South from session 129 (2003) onwards), (5) dummy variable equal to one if ever held a ministerial position prior to this session, (6) dummy variable equal to one if chaired a committee at any point during the previous session, (7) number of sessions since first session as Althingi member. The outcomes for columns (8) and (9) are, respectively, the wages received, and other expenses claimed in Althingi during the calendar year prior to the current session, with the data for these variables only available from session 136 (2008/9) onwards. Proportion Other-Party Neighbor is the proportion of left-right seating neighbors from a different party. MP-clustered standard errors are in parentheses and p-values are in square brackets. Randomization inference p-values (1000 draws) are in curly brackets. Special sessions and a short session (2017) are excluded. Strata FE is a dummy variable for whether MP was pre-assigned a seat. *** p<0.01, ** p<0.05, * p<0.1.

Table A3: Pair-Level Balance Table: Coalition Heterogeneity

	Same...					Difference in...			
	Neighbor (t-1)	Gender	Ever Minister	Committee (t-1)	Constit.	Age	Sessions Experience	Wages (t-1)	Expenses (t-1)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Neighbor × Different Coalition	-.00082	-.0074	.0054	.00035	-.0064	-.17	.1	-204889	-51900
	[.94]	[.68]	[.69]	[.98]	[.64]	[.57]	[.69]	[.47]	[.69]
	{.93}	{.71}	{.67}	{.98}	{.68}	{.56}	{.7}	{.36}	{.57}
Neighbor × Same Coalition	.0028	.0031	-.012	.0016	.0072	.41	.19	-355686	-22256
	[.83]	[.86]	[.27]	[.92]	[.52]	[.089]*	[.49]	[.13]	[.83]
	{.79}	{.87}	{.42}	{.91}	{.62}	{.17}	{.48}	{.1}	{.8}
Same = Different	[.8]	[.63]	[.38]	[.96]	[.46]	[.17]	[.84]	[.69]	[.85]
	{.81}	{.71}	{.43}	{.96}	{.53}	{.2}	{.84}	{.64}	{.85}
Observations	21954	35314	35314	35314	35314	35314	35314	13579	13579
Session × Party Pair × Strata FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Outcome Mean	.034	.54	.73	.6	.15	10	7.6	3512167	1511681

Notes: The outcome in column (1) is a dummy variable equal to one if the two MPs in a pair were seating neighbors in the previous session (only for non-short/special sessions in same parliamentary term). Each outcome in columns (2) to (5) is a dummy variable equal to one if the two MPs in a pair share the same value for the following variables: (2) dummy variable equal to one if MP is male, (3) dummy variable equal to one if ever held a ministerial position prior to this session, (4) dummy variable equal to one if chaired a committee at any point during the previous session, and (5) constituency. The outcomes in columns (6) to (9) are the absolute difference between the two MPs in a pair for the following variables: (6) age in years as of the start of the current session, (7) number of sessions since first session as Althingi member, (8) wages received in Althingi during the calendar year prior to the current session, and (9) expenses claimed in Althingi during the calendar year prior to the current session. The waves and expenses data are only available from session 136 (2008/9) onwards. 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 Coalition is equal to one if both MPs in the pair are in the same coalition for that session. Dyadic-robust p-values are in square brackets. Randomization inference p-values (1000 draws) are in curly brackets. Special sessions and a short session (2017) are excluded. 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. *** p<0.01, ** p<0.05, * p<0.1.

Table A4: MP-level Balance Table: Coalition Heterogeneity

	Proportion Oth-Party Neighbor (t-1) (1)	Male (2)	Age (3)	Reykjavik Constit. (4)	Ever Minister (5)	Committee (t-1) (6)	Sessions Experience (7)	Wages (t-1) (8)	Expenses (t-1) (9)
Proportion Other-Coalition Neighbor	-.0067	-.036	.11	.036	-.018	.026	.13	-129312	-197475
	(.037)	(.037)	(.73)	(.037)	(.029)	(.032)	(.59)	(516001)	(235432)
	[.86]	[.33]	[.88]	[.33]	[.54]	[.42]	[.83]	[.8]	[.4]
	{.84}	{.33}	{.85}	{.3}	{.5}	{.37}	{.8}	{.78}	{.33}
Observations	924	1420	1420	1420	1420	1420	1420	536	536
Session × Party × Strata FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Outcome Mean	.49	.64	49	.31	.17	.27	8.6	2968661	1119860

Notes: Outcome variables are: (1) proportion other-party neighbor in previous session (only for non-short/special sessions in same parliamentary term), (2) dummy variable equal to one if MP is male, (3) age in years as of the start of the current session, (4) dummy variable equal to one if elected from Reykjavik constituency (North or South from session 129 (2003) onwards), (5) dummy variable equal to one if ever held a ministerial position prior to this session, (6) dummy variable equal to one if chaired a committee at any point during the previous session, (7) number of sessions since first session as Althingi member. The outcomes for columns (8) and (9) are, respectively, the wages received, and other expenses claimed in Althingi during the calendar year prior to the current session, with the data for these variables only available from session 136 (2008/9) onwards. Proportion Other-Coalition Neighbor is the proportion of left-right seating neighbors from a different coalition. MP-clustered standard errors are in parentheses and p-values are in square brackets. Randomization inference p-values (1000 draws) are in curly brackets. Special sessions and a short session (2017) are excluded. Strata FE is a dummy variable for whether MP was pre-assigned a seat. *** p<0.01, ** p<0.05, * p<0.1.

Table A5: Do Front-Back Neighbors Vote More Alike Too?

	Contemporaneous Effect (t)	
	Compliance (1)	Similarity (2)
Neighbor × Different Party	.0051	.007
	{.057}*	{.01}**
Neighbor × Same Party	.0038	.0039
	{.56}	{.55}
Front-Back Neighbor × Different Party	-.00059	-.00072
	{.85}	{.83}
Front-Back Neighbor × Same Party	.0051	.0065
	{.52}	{.43}
Observations	35259	35259
Session × Party Pair × Strata FE	Y	Y

Notes: Compliance is the proportion of times the two MPs in a pair vote the same way in a given session. 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. Front-Back Neighbor is a dummy variable equal to one if the MPs in the pair are sitting immediately in front of or behind each other. Same Party is equal to one if both MPs in the pair are in the same party for that session. Randomization inference p-values (1000 draws) are in curly brackets. Special sessions and a short session (2017) 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. 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. *** p<0.01, ** p<0.05, * p<0.1.

Table A6: Pair-Level Effects on Bipartisan Voting: Effects with Undivided Attention

	Contemporaneous Effect (t)		One Year Later (t+1)		Previous Year (Placebo) (t-1)	
	Compliance (1)	Similarity (2)	Compliance (3)	Similarity (4)	Compliance (5)	Similarity (6)
Neighbor × Corner	.013	.013	.0043	.0013	-.012	-.012
	[.046]**	[.041]**	[.68]	[.9]	[.19]	[.16]
	{.092}*	{.079}*	{.59}	{.88}	{.14}	{.14}
Neighbor × Middle	.0024	.0047	.0012	.0014	.0045	.0043
	[.42]	[.084]*	[.81]	[.79]	[.19]	[.24]
	{.45}	{.16}	{.78}	{.75}	{.29}	{.31}
Corner = Middle	[.14]	[.24]	[.79]	[.99]	[.07]*	[.06]*
	{.2}	{.31}	{.75}	{.99}	{.085}*	{.072}*
Observations	22652	22652	14140	14140	13863	13863
Session × Corner FE	Y	Y	Y	Y	Y	Y
Session × Party Pair FE	Y	Y	Y	Y	Y	Y
Outcome Mean	.56	2.5	.54	2.5	.55	2.5
Outcome S.d.	.13	.16	.12	.15	.12	.15

Notes: Regressions include different-party dyads only, with neither MP pre-assigned seats. Compliance is the proportion of times the two MPs in a pair vote the same way in a given session. 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 pair has only one seating neighbor. Middle is equal to one minus Corner. Dyadic-robust p-values are in square brackets. Randomization inference p-values (1000 draws) are in curly brackets. Special sessions and a short session (2017) 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. *** p<0.01, ** p<0.05, * p<0.1.

Table A7: Pair-Level Effects: Voting Similarity without Absenteeism

	Compliance	Yes-Yes/No-No/Abstain-Abstain		
	All	All	Below 50th	Below 25th
	(1)	(2)	(3)	(4)
	Panel A: Contemporaneous Effect (t)			
Neighbor × Different Party	.0051	.0033	.0061	.0033
(proximity effect on bipartisanship)	[.057]*	[.27]	[.034]**	[.27]
	{.057}*	{.2}	{.022}**	{.2}
Neighbor × Same Party	.0036	.0025	.0021	.0025
	[.57]	[.77]	[.81]	[.77]
	{.58}	{.72}	{.78}	{.72}
Observations	35259	35259	35205	35259
	Panel B: One Year Later (t+1)			
Neighbor × Different Party	.0008	-.00048	-.0051	-.00048
(proximity effect on bipartisanship)	[.86]	[.93]	[.34]	[.93]
	{.81}	{.88}	{.14}	{.88}
Neighbor × Same Party	.011	.0052	.013	.0052
	[.19]	[.61]	[.25]	[.61]
	{.13}	{.56}	{.16}	{.56}
Observations	21589	21589	21589	21589
	Panel C: Previous Year (Placebo) (t-1)			
Neighbor × Different Party	.0013	.0001	-.00013	.0001
(proximity effect on bipartisanship)	[.68]	[.98]	[.97]	[.98]
	{.73}	{.98}	{.96}	{.98}
Neighbor × Same Party	.0044	.0029	.0043	.0029
	[.59]	[.77]	[.71]	[.77]
	{.57}	{.75}	{.65}	{.75}
Observations	21638	21638	21638	21638
Session × Party Pair × Strata FE	Y	Y	Y	Y
Outcome Mean	.57	.49	.33	.49
Outcome S.d.	.13	.17	.17	.17

Notes: Each panel shows the estimates from four linear regressions. Compliance is the proportion of times the two MPs in a pair vote the same way in a given session. Yes-Yes/No-No/Abstain-Abstain is the proportion of times the two MPs in a pair both vote yes, or both vote no, or both abstain 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 (1000 draws) are in curly brackets. Special sessions and a short session (2017) 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. 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 Standard Deviation are for the sample included in the Panel A regressions. *** p<0.01, ** p<0.05, * p<0.1.

Table A8: Pair-Level Effects: Voting Similarity without Absenteeism and Abstention

	Compliance	Yes-Yes/No-No		
	All	All	Below 50th	Below 25th
	(1)	(2)	(3)	(4)
	Panel A: Contemporaneous Effect (t)			
Neighbor × Different Party	.0051	.0034	.0058	.0034
(proximity effect on bipartisanship)	[.057]*	[.25]	[.045]**	[.25]
	{.057}*	{.19}	{.027}**	{.19}
Neighbor × Same Party	.0036	.0012	-6.2e-06	.0012
	[.57]	[.88]	[1]	[.88]
	{.58}	{.86}	{1}	{.86}
Observations	35259	35259	35205	35259
	Panel B: One Year Later (t+1)			
Neighbor × Different Party	.0008	-.00012	-.0042	-.00012
(proximity effect on bipartisanship)	[.86]	[.98]	[.42]	[.98]
	{.81}	{.98}	{.22}	{.98}
Neighbor × Same Party	.011	.0032	.01	.0032
	[.19]	[.74]	[.35]	[.74]
	{.13}	{.71}	{.25}	{.71}
Observations	21589	21589	21589	21589
	Panel C: Previous Year (Placebo) (t-1)			
Neighbor × Different Party	.0013	.000081	-.00016	.000081
(proximity effect on bipartisanship)	[.68]	[.98]	[.96]	[.98]
	{.73}	{.98}	{.95}	{.98}
Neighbor × Same Party	.0044	.0033	.0045	.0033
	[.59]	[.73]	[.68]	[.73]
	{.57}	{.71}	{.61}	{.71}
Observations	21638	21638	21638	21638
Session × Party Pair × Strata FE	Y	Y	Y	Y
Outcome Mean	.57	.48	.31	.48
Outcome S.d.	.13	.17	.17	.17

Notes: Each panel shows the estimates from four linear regressions. Compliance is the proportion of times the two MPs in a pair vote the same way in a given session. 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 (1000 draws) are in curly brackets. Special sessions and a short session (2017) 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. 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 Standard Deviation are for the sample included in the Panel A regressions. *** p<0.01, ** p<0.05, * p<0.1.

Table A9: Pair-Level Effects on Voting: Robustness

	Contemporaneous Effect (t)				
	Compliance (1)	Similarity (2)	Compliance 3-Cat (3)	Similarity 3-Cat (4)	Similarity Recode (5)
Neighbor × Different Party	.0051	.0071	.0057	.0062	.01
(proximity effect on bipartisanship)	[.057]*	[.0047]***	[.022]**	[.0096]***	[.052]*
	{.057}*	{.009}***	{.037}**	{.022}**	{.057}*
Neighbor × Same Party	.0036	.0037	.0032	.0033	.0069
	[.57]	[.57]	[.58]	[.57]	[.56]
	{.58}	{.57}	{.62}	{.61}	{.59}
Same = Different	[.82]	[.61]	[.68]	[.63]	[.8]
	{.84}	{.65}	{.73}	{.69}	{.84}
Observations	35259	35259	35259	35259	35259
Session × Party Pair × Strata FE	Y	Y	Y	Y	Y
Outcome Mean	.57	2.5	.59	1.6	2.2
Outcome S.d.	.13	.17	.13	.13	.25

Notes: The first two columns replicate the core results of Table 1. Compliance 3-Cat is the proportion of times the two MPs in a pair vote the same way in a given session, with absence considered equivalent to abstention. Similarity 3-Cat is the average vote similarity between the two MPs in a pair, with absence considered equivalent to abstention. Similarity Recode is the pair-level average vote similarity, with absence coded as closer to a no vote than abstention. 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 (1000 draws) are in curly brackets. Special sessions and a short session (2017) 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. 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. *** p<0.01, ** p<0.05, * p<0.1.

Table A10: Pair-Level Effects on Contested Voting: Robustness

	Contemporaneous Effect (t), Below 50th				
	Compliance (1)	Similarity (2)	Compliance 3-Cat (3)	Similarity 3-Cat (4)	Similarity Recode (5)
Neighbor × Different Party (proximity effect on bipartisanship)	.0096 [<0.001]*** {.006}***	.013 [<0.001]*** {.001}***	.01 [<0.001]*** {.002}***	.011 [<0.001]*** {.002}***	.02 [<0.001]*** {.003}***
Neighbor × Same Party	.0085 [.12] {.2}	.0096 [.12] {.17}	.0078 [.13] {.21}	.008 [.13] {.21}	.016 [.12] {.2}
Same = Different	[.84] {.9}	[.61] {.68}	[.68] {.78}	[.58] {.69}	[.7] {.79}
Observations	35205	35205	35205	35205	35205
Session × Party Pair × Strata FE	Y	Y	Y	Y	Y
Outcome Mean	.46	2.3	.49	1.4	2
Outcome S.d.	.15	.32	.14	.19	.3

Notes: The first two columns replicate results from Table 2. Compliance 3-Cat is the proportion of times the two MPs in a pair vote the same way in a given session, with absence considered equivalent to abstention. Similarity 3-Cat is the average vote similarity between the two MPs in a pair, with absence considered equivalent to abstention. Similarity Recode is the pair-level average vote similarity, with absence coded as closer to a no vote than abstention. 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 (1000 draws) are in curly brackets. Special sessions and a short session (2017) 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. 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. *** p<0.01, ** p<0.05, * p<0.1.

Table A11: Pair-Level Effects on Voting (Reweighted)

	Contemporaneous Effect (t)		One Year Later (t+1)		Previous Year (Placebo) (t-1)	
	Compliance (1)	Similarity (2)	Compliance (3)	Similarity (4)	Compliance (5)	Similarity (6)
Neighbor × Different Party	.005	.0067	.0018	.0016	.000015	-.0001
(proximity effect on bipartisanship)	(.0034)	(.004)	(.0046)	(.0047)	(.0042)	(.0046)
	[.14]	[.094]*	[.7]	[.73]	[1]	[.98]
	{.13}	{.043}**	{.68}	{.71}	{1}	{.98}
	<1>	<1>	<1>	<1>		
Neighbor × Same Party	.0032	.0033	.011	.0097	-.00091	-.0023
	(.0078)	(.0084)	(.0085)	(.0091)	(.0094)	(.0098)
	[.68]	[.69]	[.2]	[.29]	[.92]	[.81]
	{.63}	{.63}	{.18}	{.25}	{.89}	{.77}
	<1>	<1>	<1>	<1>		
Same = Different	[.81]	[.66]	[.32]	[.41]	[.92]	[.83]
	{.82}	{.65}	{.35}	{.42}	{.93}	{.83}
Observations	35259	35259	21589	21589	21638	21638
Session × Party Pair × Strata FE	Y	Y	Y	Y	Y	Y
Outcome Mean	.57	2.5	.55	2.5	.57	2.5
Outcome S.d.	.13	.17	.12	.16	.12	.16

Notes: Observations are weighted by the block-level inverse probability of treatment assignment, following Gerber and Green (2012). Compliance is the proportion of times the two MPs in a pair vote the same way in a given session. 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 standard errors are in parentheses and dyadic-robust p-values are in square brackets. Randomization inference p-values (1000 draws) are in curly brackets. Sharpened q-values (Anderson 2008) for non-placebo tests are in <>. Special sessions and a short session (2017) 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. 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. *** p<0.01, ** p<0.05, * p<0.1.

Table A12: Pair-Level Effects: Voting on Contested Votes (Reweighted)

	Below 50th Votes		Below 25th Votes	
	Compliance (1)	Similarity (2)	Compliance (3)	Similarity (4)
	Panel A: Contemporaneous Effect (t)			
Neighbor × Different Party	.0092	.013	.005	.0067
(proximity effect on bipartisanship)	[.0024]***	[<0.001]***	[.14]	[.096]*
	{.019}**	{.004}***	{.13}	{.043}**
	<.019>**	<.007>***	<.62>	<.62>
Neighbor × Same Party	.008	.0093	.0032	.0033
	[.22]	[.22]	[.68]	[.7]
	{.26}	{.23}	{.63}	{.63}
	<.62>	<.62>	<.78>	<.78>
Observations	35205	35205	35259	35259
	Panel B: One Year Later (t+1)			
Neighbor × Different Party	-.0019	-.0031	.0018	.0016
(proximity effect on bipartisanship)	[.66]	[.54]	[.69]	[.73]
	{.68}	{.56}	{.68}	{.71}
	<.78>	<.78>	<.78>	<.78>
Neighbor × Same Party	.013	.011	.011	.0097
	[.14]	[.29]	[.19]	[.29]
	{.12}	{.22}	{.18}	{.25}
	<.62>	<.68>	<.62>	<.68>
Observations	21589	21589	21589	21589
	Panel C: Previous Year (Placebo) (t-1)			
Neighbor × Different Party	.0017	.0025	.000015	-.0001
(proximity effect on bipartisanship)	[.65]	[.6]	[1]	[.98]
	{.72}	{.61}	{1}	{.98}
Neighbor × Same Party	.0035	.0029	-.00091	-.0023
	[.71]	[.78]	[.92]	[.81]
	{.67}	{.75}	{.89}	{.77}
Observations	21638	21638	21638	21638
Session × Party Pair × Strata FE	Y	Y	Y	Y
Outcome Mean	.46	2.3	.57	2.5
Outcome S.d.	.15	.32	.13	.17

Notes: Each panel shows the estimates from four linear regressions. Observations are weighted by the block-level inverse probability of treatment assignment, following Gerber and Green (2012). 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. Compliance is the proportion of times the two MPs in a pair vote the same way in a given session. 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 (1000 draws) are in curly brackets. Sharpened q-values (Anderson 2008) for non-placebo tests are in <>. Special sessions and a short session (2017) 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. 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

Table A13: Pair-Level Effects on Voting: Heterogeneity by Coalition

	Contemporaneous Effect (t)		One Year Later (t+1)		Previous Year (Placebo) (t-1)	
	Compliance (1)	Similarity (2)	Compliance (3)	Similarity (4)	Compliance (5)	Similarity (6)
Neighbor × Different Coalition	.0079	.0093	.0067	.004	.0053	.0056
	(.0043)	(.0038)	(.0057)	(.0056)	(.004)	(.0037)
	[.066]*	[.014]**	[.24]	[.48]	[.19]	[.13]
	{.029}**	{.013}**	{.15}	{.37}	{.25}	{.2}
	<.3>	<.13>	<.92>	<1>		
Neighbor × Same Coalition	.0017	.0034	.00019	.00096	-.0011	-.0015
	(.0041)	(.0041)	(.0057)	(.0061)	(.0056)	(.0058)
	[.68]	[.41]	[.97]	[.87]	[.84]	[.8]
	{.66}	{.38}	{.97}	{.84}	{.83}	{.77}
	<1>	<1>	<1>	<1>		
Same = Different	[.33]	[.33]	[.43]	[.72]	[.42]	[.36]
	{.3}	{.3}	{.4}	{.67}	{.38}	{.33}
Observations	35259	35259	21589	21589	21638	21638
Session × Party Pair × Strata FE	Y	Y	Y	Y	Y	Y
Outcome Mean	.57	2.5	.55	2.5	.57	2.5
Outcome S.d.	.13	.17	.12	.16	.12	.16

Notes: Compliance is the proportion of times the two MPs in a pair vote the same way in a given session. 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 Coalition is equal to one if both MPs in the pair are in the same coalition for that session. Dyadic-robust standard errors are in parentheses and dyadic-robust p-values are in square brackets. Randomization inference p-values (1000 draws) are in curly brackets. Sharpened q-values (Anderson 2008) for non-placebo tests are in <>. Special sessions and a short session (2017) 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. 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. *** p<0.01, ** p<0.05, * p<0.1.

Table A14: Pair-Level Effects on Contested Votes: Heterogeneity by Coalition

	Below 50th Votes		Below 25th Votes	
	Compliance (1)	Similarity (2)	Compliance (3)	Similarity (4)
	Panel A: Contemporaneous Effect (t)			
Neighbor × Different Coalition	.0085	.01	.0079	.0093
	[.018]**	[.0028]***	[.069]*	[.015]**
	{.036}**	{.026}**	{.029}**	{.013}**
	<.054>*	<.022>**	<.14>	<.054>*
Neighbor × Same Coalition	.01	.014	.0017	.0034
	[.015]**	[.001]***	[.68]	[.4]
	{.03}**	{.005}***	{.66}	{.38}
	<.054>*	<.017>**	<1>	<.75>
Observations	35205	35205	35259	35259
	Panel B: One Year Later (t+1)			
Neighbor × Different Coalition	-.00044	-.0058	.0067	.004
	[.94]	[.36]	[.24]	[.47]
	{.92}	{.25}	{.15}	{.37}
	<1>	<.75>	<.51>	<.75>
Neighbor × Same Coalition	.0039	.005	.00019	.00096
	[.54]	[.47]	[.97]	[.87]
	{.48}	{.4}	{.97}	{.84}
	<.81>	<.75>	<1>	<1>
Observations	21589	21589	21589	21589
	Panel C: Previous Year (Placebo) (t-1)			
Neighbor × Different Coalition	.0036	.0051	.0053	.0056
	[.39]	[.27]	[.19]	[.13]
	{.42}	{.32}	{.25}	{.2}
Neighbor × Same Coalition	.00024	-.00093	-.0011	-.0015
	[.97]	[.88]	[.85]	[.79]
	{.97}	{.87}	{.83}	{.77}
Observations	21638	21638	21638	21638
Session × Party Pair × Strata FE	Y	Y	Y	Y
Outcome Mean	.46	2.3	.57	2.5
Outcome S.d.	.15	.32	.13	.17

Notes: Each panel shows the estimates from four linear 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. Compliance is the proportion of times the two MPs in a pair vote the same way in a given session. 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 (1000 draws) are in curly brackets. Sharpened q-values (Anderson 2008) for non-placebo tests are in <>. Special sessions and a short session (2017) 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. 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 Coalition is equal to one if both MPs in the pair are in the same coalition for that session. Outcome Mean and Standard Deviation are for the sample included in the Panel A regressions. *** p<0.01, ** p<0.05, * p<0.1.

Table A15: Pair-Level Effects by Vote Type

	Contemporaneous Effect (t) on Similarity				
	Bill	Amendment	Document	Resolution	Other
	(1)	(2)	(3)	(4)	(5)
Neighbor × Different Party	.01	.0033	.0045	.013	.0073
(proximity effect on bipartisanship)	[<0.001]***	[.27]	[.12]	[.0042]***	[.011]**
	{.005}***	{.37}	{.17}	{.002}***	{.025}**
Neighbor × Same Party	.0061	.012	.0094	.0064	.016
	[.39]	[.18]	[.29]	[.49]	[.012]**
	{.36}	{.17}	{.23}	{.47}	{.032}**
Same = Different	[.58]	[.37]	[.6]	[.51]	[.25]
	{.62}	{.37}	{.58}	{.52}	{.31}
Observations	35205	35150	35151	35159	35256
Session × Party Pair × Strata FE	Y	Y	Y	Y	Y
Outcome Mean	2.5	2.4	2.4	2.5	2.5
Percentage of All Votes	22	32	34	7	6

Notes: Similarity is the average vote similarity between the two MPs in a pair. The results are shown separately for voting on the four most common vote categories, plus a residual category. The four main categories are: draft bills, amendments, resolutions or parliamentary resolutions, and parliamentary documents. 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 (1000 draws) are in curly brackets. Special sessions and a short session (2017) 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. 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. *** p<0.01, ** p<0.05, * p<0.1.

Table A16: Pair-Level Effects by Bill Topic

	Contemporaneous Effect (t) on Similarity										
	Industry (1)	Foreign (2)	Economic (3)	Health (4)	Law (5)	Educ. (6)	Community (7)	Transport (8)	Admin. (9)	Religion (10)	Environ. (11)
Neighbor × Different Party	.019	.015	.016	.0045	.022	.011	.014	.0016	.015	.021	.019
	[<0.001]***	[.0036]***	[<0.001]***	[.64]	[.0016]***	[.35]	[.0055]***	[.87]	[.12]	[.48]	[.082]*
	{.003}***	{.092}*	{.007}***	{.69}	{.012}**	{.28}	{.042}**	{.86}	{.16}	{.41}	{.05}*
Neighbor × Same Party	.005	-.002	.0063	.019	.013	.0066	.015	.02	-.0076	.092	-.0048
	[.53]	[.9]	[.52]	[.22]	[.2]	[.7]	[.2]	[.09]*	[.67]	[.014]**	[.69]
	{.67}	{.91}	{.55}	{.33}	{.35}	{.7}	{.19}	{.26}	{.66}	{.026}**	{.78}
Same = Different	[.11]	[.32]	[.35]	[.48]	[.43]	[.83]	[.94]	[.18]	[.26]	[.17]	[.19]
	{.32}	{.38}	{.42}	{.54}	{.57}	{.84}	{.93}	{.37}	{.25}	{.14}	{.22}
Observations	22,090	21,995	22,093	21,631	21,896	20,459	21,943	20,464	21,696	10,642	22,024
Session × Party Pair × Strata FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Outcome Mean	2.3	2.3	2.2	2.3	2.3	2.3	2.2	2.3	2.2	2.3	2.3
Percentage of All Bills	34	14	40	12	16	15	33	9.4	12	.79	14

Notes: Similarity is the average vote similarity between the two MPs in a pair. We include only the contested votes in which the share of MPs voting the modal vote is less than the median among all votes. The results are shown separately for voting on legislation belonging to the following categories: (1) Industry, (2) Foreign Relations, (3) Economic Management, (4) Health, (5) Law and Justice, (6) Education and Culture, (7) Community Issues, (8) Transport, (9) Administration and Local Governance, (10) Religion, and (11) Environment. Each piece of legislation may belong to more than one category (explaining why the percentage of all bills does not sum to 100%). Sample includes only 2001-02 session onwards as bill topic data only goes back to 2001-02. 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 (1000 draws) are in curly brackets. Special sessions and a short session (2017) 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. 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. *** p<0.01, ** p<0.05, * p<0.1.

Table A17: Is Influence Greater on Days With Many Votes?

	Contemporaneous Effect (t)		One Year Later (t+1)		Previous Year (Placebo) (t-1)	
	Similarity Busy (1)	Similarity Light (2)	Similarity Busy (3)	Similarity Light (4)	Similarity Busy (5)	Similarity Light (6)
Neighbor × Different Party	.007	.0082	-.0035	.0035	.00079	.00066
(proximity effect on bipartisanship)	[.046]**	[<0.001]***	[.5]	[.51]	[.89]	[.86]
	{.12}	{.008}***	{.55}	{.4}	{.89}	{.89}
Neighbor × Same Party	.0086	.0019	.0013	.018	.00022	.0042
	[.4]	[.77]	[.92]	[.051]*	[.99]	[.55]
	{.42}	{.77}	{.91}	{.028}**	{.99}	{.59}
Same = Different	[.89]	[.33]	[.74]	[.17]	[.97]	[.69]
	{.87}	{.39}	{.76}	{.12}	{.97}	{.7}
Observations	35024	35207	21482	21543	21626	21589
Session × Party Pair × Strata FE	Y	Y	Y	Y	Y	Y
Outcome Mean	2.5	2.5	2.5	2.5	2.5	2.5
Outcome S.d.	.24	.16	.23	.15	.23	.14

Notes: Similarity is the average vote similarity between the two MPs in a pair. The Busy measure considers only votes on days with at least 50 votes. The Light measure considers the remaining 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 (1000 draws) are in curly brackets. Special sessions and a short session (2017) 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. 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. *** p<0.01, ** p<0.05, * p<0.1.

Table A18: Pair-Level Effects on Voting: Heterogeneity by Gender

	Contemporaneous Effect (t)		One Year Later (t+1)		Previous Year (Placebo) (t-1)	
	Compliance (1)	Similarity (2)	Compliance (3)	Similarity (4)	Compliance (5)	Similarity (6)
Neighbor × Different Gender	.01	.012	-.0012	-.0021	.0014	.0019
	[.006]***	[<0.001]***	[.86]	[.75]	[.8]	[.71]
	{.017}**	{.005}***	{.82}	{.69}	{.76}	{.71}
Neighbor × Same Gender	-.00011	.00099	.0074	.0064	.0026	.002
	[.98]	[.8]	[.11]	[.21]	[.63]	[.7]
	{.98}	{.77}	{.11}	{.17}	{.58}	{.67}
Same = Different	[.06]*	[.04]**	[.32]	[.31]	[.9]	[.99]
	{.093}*	{.068}*	{.27}	{.28}	{.88}	{.99}
Observations	35259	35259	21589	21589	21638	21638
Same Gender Dummy	Y	Y	Y	Y	Y	Y
Session × Party Pair × Strata FE	Y	Y	Y	Y	Y	Y
Outcome Mean	.57	2.5	.55	2.5	.57	2.5
Outcome S.d.	.13	.17	.12	.16	.12	.16

Notes: Compliance is the proportion of times the two MPs in a pair vote the same way in a given session. 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 Gender is equal to one if both MPs in the pair have the same gender. Different Gender is equal to one minus Same Gender. Dyadic-robust p-values are in square brackets. Randomization inference p-values (1000 draws) are in curly brackets. Special sessions and a short session (2017) 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. 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. *** p<0.01, ** p<0.05, * p<0.1.

Table A19: How Do Neighbor Effects Depend on Ideological Distance? (Split By Median)

	Contemporaneous Effect (t)		One Year Later (t+1)		Previous Year (Placebo) (t-1)	
	Compliance (1)	Similarity (2)	Compliance (3)	Similarity (4)	Compliance (5)	Similarity (6)
Neighbor × Distant Other-Party	.011	.012	.0012	-.0007	.0018	.0024
	[.0082]***	[.0012]***	[.85]	[.91]	[.69]	[.58]
	{.011}**	{.005}***	{.84}	{.89}	{.73}	{.66}
Neighbor × Close Other-Party	-.0011	.0017	.00061	.0013	-.00016	.000039
	[.8]	[.67]	[.92]	[.83]	[.97]	[.99]
	{.78}	{.71}	{.91}	{.81}	{.98}	{.99}
Close = Distant	[.05]*	[.061]*	[.94]	[.8]	[.78]	[.73]
	{.069}*	{.12}	{.94}	{.82}	{.8}	{.79}
Observations	26599	26599	16252	16252	16185	16185
Session × Party Pair × Strata FE	Y	Y	Y	Y	Y	Y
Outcome Mean	.56	2.5	.54	2.5	.55	2.5
Outcome S.d.	.13	.16	.12	.16	.12	.15

Notes: Sample includes only different-party pairs. Compliance is the proportion of times the two MPs in a pair vote the same way in a given session. 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. Distant Other-Party is a dummy variable equal to one if the difference between the left-right score (from ParlGov) of the two parties in a pair is above-median. Close Other-Party is a dummy variable equal to one if the difference is below-median. Dyadic-robust p-values are in square brackets. Randomization inference p-values (1000 draws) are in curly brackets. Special sessions and a short session (2017) 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. 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. *** p<0.01, ** p<0.05, * p<0.1.

Table A20: How Do Neighbor Effects Depend on Ideological Distance? (Split Into Terciles)

	Contemporaneous Effect (t)		One Year Later (t+1)		Previous Year (Placebo) (t-1)	
	Compliance (1)	Similarity (2)	Compliance (3)	Similarity (4)	Compliance (5)	Similarity (6)
Neighbor × Distant Other-Party	.013	.013	.00096	-.002	-.00034	-.00013
	[.016]**	[.0053]***	[.89]	[.77]	[.95]	[.98]
	{.006}***	{.005}***	{.88}	{.73}	{.95}	{.97}
Neighbor × Middle Other-Party	.0019	.0034	.0018	.0031	.0007	.00098
	[.69]	[.44]	[.82]	[.7]	[.91]	[.88]
	{.73}	{.56}	{.79}	{.69}	{.91}	{.9}
Neighbor × Close Other-Party	-.0019	.003	-.00061	-.00082	.0034	.0043
	[.76]	[.6]	[.94]	[.91]	[.53]	[.42]
	{.74}	{.6}	{.94}	{.89}	{.67}	{.56}
Observations	26599	26599	16252	16252	16185	16185
Session × Party Pair × Strata FE	Y	Y	Y	Y	Y	Y
Outcome Mean	.56	2.5	.54	2.5	.55	2.5
Outcome S.d.	.13	.16	.12	.16	.12	.15

Notes: Sample includes only different-party pairs. Compliance is the proportion of times the two MPs in a pair vote the same way in a given session. 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. Distant Other-Party is a dummy variable equal to one if the difference between the left-right score (from ParlGov) of the two parties in a pair is in the top tercile. Middle Other-Party is a dummy variable for the middle tercile, Close Other-Party is a dummy variable for the bottom tercile. Dyadic-robust p-values are in square brackets. Randomization inference p-values (1000 draws) are in curly brackets. Special sessions and a short session (2017) 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. 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. *** p<0.01, ** p<0.05, * p<0.1.

Table A21: Pair-Level Effects Before and After the Economic Crisis

	Compliance		Similarity	
	Before 2009/10	2009/10 Onwards	Before 2009/10	2009/10 Onwards
	(1)	(2)	(3)	(4)
Neighbor × Different Party	.0075	.00076	.0078	.0056
(proximity effect on bipartisanship)	[.037]**	[.86]	[.021]**	[.19]
	{.029}**	{.86}	{.018}**	{.26}
Neighbor × Same Party	-.00057	.014	.00037	.012
	[.93]	[.19]	[.96]	[.34]
	{.94}	{.27}	{.96}	{.37}
Same = Different	[.24]	[.25]	[.3]	[.63]
	{.35}	{.35}	{.39}	{.66}
Observations	22907	12352	22907	12352
Session × Party Pair × Strata FE	Y	Y	Y	Y
Outcome Mean	.57	.58	2.5	2.5
Outcome S.d.	.11	.16	.12	.22

Notes: Compliance is the proportion of times the two MPs in a pair vote the same way in a given session. 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 (1000 draws) are in curly brackets. Special sessions and a short session (2017) are excluded. 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. *** p<0.01, ** p<0.05, * p<0.1.

Table A22: Effects of Other-Party Neighbors on Rebellious Voting (Reweighted)

	Contemporaneous Effect (t)		One Year Later (t+1)		Previous Year (Placebo) (t-1)	
	Leader Non-Compliance (1)	Rebel Rate (2)	Leader Non-Compliance (3)	Rebel Rate (4)	Leader Non-Compliance (5)	Rebel Rate (6)
Proportion Other-Party Neighbor	-.004	-.00051	-.01	.00049	.0073	-.0009
	(.009)	(.00059)	(.011)	(.00057)	(.0089)	(.0006)
	[.66]	[.39]	[.35]	[.39]	[.41]	[.14]
	{.63}	{.41}	{.34}	{.46}	{.5}	{.15}
	<1>	<1>	<1>	<1>		
Observations	1294	1294	826	826	835	835
Session × Party × Strata FE	Y	Y	Y	Y	Y	Y
Outcome Mean	.42	.005	.44	.0044	.43	.005
Outcome S.d.	.13	.011	.11	.01	.11	.0073

Notes: Observations are weighted by the block-level inverse probability of treatment assignment, following Gerber and Green (2012). Leader Non-Compliance is the proportion of times the MP votes differently from their party leader in a given session. Rebel Rate is the proportion of times the MP voted yes/abstain (no/abstain) when their party leader voted no (yes) in a given session. Proportion Other-Party Neighbor is the proportion of left-right seating neighbors from a different party. MP-clustered standard errors are in parentheses and p-values are in square brackets. Randomization inference p-values (1000 draws) are in curly brackets. Sharpened q-values (Anderson 2008) for non-placebo tests are in <>. Special sessions and a short session (2017) 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. Strata FE is a dummy variable for whether MP was pre-assigned a seat. *** p<0.01, ** p<0.05, * p<0.1.

Table A23: Are Outparty Exposure Effects Larger For the Inexperienced?

	Contemporaneous Effect (t)		One Year Later (t+1)		Previous Year (Placebo) (t-1)	
	Leader Non-Compliance (1)	Rebel Rate (2)	Leader Non-Compliance (3)	Rebel Rate (4)	Leader Non-Compliance (5)	Rebel Rate (6)
Proportion Other-Party Neighbor	-.012	-.0018*	-.00072	.00045	-.0042	-.000014
	(.012)	(.00099)	(.014)	(.0007)	(.014)	(.00076)
Proportion Other-Party Neighbor × Experience	.0018*	.00014*	.00024	-.000036	.0019*	-.000044
	(.001)	(.000074)	(.0016)	(.000068)	(.0011)	(.00007)
Experience	-.00017	-.00005	.00079	.000076	-.00016	.000067
	(.00085)	(.000055)	(.0016)	(.000057)	(.00087)	(.000049)
Observations	1294	1294	826	826	835	835
Session × Party × Strata FE	Y	Y	Y	Y	Y	Y
Outcome Mean	.42	.005	.44	.0044	.43	.005

Notes: Leader Non-Compliance is the proportion of times the MP votes differently from their party leader in a given session. Rebel Rate is the proportion of times the MP voted yes/abstain (no/abstain) when their party leader voted no (yes) in a given session. Proportion Other-Party Neighbor is the proportion of left-right seating neighbors from a different party. Experience is the number of sessions since first session as Althingi member. MP-clustered standard errors are in parentheses. Special sessions and a short session (2017) 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. Strata FE is a dummy variable for whether MP was pre-assigned a seat. *** p<0.01, ** p<0.05, * p<0.1.

Table A24: Are Pair-Level Effects Larger When MP Pairs Differ in Experience?

	Contemporaneous Effect (t)		One Year Later (t+1)		Previous Year (Placebo) (t-1)	
	Compliance (1)	Similarity (2)	Compliance (3)	Similarity (4)	Compliance (5)	Similarity (6)
Neighbor × Different Experience	.0051	.0064	.0061	.0062	.0094	.0091
	[.19]	[.073]*	[.12]	[.12]	[.021]**	[.019]**
	{.19}	{.11}	{.2}	{.2}	{.06}*	{.069}*
Neighbor × Same Experience	.005	.0067	.0013	-.00057	-.004	-.004
	[.14]	[.049]**	[.81]	[.92]	[.37]	[.36]
	{.14}	{.055}*	{.78}	{.9}	{.36}	{.36}
Same = Different	[.98]	[.95]	[.37]	[.25]	[.047]**	[.032]**
	{.99}	{.96}	{.5}	{.35}	{.066}*	{.078}*
Observations	35259	35259	21589	21589	21638	21638
Same Experience Dummy	Y	Y	Y	Y	Y	Y
Session × Party Pair × Strata FE	Y	Y	Y	Y	Y	Y
Outcome Mean	.57	2.5	.55	2.5	.57	2.5
Outcome S.d.	.13	.17	.12	.16	.12	.16

Notes: Compliance is the proportion of times the two MPs in a pair vote the same way in a given session. 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 Experience is equal to one if the difference in political experience between the two MPs in the pair is five sessions or less. Different Experience is equal to one if the difference in experience is more than five sessions. Dyadic-robust p-values are in square brackets. Randomization inference p-values (1000 draws) are in curly brackets. Special sessions and a short session (2017) 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. 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. *** p<0.01, ** p<0.05, * p<0.1.

Table A25: Effects on Rebellious Voting: Alternative Outcomes

	Contemporaneous Effect (t)		One Year Later (t+1)		Previous Year (Placebo) (t-1)	
	Party Non-Compliance (1)	Leader Diff. (2)	Party Non-Compliance (3)	Leader Diff. (4)	Party Non-Compliance (5)	Leader Diff. (6)
Proportion Other-Party Neighbor	.0079	.0021	-.0015	.0019	.013	.012
	(.011)	(.008)	(.014)	(.01)	(.014)	(.0098)
	[.47]	[.8]	[.91]	[.85]	[.35]	[.23]
	{.48}	{.79}	{.92}	{.85}	{.33}	{.23}
Observations	1294	1294	826	826	835	835
Session × Party × Strata FE	Y	Y	Y	Y	Y	Y
Outcome Mean	.24	.43	.26	.45	.25	.45
Outcome S.d.	.13	.13	.12	.11	.13	.11

Notes: Party Non-Compliance is the proportion of times the MP votes differently from the modal vote in their party in a given session. Leader Diff. is the average vote difference score between the MP and their party leader. Proportion Other-Party Neighbor is the proportion of left-right seating neighbors from a different party. MP-clustered standard errors are in parentheses and p-values are in square brackets. Randomization inference p-values (1000 draws) are in curly brackets. Special sessions and a short session (2017) 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. Strata FE is a dummy variable for whether MP was pre-assigned a seat. *** p<0.01, ** p<0.05, * p<0.1.

Table A26: Effects on Rebellious Voting on Contested Votes

	Below 50th Votes		Below 25th Votes	
	Leader Non-Compliance (1)	Rebel Rate (2)	Leader Non-Compliance (3)	Rebel Rate (4)
	Panel A: Contemporaneous Effect (t)			
Proportion Other-Party Neighbor	-.001	-.00036	.0028	-.00061
	(.0089)	(.00082)	(.0076)	(.00057)
	[.91]	[.66]	[.71]	[.29]
	{.9}	{.67}	{.71}	{.31}
Observations	1292	1292	1294	1294
	Panel B: One Year Later (t+1)			
Proportion Other-Party Neighbor	-.0017	-.00018	.0014	.00017
	(.012)	(.00092)	(.0098)	(.00051)
	[.89]	[.84]	[.89]	[.73]
	{.88}	{.88}	{.89}	{.77}
Observations	825	825	826	826
	Panel C: Previous Year (Placebo) (t-1)			
Proportion Other-Party Neighbor	.014	-.00041	.012	-.00049
	(.011)	(.0011)	(.0097)	(.00054)
	[.19]	[.71]	[.2]	[.37]
	{.19}	{.68}	{.19}	{.37}
Observations	835	835	835	835
Session × Party × Strata FE	Y	Y	Y	Y
Outcome Mean	.45	.0078	.42	.005
Outcome S.d.	.13	.013	.13	.011

Notes: Each panel shows the estimates from four linear 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. Rebel Rate is the proportion of times the MP voted yes/abstain (no/abstain) when their party leader voted no (yes) in a given session. Proportion Other-Party Neighbor is the proportion of left-right seating neighbors from a different party. MP-clustered standard errors are in parentheses and p-values are in square brackets. Randomization inference p-values (1000 draws) are in curly brackets. Special sessions and a short session (2017) 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. Strata FE is a dummy variable for whether MP was pre-assigned a seat. Outcome Mean and Standard Deviation are for the sample included in the Panel A regressions. *** p<0.01, ** p<0.05, * p<0.1.

Table A27: Effects of Other-Coalition Exposure on Rebellious Voting

	Contemporaneous Effect (t)		One Year Later (t+1)		Previous Year (Placebo) (t-1)	
	Leader Non-Compliance (1)	Rebel Rate (2)	Leader Non-Compliance (3)	Rebel Rate (4)	Leader Non-Compliance (5)	Rebel Rate (6)
Proportion Other-Coalition Neighbor	-.002	.00039	-.0025	.00087	.0095	-.000067
	(.0068)	(.00046)	(.0077)	(.00065)	(.0078)	(.00045)
	[.77]	[.4]	[.74]	[.18]	[.23]	[.88]
	{.76}	{.51}	{.78}	{.27}	{.28}	{.89}
	<1>	<1>	<1>	<1>		
Observations	1294	1294	826	826	835	835
Session × Party × Strata FE	Y	Y	Y	Y	Y	Y
Outcome Mean	.42	.005	.44	.0044	.43	.005
Outcome S.d.	.13	.011	.11	.01	.11	.0073

Notes: Leader Non-Compliance is the proportion of times the MP votes differently from their party leader in a given session. Rebel Rate is the proportion of times the MP voted yes/abstain (no/abstain) when their party leader voted no (yes) in a given session. Proportion Other-Coalition Neighbor is the proportion of left-right seating neighbors from a different coalition. MP-clustered standard errors are in parentheses and p-values are in square brackets. Randomization inference p-values (1000 draws) are in curly brackets. Sharpened q-values (Anderson 2008) for non-placebo tests are in <>. Special sessions and a short session (2017) 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. Strata FE is a dummy variable for whether MP was pre-assigned a seat. *** p<0.01, ** p<0.05, * p<0.1.

Table A28: Effects on Rebellious Voting by Intensity of Contact

	Contemporaneous Effect (t)		One Year Later (t+1)		Previous Year (Placebo) (t-1)	
	Leader Non-Compliance (1)	Rebel Rate (2)	Leader Non-Compliance (3)	Rebel Rate (4)	Leader Non-Compliance (5)	Rebel Rate (6)
Proportion Other-Party Neighbor = 1/2	.0078	.00069	-.014	.00015	.0041	.00031
	[.4]	[.36]	[.28]	[.7]	[.66]	[.54]
	{.42}	{.25}	{.2}	{.81}	{.69}	{.62}
Proportion Other-Party Neighbor = 1	.0057	-.00016	-.0047	.0002	.012	-.00024
	[.49]	[.74]	[.67]	[.66]	[.2]	[.62]
	{.49}	{.78}	{.65}	{.74}	{.24}	{.69}
Observations	1294	1294	826	826	835	835
Session × Party × Strata FE	Y	Y	Y	Y	Y	Y
Outcome Mean	.42	.005	.44	.0044	.43	.005
Outcome S.d.	.13	.011	.11	.01	.11	.0073

Notes: Leader Non-Compliance is the proportion of times the MP votes differently from their party leader in a given session. Rebel Rate is the proportion of times the MP voted yes/abstain (no/abstain) when their party leader voted no (yes) in a given session. Proportion Other-Party Neighbor is the proportion of left-right seating neighbors from a different party. MP-clustered p-values are in square brackets. Randomization inference p-values (1000 draws) are in curly brackets. Special sessions and a short session (2017) 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. Strata FE is a dummy variable for whether MP was pre-assigned a seat. *** p<0.01, ** p<0.05, * p<0.1.

Table A29: Effects of Other-Coalition Exposure by Intensity of Contact

	Contemporaneous Effect (t)		One Year Later (t+1)		Previous Year (Placebo) (t-1)	
	Leader Non-Compliance (1)	Rebel Rate (2)	Leader Non-Compliance (3)	Rebel Rate (4)	Leader Non-Compliance (5)	Rebel Rate (6)
Proportion Other-Coalition Neighbor = 1/2	.00046	.00091	-.0052	.001	-.0066	-.00064
	[.95]	[.07]*	[.53]	[.22]	[.4]	[.17]
	{.94}	{.089}*	{.51}	{.15}	{.39}	{.17}
Proportion Other-Coalition Neighbor = 1	-.002	.00038	-.0023	.00084	.0099	-.000046
	[.77]	[.42]	[.77]	[.18]	[.21]	[.92]
	{.76}	{.51}	{.79}	{.29}	{.26}	{.93}
Observations	1294	1294	826	826	835	835
Session × Party × Strata FE	Y	Y	Y	Y	Y	Y
Outcome Mean	.42	.005	.44	.0044	.43	.005
Outcome S.d.	.13	.011	.11	.01	.11	.0073

Notes: Leader Non-Compliance is the proportion of times the MP votes differently from their party leader in a given session. Rebel Rate is the proportion of times the MP voted yes/abstain (no/abstain) when their party leader voted no (yes) in a given session. Proportion Other-Coalition Neighbor is the proportion of left-right seating neighbors from a different coalition. MP-clustered p-values are in square brackets. Randomization inference p-values (1000 draws) are in curly brackets. Special sessions and a short session (2017) 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. Strata FE is a dummy variable for whether MP was pre-assigned a seat. *** p<0.01, ** p<0.05, * p<0.1.

Table A30: Effects on Absence and Abstention

	Contemporaneous Effect (t)		One Year Later (t+1)		Previous Year (Placebo) (t-1)	
	Absent (1)	Abstain (2)	Absent (3)	Abstain (4)	Absent (5)	Abstain (6)
Proportion Other-Party Neighbor	.012	-.0019	-.000016	-.0045	.0043	.00066
	(.014)	(.0014)	(.018)	(.0019)	(.018)	(.0013)
	[.41]	[.18]	[1]	[.018]**	[.81]	[.61]
	{.36}	{.13}	{1}	{.004}***	{.79}	{.6}
Observations	1294	1294	826	826	835	835
Session × Party × Strata FE	Y	Y	Y	Y	Y	Y
Outcome Mean	.27	.032	.29	.032	.27	.031
Outcome S.d.	.16	.046	.15	.048	.15	.04

Notes: Absent is the proportion of times the MP was absent for a vote in a given session. Abstain is the proportion of times the MP abstained from voting in a given session. Proportion Other-Party Neighbor is the proportion of left-right seating neighbors from a different party. MP-clustered standard errors are in parentheses and p-values are in square brackets. Randomization inference p-values (1000 draws) are in curly brackets. Special sessions and a short session (2017) 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. Strata FE is a dummy variable for whether MP was pre-assigned a seat. *** p<0.01, ** p<0.05, * p<0.1.

Table A31: Pair-Level Effects on Co-Sponsorship Links with Undivided Attention

	Contemporaneous Effect (t)		One Year Later (t+1)		Previous Year (Placebo) (t-1)	
	Number (1)	IHS (2)	Number (3)	IHS (4)	Number (5)	IHS (6)
Neighbor × Corner	-.07	.0015	.29	.089	-.14	.038
	[.61]	[.98]	[.075]*	[.12]	[.48]	[.6]
	{.67}	{.98}	{.15}	{.17}	{.45}	{.56}
	<1>	<1>	<.92>	<.92>		
Neighbor × Middle	-.0085	-.0071	.043	.011	-.0051	.0055
	[.92]	[.8]	[.71]	[.78]	[.95]	[.89]
	{.9}	{.76}	{.69}	{.76}	{.95}	{.88}
	<1>	<1>	<1>	<1>		
Corner = Middle	[.62]	[.87]	[.071]*	[.16]	[.55]	[.7]
	{.7}	{.87}	{.3}	{.3}	{.51}	{.67}
Observations	22687	22687	15172	15172	15130	15130
Session × Corner FE	Y	Y	Y	Y	Y	Y
Session × Party Pair FE	Y	Y	Y	Y	Y	Y
Outcome Mean	1.9	.98	1.9	.97	1.8	.93
Outcome S.d.	2.7	.94	3	.97	2.6	.92

Notes: Regressions include different-party dyads only, with neither MP pre-assigned seats. Number is the total number of co-sponsorship links between the two MPs in a pair in a given session. IHS is the inverse hyperbolic sine transformation of 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 pair has only one seating neighbor. Middle is equal to one minus Corner. Dyadic-robust p-values are in square brackets. Randomization inference p-values (1000 draws) are in curly brackets. Sharpened q-values (Anderson 2008) for non-placebo tests are in <>. Special sessions and a short session (2017) 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. *** p<0.01, ** p<0.05, * p<0.1.

Table A32: Effects on Bipartisan Co-Sponsorship Links (Reweighted)

	Contemporaneous Effect (t)		One Year Later (t+1)		Previous Year (Placebo) (t-1)	
	Number (1)	IHS (2)	Number (3)	IHS (4)	Number (5)	IHS (6)
Proportion Other-Party Neighbor	-.77	.02	10	.096	5.4	.039
	(3.4)	(.079)	(4.7)	(.11)	(3.7)	(.11)
	[.82]	[.8]	[.03]**	[.38]	[.14]	[.72]
	{.86}	{.83}	{.052}*	{.45}	{.29}	{.74}
	<1>	<1>	<.14>	<1>		
Observations	1420	1420	941	941	946	946
Session × Party × Strata FE	Y	Y	Y	Y	Y	Y
Outcome Mean	82	4.7	83	4.5	76	4.5
Outcome S.d.	76	1.1	82	1.3	73	1.2

Notes: Observations are weighted by the block-level inverse probability of treatment assignment, following Gerber and Green (2012). Number is the total number of co-sponsorship links between the MP and any other-party MP in a given session. IHS is the inverse hyperbolic sine transformation of Number. Proportion Other-Party Neighbor is the proportion of left-right seating neighbors from a different party. MP-clustered standard errors are in parentheses and p-values are in square brackets. Randomization inference p-values (1000 draws) are in curly brackets. Special sessions and a short session (2017) 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. Strata FE is a dummy variable for whether MP was pre-assigned a seat. *** p<0.01, ** p<0.05, * p<0.1.

Table A33: Effects on Bipartisan Co-Sponsorship Links by Intensity of Contact

	Contemporaneous Effect (t)		One Year Later (t+1)		Previous Year (Placebo) (t-1)	
	Number	IHS	Number	IHS	Number	IHS
	(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]
	{.19}	{.13}	{.14}	{.76}	{.33}	{.74}
Proportion Other-Party Neighbor = 1	3.5	.095	11	.17	5.7	.068
	[.36]	[.22]	[.013]**	[.19]	[.12]	[.41]
	{.39}	{.26}	{.029}**	{.17}	{.24}	{.59}
Observations	1420	1420	941	941	946	946
Session × Party × Strata FE	Y	Y	Y	Y	Y	Y
Outcome Mean	82	4.7	83	4.5	76	4.5
Outcome S.d.	76	1.1	82	1.3	73	1.2

Notes: Number is the total number of co-sponsorship links between the MP and any other-party MP in a given session. IHS is the inverse hyperbolic sine transformation of Number. Proportion Other-Party Neighbor is the proportion of left-right seating neighbors from a different party. MP-clustered p-values are in square brackets. Randomization inference p-values (1000 draws) are in curly brackets. Special sessions and a short session (2017) 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. Strata FE is a dummy variable for whether MP was pre-assigned a seat. *** p<0.01, ** p<0.05, * p<0.1.

Table A34: Effects of Other-Coalition Exposure on Bipartisan Co-Sponsorship Links

	Contemporaneous Effect (t)		One Year Later (t+1)		Previous Year (Placebo) (t-1)	
	Number	IHS	Number	IHS	Number	IHS
	(1)	(2)	(3)	(4)	(5)	(6)
Proportion Other-Coalition Neighbor	3.2	.028	9	.15	6.1	.044
	(3.3)	(.056)	(4.5)	(.089)	(4.5)	(.082)
	[.33]	[.62]	[.047]**	[.1]	[.18]	[.59]
	{.29}	{.65}	{.046}**	{.14}	{.13}	{.61}
	<.29>	<.45>	<.23>	<.23>		
Observations	1420	1420	941	941	946	946
Session × Party × Strata FE	Y	Y	Y	Y	Y	Y
Outcome Mean	82	4.7	83	4.5	76	4.5
Outcome S.d.	76	1.1	82	1.3	73	1.2

Notes: Number is the total number of co-sponsorship links between the MP and any other-party MP in a given session. IHS is the inverse hyperbolic sine transformation of Number. Proportion Other-Coalition Neighbor is the proportion of left-right seating neighbors from a different coalition. MP-clustered standard errors are in parentheses and p-values are in square brackets. Randomization inference p-values (1000 draws) are in curly brackets. Special sessions and a short session (2017) 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. Strata FE is a dummy variable for whether MP was pre-assigned a seat. *** p<0.01, ** p<0.05, * p<0.1.

Table A35: Do Effects of Other-Coalition Exposure Compound?

	Voting		Co-Sponsorship	
	Leader Non-Compliance (1)	Rebel Rate (2)	Number (3)	IHS (4)
Proportion Other-Coalition Neighbor	-.00075	-.00037	-6	-.095
	(.015)	(.00068)	(6.4)	(.11)
Proportion Other-Coalition Neighbor (t-1)	.000063	-.00067	.94	.0058
	(.012)	(.00055)	(6)	(.095)
Prop. Oth.-Coalition Neigh. $\times$ Prop. Oth.-Coalition Neigh. (t-1)	-.0053	.0019*	18*	.26
	(.022)	(.0011)	(9.4)	(.17)
Observations	840	840	924	924
Session $\times$ Party $\times$ Strata FE	Y	Y	Y	Y
Outcome Mean	.44	.0044	85	4.7

Notes: Leader Non-Compliance is the proportion of times the MP votes differently from their party leader in a given session. Rebel Rate is the proportion of times the MP voted yes/abstain (no/abstain) when their party leader voted no (yes) in a given session. Number is the total number of co-sponsorship links between the MP and any other-party MP in a given session. IHS is the inverse hyperbolic sine transformation of Number. Proportion Other-Coalition Neighbor is the proportion of left-right seating neighbors from a different coalition. MP-clustered standard errors are in parentheses. Special sessions and a short session (2017) are excluded. Sessions are also dropped where lag would be a special/short session or a session in a different parliamentary term. Strata FE is a dummy variable for whether MP was pre-assigned a seat. *** p<0.01, ** p<0.05, * p<0.1.

B Expert Survey

B.1 Script

We received ethics approval from UBC (#H23-00154) to contact Icelandic MPs with a short survey. We successfully emailed 64 sitting main and deputy MPs using the email addresses listed on the *Althingi* website here. We also successfully contacted 36 ex-MPs elected since 2013, by email, LinkedIn, and social media. 19% (12) of sitting MPs and 6% (2) of ex-MPs gave answers. Our email included three questions to provide short qualitative answers to three questions. The full script is as follows:

Dear [MP name],

I am an Assistant Professor of Economics at the University of British Columbia, and I have written a paper on the effects of the random seating arrangement in the Alþingi. This paper has been invited for resubmission at a leading political science journal (the Journal of Politics).

To check that I am not misrepresenting how the Alþingi functions, I wondered whether you would have a few minutes to share your expert thoughts on the three questions below? Since your time is valuable, as a small thank you, I will give 3,000 ISK to a charity of your choice for your thoughts.

1. How much do MPs assigned to sit next to each other interact, and what types of interactions do they have? For example: is it common for seating neighbours to share conversations on voting days or otherwise, do seating neighbours ever become close friends and interact outside of the chamber, etc.

2. If you had to guess, how do you think an MP might influence the voting (if only a little bit) of another MP that sits next to them?

3. In American politics, co-sponsorship networks are often used to proxy for social networks. Is that a reasonable assumption in the Icelandic context? For example, do co-sponsors of legislation in the Alþingi tend to spend time working together, or might they not even interact at all?

Note: while your answers may be quoted in the paper, your identity would be kept anonymous, with the quote attributed to "an ex-MP."

Thank you for helping with my research!

Best,

Matt Lowe

mattlowe.site

Study Title: Legislature Integration and Bipartisanship: Expert Survey

If you have any concerns or complaints about your rights as a research participant and/or your experiences while participating in this study, contact the Research Participant Complaint Line in the UBC Office of Research Ethics at 604-822-8598 or if long distance e-mail RSIL@ors.ubc.ca or call toll free 1-877-822-8598.

B.2 Anonymized Responses

We report the responses to each of the three questions in full below, with a randomly assigned ID uniquely identifying a given MP's answers across questions. Asterisks indicate responses from ex-MPs, while all other responses are from sitting MPs.

1. **How much do MPs assigned to sit next to each other interact, and what types of interactions do they have? For example: is it common for seating neighbours to share conversations on voting days or otherwise, do seating neighbours ever become close friends and interact outside of the chamber, etc.**

(1) The amount of and nature of interactions depends on the people in question. People with a similar outlook, irrespective of party, tend talk [sic] and "joke around" more than those with conflicting views. This is by no means a rule though, a bigger factor is the personality of the MP's sitting together. Obviously talkative or outgoing people will converse more than others.

MP's often have a personal affinity with people they disagree with politicly [sic] (even more than with their own party members (competitors)). The seating arrangement is how ever [sic] not a big factor in this regard. Such friendships are more often the result of communications outside

the chamber, in committees, on travels, over coffee etc. Hence MP's that already like each other will make use of their proximity rather than the seating arrangement being a deciding factor.

(2) The interaction depends on people and personality, but overall there is an amicable interaction between parliamentarians sitting next to each other across party lines. Some voting days may be a bit fraught but MPs take care not to make it personal while sitting in their assigned seats. Sometimes you get to know someone well sitting next to you who you would otherwise not be in contact with, as you might not be sitting on the some [sic] committees or be from different parties.

(3) It is only my 2nd year in Alþingi so I only have my experience to share and those I can watch from my seat. I would say it varies. You can end up sitting with someone from your party and that colours the interactions. I would say that we talk during voting days for sure, when we don't have to be quiet not to miss out on anything. Some of the MPs have had more collision with other MP sand [sic] interactions with Ministers (who are this term all MPs) are usually more formal. It is quite common for MPs to form friendships throughout party lines. May [sic] there is some division between those in government and those in opposition each term. We are a nation of only 370 thousand so some of us have even had close ties before being elected.

(4) Friendly conversation mostly smal [sic] talk

(5) To sum up, I would say the atmosphere in the parliament is professional and friendly, in general at least.

(6) We "chat" (whisper) things to each other while other MPs are at the podium speaking, or challenging what they are saying. We will also often give each other positive feedback after a neighbor has given a speech or asked a minister a good challenging question. In my case both my neighbors, like me are from opposition parties, so I guess the interaction is different when you have someone from the coalition party sitting next to you. Last year, I did have one coalition member sitting next to me and since we also shared a committee we got to know each other much better - and that then also led to us working closer on finding common grounds on some bills being discussed in the committee. On voting days, the interaction is not much different, we usually vote more as a party group rather than being influenced by someone sitting next to you.

The seating is changed every year (draw of a ball from a box), so you get to know new neighbors each year. I am sure some people become good friends, but I think it is more common that people at least become a bit closer - which then enables them to do better discussions with each other outside of the chamber.

(7) Not much. MPs generally don't sit much in the chamber during discussions and since it's a small chamber whispering interrupts the proceedings. I would say seating arrangement has little to no impact outside of the chamber.

(8) Alþingi is small, only 63 MPs and it is inevitable that we get to know people and become friendly. It is also inevitable that we like some people more than others. I have never felt that who I'm sat next to during voting has any effect as the time spent voting in assembly is limited. If I like the person sitting next to me I'll chat with them, otherwise not. Much more of our time is spent in committee meetings. The MPs we get to know the best are the ones we work with them ost [sic] in committees or other groups.

(9) Lot of chat, sometimes about the issue sometimes just friendly chat. We interact a lot outside of the chamber and important to have broader support with Your [sic] ideas than the majority. It is different between politicians though but parliamentarians have greater connection between themselves than ministers can. They aren't as much in the parliaments even though they are parliamentarians. As a parliamentarian before I was appointed minister I had good friendship with MPs from the minority.

(10) My party forms part of the minority/opposition in parliament. I have members of other parties in the opposition on both sides now, but before I had members of the majority on both sides (we draw again at the beginning of every session, for approximately one year). The members of the majority rarely attend parliament sessions, mainly for voting, but when they are there we may exchange jokes and friendly comments, but not too much so as not to interrupt the ongoing discussion (the room is too small and echo-ing for much conversation outside the podium). I feel little difference whether my neighbors are from "friendly" parties or not.

(11) It really depends on the relationship between the members how much they interact. I have been very interactive with some who sit next to me, others I barely speak to. The parliament

room is very small, so often you are rather speaking with the person sitting in front of you or behind you if you have a friendlier relationship with them. Personally, I can not attribute sitting next to someone as the basis for our friendship. Often there is chitchat between people sitting next to each other.

(12) From my experience, seating neighbours mainly chit chat and joke about what is going on in the hall. When the conversation turns towards the voting itself, it is not about the politics but practical questions ('ah we're voting about this particular section now' and so on). The peer pressure and the endeavour to influence the vote of other MPs would be within each parliamentary party via internet chats (but most of it has hapoened [sic] before the vote itself of course). One thing: the Parliament room is incredibly small, almost like a class room. You are close to the obe [sic] sitting next to you, but probably an MP from your party sits right behind you or in front of you, so the political discussion would rather be amongst you and them.

(13*) Depends on the person you're sitting next to. If you personally like the person you're sitting next to you'll interact quite a lot. Mostly on non-political stuff. Everyday kind of things like you would a co-worker. Jokes are quite common.

(14*) Usually, the MPs do not spend that much time in the main chamber. During longer plenary discussion mostly just the couple of MPs actively taking part in the discussion are present. Others are in their offices og [sic] in party meeting rooms. So you do not necessarily spend that much time with your assigned neighbour. This is different during voting sessions when most MPs are actually in the room. But then the communication is mostly limited to one-line sentences ranging from "what item is this again?" to political trash-talk. I usually did not find these randomly assigned neighbours becoming close friends or interacting outside the chamber, unless they are good buddies to begin with.

2. **If you had to guess, how do you think an MP might influence the voting (if only a little bit) of another MP that sits next to them?**

(1) Seating arrangements have virtually no influence on how MP's vote. Usually, they are following the party line and once in a while strong convictions that go against the party. The exception is when MP's point out that the person sitting next to them has forgotten to vote or voted the

"wrong" way, i.e., made an obvious mistake by pressing the wrong button. A joke might be made about whether someone is really going to vote a certain way, but no one tries to use the proximity to influence the vote.

(2) There is very little "cross-contamination" when it comes to voting. The parliamentary groups have in almost all cases taken a joint decision on how they are going to vote beforehand.

(3) I would think that is minimum and to my knowledge MPs usually don't discuss how they are voting. You can see it on a table on the wall and it is quite common for MPs to go up and explain how they are voting. MPs also usually vote according to party lines so. . . .

(4) Never any influence on voting.

(5) To this date I have not cast a vote on a legislation as my participation has only been about the parliament schedule – i.e. if the president of the parliament suggests for a lengthened working day or if an MP suggests for a legislation to be sent back to a committee. These votes can become political. Our party chairman gives out a suggestion of a stance (yes, no, no vote) via Signal, and we usually follow suit. I share very limited interaction with my seating neighbors, although it is an amiable environment. It is hard to imagine an influence in that matter, though not impossible, but my experience is lacking in this matter.

(6) I think it is very unlikely it influences the voting – the party whips are more in control of what you are told to vote each time. Very few parties are like we in the Pirate Party where we don't have to follow the party line if our conscious tells us otherwise.

(7) Not at all.

(8) I dont believe seeting [sic] in any way influences the votes. Votes are always decided before hand during party meetings.

(9) Not a lot, but ofcourse [sic] close talk will always help and could get majority extra support.

(10) Not at all. Voting is mainly determined by each party beforehand and not much decided in the room itself. My party may be a bit different in this respect (we are the weird quirky ones) but my vote could be influenced by something said by another MP giving a formal comment from

the podium during the voting, or by a comment made from anywhere in the room, but no [sic] rather from a person sitting next to me than others.

(11) I think the biggest influence is helping in keeping track of where we are in the voting process, it can get confusing and people loose [sic] track. The person next to you will tell you what exactly we are voting on, which helps you vote, normally within your party's predecided vote. Party affiliation is by far the strongest influencer of voting behavior. That said, in opposition it is relatively common for people/whole parties within the opposition changing their vote after a good explanation of a fellow opposition member (from a different party) in the pulpit.

(12) It would be by pointing out that the neighbour is misunderstanding what article or section the particular vote is about, pointing to a mistake ('hmm you sure?')

(13*) For most parties the votes are decided beforehand. The whole party votes in an certain way. I don't think that an MP sitting next to you could influence the voting in any way.

(14*) I would not think that it does really. The parliamentary clubs hold meetings on mondays and wednesdays and coordinate their voting beforehand. During the votes each MP follows a script and they are unsure, they look at the light on the voting panel and check on how the club chair is voting and copy them. For that purpose the club chairs are seated next to the walkway, so that their votes could be tracked easily by others. Of course, there are sometimes big high profile issues where parties split but then I don't think that seating has much impact on how people vote. The club chairs today sit in these spots so other MPs could track their votes easily.

3. **In American politics, co-sponsorship networks are often used to proxy for social networks. Is that a reasonable assumption in the Icelandic context? For example, do co-sponsors of legislation in the Alþingi tend to spend time working together, or might they not even interact at all?**

(1) Co-sponsorship primarily depends on the matter in question. MP's will try to get those that they believe might agree with (or have difficulty opposing) the proposal to sign. Quite often MP's will ask others from their constituency to co-sponsor a bill that has significance for the

constituency (fix a certain bridge etc). Co-sponsors might work together on a bill, particularly if they are in the same party but more often one MP will get staff to help him or her in writing a bill and then ask likely takers if they want to take part or simply send a message to all parliamentary groups asking who would like to join.

(2) Co-sponsoring bills and proposal is common and happens across party lines, but not as often across the lines that divide parties that support the government and those who are in opposition. In a small 63 member parliament there is often close cooperation across party lines in the parliamentary committees even if people disagree on the matter at hand.

(3) I don't think that is very common. Also, in Iceland a very high percentage of new legislation comes from the Ministers. Almost no laws pass that origin from MPs...

(4) No working gether [sic]

(5) As regarding the last question, I'm sorry to say that I have no valuable insight on the subject.

(6) It is not as formal in Iceland as it is for example in the UK, where they have All Party Parliamentary Groups on certain subjects. Since co-sponsorship is often done via email (requests go out to all MPs) - that doesn't have any social implications. There are however exceptions to this, where we go out to likeminded MPs in other parties with bills related to topics we are passionate about. For example when it comes to Gender Based Violence, there is a good-sized group of us, from all parties that works together and co-sponsors bills related to this. That then in turn leads to us working more closely together on other related subjects and also enables us to talk more openly and freely in confidence with each other. Since bills proposed by MPs have a very very small chance of passing, having a broad co-sponsorship increases the likelihood of it being the 1-2 MP bills per party that get approved each year.

(7) Co-sponsorship is most common within parties and within coalition parties (government / opposition). In some cases there are issues that span an ideology and as such rally people to co-sponsorship across the isle. The issues might also be related to a geographical area and thus MPs from that area co-sponsor the issue. Other social connections than that are incredibly rare.

(8) Co-sponsors do not work together at all. If someone asks met o [sic] co-sponsor, I will read the legislation and perhaps offer up advice or ask for slight changes, but that's the extent of it.

(9) I dont know it.

(10) I'm not sure if I understand the concept of co-sponsorship very well in this context, but sometimes we put our name on bills from MP's from other parties, and we may request such support from others as well. When this happens we may chat in the halls, but there's no cooperation and usually we don't interact at all except by confirming our support by email or orally during breaks. Let me know if I have misunderstood the question.

(11) They don't usually spend time together in that context.

(12) I've not noticed this in the Icelandic context no.

(13*) They will spend some time working together within their committees when co-sponsoring legislation from within their committee. If the legislation doesn't origin [sic] from within a committee then people who are co-sponsoring it might not even interact at all. If you have something that you're putting forward as an MP you might send an email out to all the MPs asking simply "Hey, I'm putting this forward soon. Anyone want to co-sponsor?" and that will usually get you a few other MPs. Yes, this has led to some unfortunate "Reply All" incidents.

(14*) Very interesting subject. As you probably know most of the legislation that actually passes in Iceland comes from the government. However bills sponsored by MPs are common, they often start a discussion. They get debated, consulted and a handful of them pass each session (usually if they are relatively uncontroversial).

They come in different forms. It is common for a party (i.e. all its club members) to present a bill as a political statement. Sometimes members from a single district present a bill together if it represents their area interests. This might be something like a proposal to build a road etc.

Then there are other bills which are presented by people from different parties which I guess tell you the most about cross-party alliances. In 99% of those cases however the first MP mentioned is the real author of the bill. He or she then sends out a call to other MPs, asking for co-sponsors. So the MPs usually do not work on those bills together. Exceptions are very few

bills which are likely to be highly debated (e.g. abolition of alcohol monopoly, referendum on eu accession talks) where sometimes some negotiations take place beforehand to get as many people on board as possible. Having said that I would definitely say tha [sic] co-sponsorships do provide some indications on who is friends with whom in the Parliament, because, although some parties do have policies where sponsorships of single party members must be discussed in the club, there is generally less discipline in co-sponsorships then there is when it comes to voting.

C 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 includes each MP's party, constituency, gender, whether and when the MP was the chair of a parliamentary group, ministerial and committee history, 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.
- We use data from ParlGov (from here) to measure the left-right ideological position of each political party – their measure is a time-invariant unweighted mean value of information from party expert surveys on a 0 to 10 scale. We use this measure to estimate proximity effects separately for different-party neighbors that belong to ideologically-similar vs. ideologically-distant parties.
- For additional balance checks, we collected data on wages and expense claims since 2007 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. We drop the (less frequent) votes taken by deputy MPs. These deputies are called upon when MPs are absent due to (i) government duties lasting more than 5 days, (ii) duties abroad, and (iii) health reasons. We web-scraped the topic of each bill from the *Althingi* website here (only available since the 2001-02 session).

- The identities and terms of party leaders were kindly provided by Axel Viðar Egilsson, Project Manager at the Research Service of the *Althingi*. We linked this data with voting data to construct our MP-level outcome variables *Leader Non-Compliance* and *Rebel Rate*.

- 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.

D Discussion of 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 MP-level 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-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.[21] 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

[21] 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).

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*$_{iv}$).

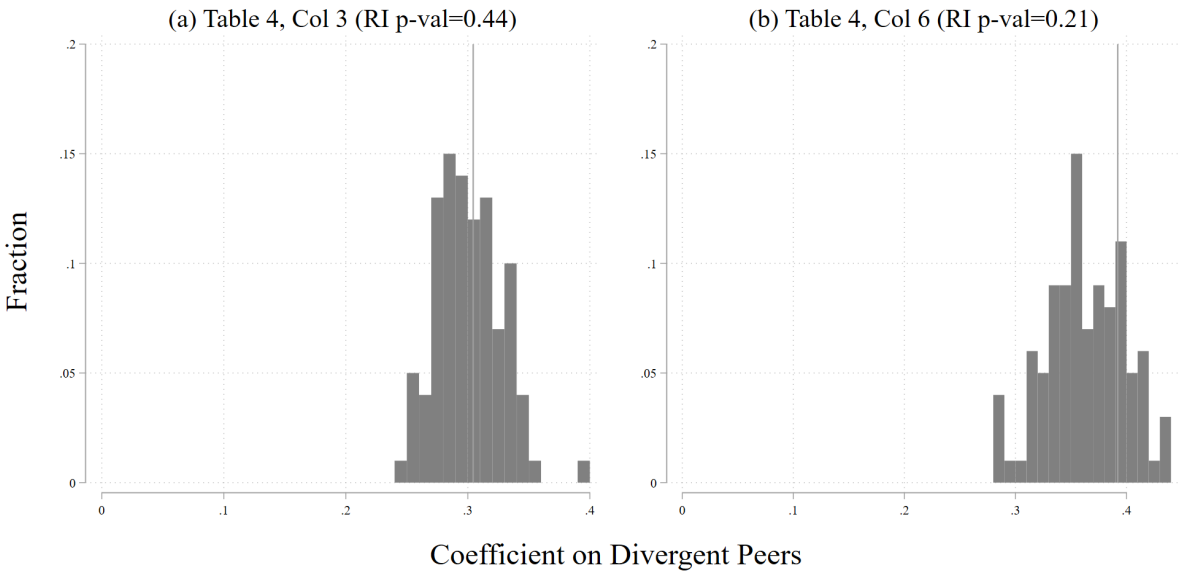
Table C1: Replication of Table 4 in Saia (2018) and raising concerns

	Col.3 replication	Party leader vote				Col.3 with appropriate FEs	
	Non-compl-iance (1)	No (2)	Yes (3)	Abstain (4)	Absent (5)	Non-compl-iance (6)	Non-compl-iance (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 to their party leader for the particular voting procedure. In Columns 2-5, the dependent variable is dummy variable indicating the vote of the MP's party leader. Standard errors 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 *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 *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 *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.

E Reconciling Pair-Level With Individual-Level Effects

E.1 Theory

We outline a simple model to show that cue-taking from different-party neighbors need not imply a fall in party discipline. We make the following assumptions. There exist N MPs (where N is a multiple of eight), out of which half belong to party A and half belong to party B. N is large, allowing us to simplify expressions below by taking N to infinity. Simpler than the Icelandic setting, assume that each MP has only one seating neighbor, with seating randomly assigned such that $\frac{N}{2}$ MPs sit next to a same-party MP and $\frac{N}{2}$ MPs sit next to a different-party MP.

MPs vote once, without loss of generality. The party line for party A is "yes" and the party line for party B is "no." Each MP has a "natural vote" – the vote choice they would make in the absence of any neighbor influence. Fraction r of each party are rebels: they have a natural vote which is against the party line.

For simplicity, we assume no influence between same-party neighbors, but influence between different-party neighbors is possible. In particular, an MP can influence their different-party neighbor's vote with probability p_l when the two neighbors would otherwise vote the party line. Otherwise, when the two neighbors would both rebel without influence, we say that the probability of influence is p_r. Influence is symmetric: p_l and p_r do not vary by party. Finally, when both MPs have the same natural vote, no influence occurs. In more detail, the three possible cases for different-party MP pairs are:

1. With probability $(1-r)^2$, the natural votes are party line voting (Party A MP votes "yes," Party B MP votes "no"). We call the Party A MP a and the Party B MP b. With probability p_l b influences a, such that both vote no; with probability p_l a influences b, such that both vote yes; with probability $1-2p_l$, neither influence the other, such that both vote their natural vote.

2. With probability r^2, the natural votes are rebellious voting (Party A MP votes "no," Party B MP votes "yes"). With probability p_r b influences a, such that both vote yes; with probability p_r a influences b, such that both vote no; with probability $1-2p_r$, neither influence the other, such that both vote their natural vote.

3. With probability $2r(1-r)$, the natural votes are aligned – both MPs in the pair vote the same way. Here there is no scope for influence (ruling out the possibility of negative cue-taking).

With these assumptions, we can now solve for the individual-level and pair-level effects of outparty exposure as a function of the underlying persuasion parameters (p_l and p_r) and the rebellion rate (r).

First, we define P_{ls} as the probability that an MP with a same-party neighbor votes the party line, and P_{ld} as the probability that an MP with a different-party neighbor votes the party line. The former is simply the probability that the MP does not rebel, since same-party neighbors are assumed to exert no influence. The latter is the sum of three probabilities: (i) the probability of both MPs having a party-line natural vote and the focal MP not being persuaded, $(1-r)^2(1-p_l)$; (ii) the probability of both MPs having a rebelling natural vote and the focal MP being persuaded, $r^2 p_r$; and (iii) the probability of both MPs having an aligned natural vote equal to the focal MP's party line, $r(1-r)$. This gives us the following two lemmas, which we then use to state the individual-level effect of outparty exposure.

Lemma 1.

$$\begin{aligned} P_{ls} &= P[\textit{vote party line if have same-party neighbor}] \\ &= 1-r \end{aligned}$$

Lemma 2.

$$\begin{aligned} P_{ld} &= P[\textit{vote party line if have different-party neighbor}] \\ &= (1-r)(r+(1-p_l)(1-r)) + p_r r^2 \end{aligned}$$

Proposition 1. *Δ_i is the individual-level effect of outparty exposure on party-line voting, defined as the difference in party-line voting between MPs with a different-party neighbor and those with a same-party neighbor (as in our MP-level regression specification):*

$$\Delta_i = P_{ld} - P_{ls} = p_r r^2 - p_l(1-r)^2$$

All proofs are below. The first proposition establishes that cross-party peer influence need not reduce party-line voting – Δ_i can be positive or negative. Two forces counteract each other to determine the sign. First, in settings with strong parties (like Iceland), the rebellion rate r will tend to be low, pushing towards a negative Δ_i. Second, the voting of outparty MPs may be more persuasive when it comes through rebellion than when it comes through obedience – rebellion is a stronger signal of the neighbor's support for the legislation. Chiang and Knight (2011) report empirical evidence for this idea in the context of newspaper endorsements of political candidates – endorsements are more effective when they go against the political slant of the newspaper. This force suggests that $p_r > p_l$, which pushes toward a positive Δ_i, with outparty exposure *increasing* party-line voting.

Next we solve for the pair-level effects of outparty exposure.

Proposition 2. *Δ_p is the pair-level outparty exposure effect, defined as the difference in compliance between neighboring and non-neighboring different-party pairs (as in our pair-level regression specification).*

(i)

$$\begin{aligned}\Delta_p = {} & 2p_l(1-r)^2 + 2p_r r^2 + 2r - \frac{3}{2}r^2 - \frac{1}{2} + \frac{1}{2}(1-r)^4(1-p_l)^2 \\ & + (r^2 p_r - r - r^2)\left[\frac{1}{2}(1-r)^2(1-p_l) + \frac{1}{2}r(1-r) + \frac{1}{2}r^2 p_r\right] \\ & + \frac{1}{2}r(1-r)^3(1-p_l) + \frac{1}{2}r^2 p_r(1-r)^2(1-p_l)\end{aligned}$$

(ii) The first derivative of Δ_p with respect to p_l and p_r is positive.

(iii) Δ_p is weakly positive.

The second proposition establishes that, unlike the individual-level effect, the pair-level effect cannot be negative – a result that hinges on our assumption of no negative cue-taking. This confirms that, theoretically, the signs of the pair-level and individual-level effects need not coincide. Otherwise, this proposition shows that increases in both types of peer influence – influence from rebels (p_r) and from the obedient (p_l) – make the pair-level effect larger.

Proofs of Propositions.

PROOF OF *Proposition 1*:

$$\begin{aligned}\Delta_i &= P_{ld} - P_{ls} \\ &= (1-r)^2(1-p_l) + p_r r^2 + (1-r)r - 1 + r \\ &= 1 + r^2 - 2r - (1-r)^2 p_l + p_r r^2 + 2r - r^2 - 1 \\ &= p_r r^2 - p_l(1-r)^2\end{aligned}$$

PROOF OF *Proposition 2(i):*

Definition 1. (Pair Compliance Rate or PCR): The percentage of MP pairs that vote the same way.

- *PCR* for neighboring different-party pairs:

$$PCR_n = 2r(1-r) + 2p_l(1-r)^2 + 2p_r r^2 \tag{4}$$

- *PCR* for non-neighboring (separate) different-party pairs:
  - If both MPs have same-party neighbors
    - Note: We have $\frac{N^2}{4}$ different-party pairs, $\frac{N}{4}$ of which are neighbor pairs. So $\frac{N(N-1)}{4}$ are non-neighboring different-party pairs.
    - $\frac{1}{4}$ of different-party pairs; and as fraction of diff-party non-neighbor pairs: $\frac{N^2}{16} / \frac{N(N-1)}{4} = \frac{N}{4(N-1)} \to \frac{1}{4}$ for large N

$$\begin{aligned}PCR_s^{ss} &= 2P_{ls}(1-P_{ls}) \\ &= 2r(1-r)\end{aligned}$$

  - If one MP has a different-party neighbor:
    - $\frac{1}{2}$ of different-party pairs; and as a fraction of diff-party non-neighbor pairs:

$\frac{N^2}{8} / \frac{N(N-1)}{4} = \frac{N}{2(N-1)} \to \frac{1}{2}$ for large N

$$\begin{aligned} PCR_s^{sd} &= P_{ls}(1-P_{ld}) + (1-P_{ls})P_{ld} \\ &= (1-r)(1-P_{ld}) + rP_{ld} \\ &= 1 - P_{ld} - r + 2rP_{ld} \\ &= 1 - r + P_{ld}(2r-1) \end{aligned}$$

- If both MPs have different-party neighbors (but they aren't neighbors of each other):
  - $\frac{1}{4}$ of different-party pairs; and as a fraction of diff-party non-neighbor pairs: $\left(\frac{N^2}{16} - \frac{N}{4}\right) / \frac{N(N-1)}{4} = \frac{(N-4)}{4(N-1)} \to \frac{1}{4}$ for large N

$$PCR_s^{dd} = 2P_{ld}(1-P_{ld})$$

- Mean PCR for non-neighboring different-party pairs:

$$\begin{aligned} PCR_s &= \frac{1}{4}PCR_s^{ss} + \frac{1}{2}PCR_s^{sd} + \frac{1}{4}PCR_s^{dd} \\ &= \frac{1}{2}r(1-r) + \frac{1}{2}\left[1 - r + P_{ld}(2r-1)\right] + \frac{1}{2}P_{ld}(1-P_{ld}) \end{aligned} \tag{5}$$

$$\begin{aligned} \Delta_p &= PCR_n - PCR_s = (4) - (5) \\ &= 2r(1-r) + 2p_l(1-r)^2 + 2p_r r^2 - \frac{1}{2}r(1-r) - \frac{1}{2}\left[1 - r + P_{ld}(2r-1)\right] - \frac{1}{2}P_{ld}(1-P_{ld}) \\ &= 2p_l(1-r)^2 + 2p_r r^2 + 2r - 2r^2 - \frac{1}{2}r + \frac{1}{2}r^2 - \frac{1}{2}(1-r) - rP_{ld} + \frac{1}{2}P_{ld}^2 \\ &= 2p_l(1-r)^2 + 2p_r r^2 + 2r - \frac{3}{2}r^2 - \frac{1}{2} + \frac{1}{2}P_{ld}(P_{ld} - 2r) \\ &= 2p_l(1-r)^2 + 2p_r r^2 + 2r - \frac{3}{2}r^2 - \frac{1}{2} \\ &+ \frac{1}{2}\left[(1-r)^2(1-p_l) + r(1-r) + r^2 p_r\right]\left[(1-r)^2(1-p_l) + r - r^2 + r^2 p_r - 2r\right] \\ &= 2p_l(1-r)^2 + 2p_r r^2 + 2r - \frac{3}{2}r^2 - \frac{1}{2} + \frac{1}{2}(1-r)^4(1-p_l)^2 \\ &+ \frac{1}{2}(1-r)^2(1-p_l)(r^2 p_r - r - r^2) + \frac{1}{2}r(1-r)^3(1-p_l) + \frac{1}{2}r(1-r)(r^2 p_r - r - r^2) \\ &+ \frac{1}{2}r^2 p_r(1-r)^2(1-p_l) + \frac{1}{2}r^2 p_r(r^2 p_r - r - r^2) \end{aligned}$$

PROOF OF *Proposition 2(ii):*

- Differentiating Δ_p with respect to p_l

$$\begin{aligned}
&\implies 2(1-r^2)-(1-r)^4(1-p_l)-\frac{1}{2}(1-r^2)(r^2p_r-r-r^2)-\frac{1}{2}r(1-r)^3-\frac{1}{2}r^2p_r(1-r)^2\\
&=(1-r)^2\left[2-(1-r)^2(1-p_l)-\frac{1}{2}(r^2p_r-r-r^2)-\frac{1}{2}r(1-r)-\frac{1}{2}r^2p_r\right]\\
&=(1-r)^2\left[2-1+p_l+r^2p_l+2r-2rp_l-r^2p_r\right]\\
&=(1-r)^2\left[1+p_l(1+r^2-2r)+2r-r^2p_r\right]\\
&=(1-r)^2\left[1+p_l(1-r)^2+r(2-rp_r)\right]\\
&>0
\end{aligned}$$

given that $r, p_l, p_r \in (0,1)$.

- Differentiating Δ_p with respect to p_r

$$\begin{aligned}
&\implies 2r^2+\frac{1}{2}r^2(1-r)^2(1-p_l)+\frac{1}{2}r^3(1-r)+\frac{1}{2}r^2(1-r)^2(1-p_l)+r^4p_r-\frac{1}{2}r^3-\frac{1}{2}r^4\\
&=r^2\left[2+(1-r)^2(1-p_l)+\frac{1}{2}r(1-r)+r^2p_r-\frac{1}{2}r-\frac{1}{2}r^2\right]\\
&=r^2\left[2+1-p_l-r^2p_l-2r+2rp_l+r^2p_r\right]\\
&=r^2\left[2+(1-p_l)-2r(1-p_l)+r^2(p_r-p_l)\right]\\
&>0
\end{aligned}$$

given that $r, p_l, p_r \in (0,1)$ since

$$2+(1-p_l)-2r(1-p_l)+r^2(p_r-p_l)=2+(1-p_l)(1-2r)+r^2(p_r-p_l)$$

where the second and third terms are each at least equal to minus one.

PROOF OF *Proposition 2(iii):*

- Recall that $PCR_s=\frac{1}{4}PCR_s^{ss}+\frac{1}{2}PCR_s^{sd}+\frac{1}{4}PCR_s^{dd}$, and for $p_l=p_r=0$, note that $PCR_s=PCR_n=2r(1-r)$.

- It follows that $\Delta_p = 0$ whenever $p_l = p_r = 0$, for any $r \in (0,1)$.

- From *Proposition 2(ii)*, it follows that $\Delta_p > 0$ when $p_l, p_r \in (0,1)$.

E.2 Simulations

The theory demonstrates that positive pair-level effects of being seating neighbours need not imply that sitting next to other-party MPs reduces party-line voting. Despite this theoretical possibility, cue-taking only increases party discipline when $\frac{p_r}{p_l} > \left(\frac{1-r}{r}\right)^2$, and this condition is unlikely to hold in the Icelandic context, where r is close to zero, given that party discipline is high.

An alternative explanation for the disconnect between our pair-level and MP-level results is statistical: we may have more statistical power to detect peer influence in a pair-level specification than in an MP-level specification. We illustrate that this is the case with simulations. Our simulations follow the model in Section E.1 with only minor deviations, which we note. We take the following steps:

- We take the vector of actual votes of MPs,[22] and we call this variable $vote_natural$. We interpret these votes as the votes MPs would take in the absence of peer influence. In reality, these votes *have* been influenced, but since we estimate only small peer effects, these votes still approximate the main features of votes made in the absence of influence (for example, the rebellion rate, a key input into the theory, will be similar).

- We re-randomize the seating 50 times.[23] We use each of these counterfactual seating arrangements to define counterfactual peers, and we allow these peers to influence each MP's vote.

- For each random draw s of the seating arrangement, we simulate peer effects, leaving us with a final vector of votes, $vote_influenced_s$. We assume that only different-party neighbors can influence an MP's vote, and that they do so with probability p, which is the key parameter that we vary across simulations. p is the parallel to p_l and p_r in the model. The

[22] These votes are exactly those used for our main analysis.

[23] As with our approach to randomization inference, we replicate the exact random process by which the *Althingi* assigns MPs to seats.

simplifying differences here are that (i) we set $p_l = p_r$, and (ii) we allow neighboring MPs to persuade one another at the same time, meaning that the two neighboring MPs could swap votes (although this happens only with low probability when p is small). A third difference adds realism: we allow an MP to partially influence their neighbor, switching the neighbor's vote part of the way towards their own vote.

- In detail, we simulate the process of influence as follows:
  - We set a given MP's $vote_influenced = vote_natural$ whenever the MP has no different-party neighbors to influence them (which depends on the particular seating arrangement s), or whenever all different-party neighbors have the same natural vote as the MP (which depends on both the seating arrangement s and the specific vote in question).
  - When an MP has one different-party neighbor, we allow the MP's vote to be influenced by that neighbor with probability p. When an MP has two different-party neighbors, we first randomly select one of the two neighbors to be the influencer, and then we again allow the MP's vote to be influenced by that neighbor with probability p.
  - When an MP is successfully influenced by a neighbor, we replace the MP's $vote_influenced$ with a vote that is closer to the vote of the neighbor. We do so by considering votes in order of the strength of support: from yes, to absent, to abstain, to no. For any given instance of influence, we find the votes that are closer or equal to the influencer, and we randomly select one of these votes to become the influenced MP's new vote. For example, suppose that MP a with natural vote "yes" influences MP b with natural vote "no." In this case, we would set MP b's $vote_influenced$ to "abstain" with one-third probability, to "absent" with one-third probability, and to "yes" with one-third probability.
- After simulating the influence process for the entire vector of natural votes, for each of seating arrangement s, and for different learning parameters p, we have a set of vectors of simulated votes, $vote_influenced_{sp}$ – one vector for each of the 50 seating draws, for each

possible parameter p. With the votes simulated, we collapse the data to the pair-session and MP-session-level, recreating our main outcome variables.

We re-estimate contemporaneous effects using both the pair-level and MP-level specifications (specification 1 and 3) using the vectors of simulated votes, and with the neighbor variables defined as per the seating arrangement draw s associated with the vector of simulated votes. For each parameter p, we re-estimate both specifications 50 times, once for each re-randomized seating arrangement. For simplicity, we use only the contested votes in which the share of MPs voting the modal vote is less than the median (as in Tables 2 and A26, Columns 1 and 2), and we use only the Similarity outcome for the pair-level effects, and the Rebel Rate outcome for the MP-level effects.

We summarize the results of the estimation in the table below, reporting for each value of p: (i) the mean $\hat{\gamma}_2$ (the cross-party pair-level neighbor effect), (ii) the percentage of the 50 counterfactual seating arrangements for which we can reject the null that $\gamma_2 = 0$ at the 5% level (a measure of statistical power in the pair-level specification), (iii) the mean $\hat{\beta}$ (the MP-level effect of other-party neighbors), and (iv) the percentage of the 50 counterfactual seating arrangements for which we can reject the null that $\beta = 0$ at the 5% level (a measure of statistical power in the MP-level specification). For comparison, we also include the estimates and p-values using the real data and seating arrangement. Table C2 reveals several key findings:

1. For a large enough p, positive pair-level effects come hand-in-hand with positive MP-level effects on rebellion – i.e. we are not in the theoretical case where cross-party peer influence *increases* party-line voting.

2. For any given p, we have considerably more power to detect pair-level effects than MP-level effects. Given (1), this is not because we are in the theoretical case where general effects are null or of opposite sign.

3. Estimated pair-level effects are roughly half as large as the underlying structural parameter p – the fact that they are smaller is natural, since whenever MP pairs would vote the same in the absence of influence, there is no scope for neighbors to vote more similarly than non-neighbors. Taking the model literally, our pair-level effects estimated on the real data are consistent with a probability of influence equal to two percent.

Table C2: Estimated Effects Using Simulated Votes With Known Peer Influence

Influence Probability	Pair Effects $\hat{\gamma}_2$	Individual Effects $\hat{\beta}$
Actual	.0096	-.00036
	$p < 0.001$	$p = 0.66$
1%	.00546	.000614
	38%	8%
2%	.0106	.00116
	90%	28%
3%	.0157	.00172
	100%	40%
5%	.0256	.00283
	100%	88%
7%	.0352	.00392
	100%	98%
10%	.0487	.00564
	100%	100%
15%	.069	.00853
	100%	100%
20%	.0866	.0112
	100%	100%

Notes: The table reports estimates of $\hat{\gamma}_2$ from the pair-session-level specification 1 with the Compliance outcome, and estimates of $\hat{\beta}$ from the MP-session-level specification 3 with the Rebel Rate outcome. The first row reports the estimates and dyadic-robust p-values when using the real data and seating assignment (repeated from column 1 of Table 2 and column 2 of Table A26). The remaining rows show the results from simulations, parameterized with the probability of influence ranging from 1% to 20% (far-left column). In each cell, the top row is the mean $\hat{\gamma}_2$ and mean $\hat{\beta}$ from across the 50 simulated counterfactual seating arrangements. The second row is the percentage of the 50 simulations for which we reject the null hypothesis that $\gamma_2 = 0$ and the null hypothesis that $\beta = 0$ at the 5% level.