Legislature Integration and Bipartisanship: A Natural Experiment in Iceland

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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 cosponsorship, despite the lack of persistent effects on voting.

Politicians in almost all countries are segregated at the workplace. Members of Parliament (MPs) in the United Kingdom are seated with the government on one side of a 3.96-m aisle, and the opposition facing them on the other side. This adversarial arrangement is reflected in the history of the aisle width: 3.96 m 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, Clark, and Patterson 1993), and even change political behaviors (Ringe et al. 2013; Wahlke, Eulau, and Buchanan 1962). The decline of such cross-party social interactions may even lie behind the deepening partisan divide in the United States (Haidt 2012; Ripley 2023). Does the integration of politicians increase bipartisanship?

A broad body of work argues that political behaviors are socially determined and are 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 (Fong 2020; Harmon, Fisman, and Kamenica 2019; Zelizer 2019). 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 cosponsorship 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

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We received ethics approval for the survey of current and past MPs from UBC's Behavioural Research Ethics Board (H23-00154). Replication files are available in the *JOP* Dataverse (https://dataverse.harvard.edu/dataverse/jop). The empirical analysis has been successfully replicated by the *JOP* replication analyst. An online appendix with supplementary material is available at https://doi.org/10.1086/734285.

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(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, although 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 cosponsorship links, an indicator of weak social ties and interest overlap between legislators (Fowler 2006; Kessler and Krehbiel 1996; Ringe, Victor, and Cho 2017). There were 10 (19%) more bipartisan cosponsorship 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 .16 and .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 cosponsorship ties. Of course, even in the absence of enduring effects on voting, a more bipartisan cosponsorship 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 cosponsorship ties then merits future research to establish whether our estimated effects are real or merely false-positives.

Our article 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, whereas 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) who 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-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. Fifth, in an article written contemporaneously with ours, Saia (2018) conducts MP-level analysis using the same Icelandic experiment but does not study persistence, effects on cosponsorship, or survey MPs or distinguish between the

1. Because 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.

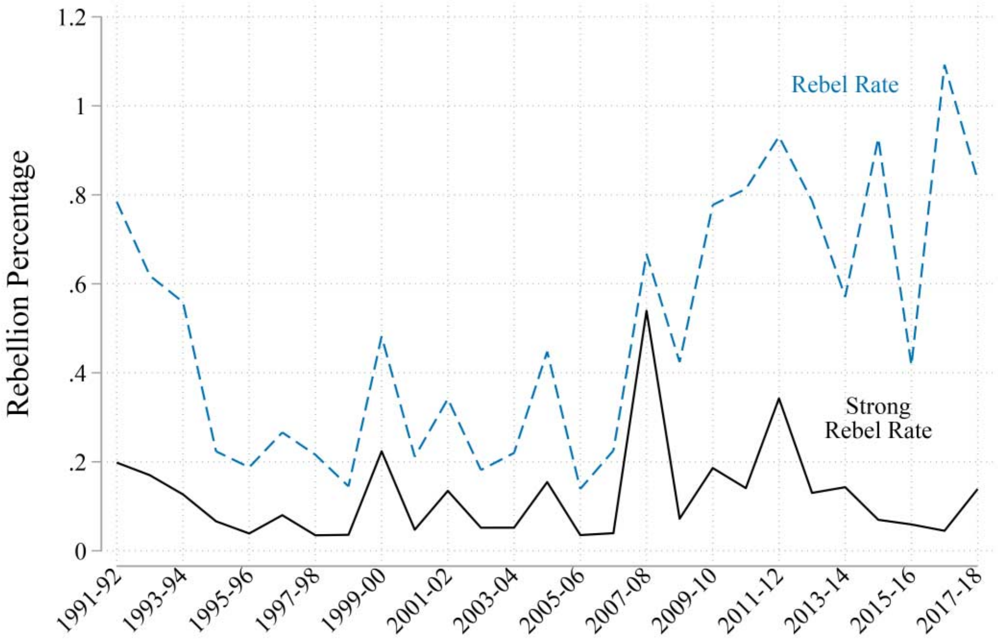

Figure 1. *Althingi* MPs rarely dissent from the party line. *Rebel Rate* is the percentage of votes for which an MP votes yes or abstain when their party leader votes no, or for which an MP votes no or abstain when their party leader votes yes. *Strong Rebel Rate* is the percentage of votes for which 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.

mechanisms that we have outlined above.[2] Finally, Darmofal, Finocchiaro, and Indridason (2023) use Moran's I to estimate spatial dependence in voting in the *Althingi* from 1995 to 2015, with outcomes most similar to those we use for our MP-session–level analysis. They find little evidence of spatial dependence, consistent with our MP analysis null effects. Our pair-level specifications uncover complementary findings to theirs, as we will explain.

ICELANDIC POLITICS AND THE SEATING LOTTERY

Iceland's political system

Like the other Nordic countries, Iceland has a unicameral parliamentary democracy with a multiparty 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 nonpartisan 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 European parliaments (Kristinsson 2011; fig. 1). Furthermore, legislative productivity has been relatively stable since 1991–1992 (fig. A1), although with some gridlock during

2. Different to us, Saia (2018) finds that those sat next to all other-party legislators are 30–50 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 semipresidential 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 semipresidential constitution, "the system functions in practice much like a parliamentary system characterized by a high degree of 'partyness'."

periods of weak coalitions.[4] Finally, parties are substantially more polarized along the left-right dimension than those of the United Kingdom and the United States, while slightly more polarized when compared with the other Nordic countries (Bengtsson et al. 2013, 30).

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 (fig. 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 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. Ministers sit at special desks shown on the right side of figure 2, 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 preassigned 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 estimates using the initial assignment of seating.

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] Although 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. Twelve sitting MPs and two ex-MPs gave answers (full details and complete anonymized answers in app. 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 and 8, ex-MP 14); MPs interact with neighbors on voting days, although 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). 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 some bills being discussed in the committee."

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 at https://www.althingi.is/altext/upptokur/lidur/?lidur=lid20140909T160704.

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 preassigned that seat or assigned it by lottery. We address this issue by assuming that any chairs of parliamentary groups in aisle seats were preassigned 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.

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

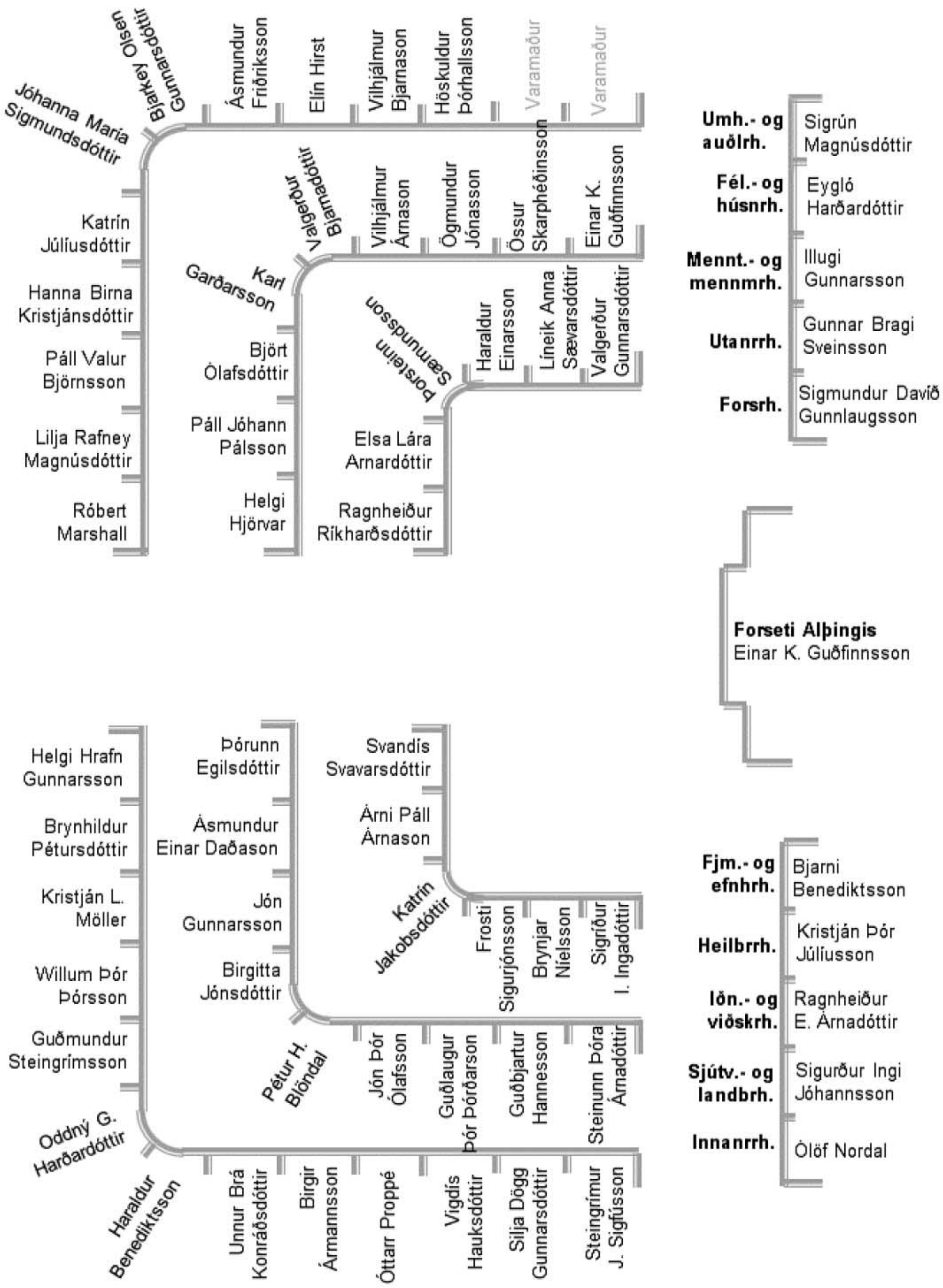

Figure 2. Seating assignment for 2014–15. Source: http://www.althingi.is/.

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.

MECHANISMS OF INFLUENCE AND LAWMAKER BEHAVIORS

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 (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; Lowe 2021; Paluck, Green, and Green 2018). 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 (fig. 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 cosponsorship 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 cosponsorship of each other's bills (Fong 2020). Zelizer (2019) finds compelling evidence of cue-taking using this approach. In his setting, cosponsorship 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 (Harmon et al. 2019; Masket 2008; Saia 2018). Our article 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, whereas 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. Because 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, Green, and Christopher Larimer 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.

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

Measures of legislator behavior

We analyze two formal political behaviors—roll-call votes and cosponsorship.[10] We treat voting as the main measure of lawmakers' revealed preferences, as in a large body of existing work (Clinton, Simon Jackman, and Rivers 2004; Poole and Rosenthal 2011). Therefore, voting is used to distinguish between the mechanisms outlined above.

Although cosponsorship is considered a proxy for social connectedness in American politics (Fowler 2006), we know of no scholarship that explores whether cosponsorship ties in Iceland imply social connection. To make progress, we included a question on the nature of cosponsorship in our survey of past and present MPs (full anonymized responses in app. B). The responses establish that cosponsors 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 cosponsoring 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 cosponsors will work closely on bills which are likely to be highly debated. The same respondent also notes that cosponsorships provide "some indications on who is friends with whom in Parliament," given that there is weaker party discipline with cosponsorship than with voting. In light of the experts' opinion, we consider cosponsorship 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.

DATA AND SPECIFICATION

Data description

We compiled data on initial seating assignments, voting, and cosponsorship 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-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 these data to construct two MP-session–level voting outcomes. *Leader Noncompliance* 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 noncompliant when the vote chosen from among these four categories is different from that chosen by their party leader. We consider *Leader Noncompliance* to be a measure of general bipartisanship.

A limitation of our *Leader Noncompliance* 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, 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 fig. 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 Noncompliance*. Our second pair-level measure is *Similarity*, which 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

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 2001–03 and resultant economic crisis of 2008 (SIC 2010; Viken 2011; Wade and Sigurgeirsdottir 2011). Nevertheless, our approach has an important advantage: by focusing 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.

11. 1991–92 is the first regular session for which the seating assignment is available.

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—that is, not wanting to go on record as either a supporter or opposer.

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, whereas 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* that do not count both MPs being absent as the two voting the same way.

Cosponsorship. We web-scraped cosponsorship data from the *Althingi* website, covering bills, resolutions, and reports. We used these data to construct two MP-session–level cosponsorship outcomes. *Raw Number of Cosponsorship Links* is the total number of links an MP has with other-party members through sponsorship or cosponsorship 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 cosponsorship 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, as either sponsor or cosponsor. The second measure is the inverse hyperbolic sine of the first.

Empirical specification

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

$$y_{ab\{t-1,t,t+1\}} = \alpha_{p(a)p(b)st} + \gamma_1(\text{Neighbor}_{abt} \times \text{Same Party}_{abt}) + \gamma_2(\text{Neighbor}_{abt} \times \text{Different Party}_{abt}) + u_{abt}. \quad (1)$$

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 1 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 (fig. 2). Same Party$_{abt}$ is a dummy variable equal to 1 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 1 when both MPs in the pair were preassigned seats. For these pairs it is always the case that Neighbor$_{abt}$ = 0. The second strata equals 1 when either one, but not both, of them was preassigned. The third equals 1 when neither were preassigned. We include preassigned MPs because, 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]

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 we 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

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 (fig. 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 the "Pair-Specific Effects on Voting" section.

15. Note that the noninteracted 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 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 cosponsorship 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.

to 1 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 rerandomized. 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 cosponsor frequently with some MPs may also tend to cosponsor frequently with others in the same session) and across-time correlation (e.g., MPs who cosponsor frequently with others at time t may also tend to cosponsor 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 (Imbens and Rubin 2015; Young 2015).

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

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

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 nonplacebo 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:

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

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 cosponsorship or voting rebellion outcome for MP i during session t, while $\text{Proportion Other Party Neighbor}_{it} \in \{0, \frac{1}{2}, 1\}$ 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 $\text{Proportion Other Coalition Neighbor}_{it}$ 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—because 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 preassigned a seat during session t as opposed to having participated in the seating lottery. The estimates then only come from within-strata variation—that is, 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 cosponsorship 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 because 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 predetermined 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, 2 of 27 coefficients are statistically significant at the 10% level (tables A1 and 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 and 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

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.

effects on party-line voting of either sign (full model and proofs in app. 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, app. 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, app. 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 $p_r > p_l$, because 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 (fig. 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 (app. E). As we elaborate further in the next section, our null effects on rebellious voting at the individual level are likely due to this limitation of power.

RESULTS

Pair-specific effects on voting

MPs from different parties vote 0.5 percentage points (randomization inference [RI] $p = .06$) 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 $p = .009$) higher (column 2). The former effect is sensitive to correcting for multiple hypothesis testing ($q = .26$), whereas the latter is more robust ($q = .038$).

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 $< .01$, 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 (fig. 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 preassigned to corner seats are different to other MPs (e.g., they are more likely to be chairs of parliamentary groups and thus may be more influential).

Consistent with our hypothesis, proximity effects on voting for corner-seat MPs are three to five times larger than for

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 (proximity effect on bipartisanship)	.0051	.0071	.0008	.000057	.0013	.0017
Dyadic-robust p value	.057*	.0047***	.86	.99	.68	.59
Randomization inference p value[a]	.057*	.009***	.81	.99	.73	.64
Neighbor × same party	.0036	.0037	.011	.0099	.0044	.0027
Dyadic-robust p value	.57	.57	.19	.28	.59	.75
Randomization inference p value[a]	.58	.57	.13	.19	.57	.71
Same = different						
Dyadic-robust p value	.82	.61	.32	.35	.74	.92
Randomization inference p value[a]	.84	.65	.26	.28	.74	.91
Observations	35,259	35,259	21,589	21,589	21,638	21,638
Session × party pair × strata FE	Y	Y	Y	Y	Y	Y
Outcome mean	.57	2.5	.55	2.5	.57	2.5
Outcome SD	.13	.17	.12	.16	.12	.16

Note. *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 1 if the MPs in the pair are randomly assigned to sit next to each other during that session. 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 preassigned seats, one MP in a pair was preassigned a seat, or neither MP in a pair was preassigned a seat. FE, fixed effects; Y, yes.

[a] 1,000 draws.

* $p < .1$.

** $p < .05$.

*** $p < .01$.

middle-seat MPs, although 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 coabsenteeism, 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 and 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 article. 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 app. 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

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 (proximity effect on bipartisanship)	.0096	.013	.0051	.0071
Dyadic-robust p value	0.001***	0.001***	.057*	.0047***
Randomization inference p value[a]	.006***	.001***	.057*	.009***
Sharpened q value[b]	.003***	.001***	.23	.023**
Neighbor × same party	.0085	.0096	.0036	.0037
Dyadic-robust p value	.12	.12	.57	.57
Randomization inference p value[a]	.2	.17	.58	.57
Sharpened q value[b]	.33	.33	.64	.64
Observations	35,205	35,205	35,259	35,259
Panel B: One year later ($t + 1$)				
Neighbor × different party (proximity effect on bipartisanship)	−.0023	−.0042	.0008	.000057
Dyadic-robust p value	.61	.36	.86	.99
Randomization inference p value[a]	.57	.35	.81	.99
Sharpened q value[b]	.64	.5	.8	.8
Neighbor × same party	.014	.012	.011	.0099
Dyadic-robust p value	.15	.26	.19	.28
Randomization inference p value[a]	.072*	.15	.13	.19
Sharpened q value[b]	.33	.5	.41	.5
Observations	21,589	21,589	21,589	21,589
Panel C: Previous year (placebo) ($t - 1$)				
Neighbor × different party (proximity effect on bipartisanship)	.00052	.0012	.0013	.0017
Dyadic-robust p value	.88	.77	.68	.59
Randomization inference p value[a]	.9	.81	.73	.64
Neighbor × same party	.0061	.0046	.0044	.0027
Dyadic-robust p value	.49	.61	.59	.75
Randomization inference p value[a]	.44	.61	.57	.71
Observations	21,638	21,638	21,638	21,638
Session × party pair × strata FE	Y	Y	Y	Y
Outcome mean	.46	2.3	.57	2.5
Outcome SD	.15	.32	.13	.17

Note. 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. 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 preassigned seats, one MP in a pair was preassigned a seat, or neither MP in a pair was preassigned a seat. *Same party* is equal to 1 if both MPs in the pair are in the same party for that session. Outcome mean and SD are for the sample included in the panel A regressions. FE, fixed effects; Y, yes.

[a] 1,000 draws.

[b] For nonplacebo tests (Anderson 2008).

* $p < .1$.

** $p < .05$.

*** $p < .01$.

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, given that 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 1 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 ordinary least squares 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 articles 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 1 percentage point is consistent with an underlying probability of cross-party peer influence of 2%—because influence is only possible in cases where neighbors would otherwise vote differently, our estimated effect understates the probability of influence.

Although cross-party cue-taking has been observed between those linked through cosponsorship (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 and 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, Ryan, and Sokhey 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 fig. 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, Huber, and Manow [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 (fig. 3). Cross-party neighbor effects are much stronger, and only statistically significant, prior to the 2009–10 session (table A21), whereas same-party neighbor effects show the opposite pattern. Although more suggestive, these results support the hypothesis that cross-party influence is possible, although perhaps only during periods of cross-party consensus.

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).

Because 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).

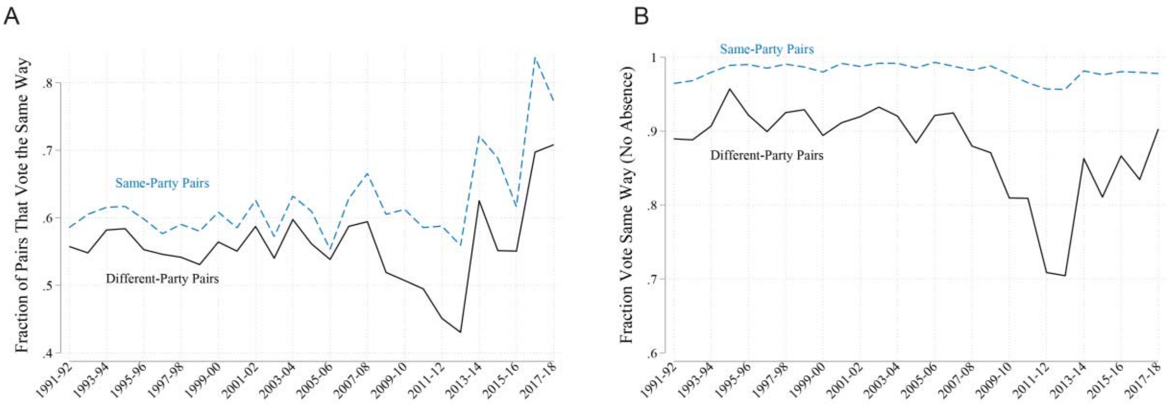

Figure 3. Cross-party consensus fell after the 2008 economic crisis. (*A*) Outcome is the average fraction of MP pairs that voted the same way (both yes, both no, both abstain, or both absent) for a given session. (*B*) Outcome is the same but calculated only for votes in which both MPs in the pair were not absent. In both panels, the prime minister, ministers, and speaker are excluded.

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 and 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 = .089). Although 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, 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%

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

	Contemporaneous Effect (t)		One Year Later (t+1)		Previous Year (Placebo) (t−1)	
	Leader Noncompliance (1)	Rebel Rate (2)	Leader Noncompliance (3)	Rebel Rate (4)	Leader Noncompliance (5)	Rebel Rate (6)
Proportion other-party neighbor	.0028	−.00061	.0014	.00017	.012	−.00049
MP-clustered SE	.0076	.00057	.0098	.00051	.0097	.00054
p value	.71	.29	.89	.73	.2	.37
Randomization inference p value[a]	.71	.31	.89	.77	.19	.37
Sharpened q value[b]	1	1	1	1		
Observations	1,294	1,294	826	826	835	835
Session × party × strata FE	Y	Y	Y	Y	Y	Y
Outcome mean	.42	.005	.44	.0044	.43	.005
Outcome SD	.13	.011	.11	.01	.11	.0073

Note. *Leader Noncompliance* 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. 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 preassigned a seat. FE, fixed effects; Y, yes.

[a] 1,000 draws.

[b] For nonplacebo tests (Anderson 2008).

* $p < .1$.

** $p < .05$.

*** $p < .01$.

Table 4. Pair-Level Effects on Cosponsorship 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 (proximity effect on bipartisanship)	−.037	−.013	.07	.023	−.025	.016
Dyadic-robust p value	.65	.6	.5	.52	.76	.59
Randomization inference p value[a]	.56	.56	.41	.39	.75	.54
Sharpened q value[b]	.95	.95	.95	.95		
Neighbor × same party	−.24	−.022	−.37	−.093	−.43	−.055
Dyadic-robust p value	.22	.52	.1	.011**	.1	.22
Randomization inference p value[a]	.21	.55	.14	.065*	.11	.3
Sharpened q value[b]	.79	.95	.54	.097*		
Same = different						
Dyadic-robust p value	.34	.83	.088*	.019**	.15	.23
Randomization inference p value[a]	.32	.84	.12	.067*	.14	.25
Observations	35,314	35,314	23,265	23,265	23,472	23,472
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 SD	4.8	1.1	5	1.2	4.8	1.1

Note. *Number* is the total number of cosponsorship 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 1 if the MPs in the pair are randomly assigned to sit next to each other during that session. *Same party* is equal to 1 if both MPs in the pair are in the same party for that session. 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 preassigned seats, one MP in a pair was preassigned a seat, or neither MP in a pair was preassigned a seat. FE, fixed effects; IHS, inverse hyperbolic sine; Y, yes.

[a] 1,000 draws.

[b] For nonplacebo tests (Anderson 2008).

* $p < .1$.

** $p < .05$.

*** $p < .01$.

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 out-party 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 out-party exposure decreasing confidence in position taking.

Effects on cosponsorship

Bipartisan proximity does not lead to increased cosponsorship 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

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 $p = .065$). 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.

Table 5. Effects on Bipartisan Cosponsorship 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
MP-clustered SEs	3.6	.068	4.7	.12	3.8	.086
Dyadic-robust p value	.69	.42	.035**	.12	.24	.21
Randomization inference p value[a]	.69	.48	.037**	.095*	.31	.32
Sharpened q value[b]	.53	.38	.16	.21		
Observations	1,420	1,420	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 SD	76	1.1	82	1.3	73	1.2

Note. *Number* is the total number of cosponsorship 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. 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 preassigned a seat. FE, fixed effects; IHS, inverse hyperbolic sine; Y, yes.

[a] 1,000 draws.

[b] For nonplacebo tests (Anderson 2008).

* $p < .1$.

** $p < .05$.

*** $p < .01$.

can strengthen the treatment effect on cosponsorship. We find 0.29 more cosponsorship links (RI $p = .15$) 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 ($q = .92$). 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 cosponsorship 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%), although it becomes marginally insignificant when we use the inverse hyperbolic sine transformation instead of the raw number or when correcting for multiple hypothesis testing ($q = .16$ and $q = .21$ 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 cosponsorship links offer some hope that bipartisan seating can create weak social ties and lead to interest overlap.

CONCLUSION

Icelandic legislators randomly assigned to sit next to each other are 0.5–1 percentage point more likely to vote alike. Most surveyed legislators are not aware of this small peer influence. Nevertheless, 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 cosponsorship 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, although perhaps only those with a reasonable amount of cross-party consensus, given the fall in cross-party influence after the Icelandic economic crisis. 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, whereas 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, Theriault, and Guthrie 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."

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