

The Effects of Sharing a Meal on Legislative Bill Cosponsorship Patterns

This version: October 20, 2026

Abstract

Reformers frequently argue that increasing social interactions between legislators from opposing parties will increase bipartisan collaboration. However, existing evidence for this claim is often confounded by selection bias, as collaborative legislators are more likely to seek out social contact with the out-party in the first place. We conducted a randomized field experiment in a state legislature where we assigned bipartisan pairs of lawmakers to share meals. We find no evidence that the intervention increased subsequent bill cosponsorship or roll call agreement between paired legislators. Our results suggest that while legislators reported positive social experiences, modest social interventions are unlikely to serve as a durable solution to breaking policy gridlock.

Word count: 5,989

Bipartisanship | Legislative politics | Bill sponsorship | Field experiments

In 2026, a Minnesota restaurant offered a unique promotion to state legislators. If they would “Dine Across the Aisle,” eating lunch with a legislator of the other party, their meals would be free ([Fleming, 2026](#); [Lambe, 2026](#)). The 2025-26 Minnesota state legislative session had been hard and divisive, coming against the backdrop of political violence in the state ([Day, 2026](#); [U.S. Attorney’s Office, District of Minnesota, 2025](#)). A local paper described that time in the legislature in stark terms, titling their article, “Short tempers, long debates: A year of trauma colors rancorous session at Minnesota Capitol” ([Ferguson, 2026](#)). The owner of the restaurant behind the promotion wanted to lower the temperature in politics and to increase bipartisanship ([Fleming, 2026](#)). Can contact-based initiatives like this increase bipartisan action among legislators?

American legislatures have failed to address a range of pressing problems in recent decades, from dealing with K-12 student struggles to balancing the budget and more ([Binder, 2014](#)). Many voters are frustrated by the legislative gridlock ([Newport and Saad, 2016](#)). But increasing divides between the parties means it is harder to find common ground ([Anderson, Butler and Harbridge-Yong, 2020](#); [Binder, 2014](#); [Mann and Ornstein, 2012](#); [McCarty, Poole and Rosenthal, 2016](#); [Shor and McCarty, 2011](#)). The complexity of American government, with its numerous veto points often requiring supermajoritarian coalitions to implement policy ([Krehbiel, 1998](#)), makes it even more difficult to pass policy. In practice, bipartisanship is necessary to break gridlock and pass legislation ([Curry and Lee, 2020](#); [Harbridge, 2015](#); [Harbridge-Yong, Volden and Wiseman, 2023](#)).

Legislators, reformers, and commentators alike have cited declining cross-party relationships to explain why it is difficult for legislators to get things done ([Barber and McCarty, 2015](#); [Mann and Ornstein, 2012](#)). Many have promoted social interactions as a way to increase bipartisanship and improve legislative productivity. Through hearings and reports, the Select Committee on the Modernization of Congress in the U.S. House of Representatives called for actions designed to increase social interactions between members, including “a

bipartisan Members-only space in the Capitol to encourage more collaboration across party lines” and “biennial bipartisan retreats for Members and their families at the start of each Congress” (e.g., [Phillips, 2019](#); [House Select Committee on the Modernization of Congress, 2019, 2021](#)). Similarly, a report by the National Conference of State Legislatures identified “a decline in cross-party friendships, socializing and collaboration” as one of the factors exacerbating “the effects of political polarization on policymaking” ([National Conference of State Legislatures, 2018](#)). In addition to the “Dine Across the Aisle” program in Minnesota, nonpartisan and bipartisan groups outside of legislatures have turned to increasing members’ social interactions with the goal of increasing bipartisanship inside the legislature ([Bipartisan Policy Center, 2021](#); [Rubinton, 2016](#)).

Researchers have found that social interactions can affect legislators’ willingness to engage in bipartisan policymaking in some circumstances. Participation in voluntary caucuses is related to bipartisan collaboration ([Victor, 2025](#)), and January 6th, which strained legislators’ bipartisan social relationships, reduced their legislative output ([Curry and Roberts, 2025](#)). Although large interventions that affect legislator social interactions can produce changes in legislator behavior (e.g., living in the same boardinghouse, as in [Minozzi and Caldeira, 2021](#)), an important question is whether modest interventions can lead to more collaboration. Reformers designing socialization-based interventions must operate in an environment in which legislators are increasingly busy representing their constituents, campaigning for office, and traveling ([House Select Committee on the Modernization of Congress, 2021](#)). Intensive interventions can be impractical.

While there are some indications that a modest intervention can make a difference (e.g., [Gaynor and Millburne, 2026](#); [Masket, 2008](#); [Victor, 2025](#)), it is possible that the observational association between interaction and collaboration is simply the result of self-selection of partners. When interviewed about his participation in the “Dine Across the Aisle” in Minnesota, Representative Tabke, a Democrat, said:

I work on legislation with Republicans every day, but who would have thought having a beer while doing it would get so many people to pay attention? Rep. Olson and I have a strong working relationship, working on transportation issues at the Capitol, and we were more than happy to take Sweeney’s up on its ‘Dine Across the Aisle’ special ([Lambe, 2026](#)).

The implication is that Representative Tabke and Olson would have been collaborating without the meal program. This self-selection of partners raises concerns about using observational data to estimate the effectiveness of social bipartisan programs. While bipartisanship is only possible with people who choose to participate (which is also true in our study), we do not know if it is also driven by self-selection of partners. What would have happened if Rep. Tabke had been assigned to have meals with a different Republican and Rep. Olson had been assigned to have meals with a different Democrat? Tests are needed to see whether simple interventions like the Minnesota “Dine Across the Aisle” promotion can increase bipartisanship.

We conduct a unique experiment where state legislators are randomly assigned to have a meal with a legislator from the other party in their own state. Our study is an experimental test of the Minnesota restaurant’s “Dine Across the Aisle” promotion. For the experiment, we worked with leaders of the bipartisan caucus in a northeastern state legislature to recruit legislators to participate in the “Breaking Bread” program. Legislators who participated were randomly assigned a partner from the opposing party who had also signed up. We test whether legislators cosponsored more bills with the legislator they were paired with.

We find that the treatment has no effect on bipartisan legislative collaboration. Legislators were no more likely to cosponsor with their assigned partner than with other out-party legislators in the program. Our findings suggest potential limitations on the impacts of simple interventions like meals or sporting events (e.g., [Booker, 2015](#)) on levels of bipartisanship.

The Case for Social Interactions to Increase Bipartisanship

There are several theoretical reasons social interaction among state legislators might increase legislative collaboration across party lines. First, state legislators have few institutional resources for information gathering. Many legislative sessions are short in duration, and legislators are busy with travel, fundraising, addressing constituent concerns, and, in less professionalized legislatures, their non-legislative careers. These factors mean that legislators have limited information about policy proposals ([Hertel-Fernandez, 2019](#)). In a setting of limited information, social interaction can be valuable: legislators may learn new information from colleagues who specialize in a policy area, and they may become more knowledgeable about policy proposals. Several studies find that such informational exchange influences legislators' policy decisions ([Kingdon, 1973](#); [Zelizer, 2019a](#)). Because much of this informational exchange occurs within parties ([Grumbach, 2022](#)), cross-partisan exchanges have a potentially large impact.

Second, intergroup contact can reduce animosity between people ([Allport, 1954](#)). Partisan prejudice is growing in America (e.g., [Iyengar et al., 2019](#)), and politicians express animus toward the opposing party ([Costa, 2025](#); [Druckman et al., 2023](#); [Enders, 2021](#)). If partisan animosity inhibits policy collaboration across party lines among legislators, interventions to promote positive intergroup contact may have the downstream consequence of increasing willingness to work with members of the opposing party on policy. In the legislative setting, interpartisan contact may reduce partisan prejudice by humanizing members of the out-group ([Kalla and Broockman, 2023](#)), correcting misperceptions about the out-party ([Ahler and Sood, 2018](#)), or highlighting superordinate identities ([Levendusky, 2018](#)). [Lee \(2016\)](#) argues that constantly denigrating legislators from the opposing party is not conducive to positive interpersonal collaboration on policy. The inverse may also be true: positive interpersonal interactions may make it harder to denigrate legislators from the opposing party,

fostering greater willingness to engage in collaboration. Moreover, previous work finds that misperceptions drive partisan affective polarization ([Ahler and Sood, 2018](#); [Druckman et al., 2023](#)). By interacting with their out-party peers, legislators may learn new information that contradicts misperceptions and reduces partisan prejudice. Finally, social interactions may highlight common goals or identities that reduce animus ([Levendusky, 2018, 2023](#)) and form the basis for legislative collaboration. Thus, social interactions could provide legislators with valuable policy information and increase legislative collaboration through information provision. Social interactions could also decrease partisan prejudice and reduce barriers to bipartisan collaboration rooted in animus or misperceptions.

Consistent with these theoretical expectations, researchers have found that large shifts in legislators' social interactions can affect bipartisan cooperation. [Curry and Roberts \(2025\)](#) look at the legislative process in the aftermath of January 6th. They find that Republican members of Congress who voted against certification of state electors became less bipartisan and less effective in passing legislation after their votes. The authors argue that this deterioration of bipartisanship stemmed from the negative impact of January 6th on legislators' interpersonal bipartisan relationships.

Other scholars have exploited variation in legislators' physical proximity to one another to study social influence on behavior, examining the impacts of sustained social contact induced by lodging, seating arrangements, and office locations. In an extensive study of Congress during the pre-Civil War period, [Minozzi and Caldeira \(2021\)](#) find that members of Congress who lived at the same boardinghouses in Washington were more likely to vote in a similar fashion. In the boardinghouses, legislators could have repeated interactions and discussions about politics in a private setting. In the state legislative context, [Masket \(2008\)](#) shows that legislators who sit next to one another express more agreement in roll call voting than those who do not. On the other hand, in a quasi-experimental design, [Rogowski and Sinclair \(2012\)](#) uncover little evidence that members of Congress whose offices are closer to

one another are more likely to agree on roll call votes or to cosponsor legislation together. Though [Zelizer \(2019a\)](#) doesn't randomly assign legislator interactions, he demonstrates that lobbying treatments can indirectly affect treated legislators' office suite-mates, confirming that information diffusion occurs among legislators based on physical proximity. Overall, these results show that sustained interpersonal contact can influence legislative behavior.¹

While large shocks to social interactions can clearly lead to changes in bipartisanship, an important question is whether more modest interventions can reliably affect bipartisan collaboration. Intense interventions are impractical due to growing demands on legislators' time: members of Congress have to devote considerable time to campaign fundraising and travel back to their home district ([House Select Committee on the Modernization of Congress, 2021](#)). As for state legislators, many serve in part-time capacities and must maintain careers outside of the legislature. The type of socialization found in pre-Civil War boardinghouses is unrealistic in the modern Congress and many state legislatures. Practitioners aiming to bolster bipartisanship through social interactions must turn to interventions that are brief and less demanding for legislators. The effects of these less intensive interventions are unclear, however.

Work in sociology suggests that weak connections to new people have important consequences for behavior. In particular, [Granovetter \(1973\)](#) argues that these "weak ties" are more likely to provide novel information by bridging distinct groups (like political parties). In other words, brief social contacts with new individuals present an opportunity for information exchange. Indeed, [Zelizer \(2019b\)](#) finds that policy discussion in a bipartisan caucus can increase support for the policy under consideration, reflecting this potential for information dissemination through brief interactions across party lines. Moreover, though the literature

¹A related literature studies the impacts of legislative staff on legislator behavior, suggesting that interactions with staff may influence legislator voting behavior and effectiveness through information provision and the facilitation of interpersonal relationships ([Camp, Schwam-Baird and Zelizer, 2024](#); [Montgomery and Nyhan, 2017](#); c.f., [Landgrave, 2024](#)).

on contact-based prejudice reduction is built around sustained, cooperative contact ([Allport, 1954](#)), work on affective polarization demonstrates that relatively short interactions between opposing partisans can reduce partisan animosity ([Levendusky and Stecula, 2021](#); [Rossiter and Carlson, 2024](#)). That said, it seems reasonable to assume that less intensive social interactions are likely to have more modest impacts than the sustained contact envisioned by contact theorists ([Allport, 1954](#)). Still, research suggests even modest social interventions could nonetheless increase information and reduce affective polarization, generating a greater willingness to engage in bipartisan collaboration as a result.

In the congressional context, [Victor \(2025\)](#) finds that having more voluntary relationships—such as being part of the same voluntary caucus—is associated with higher bipartisan cooperation. [Gaynor and Millburne \(2026\)](#) study the Annual Congressional Baseball Game. They find that the people who participated in the game had a higher likelihood of legislative collaboration. Participation in voluntary congressional trips is also associated with more bipartisanship ([Curry and Roberts, 2023](#)). While there is some suggestive evidence of a relationship between social interaction and bipartisanship, previous work relies on observational data that is subject to selection bias. Legislators choose whether to participate in caucuses, baseball games, and congressional trips. Even studies of large shocks to social interactions involve some self-selection by legislators: [Minozzi and Caldeira \(2021\)](#) show that members of Congress were more likely to select into the same boardinghouse if they were members of the same party or agreed on policy and were more likely to move out of the boardinghouse if they disagreed. The shock from January 6th involved members of Congress choosing to vote against electoral certification. Thus, it is challenging to determine whether observed associations between social interactions and bipartisanship are caused by the social interactions or by an underlying agreement that generates the social interactions.

Notably, one study does probe the influence of social contact with legislators in a field experiment: [Grose et al. \(2021\)](#) compare positions of legislators assigned to contact with

lobbyists in a social setting, in their office, or a no-contact control. They find that only lobbying in an informal, social setting increases legislators’ support for relevant legislation. However, this lobbying is most effective among legislators who are already allies of the lobbying organization. Still, this study does support the idea that brief social contacts can impact legislative behavior.

Practitioners’ Position

Though theory leads us to suspect that modest social interventions may have more limited effects than sustained contact based on physical proximity (e.g., sharing desks, offices, or lodging), it is also important to test for the impact of sharing a meal because of the importance that reformers put on social interactions as a means for increasing bipartisanship. What impacts, if any, do the social initiatives, like the “Dine Across the Aisle” initiative in Minnesota, frequently promoted by politicians and practitioners have on bipartisanship?

In 2019, the House of Representatives established the Select Committee on the Modernization of Congress. The committee was bipartisan with an equal number of Democrats and Republicans serving together. The committee worked together for four years. In a 2019 report, the committee made recommendations to increase the level of bipartisan collaborations, including the creation of “a bipartisan Members-only space in the Capitol to encourage more collaboration across party lines” and the institution of “biennial bipartisan retreats for Members and their families at the start of each Congress” ([House Select Committee on the Modernization of Congress, 2019](#)). The purpose of these recommendations was to encourage “civility and bipartisanship in Congress.”

Many state legislators also believe that social interactions can increase bipartisanship in office ([National Conference of State Legislatures, 2018](#)). A report from the [National Conference of State Legislatures \(2018\)](#) highlighted how legislators felt that social interactions

helped their state continue to be productive even in times of high partisan-animosity. An Iowa legislator told the National Conference of State Legislatures, “I find the time to talk to colleagues from the other party at the large social receptions. I play racquetball and basketball with colleagues. It gives us the opportunity to talk about our families and interests” ([National Conference of State Legislatures, 2018](#)). The report also quoted legislators from Colorado and Washington who credited dinners and bipartisan social outings as key to forming bipartisan collaborations.

Outside the legislature, several groups have implemented programs to increase social opportunities across the aisle. With the express goal of forging “trusted relationships across the aisle to advance durable bipartisan solutions in Congress,” the Bipartisan Policy Center (BPC) implemented a program to increase the social interactions between members of Congress. Under their special initiative, American Congressional Exchanges, the participating Members of Congress took turns visiting each other in their districts. As of May 2021, the BPC had reported facilitating over 50 trips with the goal of forging “trusted relationships across the aisle to advance durable bipartisan solutions in Congress” ([Bipartisan Policy Center, 2021](#)). Other programs with similar aims focus on other members of congressional offices such as chiefs of staff, hoping to increase bipartisanship among staff members ([Rubinton, 2016](#)).

Details of Our Breaking Bread Experiment

We address the potential selection of partners by using a randomized experiment to study the impacts of a modest social interaction on bipartisan collaboration in a state legislature. We worked with leaders of the bipartisan caucus in a northeastern state legislature to recruit their colleagues to participate in the “Breaking Bread” program. The bipartisan caucus was formed to increase bipartisanship and primarily organizes trainings for produc-

tive cross-partisan dialogue as well as social outings across party lines. Legislators were told that each participant would have 1-3 meals with a legislator from the other party, with the meals paid for by the researchers.² Recruitment included a speech on the floor of the House chamber and a caucus social media post. To mitigate potential experimenter demand effects and to more precisely access the organic social interactions promoted by practitioners, recruitment materials did not provide any restrictions on topics of conversation during meals; instead, materials stated that the program was “to encourage bipartisanship in our politics and to foster positive cross-partisan relationships and understanding.”

In total, 45 legislators agreed to participate: 26 Democrats and 19 Republicans. Legislators signed up by filling out a webform where they could indicate any legislators from the other party that they would be unwilling to meet.³ Legislators who signed up were randomly assigned to a legislator from the opposing party. Because more Democrats than Republicans signed up, some Democratic legislators were not matched. For each Republican participant, we randomly selected a Democratic participant until all Republicans were paired. The analyses are based on the universe of cross-party pairs (or dyads) that could have been created; these dyads represent our sample. In total, after excluding one Democratic legislator and one pairing by legislator request, there were 474 possible pairs of legislators across party lines, including Democrats who were not ultimately matched to a Republican. The treatment condition includes the 19 pairs that were matched.

We partnered with several restaurants near the state capitol so legislators could meet for a meal and have it charged directly to our research team. The lunches took place between

²No deception was used, and legislators willingly signed up for the program. There was no concern that participation in the study would harm any legislator involved. The experiment was approved by the Institutional Review Boards at REDACTED University and REDACTED University. See Appendix A for further details on ethical considerations.

³Only nine participants listed any preferred exclusions. One Democratic legislator listed an ideological faction in the opposing party. Because we were unable to identify members of this faction, we excluded this legislator from participation altogether. After excluding this legislator, only one pair of legislators was affected by exclusion requests.

May and August 2025. We chose to have the treatment during this period because it occurred after all the bills were proposed during the 2025 legislative session but before the bills for the 2026 legislative session had been filed. Additionally, this left more than one full month of legislative session remaining during which legislators could schedule meals. In this way, we attempted to minimize the time between treatment and outcome measures while maximizing the amount of time legislators had to participate in meals.

While we offered to pay for up to three meals, the modal legislative pair met just once. For legislative pairs that had not met yet, we sent several follow-ups and reminders. Ultimately, 14 (74%) of the treatment pairs met at least one time, and two pairs met more than once.

Outcomes

Following previous literature on legislative behavior, we use legislative cosponsorship as our primary outcome measure (e.g., [Camp, Schwam-Baird and Zelizer, 2024](#); [Rogowski and Sinclair, 2012](#); [Zelizer, 2019a](#)). We rely on cosponsorships for several reasons. First, a social contact intervention could affect behavior through relationship-building: friendly social interactions may build trust and reciprocity and reduce interpartisan hostility. Legislators are presumably more likely to reach out to colleagues with whom they have preexisting relationships when seeking cosponsors for their legislation and in turn, may be more responsive to requests from trusted peers than others. Because cosponsorship is optional and often reciprocal, cosponsorships represent an indirect measure of relationships between legislators ([Fowler, 2006a,b](#)).⁴ Second, as [Zelizer \(2019a\)](#) highlights, cosponsorship measures may be less reflective of party influence than roll call voting, and they sidestep the issue of partisan agenda control which determines the bills obtaining floor votes ([Cox and McCubbins, 2005](#);

⁴We do not contend that the *only* motivation for cosponsoring legislation is relational—indeed, electoral, ideological, and policy considerations undoubtedly play a role; however, among available legislative behavioral measures, it presents an indirect and observable metric of legislator relationships ([Fowler, 2006a,b](#)).

[Harbridge, 2015](#)).

We constructed dyadic cosponsorship measures by scraping all bills introduced in 2025 and 2026 from the state legislature’s website. From each bill, we extracted the list of cosponsors and expanded each list into all pairs of Republican and Democratic legislators for a given bill. We calculated our outcome measure (pair-level number of cosponsorships) by counting the number of bills each pair of legislators had sponsored together. Finally, we merged these dyad-level data with our treatment assignments, allowing us to examine whether treated legislators cosponsored more bills with their paired partners than did untreated pairs of legislators in the Breaking Bread program. Because legislative elections occurred in 2024, we have pre- and post-treatment cosponsorship data for all participants in our sample.

As a secondary outcome, we calculated roll call agreement rates for pairs of legislators. Specifically, we counted the number of times legislators voted the same way across bills, excluding nonvoting, and divided by the number of bills each pair cast votes on. We have roll call agreement scores for two post-treatment periods: the 2025 legislative session after study recruitment began and the 2026 legislative session. These roll call measures were not pre-registered, and as such, our roll call agreement results should be treated as exploratory.

The Legislative Setting

The legislature where our experiment occurred is less professional and could improve on its current level of bipartisanship. Table 1 presents summary statistics from the 2025 legislative session, giving the the state of affairs before our intervention later that summer. We show information about the 760 bills that had at least one House sponsor. Most of these bills were partisan (52%); 28% were bipartisan, and the remaining 20% were sponsored by a single legislator. Among individual legislators (see part B of the Table), most of the bills they cosponsored were partisan bills (approximately 40% of the cosponsored bills were bipartisan on average). However, most legislators cosponsored at least one bipartisan bill.

Legislators who registered to participate in the Breaking Bread Program were somewhat more bipartisan than their non-participant colleagues: on average, they cosponsored almost one more bipartisan bill than non-participants, and they were more likely to have cosponsored at least one bipartisan bill than non-participants (though much of this difference stems from their higher level of legislative activity). Finally, part C of the Table shows that very few bipartisan pairs of legislators worked together on bills (only about 2.4% in the 2025 legislative session). Echoing our individual-level data, bipartisan dyads containing Breaking Bread participants were more likely to have worked together prior to the program than were dyads without participants, reflecting participants’ inclination toward bipartisanship.

Table 1: Pre-Treatment Summary Statistics (2025 Legislative Session)

A. Bill-Level Summary					
Num. Bills	Num. Bipartisan Bills	Avg. Num. Cosponsors	Median Num. Cosponsors		
760	216	3.97	3.00		
B. Individual-Level Summary					
	Overall	Non-Participant	Participant		
Avg. Number of Bills Sponsored	7.54	7.45	8.20		
Avg. Number of Bills Cosponsored (2+ Leg.)	7.16	7.06	7.93		
Avg. Number of Bipartisan Bills Cosponsored	2.79	2.71	3.44		
Avg. Percent of Cosponsored Bills Bipartisan ¹	41.49%	41.57%	40.93%		
Percent of Legislators with 1+ Bipartisan Bill(s)	64.07%	63.74%	66.67%		
C. Dyad-Level Summary					
	Overall	Non-Participant	Participant	Control	Treated
Percent Cosponsoring Together	2.42%	2.17%	3.28%	3.30%	10.53%

¹ Among legislators who cosponsored at least one bill.

One advantage to conducting our study in this legislature is that it is a less professional

legislature with short sessions. Because the sessions are short and because legislators only serve part-time, there are fewer preexisting opportunities for these legislators to get to know each other than their peers in more professionalized settings. The limited opportunities for preexisting contact means that the shared meals represent an opportunity for creating deeper relationship. Second, existing dyad-level bipartisan cosponsorship was near zero (among participants, only about 3% of bipartisan dyads cosponsored together prior to treatment, see Table 1). There was room to improve and even small increases in bipartisan collaboration would represent substantively meaningful progress. Third, the legislators who signed up to participate are the type most open to bipartisan collaboration. If a treatment was going to have an effect, these are the legislator-pairs most likely to change as a result of the treatment.

Methodological Approach

We preregistered our analyses for this study.⁵ We regress our dyadic measure of cosponsorships on a treatment indicator along with the number of pre-treatment pairwise cosponsorships. We used heteroskedasticity-robust (HC2) standard errors. Likewise, for our exploratory analyses of roll call agreement, we regress post-treatment agreement on treatment and pre-treatment agreement.

By randomly assigning treatment, we avoid the main challenges associated with self-selection into treatment. However, legislators can still choose whether to have a meal with their assigned partners. Five pairs opted not to have a meal. Due to noncompliance with treatment, our estimand is the intent-to-treat effect (ITT) rather than the direct effect of meals per se.⁶

⁵An anonymized version of our preregistration can be found in Supplementary Information D or the OSF website: https://osf.io/qpxgn/overview?view_only=f03a6bc3003347f8ab460a709af02e7b

⁶In Supplementary Information C, we show that our results are similar if we estimate the Complier Average Causal Effect.

Table 2: Intent-to-Treat Estimates on Bipartisan Collaboration

	Cosponsorships	Roll Call Agreement (Exploratory)	
		2025	2026
Treatment	−0.056 (0.076)	−0.008 (0.013)	0.000 (0.011)
N	474	474	474
Pre-Treatment DV	✓	✓	✓

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: Intent-to-treat effect estimates from linear regression with HC2 standard errors in parentheses.

Results

Table 2 presents the results of our main analyses. The ITT estimate is negative and fails to reach statistical significance. Contrary to the expectations of reformers, this bipartisan social interaction does not have an impact on legislative collaboration. Because our design is well-powered to detect an effect—if only one pair of treated legislators cosponsored one additional bill, we have 80% power to detect it—our null finding is unlikely to be due to chance alone.

In the final two columns of the table, we can see that we obtain similar results with our exploratory measures of roll call agreement. Legislators assigned to have meals with one another do not agree with one another on roll call votes than other legislator pairs.⁷

It is possible that our null finding is driven by negative interactions between partners. However, feedback from legislators suggests that meals were generally quite successful (see Table 3). The main problems legislators experienced were in scheduling meetings. Even a legislator whose partner opted not to meet a second time reported that the partner “seemed interested during the first meeting” and that the first meeting was “cordial.” It appears that meals shared between opposing party legislators were largely positive experiences.

⁷In Appendix C, we show that these roll call results hold for competitive and lopsided votes.

Table 3: Email Feedback on Meals from Legislators

Representative	Quote from Email
Republican Rep. A	“[Democratic Rep. A] and I met at [restaurant] after a session where we ate, drank and talked for over 4 hours! I cannot speak for [Democratic Rep. A], but I can attest to the fact that I absolutely enjoyed myself and learned a lot about my colleague!! I have always believed that the things that unite us as American Citizens far exceed the things that divide us. And, I know that we have developed a friendship built around an understanding of who we are as humans.”
Democratic Rep. A	“I will say I feel the same! I would love to have another meal if possible. Looking forward to seeing you soon [Republican Rep. A]!”
Democratic Rep. B	“Thank you for supporting this program. We would likely not have made contact otherwise, but discovered some common legislative interests and expect to work together going forward.”
Democratic Rep. C	“Unfortunately I must report that my partner, [Republican Rep. C], is not interested in having another meeting. He did not indicate the reason in the very short email reply to request to work on a date. He seemed interested during the first meeting, and it was cordial.”

Discussion

In many experimental studies, there is concern that results could be driven by demand effects—subjects are aware that they are being studied and may alter their behavior to align with what they believe the researchers want to see. This concern is especially acute in contexts where the researcher spends money on the research subject (e.g., paying for the legislators’ meals). While we did not inform subjects about the specific outcomes we were studying, the purpose of the program was clear in recruitment and communications with legislators: fostering relationships across party lines. Because we failed to find a significant effect of treatment, the potential for demand effects in our study arguably strengthens confidence in our null results. Even though we were explicitly interested in promoting bipartisanship, treated pairs of legislators did not engage in more bipartisan collaboration.

One concern with our results may be that the modal number of bipartisan meals (one)

was insufficient to produce an effect, but higher doses of bipartisan interaction may be more effective. Yet, participants in our experiment had the option to participate in up to *three* meals with their partner. Even among these bipartisanship-oriented legislators, most chose either to meet only once or not to meet at all. Thus, it seems unlikely that simply increasing dosage would be sufficient to increase effect sizes, given that higher dosage was offered but not taken in our experiment. Instead, contact-based interventions may be more effective if they alter the physical proximity of legislators from opposing parties (e.g., [Masket, 2008](#)), generating organic social interactions but removing the “opt-in” nature of treatments like meal programs.

Another potential limitation of our study is the time between treatment and outcome measurements. Most meals occurred in May and June (before the end of the 2025 legislative session), and the process of introducing legislation for the 2026 session did not begin until the fall of 2025. Consequently, for many pairs, there were at least three months between treatment and the start of opportunities to submit bills they could collaborate on. Still, we believe our results are nonetheless informative for several reasons. First, while legislators could officially introduce legislation in the fall of 2025, they may work on and conceive of legislation outside of this window, meaning collaboration may begin before the endline official measures (closer to treatment).⁸ Second, social interaction has been presented as an important means for building personal relationships across the aisle. To the extent that social interactions are a powerful tool for increasing bipartisanship, it should be the case that treatment effects from such an intervention are lasting. That we do not find effects of social interaction suggests that it is unlikely to represent a practical or durable solution for increasing bipartisanship.

Finally, the low levels of bipartisan cosponsorship prior to treatment may reflect norms

⁸In email correspondence, one former legislator confirmed that the process of legislating varies widely across bills and legislators, with some beginning the process as much as a year prior to the official submission.

that do not favor bipartisanship in this particular legislature. It is possible that our social contact treatment would be more effective in a normative environment that favors bipartisanship.⁹ Future work could examine social interventions in more bipartisan legislatures. That said, to the extent that practitioners promote contact over meals or at sporting events as a method of reducing toxic partisanship among politicians, we believe our results in an environment that mimics the partisan conflict reformers seek to remedy are informative.

Conclusion

We present the results of a unique field experiment that randomly assigned bipartisan pairs of state legislators to have meals together. Contrary to the expectations of practitioners and legislators, we find little evidence that this form of positive social interaction increases bipartisan collaboration. Of course, this experiment occurred during a time of intense partisanship. It is possible that this intervention would have a different result in times of less partisanship and turmoil. However, it is precisely the gridlock created by this environment that practitioners hope bipartisan social contact will address. Moreover, the high degree of partisanship in American politics makes it less likely that legislators would have preexisting relationships across party lines, making the opportunities for establishing interpartisan connections through a program like this greater.

Our results join a growing body of work on the impacts of positive cross-partisan contact on bipartisan relationships (e.g., [Baron et al., 2025](#); [Rossiter and Carlson, 2024](#)). In contrast to previous findings, we find that contact is insufficient to increase bipartisanship (c.f., [Blair et al., 2025](#); [Frederick, Miller and Green, 2024](#)). While observational work finds that bipartisan contact is associated with increased legislative collaboration, we find no relationship, highlighting the importance of using randomized experiments to mitigate the

⁹We thank an anonymous reviewer for highlighting this possibility.

confounding from self-selecting collaboration partners. Reformers dedicated to increasing bipartisanship among politicians may need to seek out alternatives to opt-in social contact as means to those ends.

On a more optimistic note, despite the intensity of partisan polarization, 45 politicians agreed to have meals with their peers from the opposing party. In doing so, they recognized that interparty relations have been suboptimal and demonstrated a willingness to take concrete steps toward improving those relationships. While our meal-based intervention did not change behavior, it nevertheless revealed a demand among politicians for fixing partisan hostility.

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Supplementary Information for “The Effects of Sharing a Meal on Legislative Bill Cosponsorship Patterns”

Anonymized Author(s)

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A Ethical Considerations

Our field experiment did not involve deception of study participants, and we received approval for the study from the Institutional Review Boards at [REDACTED] and [REDACTED]. The design of our experiment, which involved paying for legislators’ meals, involved additional ethical considerations. We discussed this study with the government relations offices at our respective universities to ensure that they did not foresee any issues with the design. Further, we received approval from the Legislative Ethics Committee in the state legislature for the design: participation in the program was available to all legislators, and the provided meals were far less than the legal limits for gifts. Finally, we do not have any interests before the state legislature.

B Covariates

In our LASSO-selected covariate analysis, we consider several possible covariates. We selected covariates that we thought might be important predictors of legislative collaboration in the 2026 session. We used 10-fold cross-validated LASSO to select the lambda that minimized the Mean Squared Error, selecting the covariates with nonzero coefficient estimates at this lambda value. Averages for considered covariates are displayed in Table B.0.1. First, we include our preregistered covariate measuring the number of pre-treatment cosponsorships. Second, we gathered legislators’ seat numbers and locations in the House chamber. We created a series of dummy variables for each dyad indicating whether the pair’s seats were in the same section of the chamber, whether they were in the same section and row, whether they were adjacent to one another, and whether they were nearby (within a few seats in adjacent rows). Third, we gathered the gender of each legislator and created dummy variables indicating whether each dyad shared a gender and what gender that was if shared. Fourth, we gathered committee membership from the 2025 legislative session and counted the number of committee memberships legislators in a pair had in common. Fifth, we collected roll call votes from the 2025 legislative session and estimated legislator ideal points using these recorded votes. For each pair, we took the absolute difference between the legislators’ ideal points.

Sixth and finally, we collected a variety of district information for each legislator. We noted whether legislators in a pair represented the same county. We gathered constituency demographic information from the 2024 American Community Survey’s 5-year estimates.¹⁰ We reduced the dimensionality of the data using Principal Components Analysis, keeping only the first 11 components necessary to account for more than 80% of the variation in the data. We then calculated the Euclidean distance between these principal components for each pair of districts. We also gathered district-level election results from 2024, generating the absolute difference between districts’ Democratic presidential vote share, indicators for

¹⁰Specifically, we calculated the percent of the district population in different educational categories, racial and ethnic groups, age groups, and occupations; the percent who were women, noncitizen, foreign-born, veterans, and married; and the district population.

whether both districts in the pair were won by Harris or both by Trump and for whether one or both districts in the pair had presidential margins under 5 percentage points.

Table B.0.1: Dyad-Level Covariate Averages (Bipartisan Participant Dyads)

Variable	Average Value
Number of Cosponsorships (2025)	0.042
Chamber Seats in Same Section	0.063
Chamber Seats in Same Section and Row	0.008
Chamber Seats Adjacent	0.023
Chamber Seats Nearby	0.023
Same Gender - Woman	0.127
Same Gender - Man	0.314
Number of Same Committees	0.065
Difference between Roll Call Ideal Points	1.926
Represent Same County	0.215
Euclidean Distance District Demographics	4.501
Absolute Difference between Districts' Democratic Presidential Vote Shares (2024)	13.373
Both Districts Won By Harris	0.354
Both Districts Won By Trump	0.025
Both Districts Competitive	0.160
One District Competitive	0.536

C Alternative Effect Estimates (Exploratory)

In this section, we present alternative, unregistered estimates of our treatment's effect on bipartisan collaboration. These different estimates are presented in Table C.0.1 and confirm the null result is robust to numerous other specification decisions. First, because dyad-level pre-treatment cosponsorships did not appear to be a highly prognostic covariate, we estimate the ITT excluding pre-treatment cosponsorships. It is possible that the treatment affects the presence of a cosponsorship relationship but not the *intensity* of those relationships, so we estimate the ITT with indicator variables for whether a dyad cosponsored any bills pre- and post-treatment. Given that our outcome variables are count data,

we also test whether our results are robust to estimation with negative binomial regression. While most pieces of legislation in our data are bills, there are also constitutional and other resolutions introduced in the legislature. Resolutions are often more symbolic pieces of legislation and may be less meaningful than bills qua bills. Among participant dyads, there are no House resolutions or concurrent resolutions; however, there are constitutional amendment resolutions (though there is no variation in the pre-treatment dyad-level constitutional amendment variable, meaning we must exclude it from our regression). As such, we present separate estimates for legislative bills and constitutional amendment resolutions. Finally, we construct dyad-level covariate information and use LASSO to select prognostic covariates (Belloni, Chernozhukov and Hansen, 2014). By including prognostic covariates, we can increase power and account for any remaining imbalances between treated and control dyads. The only covariate selected by LASSO is the number of committees legislators in the dyad both served on. All estimates of the ITT are negative and statistically insignificant, consistent with our main specification.

As a further robustness check, we present the Complier Average Causal Effect (CACE). We take this step because some legislators opted not to have a meal with their partner, meaning our main estimates assess the effect of assignment to a meal dyad rather than the effects of the meals themselves. We can estimate the effect of having a meal more directly by taking the Complier Average Causal Effect (CACE). The CACE instruments for whether a dyad participated in a meal with the dyad’s assignment to treatment. To estimate the CACE in this way, we assume that dyads that were not assigned to treatment did not participate in meals. This assumption seems reasonable, given that several dyads assigned to treatment did not participate in meals—despite the meals being provided by the researchers. Our CACE estimate is substantively similar to our ITT estimates and is similarly statistically insignificant.

Finally, we gathered legislators’ roll call voting records in 2025 and in 2026.¹¹ We calculated the share of votes on which each bipartisan dyad of legislators agreed, excluding nonvotes. Because our treatment assignment occurred before the end of the session, our pre-treatment roll call agreement measure is limited to votes prior to assignment. This feature of the data also provides us with an opportunity to extract post-treatment measures closer to the time of assignment than our cosponsorship measure: we estimate the ITT for roll call agreement on votes taken in 2025 after treatment assignment. Using dyadic roll call agreement as the outcome of interest, we also estimate the ITT and CACE for 2026 cosponsorship measures. We find that all estimates are small and statistically insignificant (see the last three rows of Table C.0.1).

Continuing our exploratory analysis of roll call voting agreement, we examined whether the treatment could have affected voting more for more lopsided votes (i.e., votes on which

¹¹At the time of this writing the 2026 legislative session is ongoing, meaning that the roll call measures for the 2026 session are necessarily incomplete. However, this seems unlikely to affect our results: if there is no effect of treatment early in the legislative session (i.e., closer to the administration of treatment), it seems unlikely that such an effect would arise further from the treatment.

Table C.0.1: Alternate Estimates of Treatment Effects

Model	Estimate (S.E.)
ITT - No Pre-Treatment Cosponsorships	-0.027 (0.075)
ITT - Binary Outcome + Pre-Treatment	-0.013 (0.068)
ITT - Negative Binomial	-0.487 (0.749)
ITT - Bills Only	-0.046 (0.075)
ITT - Constitutional Resolutions, No Pre	-0.004 (0.003)
ITT - LASSO Covariates ¹	-0.043 (0.084)
CACE	-0.077 (0.146)
CACE - Roll Call Agreement (2026)	0.004 (0.016)

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

¹ For full list of possible covariates, see Supplementary Information [B](#).

there was less potential party pressure for voting a certain way).¹² We test different thresholds for lopsidedness, ranging from 70-30 to 90-10. It should be noted that the samples of votes for these analyses are quite small (between two and 22 votes). Despite the potentially lower pressure from party leadership, we find no statistically significant intent-to-treat effects from our meal program (see Figure [C.0.1](#)). Additionally, following ([Masket, 2008](#)), we examine whether there was more evidence of influence on more competitive votes. We again test different thresholds, ranging from bills that were 55-45 or closer to bills that were 70-30 or closer. Here, too, we find no evidence that assignment to treatment impacted roll call voting agreement.

¹²We thank an anonymous reviewer for suggesting this analysis.

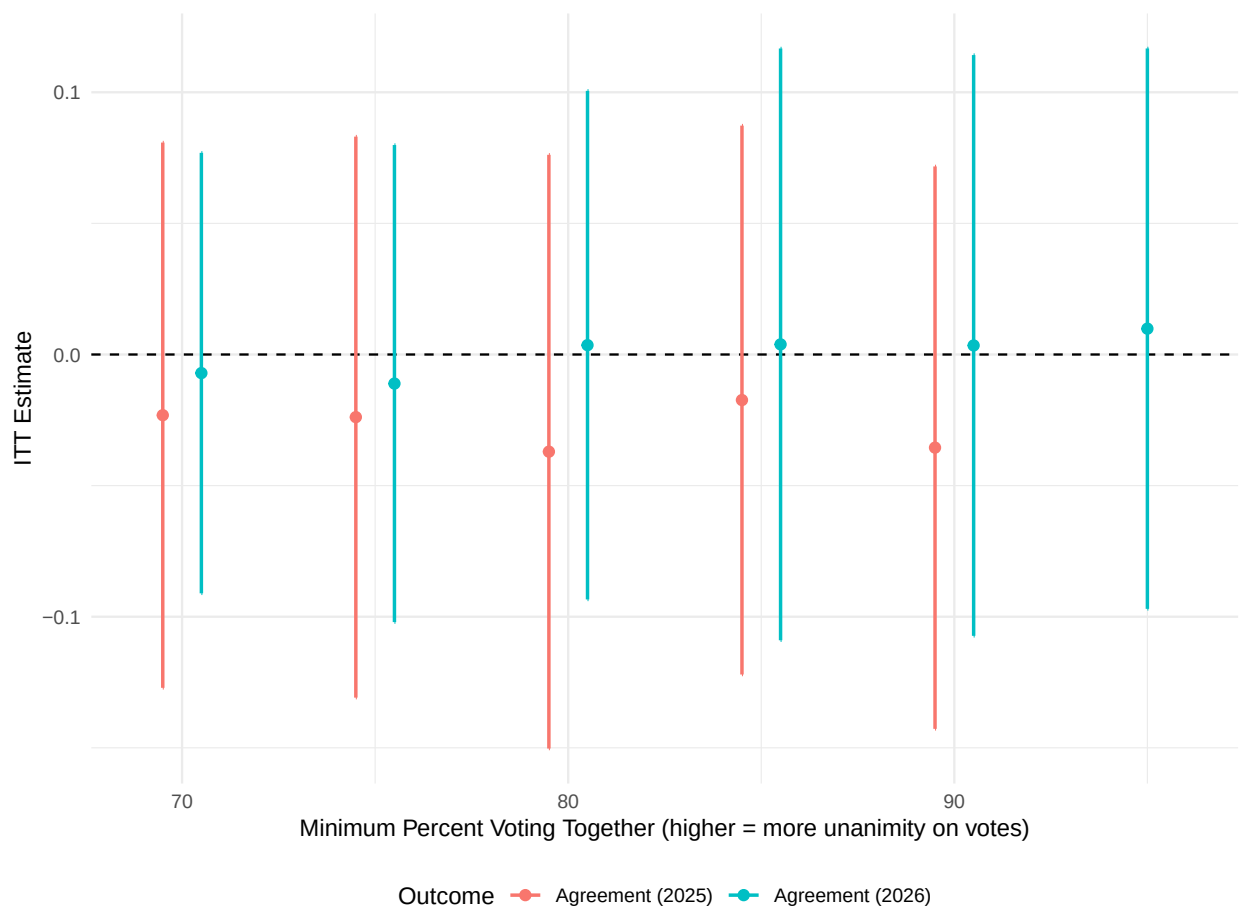


Figure C.0.1: Pairwise Agreement Rates on Lopsided Roll Call Votes

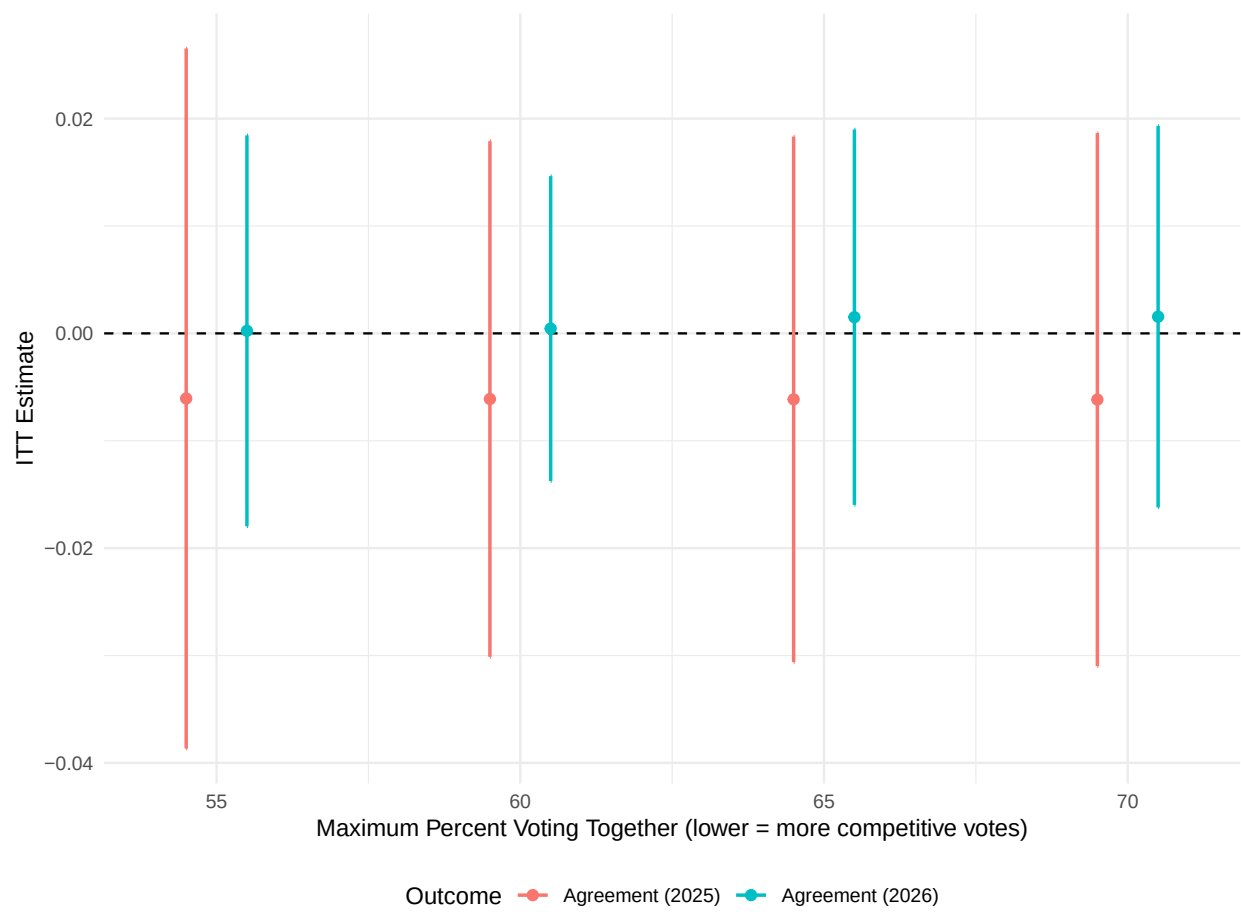


Figure C.0.2: Pairwise Agreement Rates on Competitive Roll Call Votes

D Anonymized Pre-analysis Plan

Title: The Effects of Sharing a Meal on Legislative Bill Cosponsorship Patterns

Research Team:

Anonymized

Description: State legislators in a northeastern state were recruited by leaders of a bipartisan caucus to participate in a voluntary program where they would be partnered with someone from the other party and would eat 1-3 meals together. Legislators who signed up to participate were randomly matched with someone from the other party who also signed up for the study. We then encouraged them to have a meal together at a local restaurant paid for by the research team. We will analyze whether legislators subsequently cosponsored more bills with the legislator they were partnered with than with the other-party legislators in the study that they were not partnered with.

Hypotheses:

H1: Legislators are more likely to cosponsor bills with their assigned meal partner.

Design Plan

Study Type: Experiment

Blinding: No blinding is used

Study Design: We worked with leaders of the bipartisan caucus in a northeastern state legislature to recruit legislators to participate in the “Breaking Bread” program that was being done in collaboration with researchers from [REDACTED] University. They were told that as part of the program, each legislator would have 1-3 meals with a legislator from the other party, with the meals being paid for.

Legislators signed up by filling out a form on a website. The form asked legislators to indicate any legislators from the other party that they would be unwilling to meet with. We asked legislators this question to prevent creating matches where people knew they would be unwilling to meet.

We also partnered with several restaurants in near the state capitol to create an account so legislators could meet for a meal and have it charged directly to our research team. The lunches took place between May and August 2025. While we offered to pay for up to 3 meals, the modal legislative pair met just once. For legislative pairs that had not met yet, we sent various follow ups and reminders.

We chose to have the treatment during this period because it occurred after all the bills were proposed during the 2025 legislative session, but before the bills for the 2026 legislative session have been filed. We can use the sponsorships of the 2025 session as a pre-treatment measurement of the dependent variable.

Randomization: Legislators who signed up to participate were randomly assigned to a legislator from the opposing party. Because there were more Democratic than Republican legislators who volunteered for the program, some Democratic legislators were not matched. For each Republican participant, we randomly selected a Democratic participant until all Republicans were paired.

Sampling Plan

Registration Timing: The registration occurs after the treatment has been delivered but prior to the collection of the dependent variable.

Data collection: We will collect data on bill cosponsorships from the state legislature's website.

Sample Size: We had 26 Democrats and 19 Republicans sign up to participate. Most of the analyses will be based on the universe of cross-party pairs (or dyads) that could have been created. In other words, these dyads represent our sample. The treatment is the 19 pairs that were matched. The control condition includes all the pairings that were not made, subject to the data exclusions noted below. In total, after excluding pairings based on legislator requests, there were 474 possible pairs of legislators across party lines, including Democrats who were not ultimately matched to a Republican. Of these, 19 were treated, leaving 455 control dyads.

Sample Size Rationale: Given the low rates of cross-party cosponsorships (close to 0), we are powered to detect an effect if even 1 treated pair cosponsor together.

Variables

Manipulated Variable: Whether the legislative cross-party dyad was assigned to have meals together.

Outcome: We look at how many bills each dyad cosponsor during the 2026 session. We use the same measure during the 2025 session as a control variable.

Analysis Plan

Statistical Models: For H1 we will estimate a regression model where we include the treatment variable and the lagged dependent variable. We will use HC2 standard errors.

Data Exclusion: When signing up to participate, legislators were asked if there were legislators from the other party they would be unwilling to meet with. We exclude any match that a legislator indicated from the sampling frame (and thus it is not used in either the treatment or control conditions). One legislator requested not to be matched with another legislator who also signed up, and this possible pairing was excluded from randomization. Additionally, one Democratic legislator requested not to be paired with Republicans with a specific ideological point of view. Given the challenges of identifying partners (in)consistent with this request, this legislator was excluded from randomization.

IRB: This study was approved by the (Anonymized) University IRB Board

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