

# Talking Shops: The Effects of Bipartisan Deliberation on Legislative Outcomes

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## Abstract

Group deliberation is a defining feature of representative legislatures. However, there is little evidence that legislators actually engage in good faith deliberation or the sharing of communication that influences individual or collective choice outcomes. To what extent can deliberation influence policy positions in today's polarized legislatures? This paper reports results from two field experiments in which state legislators randomly selected bills for supplemental, bipartisan group deliberation. Deliberation increased individual support for selected bills. Estimated effects are similar in magnitude for legislators regardless of whether they belong to the sponsor's party. Deliberation did not appear to affect bill-level outcomes such as bargaining over bill content or the probability of bill passage, though power limitations make studying such policy-level outcomes infeasible. Qualitative data from the experiment suggest one constraint on legislators' ability to deliberate across party lines: pressure from partisan voters.

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A defining feature of representative lawmaking, deliberation has drawn attention from statesmen and scholars for centuries. Deliberative processes are thought to legitimize the aims, methods, and outputs of democratic government (Mill 1861; Bessette 1997; Habermas 1997; Gilligan and Krehbiel 1987). Deliberation has been called the “first and capital purpose” (Wilson 1908, p. 11) of representative assemblies:

“They were meant to be talking shops. The name ‘parliament’ is no accidental indication of their function.... They were meant to be grand parleys with those who were conducting the country’s business: parleys concerning laws, concerning administrative acts, concerning policies and plans at home and abroad, in order that nothing which contravened the common understanding should be let pass without comment or stricture...”(Wilson 1908, p. 11).

Despite their perceived importance, deliberative processes have often been found lacking in American legislatures. Senator Howell Heflin said that “Few in their right mind will argue that [Congress] suffers from too much deliberation, analysis, or thought” (Loomis 2004, p. 9). A “partisan steamroller” (Sinclair 2014, p. 345) has replaced once civil and bipartisan committee proceedings (Manley 1965; Fenno 1973; Lee 2016). Long-serving Republican Senator John McCain described the current state of Congress: “Our national political campaigns never stop. We seem convinced that majorities exist to impose their will with few concessions and that minorities exist to prevent the party in power from doing anything important” (McCain 2017). One op-ed writer recently lamented that “The Senate, the world’s greatest deliberative body, no longer deliberates” (Davenport 2018).

The decline of deliberative processes coincides with increasing partisan polarization both in Congress and in the state legislatures (Shor and McCarty 2011). Some legislative leaders view policymaking more as a tool for winning elections than as the reward for winning them (Lee 2016). It is a fair question to ask whether contemporary legislators are able, or willing, to engage in bipartisan deliberation in which they listen to peers across the aisle before deciding what position to take on legislation.

Although it is clear to most observers that deliberation has become less frequent and more partisan, empirical research on deliberation has had little to say about the effects of these changes on legislative outcomes. The main reason for this silence is that existing empirical work is limited to observational designs that avoid causal questions of interest. Case studies have traced changes in the content of policy proposals as legislators discuss them and receive public feedback (Landy, Roberts, and Thomas 1990; Granstaff 1999; Derthick and Quirk 2001; Quirk and Mucciaroni 2006; Wirls 2007). Quantitative studies have measured the quality or quantity of speech, in some cases to show how deliberation varies with institutions (Sinclair 1990; Steenbergen et al. 2003; Connor and Oppenheimer 1993; Bara, Weale, and Biquelet 2007; Bächtiger et al. 2008; Taylor 2012). Neither approach can answer the important causal questions of whether deliberation influences individual or collective choice outcomes. As a result, legislative studies of deliberation have often avoided the questions of most interest to deliberation theorists (Mutz 2008).

Deliberation is nevertheless thought to influence important political behaviors and outcomes. Formal theories of communication predict that deliberation can influence both individual and collective choice outcomes (Gilligan and Krehbiel 1987; Austen-Smith 1993a,b; Austen-Smith and Feddersen 2006; Bennedsen and Feldmann 2002; Battaglini 2002; Chakraborty and Harbaugh 2010; Schnakenberg 2015, 2017; Sobel 1985; Ottaviani and Sørensen 2006; Dziuda 2011; Hafer and Landa 2007; Eliaz and Spiegler 2018). Lab and field experiments show that deliberation among the mass public can influence their policy preferences and vote choices (Luskin, Fishkin, and Jowell 2002; Dickson, Hafer, and Landa 2008; Farrar et al. 2010; Fujiwara and Wantchekon 2013; Wantchekon et al. 2017; Battaglini et al. 2019). Yet, we lack evidence on the extent to which legislative deliberation influences these same outcomes.

This study examines whether bipartisan deliberation is influential in contemporary American legislatures. The main question is the extent to which bipartisan deliberation influences legislators' individual policy positions. This micro-level effect of deliberation speaks to broad

debates about partisanship and effective lawmaking in American legislatures, as well as to the ability of legislative institutions to function during times of partisan polarization.

To estimate the effects of deliberation, I conducted two experiments in a state legislature. Leaders of a bipartisan legislative caucus randomly selected some bills sponsored by members for supplemental deliberation among the group. The main outcomes of interest are the policy positions of legislators who participated in deliberations. I also examine bill-level outcomes, whether bills selected for deliberation received more filed amendments or were more likely to be passed by the legislature.

Results show that deliberation increases legislators support for legislation. Bill sponsors were able to convince both in-partisans and out-partisans to support their bills. There is no meaningful effect of deliberation on bill-level outcomes. However, power limitations make such analyses infeasible. A case study suggests that framing is one way legislators build bipartisan support for their bills, subject to an important constraint: partisan voters limit legislators' efforts at bipartisanship.

This study contributes to the literature in several ways: 1) by illustrating how a deliberation experiment can be conducted in a legislature, where noncompliance can be acute; 2) by estimating the causal effect of deliberation on legislative behavior; 3) by assessing the impact of deliberation on bill-level outcomes; and 4) by exploring legislators' persuasion strategies and the limits of their effectiveness.

## **The Limits of Deliberation**

Legislative communication has traditionally been approached as a cheap talk game (Austen-Smith and Riker 1987; Gilligan and Krehbiel 1987, 1989; Austen-Smith 1993a,b). In the canonical cheap talk game, an expert has private information about a policy under consideration. An uninformed decision-maker would benefit from this information, but the expert can lie without incurring any cost. As a result, conflicts of interest between the expert and decision-maker make the expert's communication unreliable.

It seems every actor in a legislature has their own interests, so legislative scholars interpreted this result as suggesting that credible communication was unlikely if not impossible in most settings:

“[O]pportunities for strategic use of information are so extensive that, except under the most extreme circumstances, the collective wisdom of the committee — that is, full, precise, and truthful aggregation of private information — could not work its way into the collective choice” (Krehbiel 1992, p. 65).

Legislative scholars internalized this conclusion. Legislative studies came to be dominated by models of politics in which legislators’ policy positions are determined by factors outside the legislature rather than by communication within it. Rather than being influenced by deliberation, whipping, or lobbying, positions came to be determined by legislators’ ideology. Ideology, as a stable, exogenous factor that exists beyond the day-to-day activities of the legislature, came to be seen as the main, if not only, determinant of position-taking (Brady and Volden 1998; Krehbiel 1992, 2010; Lee 2009).

This idea that communication within the legislature does not influence policy positions is troubling to anyone who has spent time in a legislature. Legislators spend a great deal of time deliberating, building coalitions, lobbying one another, and whipping votes. These activities do not fit neatly into models of ideology-driven position-taking. Yet, cheap talk models seems to suggest that these behaviors are unlikely to be persuasive, so they can safely be ignored.

The cheap talk models of the 1980s and 1990s give an incomplete picture of costless communication. Recent work on cheap talk has shown that even costless communication is credible in a far wider range of circumstances than previously thought. The original cheap talk model applied to legislatures featured one expert, one dimension of policy, one recipient, and one period of play. Relaxing these limitations can make communication more credible and even fully informative (Bennedsen and Feldmann 2002; Battaglini 2002; Chakraborty and Harbaugh 2010; Schnakenberg 2015, 2017; Sobel 1985; Ottaviani and Sørensen 2006).

Conflicts of interest may make communication more difficult, but it does not make it impossible.

Scholars have also recently moved beyond the literature's focus on a particular type of information. Early cheap talk models focused on private policy expertise. Yet communication may contain other types of information. One promising area of work is in argumentation (Dziuda 2011; Hafer and Landa 2007; Eliaz and Spiegler 2018). When an expert recommends a course of action to a decision-maker, she will typically also provide a rationale or argument for her recommendation. There are many ways that models of arguments differ from models of technical policy expertise, but one merits particular attention. Legislators always benefit by learning policy expertise; it helps them make more informed decisions. Arguments need not do the same. A speaker can strategically frame an issue to win support from peers who, if perfectly informed about all the relevant arguments, would oppose the bill.

Cheap talk models motivate the main questions of this paper: can deliberation influence legislators' position-taking behavior, or is such communication not credible or persuasive? Can deliberation's influence reach across party lines given conflicts of interest between the parties? What types of information do legislators share when deliberating, and does communication make legislators better or worse off? To answer these questions, I conducted two field experiments in which legislators deliberated randomly selected legislation.

## **Experimental context and design**

The experimental design is similar in spirit to deliberative polls conducted among voters (Luskin, Fishkin, and Jowell 2002; Farrar et al. 2009, 2010). The intervention is an active persuasion design. Communication was prescribed on specific topics, and not others, and thus actively managed the content of deliberations. Studies with such designs have observed larger deliberation effects than an alternative, the passive design, in which the composition of groups is randomized but not the content of discussion (Farrar et al. 2009, 2010; Fujiwara and Wantchekon 2013; Wantchekon et al. 2017). Compared to prior deliberative poll experiments,

this study is notable in that deliberations occurred in a legislature, during session, among legislators, and on real policy proposals.

Two field experiments were conducted in a setting that is broadly characteristic of many state legislatures.<sup>1</sup> Squire (2007) ranks the state in the bottom half in terms of professionalism, but there are at least a dozen lower. The National Conference of State Legislatures categorizes it among the 40 part-time state legislatures. The state government featured single-party control of government, as did a majority of states in 2016 and 2019 when the interventions were fielded.

## **Subjects**

The interventions were conducted with members of the Bipartisan Freshman Caucus (BFC). Caucuses are voluntary, informal, and typically bipartisan groups that focus on policy-making (Hammond 2001; Dilger and Gerrity 2009; Ringe, Victor, and Carman 2013). Lacking formal gatekeeping or proposal powers, they are thought to affect policymaking through their role as information clearinghouses. Caucuses in Congress and state legislatures have exploded in number since the 1970s, during the same period as the floor and committees began losing their reputations for cooperation and honest deliberation (Sinclair 1990).

The BFC is unique because its members are all first-term legislators. Many legislatures feature first-term caucuses, as new members face unique challenges adjusting to the legislature.<sup>2</sup> First-term legislators are thought to be more partisan than their more senior colleagues because they lack relationships with peers across the aisle (Caldeira, Clark, and Patterson 1993; Francis 1962; Price and Bell 1970; Sarbaugh-Thompson et al. 2006). To the extent novice legislators are particularly partisan or uninformed about policy proposals or one another, they may be particularly responsive to deliberation.

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<sup>1</sup>The state is not named in order to preserve ongoing research projects.

<sup>2</sup>For example, first-term caucuses may invite audio-visual staff to discuss resources for engaging with the media. In some cases, first-term caucuses even organize across party lines to advocate for more representation of new legislators on desirable committees.

## Recruitment and assignment procedure

In 2016, two co-chairs of the BFC agreed to hold meetings in which members would pitch their legislation to the group. The experience was intended to pursue two goals: 1) to provide practice for legislators who would later need to present their bills to committees and the floor, and 2) to build policy coalitions. To determine whether deliberation was a productive use of time, we would randomly select which bills would be chosen for discussion. Bills would be chosen from a list of bills that the caucus members agreed would be suitable to present at the caucus.

In the first study, the 16 members of the BFC identified 45 bills that they would be willing to present to the group. One bill was chosen at random for each member to present to the caucus. Because each member was allowed to present only once, the probability of bills' assignment to treatment varies across members. Inverse probability weights are used in all analyses to account for differential treatment probabilities across legislators.

One feature of the experimental design requires additional attention. Legislatures are busy places, so legislators often miss important meetings. It seemed likely that attendance at the caucus meeting would not be perfect. Non-attendance could cause two important problems. First, non-attendance creates noncompliance. Legislators who do not attend would not be able to receive treatment, nor would they be able to administer treatment on their own bills. Low treatment administration rates would reduce the experiment's power. Second, attendance might be revealed post-treatment if legislators knew the agenda ahead of time. As a post-treatment covariate, we would not be able to estimate deliberation effects among just legislators who attended the meetings, since treatment might plausibly influence whether legislators attend. We would be forced to estimate deliberation's pooled effects among both attendees and absentees, despite these groups receiving fundamentally different treatments.

I accounted for legislators' likely absence using a variant of a matched rollout protocol (Nickerson 2005). Legislators were not informed which bills had been selected for treatment

in advance of the meetings. This procedure yields two advantages. First, we can drop bills from the study for which treatment administration was never attempted. Bill sponsors were not aware of the treatment assignment when they decided not to attend the meeting, so attendance is independent of treatment assignment.<sup>3</sup> Dropping these bills improves power despite the decline in sample size because it increases the treatment administration rate. Second, attendance becomes a pre-treatment covariate. We can thus estimate separate deliberation effects among attendees and non-attendees. Although I expect deliberation to be most influential among attendees, effects might spill over to legislators who did not attend as well (Zelizer 2019).

To improve power and generalizability, I replicated the experiment in the same state in 2019. The 2018 election resulted in a particularly large number of new legislators. There were 27 first-term legislators, who accounted for 64 bills that could be discussed during the caucus meeting.

As expected, attendance proved to be imperfect. Of the 16 BFC members in the first experiment, 9 legislators presented bills at the caucus; of the 27 BFC members in the second experiment, 17 presented. Combined, the two studies yielded 26 bills in treatment and 41 in control. With 99 legislators in the chamber, these 67 bills translate to 6,633 individual policy positions by legislators, including 1,031<sup>4</sup> by legislators who attended the caucus meetings.

## **Treatment**

Treatment is the opportunity to present a bill at a caucus meeting. This design encouraged sponsors to discuss selected bills, but it did not dictate the content of their appeals. This relatively light touch preserves realism and minimizes the researcher's role in crafting information. One result of this light touch, however, is that treatment cannot be defined as a

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<sup>3</sup>The estimand must also be reconsidered. Rather than the effect of deliberation on behavior for a non-random subset of bills sponsored by all first-term legislators, the estimand becomes the effect of deliberation on behavior for a non-random subset of bills sponsored by a non-random subset of first-term legislators.

<sup>4</sup>Two additional first-term legislators in 2016 and one in 2019 attended meetings but had no bills to present; these legislators are not counted in discussions of treatment administration or compliance because they submitted no bills in advance of the meetings that they were willing to present.

particular persuasive or informational message. The sponsor is herself choosing the content she thinks will be most persuasive. This is an opportunity to evaluate the kinds of appeals legislators make.

Legislators routinely shared both technical expertise and arguments for supporting the policy. Many sponsors began with a detailed discussion of technical, substantive policy information. One bill allowed public transit buses to drive on highway shoulders to ease traffic congestion. The sponsor described which highways would be eligible for the program and highlighted the program's unblemished safety record in another state. Another legislator shared the predicted fiscal cost of his bill to provide veterans with free hunting licenses, as well as the predicted costs for scaled-down versions of his proposal. These messages resemble the private expertise discussed in cheap talk models. Legislators could provide biased signals of their legislation's cost or impacts if they wanted, as listeners could not easily verify the information provided.<sup>5</sup>

Legislators also made persuasive appeals that more resemble framing arguments than policy expertise. The sponsor of a bill to provide opioid antagonists to first responders noted that his brother-in-law had passed away from an opioid overdose, invoking the personal costs of addiction. One legislator sponsored a bill to prevent individuals from providing guns to individuals who had their gun rights revoked due to mental health emergencies. She stressed that the bill was not about gun rights, which she supported, but about public health, by enforcing current laws intended to keep guns out of the hands of certain individuals. She noted that the bill was motivated by a recent mass shooting in the state carried out by an individual who was given a gun by his father despite having recently been receiving treatment for schizophrenia. Many legislators discussed the reasons why they sponsored their bills and why those reasons should be compelling to others as well. These appeals

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<sup>5</sup>The phrase "easily verifiable" addresses a complication in the application of cheap talk models to actual politics. Such models deal with unverifiable information, but it is unclear in practice what information about legislation can be verified or not, or how costly it is for a listener to verify information from a speaker. For the purposes of this paper, it seems sufficient to note that listeners were unwilling to invest their scarce time and resources to verify the speakers' messages.

incorporate arguments rather than private technical expertise.

## **Outcome measures**

Legislators' individual positions on legislation are the main outcome of interest. Cosponsorship and roll call voting are both important forms of position-taking (Mayhew 1974; Koger 2003; Highton and Rocca 2005; Kessler and Krehbiel 1996; Peress 2013; Wawro 2010; Woon 2008). Harbridge (2015) argues that cosponsorship is particularly relevant in studies of bipartisanship, as bipartisan behaviors typically occur early in the process before legislation becomes contested. Building a bipartisan cosponsorship coalition can be a strong signal to party leaders that legislation is broadly supported and merits plenary time (Kessler and Krehbiel 1996).

Cosponsorship is also a clear indicator of legislators' policy preferences. Former Senator Richard Lugar argued that cosponsorship is a better indicator of individual positions than roll call voting:

“Members' voting decisions are often contextual and can be influenced by parliamentary circumstances. Sponsorships and co-sponsorships, in contrast, exist as very carefully considered declarations of where a legislator stands on an issue” (Lugar 2017).

Roll call voting is the canonical form of position-taking in legislative studies. However, roll call voting is not the ideal experimental outcome measure. Since bills that do not reach the floor do not experience a vote, there is attrition in roll call data. If treatment affects attrition, anything but minimal attrition would preclude the precise estimation of treatment effects on roll call voting.

I can, and do, examine whether treatment increased or decreased the probability of a bill reaching a vote, but I cannot prove that treatment exerted no effect. As a result, for the

analysis of roll call voting, it is assumed.<sup>6</sup> Under this assumption, treatment effects can be estimated among the subset of bills that reach a vote regardless of treatment assignment.

Bill level outcomes include the number of amendments filed for each bill and whether bills are enacted into law. Every experimental bill that reached the floor became law, so estimated effects of treatment on bill passage conflate several steps in the policymaking process — passing committee, passing the lower chamber, passing the upper chamber, and being signed by the governor — that I would want to examine separately if not limited by the data.

## Compliance

The risk of informing legislators of treatment assignment during the caucus meetings is that sponsors may not speak about the selected bill. Table 1 displays treatment delivery by treatment assignment among the set of experimental bills sponsored by legislators who attended meetings. Twenty-one out of twenty-six (81%) bills assigned to treatment received it; two of forty-one (4.9%) bills assigned to control were discussed.<sup>7</sup> Due to these modest rates of non-compliance, analyses below focus on intent-to-treat effects.

*Table 1: Compliance with deliberation assignment.*

		Treatment Delivered?	
		No	Yes
Treatment Assigned?	No	39	2
	Yes	5	21

Bills sponsored by legislators absent from the caucus meetings are omitted from display, as are bills that received treatment but were not included as experimental bills.

<sup>6</sup>The technical assumption is that there are always attriters and never attriters. There are no if-treated attriters or if-untreated attriters. The absence of roll call data can be thought of as survival rather than attrition, but the assumptions remain the same.

<sup>7</sup>When legislators did not speak about the selected bill, in three of the five cases they discussed a bill that was not originally selected for inclusion in the studies.

## Balance

Table 2 presents tests of covariate balance for the 67 experimental bills. Balance is evaluated across two bill-level covariates: pre-treatment cosponsorship and whether the bill’s senate sponsor was a member of the same party as the house sponsor.<sup>8</sup> Both covariates might be correlated with post-treatment position-taking. Standard errors and p-values are obtained through randomization inference.

Bills assigned to treatment have modestly more pre-treatment cosponsors and are slightly more likely to have a senate sponsor of the opposing party. Neither of these differences reach conventional levels of statistical significance. An omnibus test examining whether the covariates jointly predict treatment assignment generates an F-statistic of 7.8, which is smaller than 65% of statistics from simulated random assignments. The profiles of bills across treatment and control conditions appear balanced.

*Table 2: Balance by deliberation assignment.*

	Treatment Assignment		Difference
	Control	Treatment	
Pre-treatment cosponsorship	0.021 (0.003)	0.024 (0.004)	0.003 (0.007)
Bipartisan sponsor	0.144 (0.021)	0.179 (0.040)	0.035 (0.059)
F-statistic (p-value)		7.8 (0.653)	
N	41	26	

Significance indicated at  $p < 0.10$  (\*) and  $p < 0.05$  (\*\*) two-sided. Standard errors and p-values obtained from randomization inference with 1,000 simulated treatment assignments.

## External validity

Several aspects of the intervention make it particularly well-suited to finding large effects of deliberation. Most importantly, the experimental universe of bills is not representative

<sup>8</sup>This legislature utilizes dual-track legislating which requires identical bills to be carried and passed in each chamber.

of the broader policy agenda. Legislators selected bills that they thought were appropriate for bipartisan discussion. They likely omitted highly partisan, ideological proposals. Caucus meetings in state legislatures are low-profile proceedings. Although technically open to the public, they are largely ignored by the press and by activists. Partisanship may be muted in such settings compared to committee hearings or floor debates, and personal relationships or social norms may be more important. Only some legislators attended the caucus meetings, and they may differ from their peers. These characteristics may not apply to other legislative contexts, most notably Congress. Nevertheless, the context is largely representative of policymaking in state legislatures and even in Congress. Many policy proposals receive some bipartisan support. Caucuses are prevalent across American legislatures, and they are frequently bipartisan, low-profile policymaking groups.

## Results

I begin with a measure of individual position-taking: cosponsorship. Table 3 displays weighted average cosponsorship rates for control and treatment group observations. Table 3 shows cosponsorship among all legislators in the assembly (in the third row and third column), but also for subsets of the legislature.

First, I expect deliberation effects to be largest among legislators who actually attended the caucus meetings and received the treatment first-hand. This focus on attendees motivated the experimental design. Although non-attendees did not receive the treatment directly, they may have been exposed to a form of treatment contagion, so I examine their positions as well. The columns in Table 3 break out cosponsorship by attendance.

The rows in Table 3 separate positions by whether legislators belong to the same party as the bill sponsor. The Bipartisan Freshman Caucus was selected for the intervention because it provided an opportunity for bipartisan deliberation. I want to know if deliberation only influences support among a bill sponsors' co-partisans or if it also applies to out-partisans. The primary outcomes of interest are bill positions by meeting attendees, among in-partisans

and out-partisans.

Cosponsorship rates are higher on average, and indeed for every subset of the subject pool, in treatment than control. Overall differences are relatively modest, with the average cosponsorship rate in the treatment group equal to 6.5% and in the control group 4.3%. Differences are substantially larger for specific subsets of the population. Cosponsorship rates among attendees is substantially larger in the treatment group (9.7%) than the control group (4.2%), while rates for absentees in the treatment group (5.9%) are only slightly larger than in the control group (4.3%). Cosponsorship rates are about five percentage points higher in the treatment group than the control group for attendees whether they belonged to the same party as the sponsor or not.

Estimated treatment effects are obtained using weighted least squares regression. A model that compares cosponsorship (and, later, voting) on treated bills to untreated bills is the following:

$$\begin{aligned}
 Y_{ij} = & a + b_1 d_j + b_2 \text{Pre-treatment Cosponsorship}_{ij} \\
 & g_1 \text{Leg } 1_i + g_2 \text{Leg } 2_i + \dots + g_{138} \text{Leg } 138_{138} + \\
 & h_1 \text{Sponsor } 1_j + h_2 \text{Sponsor } 2_j + \dots + h_{25} \text{Sponsor } 25_{25} + u_{ij}
 \end{aligned} \tag{1}$$

where  $Y_{ij}$  indicates cosponsorship (or roll call Yea vote) by legislator  $i$  on bill  $j$ ,  $d_j$  is an indicator variable for whether bill  $j$  was assigned to treatment, and  $u_{ij}$  represents unmeasured determinants of bill support. Weights equal the inverse of observations' probability of assignment to their realized treatment conditions. The key parameter of interest is  $b_1$ , the average intent-to-treat effect of deliberation. Standard errors and resulting p-values are obtained via randomization inference.

In order to improve the precision with which the parameter  $b_1$  is estimated, Equation 1 includes pre-treatment covariates that predict bill support.  $\text{Pre-treatment Cosponsorship}_{ij}$  is an indicator variable that equals one if the legislator cosponsored the bill prior to the caucus

*Table 3: Cosponsorship by Attendance, Party, and Treatment Condition (in %).* Un-weighted number of observations displayed in parentheses.

		Control Group		
		Attended Meetings?		
		No	Yes	All
Sponsor's Party?	No	2.3 (1,570)	4.0 (295)	2.6 (1,865)
	Yes	5.8 (1,863)	4.3 (331)	5.6 (2,194)
	All	4.3 (3,433)	4.2 (626)	4.3 (4,059)
		Treatment Group		
		Attended Meetings?		
		No	Yes	All
Sponsor's Party?	No	2.6 (872)	10.5 (180)	3.9 (1,052)
	Yes	8.5 (1,297)	9.0 (225)	8.6 (1,522)
	All	5.9 (2,169)	9.7 (405)	6.5 (2,574)

meeting. Because cosponsorships can be, and in this experiment are, withdrawn, not every pre-treatment cosponsor remained a post-treatment cosponsor. The remaining predictive covariates are legislator and bill sponsor fixed effects that account for different intercepts in bill cosponsorship (or roll call voting) that occur across bills for each legislator and bill sponsor.<sup>9</sup>

*Table 4: Estimated ITT Effects (with Standard Errors) of Deliberation on Cosponsorship (in pp).*

		Attended Meetings?		
		No	Yes	All
Sponsor's Party?	No	0.4 (1.2)	5.8** (2.9)	1.3 (1.4)
	Yes	2.2 (1.9)	4.2 (3.9)	2.5 (2.1)
	All	1.5 (1.5)	4.9** (3.0)	2.0 (1.6)

Significance indicated at  $p < 0.10$  (\*) and  $p < 0.05$  (\*\*) one-sided. Standard errors and p-values obtained from randomization inference with 1,000 simulated treatment assignments.

Table 4 displays ITT effect estimates. Estimated deliberation effects are largest among meeting attendees. Deliberation doubled cosponsorship rates among attendees. Estimated effects are similar in magnitude for in-partisans (4.2 percentage points) and out-partisans (5.8). Non-attendees were more likely to cosponsor treated bills by 1.5 percentage points, consistent with a degree of treatment spillover. However, estimated spillover effects do not attain conventional levels of statistical significance.

<sup>9</sup>There are 26 unique bill sponsors in the study and 139 unique legislators. One legislator and bill sponsor indicator variable are omitted to avoid collinearity.

## **Policy consequences of deliberation**

Cosponsorship is an important form of position-taking, but did deliberation influence bill-level outcomes? What were the consequences of deliberation on the disposition of proposals? Table 5 presents estimated effects of deliberation on bill-level outcomes. I examine whether treated bills were more likely to receive amendments and be passed into law.

Estimated intent-to-treat effects of deliberation are again estimated via weighted least squares regression with standard errors and p-values estimated through randomization inference. However, bills are now the unit of analysis. Pre-treatment covariates include the number of pre-treatment cosponsors for each bill and whether the bill has bipartisan house and senate sponsors.<sup>10</sup>

There is little evidence that treatment affects bill amendment or passage, as ITT estimates for both outcomes are close to zero. However, bill-level treatment effects are estimated with very little precision. Deliberation would have to increase the probability of amendment or passage by twenty percentage points or more to be detected with this research design. These results illustrate that while legislative experiments that assign treatments at the bill level can achieve substantial power to examine individual legislative outcomes, they are underpowered when it comes to examining bill-level outcomes. Future studies would require either a substantial increase in the number of bills included — a four-fold increase to 268 bills would still only halve standard errors — or the use of highly prognostic covariates to study the effect of interventions on bill outcomes.

## **Deliberation Effects on Roll Call Voting**

I can examine the effects of deliberation on roll call voting if we are willing to accept a substantial assumption regarding attrition. Only 20 of the 67 bills in the study (30%) reached the floor of the chamber for a vote. As a result, roll call voting data are unavailable

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<sup>10</sup>Due to the small number of bills per sponsor, sponsor fixed effects are excluded. Estimated effects do not differ, nor become more precise, with the inclusion of sponsor fixed effects.

Table 5: **Estimated deliberation effects on bill-level outcomes (in pp).**

	<b>Amendment Filed?</b>	<b>Passed</b>
$\widehat{\text{ITT}}$	-1.3	0.2
$(\widehat{\text{SE}})$	(13.0)	(12.0)
N	67	67
$\bar{Y}_{\text{Control}}$	29%	26%

Significance indicated at  $p < 0.10$  (\*) and  $p < 0.05$  (\*\*) two-sided.

for a majority of the observations. However, if deliberation did not influence whether bills reached a roll call vote, I can estimate the effect of deliberation on voting for the subset of bills that would have reached a vote whether they were assigned to treatment or control.

The previous section showed that I do not observe varying rates of attrition across treatment and control bills, but that it is difficult to do so with this research design. For the purposes of this section, I rely on an assumption that attrition is independent of treatment to allow estimation of deliberation’s effects on roll call voting.<sup>11</sup>

ITT effects are estimated using Equation (1), except bill sponsor fixed effects are excluded since some sponsors did not see multiple bills reach a vote. Again standard errors and p-values are estimated using randomization inference. As in the analysis of cosponsorship, ITT effects are estimated for distinct subsets of the subject population to see if deliberation’s effects depend on partisanship or meeting attendance.

Table 4 displays estimated ITTs. Deliberation effects are largest among meeting attendees. Deliberation increased supportive roll call voting by 3.3 percentage points among attendees. This number masks heterogeneity across partisanship, as deliberation effects were 5.5 percentage points among out-party attendees and 1.6 percentage points among in-party attendees. High baseline rates of roll call support for in-partisan bills suggest a ceiling effect

<sup>11</sup>It could be that bills reach the floor because they build large coalitions, and cosponsorship is reflective of broad support. In this case, this assumption would likely be violated. It also might be the case that bills reach the floor because a handful of key gatekeepers — committee chairs and party leaders — want the bill passed. It seems unlikely that support from first-term legislators would affect bill progress as much under this concept of legislating.

**Table 6: Estimated ITT Effects (with Standard Errors) of Deliberation on Roll Call Voting (in pp).**

		Attended Meetings?		
		No	Yes	All
Sponsor's Party?	No	0.0 (8.7)	5.5 (8.1)	0.6 (8.5)
	Yes	0.0 (1.8)	1.6 (3.1)	0.2 (1.9)
	All	0.9 (3.9)	3.3 (4.1)	1.3 (3.9)

Significance indicated at  $p < 0.10$  (\*) and  $p < 0.05$  (\*\*) one-sided. Standard errors and p-values obtained from randomization inference with 1,000 simulated treatment assignments.

is at work among in-partisans. Despite the positive treatment effects, the loss of observations due to attrition inflates standard errors such that no estimates reach conventional levels of statistical significance.

## Why is deliberation influential?

The experimental intervention illustrates a causal effect of deliberation on position-taking behavior. What can the study tell us about the type of information communicated or why it was influential?

Bill sponsors related many types of information. Most sponsors shared verifiable technical details like which districts would be affected by the proposal; they provided expert's projections of bill's costs or effects; and they made persuasive appeals concerning the considerations that they thought peers should apply when taking positions. The content of deliberations are consistent with cheap talk models of legislative communication.

It is extremely difficult to evaluate the mechanisms by which a treatment influences an outcome (Bullock, Green, and Ha 2010), but a case study of one bill suggests what kind of information was influential and why. One bill sponsor effectively framed his legislation in a

way to gain support from legislators of the opposing party. However, when constituents raised objections to the bill, legislators dropped their support, changed their positions, and punished the bill sponsor. Although they were willing to agree with his arguments to support the bill, constituents overwhelmed their efforts at bipartisanship. Further, legislators punished the sponsor by withholding support and applying greater scrutiny on his other proposals.

A first-term legislator in the minority, Democratic party sponsored a bill to update the state's child safety seat law. It would have required parents to use booster seats for children up to twelve years of age. Although not an experimental bill, the sponsor did discuss the bill at a caucus meeting and receive a cosponsor from a Republican BFC member in attendance. The cosponsorship occurred immediately after the meeting, suggesting that the meeting itself caused the cosponsorship. The sponsor framed the bill as one relating to child safety, stressing that pediatricians supported the bill. He repeated that the bill would "only update the standards that were already on the books" and bring them in line with recommendations by national child safety experts.

The bill passed through committees in the House and Senate either with unanimous support or by voice vote, passed the senate floor unanimously, and passed the house floor with two-thirds of members supporting it, including a majority of Republicans. As the bill awaited the governor's signature, Tea Party activists criticized the bill's expansion of government regulation, potential fines for parents who violated the law, and costly requirements for parents to buy new booster seats. Amplified by conservative radio and social media, the public outcry led state legislators to recall the bill from the governor, re-refer it to committee, and kill it, with now two-thirds of members in the house, and nearly all Republicans, in opposition. The Republican member of the BFC withdrew his cosponsorship.

Deliberation did not supply Republicans with the information they needed to take fully-informed positions. Republicans in the assembly may have been rational in taking his considerations at face value and not wanting to devote any more time or effort to debating the bill. Their decision was costly. They failed to anticipate the response of constituents.

The legislature as a whole lost plenary time by having to recall and reconsider the bill, and individual legislators took positions that angered their constituents. The experience of the Republican BFC member suggests that at least one legislator was persuaded by the sponsors' framing to support the bill, but that such a position was not best for him. Presumably many other Republicans reached the same conclusion after hearing from their constituents.

How do legislators respond when deliberation fails to provide them with the information they need to take positions? Republicans were not happy that the sponsor gave an incomplete picture of his bill, and they punished him. They withdrew cosponsorships of his other bills; re-referred another bill that had passed the house back to committee in an attempt to kill it; and challenged every resolution he attempted to place on the consent calendar. Republicans changed the deliberative institutions that applied to this bill sponsor. His prioritized position in speaking about his bills was eliminated, as opponents took time to speak against his bills. His reputation for being too strategic in framing his bill led to increased scrutiny of his future bills. In the repeated game of legislating, the legislator was punished by peers who felt misled by his comments about his bill.

## **Discussion**

This study makes several contributions to the field of legislative deliberation. First, legislators change their minds as a result of talking with peers. Second, communication can build policy coalitions across party lines. Conflicts of interest were, on average, not too severe to preclude persuasive communication. Third, effective communication can occur in an unstructured environment. Legislators did not need to give bill sponsors special parliamentary rights, like a closed rule, to incentivize provision of expertise. Fourth, legislators' bipartisan efforts are limited by pressure from vocal constituents. Finally, repeated interactions allow legislators to punish peers who lead them to take unpopular positions.

At least in this legislature and on these bills, some legislators exhibited open-minded, good faith deliberation. The conventional wisdom that deliberation has faded from Ameri-

can legislatures may be overstated. One possible reason for this misperception would be if scholars and journalists have been looking for deliberation in the wrong places. Committees and floor debates, historically the focus of attention, have become arenas for partisan bickering and point-scoring. Increased transparency has opened such debates to the public. Bipartisan caucuses, on the other hand, typically operate without much public attention or oversight from constituents, party leaders, or the media. It might be exactly this lack of attention that allows legislators to reach across the aisle and work with one another.

Randomizing an intervention within a legislature was a central aspect of this paper's research design. However, results showed one limitation to such approaches. Experiments are well-suited to evaluating individual-level behavioral outcomes, but less so for bill-level outcomes. The 67 bills in this study were not sufficient to analyze bill level outcomes. One reason for this lack of power is that bill-level outcomes tend to be binary: does the bill pass committee or not; reach the floor or not; get amended or not; and pass or not. Another limitation is the lack of pre-treatment covariates predicting bill-level outcomes. Future work will need to address these limitations of legislative experiments, since policy-level outcomes are among the most important products of legislatures.

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