

Legislating while Learning: How Staff Briefings, Cue-Taking,  
and Deliberation Help Legislators Take Policy Positions

Adam P. Zelizer

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## ABSTRACT

### Legislating while Learning: How Staff Briefings, Cue-Taking, and Deliberation Help Legislators Take Policy Positions

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This dissertation examines how legislators learn about policy proposals. It focuses on three common sources of policy information — staff briefings, cues, and group deliberations — to show the causal effect of information on legislators’ policy positions. It uses a new approach, field experiments, that allows me to answer questions about information, institutions, and outcomes that heretofore have been difficult to study quantitatively. Results from the three studies I conducted are largely consistent with theories of legislating under imperfect information. All three studies find that information affects position-taking. On average, information increases support by reducing legislators’ uncertainty. Information is most influential on bills that legislators are ideologically predisposed to support. In some respects, findings extend or challenge existing theories. Legislators appear responsive to repeated messaging. Cues and briefings interact to make legislators even more supportive of bills than we would expect from their separate effects. Cues determine a far greater proportion of positions than prior studies suggested. Finally, group deliberation appears to reduce partisan polarization in bill coalitions. All together, the studies illustrate that imperfect information constrains position-taking, that legislative staff, cue-taking, and deliberation can effectively communicate information, and that legislative institutions influence individual positions by providing policy-relevant information.

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# Chapter 1

## The Information Problem

“A good government implies two things: first, fidelity to the object of government, which is the happiness of the people; secondly, a knowledge of the means by which that object can be best attained. Some governments are deficient in both these qualities; most governments are deficient in the first. I scruple not to assert, that in American governments too little attention has been paid to the last.” - James Madison, Federalist 62, 1788.

In 1971, Rep. Tom Moore introduced a memorial resolution in the Texas House of Representatives. The resolution commended “Albert De Salvo [sic] on his outstanding career of public service.” It noted “He has been officially recognized... for his noted activities and unconventional techniques involving population control and applied psychology.” Like most memorials, Rep. Moore’s resolution passed unanimously, without debate, through the state house.

Astute readers will recognize that Mr. DeSalvo's name was misspelled in the resolution. They will also wonder why Mr. DeSalvo, better known as the Boston Strangler, was chosen for an official commendation. Rep. Moore considered the resolution a prank on his peers, and he tabled it after the vote (Witherspoon 2009). Nevertheless, many observers took a less sanguine perspective of the whole ordeal. If legislators could be snookered into commending the Boston Strangler, what other legislation might they approve simply because they had not done their homework?

Commemorative proposals are largely inconsequential,<sup>1</sup> but legislators commonly complain that they are not granted enough time to study significant legislation. During debate on the Affordable Care Act, then-Rep. Dean Heller complained that he had insufficient time to read the bill. Republican Minority Leader Mitch McConnell decried the lack of "full and transparent debate" on the bill (Ferraro and Smith 2009). After a change in government, Democrats raised similar objections to the 2017 tax reform bill. Senator Jon Tester complained that "I was just handed a 479-page tax bill a few hours before the vote.... This is Washington, D.C. at its worst" (Tester 2017). Despite their remonstrations, Sen. Tester had previously voted for the lengthy Affordable Care Act, and Sens. Heller and McConnell would vote for the hastily drafted tax bill.

Partisan posturing aside, hurried lawmaking does have tangible consequences for public policy. One does not have to look far for examples of legislation passed with unpredicted costs or outright errors. *The New York Times* reported that "The legislative blitz that rocketed the \$1.5 trillion tax cut through Congress in less than

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<sup>1</sup>Congress uses such resolutions to name buildings, issue stamps and coins, and grant Congressional Gold Medals.

two months created a host of errors and ambiguities in the law that businesses big and small are just now discovering and scrambling to address” (Tankersley and Rappeport 2018). While it is difficult to know whether lawmakers truly made a mistake or just chose to ignore these problems, other times it is clear, such as when *The Hill* reported that “W.Va. lawmakers try to give teachers smaller raise, accidentally pass bill giving them full raise” (Seipel 2018). In this case, legislators voted through the wrong version of a bill. Uninformed lawmaking leads to very real, and sometimes very large, costs for government and the public.

It is tempting, but wrong, to blame individual legislators for these mistakes. For some time, it has been recognized that modern policymaking is too complex for any single individual to understand. In part for this reason, legislatures have adopted institutions to support policymaking efforts. These institutions include committees, parties, research bureaus, and staff. When we observe apparently uninformed policy outcomes, we should ask not how legislators could let that happen, but instead whether legislative institutions are functioning as they should. The information problem, as it were, is less about individual competence than collective and institutional policy expertise.

Imperfect information poses problems not only for policy outcomes and for legislative organization. It raises difficult questions for political scientists. The conventional wisdom among the public and much of political science is that policies result from well-informed bargaining among competing policymakers. Legislative outcomes are then best understood in terms of which legislators, and which voters, get what they want. This zero-sum view magnifies conflict, but it could be considered reassuring.



Policy outcomes are at least predictable and understandable even if they are, in some cases, biased, unrepresentative, or normatively unappealing.

Despite Madison’s warning over two hundred years ago, political scientists still pay less attention to the knowledge of policymakers than to their responsiveness to the public. What if policies are based on a limited, asymmetric, and potentially incorrect understanding of the policy issue itself? To what extent might the various problems in contemporary legislatures, such as gridlock and polarization, actually come down to differences in legislators’ substantive information about policy? We simply don’t know the answers to these and similar questions regarding the influence of information on policymaking.

This dissertation examines how legislators learn about policy proposals. It focuses on three common sources of policy-relevant information: staff briefings, cues from close peers, and deliberations with groups of legislators. Its main contribution is to demonstrate the causal effect of policy information on legislators’ policy positions. It does so using a new approach to the empirical study of legislative procedures: field experiments. Experiments allow us to answer questions about information, institutions, and outcomes that have heretofore been difficult to study quantitatively. To understand why, we can look at the development of legislative studies of information and policymaking.

## **1.1 Policymaking under incomplete information**

Informational issues were first framed in terms of “legislative incompetency” (Rocca 1921). John Stuart Mill criticized the members of Parliament as “inexperience sitting

in judgment on experience, ignorance on knowledge” (Mill 1865, 93). Lord Bryce described the “keen, though limited, intelligence” (1906, 65) of American legislators as being no better, but no worse, than the average voter. Alabama Governor Emmet O’Neal summed up the prevailing opinion when he stated that “a session of the Legislature is looked upon as something in the nature of an unavoidable public calamity” (O’Neal 1914, 685). These observers and others questioned whether democratically-elected lawmakers could make good public policy.

Reformers and political scientists during the Progressive and New Deal Eras realized that institutions were the answer to problems of individual ignorance. During the Progressive Era, American legislatures established the first professional reference bureaus, bill drafting bureaus, and revision committees. Following the rise of dominant executive lawmaking during the New Deal, legislatures adopted councils, expanded staff, and reformed their committee systems (Hyneman 1938; Gaus 1932; Rhodes 1946; Lederle 1948; Jones 1952; Davey 1953). The academic literature’s attention to cognitive constraints, imperfect information, and the potential of institutions to overcome informational problems was ahead of its time.

The literature on these legislative reform movements failed in two respects. First, enacted reforms were not followed by empirical analyses offering support that the new rules or institutions actually improved outcomes. Identifying a causal effect of legislative institutions is difficult, but the widespread adoption of new institutions offered an opportunity. I am aware of no study that collected data before and after the reforms were adopted, across a range of legislatures, to estimate their impact. Second, the literature’s focus on policymaking under uncertainty faded from legisla-

tive studies for several decades. Rational choice models with fixed preferences and full information replaced the informational models. Institutions existed to solve distributive concerns, such as vote cycling and vote trading, not informational problems (Arrow 1951, Plott 1967, Shepsle and Weingast 1981, Weingast and Marshall 1988).

In the 1980s, formal theorists re-engaged with informational problems. Formal models of decision making under uncertainty clarified the problem and proposed a solution (Crawford and Sobel 1982; Austen-Smith and Riker 1987; Gilligan and Krehbiel 1987, 1989, 1990; Krehbiel 1991; Banks 1991). Formal theorists argued that the information most important, and most lacking, in policymaking was technical expertise, not political intelligence (Krehbiel 1991, 68; see also Zwier 1979; Webber 1979; Bradley 1980; Cooper and MacKenzie 1981; Sabatier and Whiteman 1985; Bimber 1991; Mooney 1992). Expertise was unavailable due to agency and collective action problems. Individual legislators had no incentives to acquire policy expertise and no credible way to share it. The solution, again, was institutional. Committees could be granted special parliamentary rights to collect and share policy information.

These models consider information a causal factor throughout the policymaking process. Informational concerns *cause* legislators to create committees. They *cause* committees to be granted special powers, which in turn *cause* committees to invest and share expertise. Committee expertise *causes* legislators to take certain policy positions. The logical way to test these models would have been to evaluate the causal influence of information on committee organization, procedures, and position-taking.

However, observational studies of information face substantial problems with

causal inference. Even if information can be measured, which is itself no small task, there is a fundamental endogeneity issue. Information is not randomly provided. Party leaders and committees choose when to share expertise (Gilligan and Krehbiel 1987; Curry 2015). Cue-giving is strategic (Box-Steffensmeier, Ryan, and Sokhey 2015). Even the legislature's organization and agenda are affected by informational concerns (Kessler and Krehbiel 1996). It is practically impossible to identify an effect of information on individual behavior, institutional design, or policy outcomes within this web of causal relationships.

Previous empirical studies have shed light on various aspects of policymaking under uncertainty. Surveys of legislators and archival research have cataloged the scientific and expert information provided to lawmakers (Caplan, Morrison, and Stanbaugh 1975; Bradley 1980; Mooney 1992; Amara, Ouimet, and Réjean 2004; Brasher 2006). These studies reveal that legislators face not a scarcity, but an overwhelming abundance of information that leaves them uncertain what information to trust (Schneier 1970; Kingdon 1989; Jones and Baumgartner 2005). Ethnographic analyses of position-taking describe legislators' uncertainty and their reliance on decision making heuristics (Fenno 1978; Kingdon 1989). Tests of informational models of committees compare the ideological composition of committees to the floor (Krehbiel 1991; Prince and Overby 2005). These studies built the framework for studying information. However, they did not directly take up the causal relationships at the center of the theory.

As a result, the study of information is notable for what is missing: clear causal evidence that legislators' incomplete information constrains their behavior; that spe-

cific institutions increase the quality or quantity of policy information; and that information influences individual or collective choice. A new methodological approach is needed to take up these causal claims.

## 1.2 Methodology

Experiments are a growing part of legislative studies (Grose 2014). Naturally-occurring lotteries have been used to examine the effects of committee seniority, committee membership, office location, bill sponsorship, and term length on electoral and legislative outcomes (Kellerman and Shepsle 2009; Rogowski and Sinclair 2012; Broockman and Butler 2012; Loewen et al 2014; and Titiunik 2016). Academics have also conducted their own randomized control trials (RCTs) where legislators are randomly assigned to a treatment using a proactive assignment procedure. Previous legislative RCTs have examined interactions between legislators and the public (Bergan 2009; Butler and Nickerson 2011; Butler and Broockman 2011; Malesky et al 2012; Butler, Karpowitz, and Pope 2012; Kalla and Broockman 2015; Grose, Malhotra, and Van Houweling 2015). This dissertation extends these studies to study interactions inside the legislature that are governed by legislative institutions.

Experimentation offers several benefits for the study of information. Most importantly, experimentation is the gold standard in causal inference. Randomly assigning information makes it possible to estimate its causal effects independent of other confounding factors. Identification allows new tests of information theories that are much more direct than those available to observational designs.

Second, measuring legislators' information is hard, but offering legislators infor-

mation is easy. An observational study of information and voting might require a costly and time-consuming survey of legislators. It would assume legislators gave truthful responses, and then make strong statistical assumptions about omitted variables and the relationship between information and behavior. Providing information avoids several of these assumptions and difficulties. It sidesteps endogeneity issues through random assignment. It also is more easily implemented. I have found legislators are more willing to accept information than to confess what they do not know.

Third, informational interventions can be crafted to suit specific research questions. The source of information can be specified in the research design. This high degree of control allows me in this dissertation to compare the effects of information from a staffer to information from other legislators. It is also easy to examine different types of information.

Fourth, legislative experiments are well-suited to the methodology of institutionalism (Diermeier and Krehbiel 2003). They directly compare the effectiveness of institutions on outcomes of interest. Experiments also incorporate the broader institutional context within which they are fielded. Interventions may be effective in some institutional contexts but not others.

RCTs have drawbacks, too. Experiments in every field face a similar set of issues. Results may not generalize to other contexts. Randomized interventions occur out of equilibrium. Many worthy topics of study cannot, and should not, be randomized. Any study involving human subjects should consider the basic ethical considerations of respect for persons, beneficence, and justice.

Legislative experiments face unique ethical considerations (Teele 2014). Butler and Broockman (2011) discuss three issues that arise in their legislative RCT. The issues, which are deception, personal harm, and collective burden, provide a useful foundation for evaluating the experiments in this dissertation, as well as legislative experiments more broadly.

### *Deception*

The experiments in this dissertation provide information to legislators on randomly selected bills. This information is not deceptive. Providing legislators with untruthful or misleading information about public policy would, in likely any case, be unethical. Like any scholar testifying before Congress, every effort was made to provide high-quality, unbiased information about public policy. In this dissertation, treatments contained technical information from the text of legislation, from analyses conducted by the Committee for Fiscal Review, and from other reputable sources.

Deception can also apply to subject recruitment. Party leaders, caucus leaders, and bill sponsors were all informed of and approved the projects in this dissertation. Even legislators who received information were told that briefings and deliberations were new efforts to raise awareness of legislation. They were not, however, told that their behavior would be examined ex post through publicly-available data on position-taking.

### *Personal harm*

Sophisticated political observers realize that it is impossible for a legislator to be perfectly informed about every issue on the agenda. Nevertheless, results from information experiments could be used to attack individual legislators who appear

less informed than others. While academics have not shied away from scoring the performance of individual legislators (Volden and Wiseman 2014), there is little reason to take a personalized approach to the study of information. Informational issues are ubiquitous in legislatures. At this stage, the behavior of specific individuals is less important than aggregate trends. As a result, not only are no individual subjects named in this dissertation, but the legislature itself is described, but not named.<sup>2</sup>

### *Collective burden*

Interventions should not burden legislators who already face substantial time and cognitive constraints. Surveys or audit experiments of legislators may collect data for academic projects unconnected to legislators' immediate concerns. They impose a minimal time burden. The briefings and deliberations in this dissertation occur regularly in legislatures, presumably because legislators find them useful. There is good reason to think the specific interventions helped legislators understand real policy issues.

Like campaign experiments, legislative experiments could influence policy outcomes. As such, they face an additional consideration in that they could cause harm to the public, even if the public are not the proximate subjects of the studies. This is collective harm.

### *Collective harm*

The projects in this dissertation are unlikely to have affected policy outcomes. Each exerted a light touch. Two of the experiments consisted of legislators receiving

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<sup>2</sup>The legislature's identity is no secret to my advisors and peers. My dissertation sponsor spoke with the legislator for whom I interned about my research.



research reports of information repackaged from other sources in the legislature. The third consisted of legislators discussing bills with one another. All three are encouragement designs in the sense that all legislators had access to the experimental information, even legislators in control conditions. The interventions just encouraged legislators in the treatment conditions to consider the information more intently. Nevertheless, out of an abundance of caution, experiments only addressed bipartisan, broadly supported issue areas.

A harder question is whether academics should participate in interventions that affect public policy. I firmly favor academics engaging practical, real-world policy problems. This may require providing expertise that changes the policymaking process or leads to different policy outcomes. I discuss the prospects for experimental evaluation of legislative operations in the concluding chapter, but here I will note one good rule of thumb for conducting ethical legislative experiments: interventions should be fielded with the approval and participation of legislators.

Elected officials and bureaucrats have themselves increasingly turned to RCTs to evaluate government's performance. The Behavioral Insights Team, originally part of the Cabinet Office of the British government, and the Office of Evaluation Sciences, part of the General Services Administration, are two government-affiliated groups that conduct experiments on behalf of municipal, state, and national governments. Their studies often deal directly with important public policies. OES has encouraged student borrowers to repay loans, persuaded pharmacists to prescribe fewer pain medications, and evaluated the effectiveness of foreign aid programs (OES 2018). There is no doubt these interventions influence real-world outcomes.

In cases like this, where government actors choose to alter the delivery of public services or nudge the public toward different behaviors, the researcher’s contribution is clear: to provide low-cost expertise in the design and analysis of evaluations and to make findings available to the public without financial or partisan conflicts of interest. All interventions in this dissertation were conducted alongside state legislators and staff, with their approval, support, and exceptional compliance.

### **1.3 Overview of studies**

What follows are three studies of legislators’ ability to make informed public policy. They describe informational interventions conducted over my two year residency in a state legislature. The projects were planned and conducted with approval of leaders from both parties and with the participation of caucus leaders and bill sponsors.

The three interventions examine briefings by legislative staff, cue-taking between legislators, and caucus deliberations. These are only some of the many ways legislators learn about policy. They also hear about policy during committee hearings, floor debates, and party meetings. The three interventions were chosen because they are central to the activity of legislative caucuses.

Caucuses are bipartisan groups of legislators typically organized around specific issues. They engage with policymaking by collecting and disseminating information. For example, the Democratic Study Group, one of the first organized caucuses in Congress, produced such valuable research that executive branch officials, state policymakers, and even Republican members of Congress requested its reports. Legislators cite caucuses as one of the most trusted sources of policy information (Kingdon

1989). They are present in Congress, every state legislature, and parliaments around the world.<sup>3</sup>

Unlike committees or parties, caucuses have no formal parliamentary powers. They cannot control the agenda or restrict amendments in any way. This makes them ideal for studying information's effects. Formal powers do not confound informational influence. The three interventions reflect the informal information-gathering that legislators pursue on a day-to-day basis, including through caucuses, that is often difficult to study observationally.

Caucuses are interesting information sources for theoretical reasons as well. Information signaling models are typically studied with respect to committees, but they are just as relevant to caucuses. Caucuses are groups of policy experts with heterogeneous ideological predispositions. As such, they should generally be more influential than groups with homogeneous, extreme ideological preferences (Gilligan and Krehbiel 1989). Caucuses thus may represent an upper bound against which other information sources can be compared.

Finally, practical considerations recommend studying information through caucuses. Committees, parties, and floor processes are formally governed by legislative rules. There are few openings for the researcher to provide new information. In comparison, caucuses are much more receptive. In most circumstances, caucuses receive no funding or dedicated staff from the legislature. Instead, they rely on outside experts to give presentations and answer questions. This is a great opportunity for academics to share our expertise.

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<sup>3</sup>Caucuses are sometimes called Legislative Service Organizations, Congressional Member Organizations, or, in parliamentary systems, All-Party Parliamentary Groups.

The first paper examines foundational issues in information and decision making: to what extent are legislators' positions constrained by imperfect information and, as a result, how responsive are legislators to new policy information? These micro-level questions underlie macro-level theories of committees, delegation, and lobbying. To answer these questions, I worked with the Veterans Caucus to provide legislators with supplemental policy briefings about randomly-selected pending legislation. Briefings included research reports modeled after committee and caucus reports to provide realistic and useful information. As the caucus intern, I briefed seventy-six legislators in one-on-one sessions over a period of a month.

Briefing's influence is measured primarily through legislators' cosponsorship. Briefings increased cosponsorship by 60% above baseline rates. This suggests that baseline information constraints prevented around 40% of possible cosponsorships in the control group. Briefing effects extend beyond cosponsorship. One bill, drafted with an error, was covered critically in briefings. Treatment substantially reduced cosponsorship and roll call voting support of this bill.

On the whole, briefing effects are largely consistent with predictions from information signaling models: 1) on average, providing information helps legislators take positions by reducing uncertainty; 2) information's effects are largest when sender and receiver are ideologically similar; and 3) groups of individuals with heterogeneous ideological predispositions are credible sources of information.

The next two papers examine how legislators share policy information with one another. The second paper addresses cue-taking using data from two experiments. It defines cue-taking in terms of the contagion of briefing effects between legislators.

Legislators who were not directly briefed, but who shared offices with a directly briefed legislator, may look to their officemate for a cue. Random assignment of briefings identifies this secondary, indirect treatment effect. We cannot define its content — it may result from legislators actively sharing information or passively observing one another’s behavior. But, if the contagion model is correct, we can be sure that indirect treatment effects result from cue-taking between legislators.

Direct briefing effects and secondary cue-taking effects are estimated using data from two experiments. I repurpose data from the first study and combine it with data from a replication conducted the following year. Notably, the replication was designed to study intra-office cue-taking. It features a restricted randomization to increase the number of observations assigned to the cue-taking treatment. This improves the precision with which cue-taking effects are estimated.

There is strong evidence of cue-taking. Cue-taking is nearly 80% as effective as direct briefings. Further, cue-taking appears to be purposive. It occurs for bills that reach the floor, but not bills that fail in committee. We would expect this pattern if legislators only seek out cues when they are required to vote on legislation.

The third paper addresses group deliberation. Members of the Bipartisan Freshman Caucus randomly selected bills for discussion during caucus meetings. Selected bills thus received supplemental deliberation above and beyond the discussions that ordinarily occur in committees and on the floor. This experiment differs from the prior two in a significant way. The unit of treatment assignment was the bill, not the legislator-bill dyad. This assignment procedure reduces power for estimation of information’s effects on individual position-taking, but it allows the estimation of in-

formation's effects on bill outcomes. Information may affect bills' likelihood of being amended or advancing through the policymaking process.

Position-taking on untreated bill reveals an interesting pattern. Legislators' cosponsorship and roll call voting in support for bills sponsored by in-partisans consistently exceeds their support for bills sponsored by out-partisans. This is to be expected. However, this trend holds even if we attempt to hold constant legislators' ideological predispositions. After estimating legislators' ideology using prior session roll call voting, I plot position-taking by the distance between legislators' and bill sponsors' ideal points. Supportive position-taking is lower, at any given level of ideological distance, on bills sponsored by out-partisans. This "partisan penalty" is consistent with legislators being more uncertain about bills sponsored by out-partisans.

Caucus deliberations increased both cosponsorship and roll call voting support for treated bills. Surprisingly, changes were largest among out-partisans. Although ceiling effects limit the interpretation of roll call voting effects, it is not clear why deliberation increased cosponsorship more across parties than within them. I offer one possible explanation. Among untreated bills, legislators appear to apply a penalty to bills sponsored by out-partisans. That is, even holding constant the ideological similarity of legislators to sponsors inside and outside their own party, legislators are less likely to support bills from out-partisans. Deliberation reduced and nearly eliminated this partisan penalty. Legislators' support of treated bills is still predicted by their ideological similarity to bill sponsors, but they no longer apply this extra partisan penalty.

The overall message from the suite of experiments is that legislators learn about policy during the lawmaking process. Their positions depend on what they learn. Legislators do not walk into the legislature knowing what position they will take on every bill. Instead, the day-to-day activities in a legislature influence individual choice. For legislative scholars, individual positions must be considered not only an input into models of collective choice, but also an output of the legislative process itself.

# Chapter 2

## Staff Briefings

Facts, research, and information are essential to the healthy functioning of legislatures. Alongside ideological and electoral concerns, information is a major input into legislators' decisions whether to support or oppose legislation. As a result, scholars have paid a great deal of attention to how legislators wade through a complex information environment, structure institutions to overcome asymmetric and imperfect information, and interact with outside information sources to decide which bills to support (Gilligan and Krehbiel 1987, 1989, 1990; Krehbiel 1991; Mooney 1992; Pottery and van Winden 1992; Austen-Smith 1993; Jones and Baumgartner 2005; Hall and Deardorff 2006).

However, there is little direct, empirical evidence that policy information and research generated within the legislature influence individual positions or collective policy outcomes. Few empirical studies examine how information varies across legislators, whether information affects individual behavior, and to what extent institu-



tions overcome the problem (c.f. Fenno 1973, Kingdon 1989, Jones and Baumgartner 2005). There are formidable measurement and identification challenges to studying information, as it is not randomly allocated. As a result, studies of imperfect information rely on indirect empirical tests. Informational models of committees are substantiated by the ideologies of committee members (Krehbiel 1991). Studies of lobbying examine which legislators meet with lobbyists (Hojnacki and Kimball 1998). Neither directly examines the question of interest: does providing legislators with policy research influence their likelihood of supporting a bill? Do legislative institutions affect cosponsorship or roll call voting by providing information?

This paper revisits the now-classic literature on imperfect information with a novel research design — a field experiment embedded in a state legislature — to estimate the effect of policy information on position-taking. Legislators were provided policy research by a legislative staffer on randomly selected bills. The staffer, who worked for the Veterans Caucus, conducted one-on-one briefings with subjects that provided nonpartisan, technical research about veterans bills. By randomizing briefings across bills, we can compare individual position-taking across treated and untreated bills. This approach avoids the measurement and identification challenges that characterize observational studies of policy research.

This paper contributes to the study of legislating under imperfect information in several ways. First and foremost, it shows that policy research affects position-taking. On average, legislators are 60% more likely to support bills if they are provided research. While most briefings painted bills in a positive light, one bill was covered negatively due to flaws in its drafting. Legislators briefed on this bill were

less likely to cosponsor and vote for it.

Second, the empirical design directly engages with formal models of information exchange. While previous experimental work on informational models of committees have been limited to the laboratory (Battaglini et al 2016), this paper provides several tests of these models in the field. In several respects, findings are consistent with predictions of these models: 1) on average, providing research helps legislators take supportive positions by reducing uncertainty; 2) information's effects are largest when sender and receiver are similar; and 3) groups of experts with heterogeneous ideologies and partisan affiliations make for trusted information sources.

The final contribution is to clarify the public policy implications of imperfect information. The briefings' influence reveals that at least some legislators refrain from taking positions due to information constraints. The inability or unwillingness to take positions may lead to paralysis as risk averse legislators delay approving proposals (Binder 2004, 31). However, improving information is not a free lunch, as information caused polarization. Legislators predisposed to support legislation were convinced to do so, but legislators who were unlikely to support bills were not convinced of their merits. The end result is that the treatment made it easier for legislators to align positions with their predispositions. In this surprising way, information constraints might limit polarization.

## **2.1 Imperfect information in a state legislature**

Observers have long noted that legislators cannot often draw on their own deep policy expertise when making decisions (Mill 1861; Bryce 1906; Luce 1924; Kingdon 1989;

Krehbiel 1991). Legislators have heterogeneous and incomplete information about policy. As the core of this paper examines whether policy research helps legislators make decisions, the first question to ask is whether legislators appear constrained by imperfect information in the first place.

Directly measuring information is difficult, so I instead examine whether the behavioral consequents of information vary across legislators and over time as predicted by theory.<sup>1</sup> In particular, institutions, electoral incentives, and legislators' own backgrounds should lead them to have varying expertise and information about issues. Is it the case that legislators whom we would expect to have less information are also less likely to take positions?

In this state legislature,<sup>2</sup> patterns of position-taking suggest that legislators are indeed constrained by imperfect information. Both sponsorship and cosponsorship vary across legislators and over time in the manner predicted by informational theories of position-taking. The constraints observed in this legislature are likely to be found in most legislative contexts.

### **Institutional context**

The legislature resembles many other state legislatures in the United States. It is characterized by low professionalism. It ranks in the bottom half of Squire's (2007) index with its part-time legislators, little staff support, short annual sessions, and

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<sup>1</sup>We should be hesitant to infer that information causes observed behavior — after all, that is the motivation for the experiment that constitutes the core of this paper. Nevertheless, observing that behavior changes with institutional factors would be consistent with the broader story about information effects.

<sup>2</sup>The state is not named in order to preserve ongoing research projects in the legislature.

modest legislative salaries. Nevertheless, there are at least a dozen states that rank lower.

There were unique institutional features in the legislature of interest. First, although most interest groups engaged in lobbying, veterans groups did not. Of the 579 lobbyists representing 737 separate groups registered with the state ethics commission, none represented veterans. The legislature also lacked a dedicated veterans committee. Thirty-six states convened a full standing committee on Veterans or Military Affairs in their lower chambers,<sup>3</sup> but the state where the intervention was fielded did not.

With no standing committee, the legislature had, in past years, established a bicameral committee on veterans affairs. This committee behaved as a regular standing committee, conducting hearings, communicating with interest groups, and working with the state Department of Veterans Affairs. The committee appeared to fulfill an important role. The Department of Veterans Affairs wrote that it was “most helpful in obtaining support for veterans legislation in the General Assembly” (Comptroller Report 2011, 37). Nevertheless, the joint committee was abolished as part of a legislative reorganization years before this study was conducted, so no committee held exclusive jurisdiction over veterans issues.

The termination of the joint committee offers an opportunity to examine its relationship to position-taking. Figure 2.1 plots the percentage of veterans bills cosponsored by each legislator for two assemblies during which the joint committee was operational and the assembly after which it was eliminated. Legislators serving

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<sup>3</sup>Includes the Government, Military, and Veterans Affairs Committee in the Nebraska Unicameral Legislature. Data from state legislative websites. Data collected for 2015 to 2016 sessions.

on the committee are indicated by circles; committee members who had served on the committee and remained in the legislature after its closing are indicated by triangles. Figure 2.1 shows a significant decline in cosponsorship after the joint committee was abolished. The median House member cosponsored 11.1% of veterans bills in the first assembly included in the analysis, 12.5% in the second, and 4.7% in the assembly after the committee's closing. Median Senate cosponsorship declined from 17.6% to 15.9% to 10.5% over the three assemblies.<sup>4</sup> Results are not driven by committee members, and placebo tests show that declines in bill cosponsorship were unique to veterans bills (see Appendix A.2).

A plausible interpretation of this data is that the joint committee served an important informational role. It provided legislators with policy information that they needed in order to take positions on veterans bills. Once it was eliminated, legislators were less informed and more reluctant to take positions.<sup>5</sup>

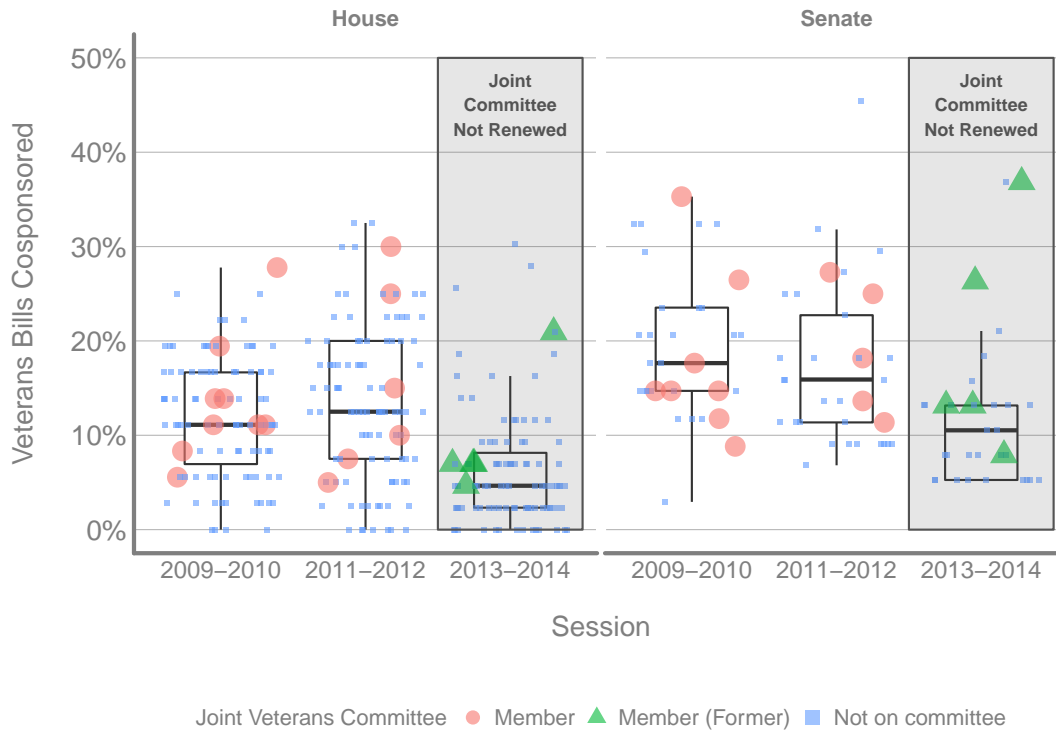
### **Legislator characteristics**

Legislators' personal experience can also affect their information. Legislators who previously had served in the military have first-hand knowledge of the issues facing veterans. As a result, we would expect veterans to be more informed and thus more likely to engage with veterans affairs than other legislators. We cannot differentiate

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<sup>4</sup>The decline in average cosponsorship from the second to third assembly in the House was 6.9 percentage points ( $p < 0.001$  two-sided from t-test) and in the Senate 5.1 percentage points ( $p = 0.016$ ).

<sup>5</sup>Alternative explanations find little support. The committee was not discontinued due to any veterans-specific issue: the committee was eliminated as part of a broader legislative restructuring driven by other committees that generated large operating costs. The number of veterans bills remained relatively stable, and even expanded slightly, over the three assemblies, from 36 to 40 to 43.



*Figure 2.1: Veterans bills cosponsored per legislator.*

information from other factors that might affect position-taking (like ideology), but we can at least examine whether veterans are more engaged in the issue.

Veterans cosponsored more veterans legislation than non-veterans. In the House, they cosponsored 10.4% of veterans bills, while non-veterans cosponsored 5.4%, a difference of 5.0 percentage points ( $p < 0.05$ ). In the Senate, there is a minimal difference in cosponsorship (12.1% among veterans and 12.0% among nonveterans).

Bill sponsorship is another important form of position-taking that requires information. Veterans were more active than others in bill sponsorship as well. Although

only 25 of 99 representatives and 4 of 33 senators were veterans, they accounted for a disproportionate share of veterans bills sponsored. 30 of the 43 veterans bills filed in the lower chamber came from veterans, as did 18 of the 38 bills filed in the upper chamber. These rates are substantially higher than we would expect by chance if all legislators were equally likely to file veterans bills.<sup>6</sup> Between sponsorship and cosponsorship, it is clear veterans were more likely to support veterans legislation than their peers, which is at least consistent with, if not demonstrably a result of, being well-informed about veterans issues.

### **Electoral incentives**

Another factor that might lead legislators to acquire varying amounts of policy information is the electoral incentive. The desire to be re-elected drives legislators to engage with important constituencies in their district. Like any constituent group, veterans make up a larger portion of some districts than others. This natural variation allows us to examine whether legislators' cosponsorship of veterans bills correlates with the number of veterans in their districts.

The veterans population in each district is calculated from data provided by the state Department of Veterans' Affairs.<sup>7</sup> Veterans make up 5.2% – 14.2% of district populations, with a statewide average of 7.7%. Districts with the largest veterans'

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<sup>6</sup>Pearson's chi-square tests indicate that it is extremely unlikely that the higher rates of veterans sponsorship arose by chance ( $p < 0.01$  for both chambers).

<sup>7</sup>The Department provided the number of veterans in each county as of 2014 which, together with the total population in the county, was used to calculate the veterans population in each county. The state legislative website lists which counties each legislator represents, although it does not break out how much of each legislator's district falls within each county. As a result, each district's veterans' population was estimated as the simple, unweighted average of the veterans' population of each county represented by the legislator.

presence feature a large military base.<sup>8</sup> These districts do not drive the results reported below.

Figure 2.2 plots legislators' cosponsorship of veterans legislation against the veterans population in their district. It covers the three general assemblies prior to the intervention. Each point represents a legislator's cosponsorship in a given assembly. To illustrate that differences across legislators do not result from membership on the select committee, joint veterans committee members are again indicated by circles and triangles. A loess curve is fit to the raw data.

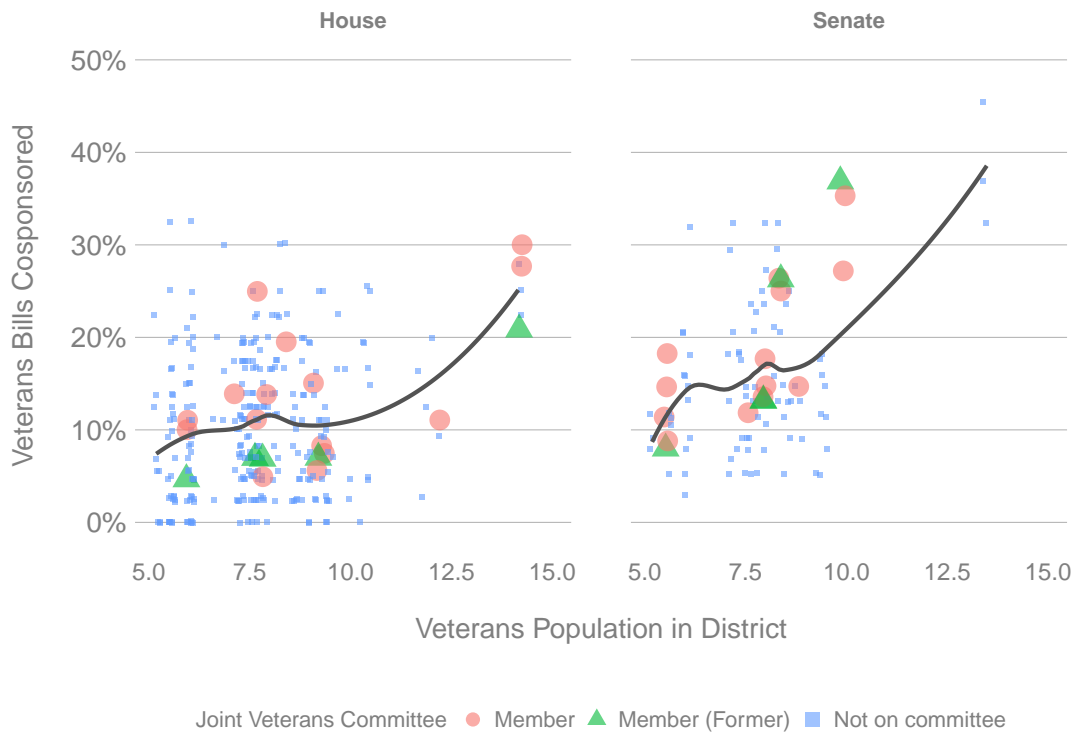
Cosponsorship increases with districts' veterans population. House and Senate members who represent districts with the most veterans are among the most supportive of veterans legislation. There is also a positive correlation for other legislators. In the House, the legislator representing the fewest veterans cosponsored 7.4% of veterans bills while the legislator representing the median number cosponsored 14.0%. In the Senate, the relationship is even stronger. These results are unique to veterans issues. Placebo tests show that legislators representing more veterans were no more likely to cosponsor non-veterans legislation (see Appendix A.2).

Legislators' position-taking varies across individuals and in response to institutional variation as we would expect if legislators are constrained by imperfect information. However, these results should be interpreted with caution. It is unclear how much information is the causal factor. Ideology also affects position-taking, and it may be correlated with information. Other factors, such as the broader political context or changes to the legislative agenda, might also drive results. These difficulties

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<sup>8</sup>The base is shared by two House districts and one Senate district. House and Senate districts are single-member, but the base is split across two districts.





*Figure 2.2: Veterans bills cosponsored per legislator, by district veterans population.*

recommend experimentation to study the role of information on position-taking.

## 2.2 An experiment on bill briefings

The experiment examines the effect of policy-relevant research on position-taking. Legislators were assigned to an in-person, one-on-one policy briefing with a staffer. The staffer discussed the problem addressed, fiscal considerations, and statutory changes the bill would effect (Bimber 1991). Technical information came from bill

sponsors, leaders, the office for fiscal review, the state code, and independent research reports from federal agencies and academics. Importantly, all printed research reports, which were handed out to legislators to guide discussion, prominently featured the sponsors of the bills. Table 2.1 displays an illustrative research report, scrubbed of information that would readily identify the state. The goal was for legislators to come away from meetings with a greater understanding of legislation.

The staffer worked for the Veterans Caucus, not an individual legislator or committee. Caucuses are trusted sources of information inside the legislature (Kingdon 1989; Hammond 2001; Ringe, Victor, and Carman 2013). They frequently employ staff to produce research reports. Like committees, they are typically bipartisan and composed of legislators with heterogeneous ideologies. According to information signaling models, this should make them more trusted than single individuals or groups of homogeneous individuals.

Care was taken to ensure that the treatment was policy information, not social pressure, valence, or political intelligence. Legislators were told that the briefing was a new initiative by the caucus to provide information, but that the caucus had not endorsed any of the bills. Indeed, the preferences of other politicians and interest groups were not discussed. It was made clear to legislators that the caucus' effort was intended to spread information about veterans legislation, but that the bills still belonged to the sponsors. The caucus had no input into the legislation, but it was responsible for the information in the briefing.

*Table 2.1: Illustrative research report.*

<b>Bill</b>	Removing Limits on ROTC Courses for Scholarship Students
<b>Sponsors</b>	House Sponsor / Senate Sponsor
<b>Overview</b>	Scholarships at public universities are not available to students who surpass a threshold number of credit hours. ROTC courses count toward this cap, causing students to become ineligible for the scholarship. Bill excludes ROTC courses from relevant cap.
<b>Current Law</b>	Requirements to be eligible for this public scholarship: 1. Receive the scholarship for no more than 8 semesters. 2. Must have completed fewer than 120 credit hours. 3. Must maintain a minimum GPA.
<b>Problem</b>	Army ROTC requires one elective and one laboratory course per semester for 2-4 years. Navy ROTC requires one naval science course per semester in addition to courses in Calculus, Physics, English, National Security, and World Culture / Regional Studies. <b>Cap can cost ROTC students 2 semesters of eligibility for the public scholarship.</b>
<b>Solution</b>	Exempt ROTC courses from the cap.
<b>Cost</b>	<b>Increase of \$200,000+ per year in state education funding.</b>

Information that could identify the state of interest is removed.

## Experimental units

To increase power, multiple bills and legislators were included in the study. Sixteen veterans bills were selected for inclusion. They represented nearly all veterans bills proposed during the session. Seventy-six legislators were included. Subjects included first-term representatives and committee chairs, members of both parties, and members and non-members of the veterans caucus.<sup>9</sup> Party leaders, the caucus chair, and the caucus chair's officemate were excluded from the study due to their familiarity with the purposes and scope of the study. Nevertheless, 75% of the chamber's membership was included.

Treatment assignment occurred at the legislator-bill dyad level.<sup>10</sup> With 76 legislators and 16 bills, there are a total of 1,216 legislator-bill dyads. Four bills were selected for treatment for each legislator through block random assignment. This created 304 observations where legislators were briefed on a bill and 912 observations where they were not briefed.

Including multiple bills offers opportunities and drawbacks. First, it yields vastly more observations than previous experimental studies of position-taking. Second, legislator-specific treatment effects are identified because each legislator is assigned to treatment for some bills and control for others. Third, bill-specific treatment effects are identified for the same reason. The downside of including multiple bills is that it requires an additional non-interference assumption: treatment is assumed

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<sup>9</sup>Eighteen legislators were caucus members and 58 were not. The caucus did not discuss the legislation during its meetings during that session.

<sup>10</sup>Because dyads and not legislators were the unit of assignment, the analyses reported below do not need to cluster standard errors at the legislator level (Green, Kim, and Yoon 2001).

not to diffuse across bills. Fortunately, there is little evidence that it does.<sup>11</sup>

## **Compliance**

A notable feature of the intervention is the high level of compliance with treatment. Of the 76 legislators who were approached for meetings, all accepted. 74 were successfully briefed in person over a three week period.<sup>12</sup> All meetings covered all assigned bills.

## **Outcome measures**

Bill cosponsorship is a key form of position-taking, frequently examined in academic research (Mayhew 1974; Koger 2003; Kessler and Krehbiel 1996; Highton and Rocca 2005; Talbert and Potoski 2002; Cho and Fowler 2010). Like any form of position-taking, cosponsorship signals legislators' priorities and their policy interests. It is important legislators cosponsor the "right" bills (Campbell 1982; Bernhard and Sulkin 2013). Cosponsoring the wrong bills can cost a legislator electoral support.

In some ways, cosponsorship is a better indicator of individual priorities than roll call voting. Former Senator Richard Lugar explains:

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<sup>11</sup>All legislative experiments with multiple legislators assume treatment does not spill over between legislators. This assumption seems strong, so it is addressed in the next chapter via a standalone study of treatment contagion across legislators. There is evidence of contagion across legislators, but allowing for it does not change the results reported in this paper. There is no evidence of contagion across bills.

<sup>12</sup>Two were not briefed in person, as they were unable to make their scheduled appointments. The first legislator was briefed by phone as she drove from her district to the Capitol. The second was unable to meet at the appointed time due to a scheduling conflict. His assistant was briefed in his absence.

“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).

Members cosponsor far fewer bills than they vote for, so the barrier to cosponsorship, whether informational or preferential, is higher. Witnessing a cosponsorship is a strong indicator of a legislator’s policy preference.

Roll call voting is a secondary outcome of interest. Only six of the sixteen bills in the study reached the House floor, and only one bill received any No votes. Bills failed in committee not because they were particularly unpopular or seriously flawed. Typically their failure came down to fiscal considerations. For example, the bill described in Table 2.1 was probably not enacted due to its \$200,000 projected cost, not because legislators found it politically advantageous to oppose scholarships for ROTC students. Even bills that failed intended to help veterans, so we would expect legislators to cosponsor them for all the same reasons that they take positions on popular bills that are unlikely to become law.

Since not all bills reached a vote, it is unclear whether intent-to-treat effects on roll call voting can be estimated for all bills that reach a vote. Doing so would require assumptions about the relationship between treatment and whether bills received a vote. To avoid making these assumptions, I take another approach. I estimate the average treatment effect for the one contested bill. The estimated average treatment effect for that bill may not be generalizable to others, but it is an unbiased estimate of the true treatment effect for that bill. Since roll call voting is not the primary

outcome of interest, observing effects on one bill is sufficient to show that policy research can influence position-taking activities other than cosponsorship.

## Results

Table 2.2 displays bill cosponsorship by treatment assignment.<sup>13</sup> The control group contains three times as many observations as the treatment group because each legislator was assigned to treatment for 25% of bills. In the control group, 8.1% of observations were cosponsored; in the treatment group, 13.5%. The difference-in-means average treatment effect estimate ( $\widehat{\text{ATE}}$ ) is 5.4 percentage points.

*Table 2.2: Summary of cosponsorship by briefing assignment.*

	Control	Treatment
Cosponsored?		
No	838	263
Yes	74	41
Cosponsorship Rate	8.1%	13.5%

$\widehat{\text{ATE}}$  is also estimated through linear regression with bill and legislator specific fixed effects:<sup>14</sup>

$$\begin{aligned}
 Y_{ij} = & \alpha + \gamma_1 \text{Legislator}_1 + \gamma_2 \text{Legislator}_2 + \dots + \gamma_{75} \text{Legislator}_{75} + \\
 & \delta_1 \text{Bill}_1 + \delta_2 \text{Bill}_2 + \dots + \delta_{15} \text{Bill}_{15} + \quad (2.1) \\
 & \beta d_{ij} + \epsilon_{ij}
 \end{aligned}$$

<sup>13</sup>Bill sponsorship is included in cosponsorship.

<sup>14</sup>Cosponsorship was concentrated on one piece of legislation that received 56 signatures. Due to this potential outlier, treatment effects are estimated using bill-specific intercepts and even bill-specific treatment effects. Results are not driven by the outlying bill.

where  $Y_{ij}$  is cosponsorship by legislator  $i$  of bill  $j$ ;  $\gamma_1$  through  $\gamma_{75}$  are estimated legislator specific fixed effects;  $\delta_1$  through  $\delta_{15}$  are estimated bill specific fixed effects;<sup>15</sup> and  $d_{ij}$  is a treatment indicator.  $\beta$  is the ATE. ATE estimates obtained from logistic regression are available in Appendix A.3.<sup>16</sup> Robust standard errors and one-tailed p-values are presented for all estimators.<sup>17</sup>

The information treatment increased cosponsorship by 5.0 to 5.4 percentage points ( $p < 0.01$  for all estimates). Including legislator and bill fixed effects does not substantially alter estimates. These effects are substantial in magnitude. Only 8.1% of bills were cosponsored in the control group, so treatment increased cosponsorship by over 60% from the baseline rate.

One bill merits individual attention. Bill 16 was the only bill to be contested on the House floor. It was also the only bill to be clearly flawed as originally drafted. The sponsor of Bill 16 stated that there was an error in its drafting. The nature of the error is beyond the scope of this paper, but it was severe enough to engender opposition from powerful lobbyists and interest groups. The sponsor quickly recognized the need to correct the error, so the bill was ultimately amended to remove the offending provisions, but not until after briefings had been held. Although it was not intended, this bill allows us to examine what happens when legislators are informed about a flawed bill.

Table 2.4 shows the effect of treatment on cosponsorship and roll call voting for

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<sup>15</sup>One legislator and one bill serve as the baseline for comparison.

<sup>16</sup>Freedman (2008) shows that logistic regression with covariates can lead to biased ATE estimates. Nevertheless, I present the logistic regression results due to possible concerns about the binary dependent variable.

<sup>17</sup>Standard errors and significance tests were verified with randomization inference, which yielded smaller standard errors and p-values in all cases.



Table 2.3: Estimated briefing effects.

DV: Cosponsorship		
	(1)	(2)
$\widehat{\text{ATE}}$	.054**	.050**
$\widehat{\text{SE}}$	(.022)	(.017)
95% C.I.	(0.011,0.096)	(0.016,0.084)
Regression Model	Simple	Multiple
Fixed Effects <sup>(a)</sup>	No	Yes
N	1,216	1,216

(a) Bill and legislator fixed effects.

Logistic regression estimates converted to predicted probabilities.

Robust standard errors and p-values presented.

One-tailed p-values indicated at  $p < .05$  (\*),  $p < .01$  (\*\*).

Bill 16. Baseline support for the bill is quite high. 78% of untreated legislators cosponsored the bill, as the sponsor called for cosponsors during floor debate after it had been amended. 93% voted for the bill. Treated legislators were less likely to cosponsor and vote for the bill. Cosponsorship was 16 percentage points lower among treated than untreated legislators ( $p < 0.10$  one-sided from randomization inference). Treated legislators also voted for the bill at a 17 percentage point lower rate ( $p < 0.05$  one-sided).<sup>18</sup> Despite the sponsors' blandishments, treated legislators hesitated when asked to support the bill.

Analyzing a single bill limits the scope of findings. Estimating treatment effects

<sup>18</sup>No votes include legislators voting “No”, “Present Not Voting”, or who elected not to vote. Since the legislature requires a majority of all 99 members to pass — not a majority of those voting — legislators often elect not to vote instead of casting a vote against a peer’s bill. Restricting the analysis to those who voted “Yes”, “No”, or “Present Not Voting” does not change the results.

Table 2.4: **Estimated briefing effects (critical coverage).**

	Cosponsorship	Roll Call
$\widehat{ATE}$	-.163	-.165*
$\widehat{SE}$	(.119)	(.089)
$\bar{Y}_{Control}$	.782	.927
Fixed Effects <sup>(a)</sup>	No	No
N	76	76

(a) Bill and legislator fixed effects.

Significance indicated at  $p < 0.05$  (\*) and  $p < 0.01$  (\*\*) one-sided. Standard errors and p-values obtained using randomization inference with 10,000 simulated treatment assignments.

on a single bill, chosen ex post, exposes these analyses to valid “garden of forking path” criticisms (Gelman and Loken 2013). It is also not clear that this bill is representative of others. Nevertheless, it illuminates a meaningful phenomenon. At least once, learning that a bill had flaws was enough to convince legislators not to support it. This is reassuring to those of us who think legislators should consider not just partisanship and ideology when taking positions, but also whether a proposal makes for good policy. Once the bill was fixed, it passed.

### **Treatment effect heterogeneity**

Now let us turn to a more direct engagement with information signaling models. These models argue that information is beneficial to all legislators because it reduces uncertainty (Gilligan and Krehbiel 1990). As long as the information provider is “not too far” in preferences from the receiver, she can truthfully communicate helpful information. However, there are two distinct reasons to expect information’s effects

to vary across legislators.

Both reasons suggest briefings should be more influential among legislators predisposed to support a bill. One of the main predictions of information signaling models is that communication is easier between like-minded individuals (Crawford and Sobel 1982; Austen-Smith and Riker 1987; Gilligan and Krehbiel 1987, 1989, 1990). Although briefings were conducted by an ostensibly nonpartisan caucus staffer, legislators still may have interpreted information in light of who sponsored the bill. Thus briefings on appealing bills might be deemed more trustworthy.

Information will be heterogeneously influential for a second reason. Although information increases all legislators' utility by reducing uncertainty, we do not observe utility. We observe a binary indicator, cosponsorship, based on utility. So we must have some idea of how utility translates into cosponsorship. Prior work argues legislators cosponsor bills only if their expected utility surpasses a utility threshold (Peress 2013). Since baseline cosponsorship rates are low, the legislators closest to the threshold (but below it) will be those predisposed to support proposals. Thus equal shifts in utility will cause only potentially supportive legislators to cross over the threshold.<sup>19</sup>

Legislators' predisposition to support experimental bills is predicted by legislators' cosponsorship of non-experimental bills. Cosponsorship has frequently been used to construct similarity scores between legislators (Talbert and Potoski 2002; Fowler 2006; Aleman et al 2009; Peress 2013), so Cosponsorship Similarity scores are calculated between each subject and bill sponsor. For this study, cosponsorship sim-

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<sup>19</sup>For more information on the choice model underlying cosponsorship, see Appendix A.1.

ilarity is more predictive of the outcome variable than other measures of ideological similarity based on roll call voting or campaign donations.<sup>20</sup> Campaign finance-based ideology scores are also not available for many of the legislators. Results below show that cosponsorship similarity is highly predictive of cosponsorship of experimental bills.

Cosponsorship Similarity is constructed as follows:

$$\text{Cosponsorship Similarity}_{ij} = \frac{\sum_{b=1}^B \text{cosponsor}_{ib} * \text{cosponsor}_{jb}}{\sum_{b=1}^B \text{cosponsor}_{jb}}$$

where Cosponsorship Similarity between experimental subject  $i$  and bill sponsor  $j$  equals the sum over all bills  $B$  not included in the study of those cosponsored by both  $i$  and  $j$  scaled by the total number of bills cosponsored by  $j$ .<sup>21</sup> Cosponsorship similarity can be understood as the prior probability of subject  $i$  cosponsoring an experimental bill by sponsor  $j$  based on the frequency of  $i$  cosponsoring  $j$ 's non-experimental bills.

Two sets of cosponsorship similarity scores are constructed. One set uses cosponsorship from the session before the intervention was fielded. This measure is pre-treatment, but it is not available for first-term subjects. A second set uses cosponsorship from the session during which the intervention was fielded. These measures are available for all legislators, but they risk bias if the intervention influenced cosponsorship of non-experimental bills. I present results using both sets of cosponsorship

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<sup>20</sup>Neither campaign donation- or roll call voting-based measures are predictive of cosponsorship in this sample.

<sup>21</sup>Results are robust to scaling by the number of bills cosponsored by  $i$ .

similarity, since there is little evidence or reason to suspect that the intervention spilled over across bills.

Heterogeneous treatment effects can be estimated by modifying Equation 2.1 to include an interaction between treatment and cosponsorship similarity:

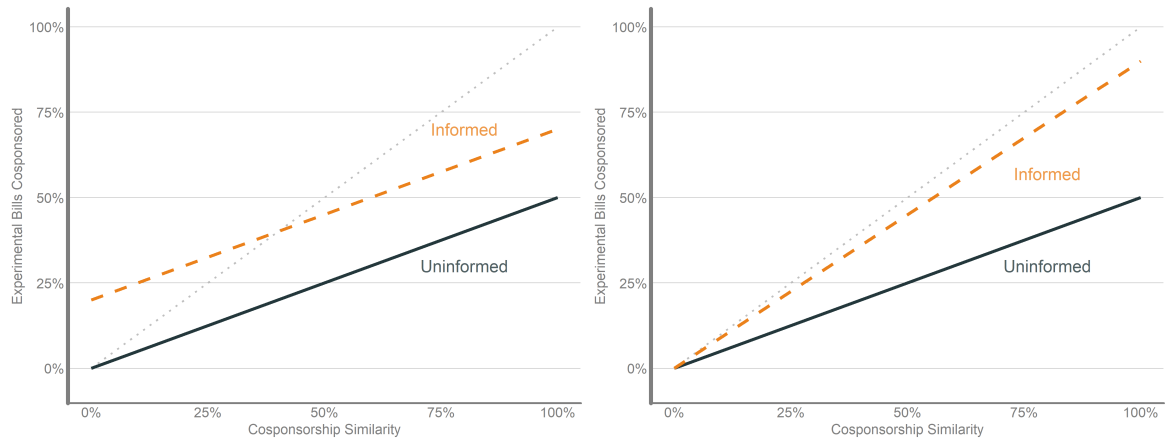
$$\begin{aligned}
 Y_{ij} = & \alpha + \gamma_1 \text{Legislator}_1 + \gamma_2 \text{Legislator}_2 + \dots + \gamma_{75} \text{Legislator}_{75} \\
 & + \delta_1 \text{Bill}_1 + \delta_2 \text{Bill}_2 + \dots + \delta_{15} \text{Bill}_{15} \\
 & + \beta_1 \text{Cosponsorship Similarity}_{ij} + \beta_2 d_{ij} + \beta_3 d_{ij} * \text{Cosponsorship Similarity}_{ij} \\
 & + \epsilon_{ij}
 \end{aligned}
 \tag{2.2}$$

The key parameters of interest are  $\beta_2$  and  $\beta_3$ . If there are similarity-independent effects of treatment,  $\beta_2$  will be positive. If there is a similarity-based marginal effect, as predicted in signaling models,  $\beta_3$  will be positive. Figure 2.3 illustrates hypothesized patterns of effects. Equation 2.2 is estimated using both measures of cosponsorship similarity, with and without legislator and bill-specific fixed effects.<sup>22</sup>

Figure 2.4 displays the experimental data, with observations binned due to the binary nature of cosponsorship. Control (dark blue, solid) and treated (light brown, dashed) observations do not differ much at low levels of cosponsorship similarity. However, at the average level of similarity (0.17), treated legislators are twice as likely

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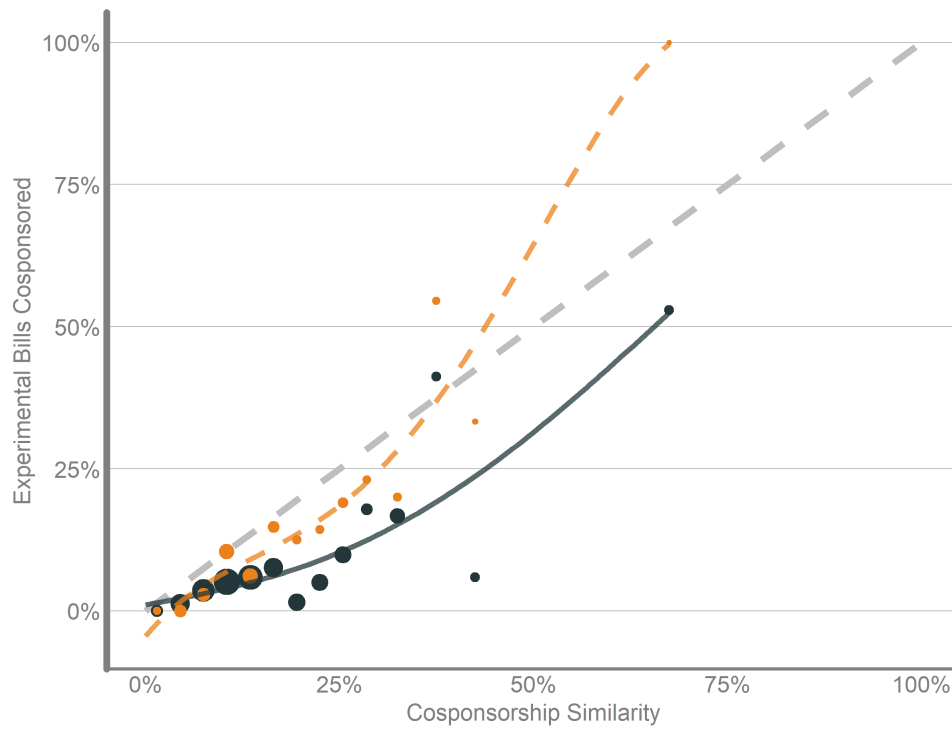
<sup>22</sup>Excluded from the display are dummy variables that indicate whether the subject sponsored the bill and whether the subject/sponsor was treated. Sponsors are defined as cosponsors and have similarity scores of 1, by construction. As a result, a mechanical relation would increase the parameters  $\beta_1$  and  $\beta_3$  because every observation with a cosponsorship similarity score of 1 corresponds to  $Y_{ij} = 1$ . Including these dummy variables, which is analogous to dropping observations where the sponsor was the subject, redefines parameter estimates as the change in cosponsorship observed among non-sponsors of the bill of interest.



(a) Similarity-independent effect.

(b) Similarity-based marginal effect.

*Figure 2.3: Hypothesized heterogeneous briefing effects.*



*Figure 2.4: Observed heterogeneous briefing effects.*

to cosponsor as untreated legislators (10.7% to 5.1%), with the difference continuing to grow as similarity increases.

Table 2.5 displays regression results. First, cosponsorship similarity is highly predictive of observed cosponsorship. A one percentage point increase in similarity is associated with a 0.59 to 0.68 percentage point increase in cosponsorship in the two specifications without legislator fixed effects. Even accounting for legislator and bill fixed effects, a one percentage point increase in cosponsorship similarity is associated with a 0.33 to 0.61 percentage point increase in cosponsorship. This is strong validation of the cosponsorship similarity measure.

We turn now to the primary estimands of interest. Estimates of the similarity-independent effect  $\hat{\beta}_2$  range from -2.9 to -0.5 percentage points. They cannot be differentiated from zero at conventional levels of statistical significance. In contrast, estimates of the similarity-based marginal effect  $\hat{\beta}_3$  are positive and substantial in magnitude in each specification. The effect ranges from 0.3 to 0.5 ( $p < 0.05$  two-tailed using current session measures). Not only was treatment more influential among subjects predisposed to support bills, but the treatment also doubled the a priori predictive relationship between cosponsorship similarity and cosponsorship of veterans bills.

How exactly should we interpret results that treatment was primarily effective among like-minded legislators? Cosponsorship similarity is an imperfect measure of ideology. It is not purely a function of ideology. It also depends on legislators' personal relationships with peers.<sup>23</sup> Thus it is safer to say that “like-minded” legis-

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<sup>23</sup>It is unclear whether cosponsorship is more susceptible to non-ideological factors than roll call voting or campaign giving, or how the distinction between ideological and non-ideological factors

Table 2.5: Estimated heterogeneous briefing effects.

	(1)	(2)	(3)	(4)
Cosponsorship Similarity	0.332**	0.678**	0.620**	0.760**
$\widehat{SE}$	(0.121)	(0.114)	(0.170)	(0.163)
$d$	-0.025	-0.029	-0.005	-0.028
	(0.029)	(0.031)	(0.050)	(0.060)
$d^*$ Cosponsorship Similarity	0.470*	0.515*	0.250	0.369
	(0.198)	(0.223)	(0.288)	(0.366)
Post-treatment covariate	Yes	Yes	No	No
Fixed effects <sup>(a)</sup>	Yes	No	Yes	No
Subjects	All Legislators		Returning Legislators	
N	1,216	1,216	915	915

Significant at  $p < 0.05$  (\*), and  $p < 0.01$  (\*\*) two-tailed.

(a) Legislator and bill-specific fixed effects.

Robust standard errors and p-values presented.

lators are more influenced by briefings than to ascribe differences in briefing effects to a particular factor like ideology. Ultimately, any heterogeneous effect should be interpreted with caution. Unlike main effects of briefings on cosponsorship, heterogeneous effects depend on non-randomized factors. Thus it might be best to interpret these results as consistent with predictions from information signaling models but not confirmative.

is determined in the first place.



## Alternative explanations

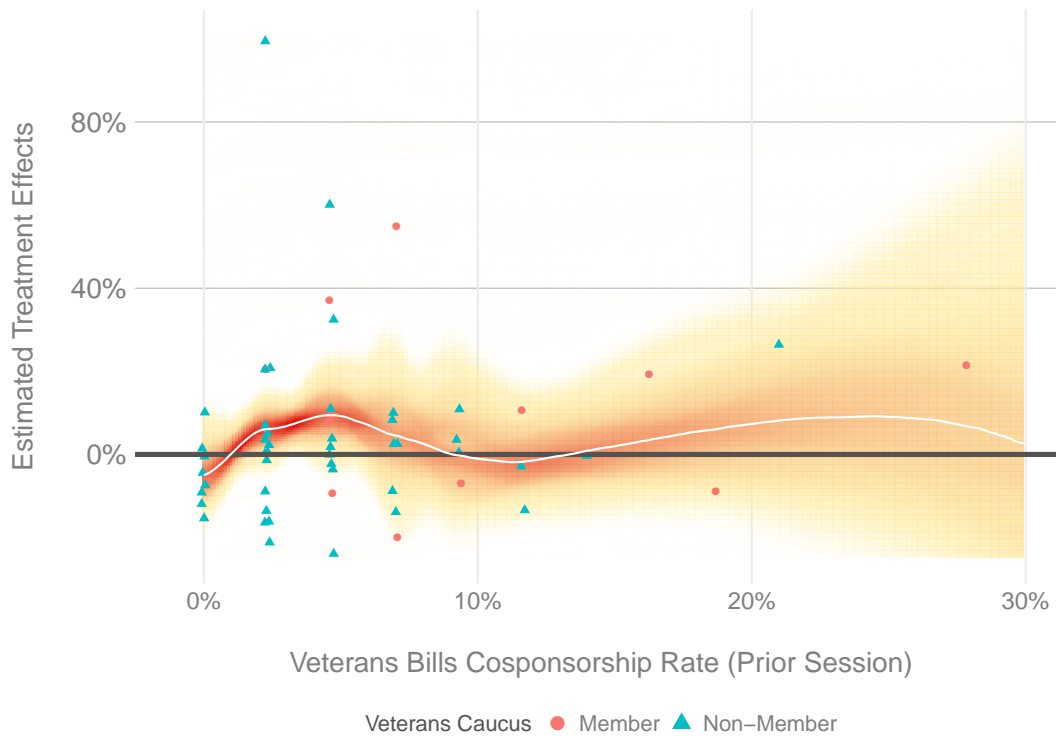
The analyses in this study assume that briefings communicated some policy-relevant information. What if that was not the case? What if legislators already knew the content of bills, or did not learn anything from the treatment? Why would the briefings be influential?

One alternative explanation is that treatment made it easier for supporters of veterans bills to do so publicly. Convincing legislators to make private support public is important, of course, but it could occur even if the briefings did not convey any bill-related information. Did the briefings just draw out latent supporters? Figure 2.5 suggests that they did not. It displays estimated individual treatment effects against legislators' cosponsorship of veterans bills in the assembly before the intervention.<sup>24</sup> There is little evidence that treatment effects increase with prior veterans cosponsorship. Effects initially increase in prior cosponsorship, but then decrease. Prior cosponsorship of veterans bills does not explain heterogeneous effects across legislators by cosponsorship similarity.

A second alternative explanation is that briefings made legislators aware of bills that, absent treatment, they would have ignored. This explanation would diminish the practical significance and generalizability of results. While educational and informative briefings might influence other forms of legislative behavior, raising awareness

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<sup>24</sup>Individual treatment effects are estimated with substantial uncertainty. Figure A.3 plots the magnitude of individual effects against their probability of occurring under the sharp null hypothesis. Individual effects reach conventional levels of statistical significance only when they approach 50 percentage points. For this reason I examine aggregate trends, not specific legislators. Overall trends are demonstrated by the white loess line fit to the individual treatment effects, with its uncertainty demonstrated by the shading. Uncertainty is estimated via simulation.



*Figure 2.5: Estimated legislator-specific briefing effects.*

would not. After all, when legislators vote on bills, they are, one hopes, aware they exist.

We can leverage bills' differential progress through the policymaking system to address this question. Assume that all legislators become aware of bills that reach a roll call vote. In fact, many legislators cosponsor bills as they are discussed on the floor, because it is the first time they become aware of them. If legislators are aware of bills that reach a vote, we would observe briefing effects due to raising awareness

only for bills that do not reach the floor.

Bills that reached the floor exhibit nearly identical estimated treatment effects on cosponsorship (5.8 percentage points) as bills that did not (5.2). The difference of 0.6 percentage points is not statistically significant ( $p = 0.88$  two-tailed from t-test). This evidence speaks against briefings simply raising awareness of legislation. It seems briefings actually communicated information that legislators were not able to get otherwise, including during floor debates.

### **2.3 Discussion of briefing's effectiveness**

The experiment described in this study is unusual in at least four respects. First, information concerning real policy proposals was delivered directly to legislators. Second, treatment was delivered through a legislative institution thought to address informational problems. Third, it examines behavioral outcomes — bill cosponsorship and voting. Fourth, it included multiple bills for a well-powered suite of tests.

Like most experiments, there are strong concerns about the generalizability of findings. A one page caucus research report probably will not change U.S. Senators' positions on Obamacare or other highly salient policies. Policy research may only be influential for broadly-supported issues, and only among relatively unprofessional legislators. It may be more important to cosponsorship than roll call voting. These are legitimate concerns, and the only way to address them is through more research. One of the benefits of this paper is that it provides a touchstone in the study of informational influence. Future work should speak to differences across legislatures, issues, and institutions.

These findings provide a benchmark in the study of legislative professionalism. Untreated legislators cosponsored legislation at approximately 60% the rate of treated, more informed legislators. If this study is indicative of other issues considered by the legislature, information constraints influence 40% of legislators' cosponsorship decisions. This is a clear, quantitative measure of information constraint. With such a measure, scholars can repeat this intervention in other legislatures, with different institutions and profiles of legislators, to see how constraints vary and how close legislators come to their fully-informed positions.

# Chapter 3

## Cue-Taking

Every session, legislators consider thousands of policy proposals, covering a wide range of issues, under severe time constraints. With such a workload, it is simply not feasible to expect any individual to develop detailed knowledge about most bills. As a result, it is little surprise that legislators are often described as poorly informed about public policy (Mill 1861; Treadway 1938; Kingdon 1989; Krehbiel 1991). Nevertheless, legislators are expected to vote for and cosponsor bills that are important to constituents (Canes-Wrone, Brady, and Cogan 2002). So how do uninformed legislators take the right positions?

Legislators can overcome individual information constraints by relying on cues from their peers (Matthews and Stimson 1975; Kingdon 1989; Masket 2008; Box-Steffensmeier, Ryan, and Sokhey 2015). Legislators often give advice to one another about which position they should take. This guidance may be formal, such as an instruction from a party leader, or informal, such as one legislator observing the

vote of another. Cues effectively allow the decision-maker to take her fully-informed position without actually becoming fully-informed about the issue. Legislators report that cue-taking is important to anywhere between 40% (Kingdon 1989) and 75% (Matthews and Stimson 1975) of their votes. Observational studies find cue-taking influences about 10% of votes (Masket 2008). It seems clear that cue-taking is an important factor in position-taking.

More rigorous research designs have found less evidence of cue-taking. Rogowski and Sinclair (2012) leverage the random assignment of office space in Congress to estimate the causal effect of office proximity on position-taking. Coppock (2014, 2016) estimates cue-taking between ideologically-similar legislators exposed to a randomized information treatment by Butler and Nickerson (2011). Unlike survey or observational studies, neither experimental study finds consequential cue-taking.

These contradictory findings illustrate how much is unknown about cue-taking. Whom do legislators look to for cues? Perhaps deskmates (Masket 2008) and friends (Matthews and Stimson 1975) share information, but not office neighbors (Rogowski and Sinclair 2012) or ideologically-similar legislators (Coppock 2016). What happens if a typical cue-giver is unavailable? Legislators may be able to replace one cue-giver with another. Did cue-taking decline between the early study of Matthews and Stimson and the later studies of Rogowski and Sinclair and Coppock?

This study addresses these questions about cue-taking with a large dataset from two legislative field experiments. The experiments randomly provided legislators with technical policy information about real bills through one-on-one briefings conducted by a legislative staffer. This information substantially affected position-taking by

legislators who were directly briefed. The primary research question of this study is whether position-taking changed among unbriefed legislators who worked in close contact with briefed peers. This study contributes to the literature on cue-taking in at least four ways: (1) by illustrating that the causal effect of cue-taking can be estimated through an experiment that models contagion; (2) by providing precisely-estimated cue-taking effects due to the number of observations and a restricted randomization that improves power; (3) by estimating heterogeneous cue-taking effects by whether bills reach the floor; and (4) by estimating cue-taking for alternative contagion models.

### **3.1 Cue-taking and contagion**

“Unlike his colleagues in the laboratory sciences, who are able to create experimental conditions at their whim, the political scientist is obliged to wait until the conditions happen to present themselves in the real world.” (Kingdon 1989, 133-134).

In the past decade, experimental research designs have gained popularity in legislative studies. Among many other interesting works, foundational experiments have examined the causal effect of information treatments on constituency service (Butler and Broockman 2011); constituents’ access to legislators (Kalla and Broockman 2015); and legislators’ voting behavior (Bergan 2009; Butler and Nickerson 2011) and home style (Butler, Karpowitz, and Pope 2012). As pathbreaking as they have been, these studies have largely ignored cue-taking or interpersonal influence between

legislators (cf. Coppock 2016).

At first, experiments might seem an odd method for studying cue-taking. Experiments often rely on the “stable unit treatment value assumption” (SUTVA), also known as “non-interference.” Non-interference declares that each subject’s potential outcome is unaffected by the treatment status of others. Treatment contagion, or spillover, violates the assumption. Non-interference is typically necessary to identify the causal effect of a treatment on individual behavior. If interference occurs in unknown ways, estimating treatment effects becomes practically impossible.

Interference can be an opportunity, not just a nuisance. Treatment effects are identified not only for those subjects directly treated, but also for subjects exposed to treatment spillovers. Spillovers are only identified, however, if the spillover model is known.

There have been two general approaches to modeling treatment contagion. One approach allows a broad network of spillover in which contagion is allowed between any subjects who interact with one another (Bowers, Frederickson, and Panagopoulos 2013; Coppock 2016). This approach is appealing because it can completely relax the strict non-interference assumption; any subject can influence any other. However, it requires strong modeling assumptions to combine the spillovers from different sources into a single measure of aggregate treatment exposure for each subject. It also requires assumptions to model behavior as a function of aggregate exposure. A second approach allows interference within narrow subgroups of subjects, but not across them. For example, Nickerson (2008) and Foos and de Rooij (2017) estimate contagion of a get-out-the-vote treatment within households assuming non-



interference between households. In choosing an approach, the researcher must decide whether contagion is likely to be more diffuse or more limited in a given context.

This study examines contagion between the legislative equivalent of households: offices. In this state, a majority of legislators share office suites. Their assistants sit in a common room beyond which each legislator has a private office. Legislators have wide latitude in choosing their office suites. As a result, officemates tend to represent similar, often neighboring districts, and to be friends. Officemates will share information even if friendship, ideology, or electoral incentives underlie the true contagion network.

Let us define the potential outcomes that can result if treatment diffuses within offices. Legislators' potential outcomes ( $Y_i$ ) are a function of their own ( $d_i$ ) and their officemate's ( $d_{-i}$ ) treatment statuses:  $Y_i(d_i, d_{-i})$ .<sup>1</sup> To fix ideas, a legislator's cosponsorship decision may be the result of whether she receives a policy briefing and whether her officemate receives a briefing.

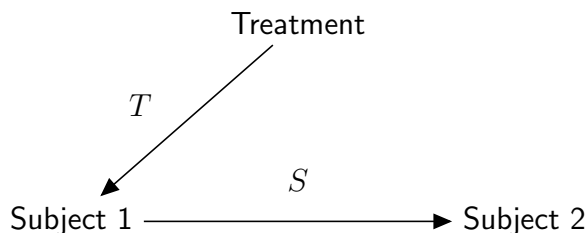
Figure 3.1 illustrates direct ( $T$ ) and secondary ( $S$ ) treatment effects between two subjects when one individual receives direct treatment (Nickerson 2008; Foos and de Rooij 2017). Secondary treatment equates to cue-taking. It may result from conscious information-sharing or from legislators' mimicking the positions of their peers. The content of the secondary treatment cannot be strictly defined, but with random assignment and the right spillover model, we are sure that secondary treatment is the portion of the briefing effects transmitted between legislators, that is, by cues.

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<sup>1</sup>This discussion assumes full compliance with treatment.

This restricted model is commonly used in experimental analyses of contagion. It is restricted because at most one individual in each pair is assigned to treatment. Assigning exactly one individual maximizes power for the estimation of  $T$  and  $S$ . What if both individuals are assigned to treatment?

*Figure 3.1: Spillover in a two-person setting.*



Assigning both subjects in a pair to direct treatment introduces new causal effects. In Figure 3.2, each effect ( $T$  and  $S$ ) is conditioned by whether the other effect is also present.  $T_{S=0}$  indicates an individual who is assigned to direct but not secondary treatment, and  $T_{S=1}$  an individual who is assigned to both. This raises a complication. Are direct effects independent of secondary effects? That is, does  $T_{S=0} = T_{S=1}$  and  $S_{T=0} = S_{T=1}$ ? We simply don't know, and the strong assumption that effects are additive is typically unwarranted in social science applications (Hudgens and Halloran 2008; Aronow and Samii 2013; Bowers, Frederickson, and Panagopoulos 2013). As a result, we cannot separately estimate  $T_{S=1}$  or  $S_{T=1}$ . We can, however, estimate the combined effect of  $T_{S=1} + S_{T=1}$ . Table 3.1 demonstrates how direct briefing ( $T_{S=0}$ ), secondary cue-taking ( $S_{T=0}$ ), and combined ( $T_{S=1} + S_{T=1}$ ) treatment effects can be estimated from the full factorial design that allows treatment by treatment interaction.<sup>2</sup>

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<sup>2</sup>Note that attempting to estimate  $T_{S=1}$  by taking the difference  $\mathbb{E}[Y_i | d_i = 1, d_{-i} = 1] -$

Figure 3.2: Spillover in a two-person setting with treatment interactions.

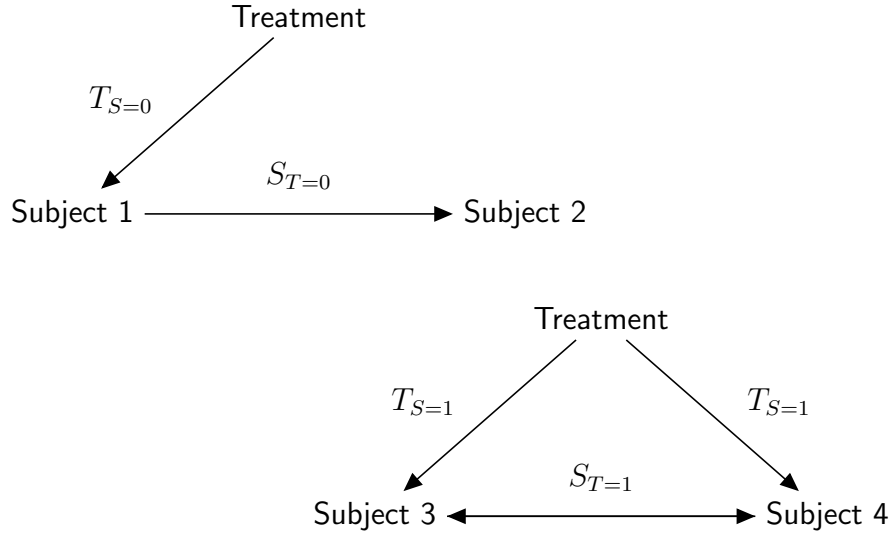


Table 3.1: Identification of briefing and cue-taking treatment effects.

	$D_{-i} = 0$	$D_{-i} = 1$	
$D_i = 0$	(1) $\mathbb{E}[Y_i   d_i = 0, d_{-i} = 0]$	(2) $\mathbb{E}[Y_i   d_i = 0, d_{-i} = 1]$	$S_{T=0} = (2) - (1)$
$D_i = 1$	(3) $\mathbb{E}[Y_i   d_i = 1, d_{-i} = 0]$	(4) $\mathbb{E}[Y_i   d_i = 1, d_{-i} = 1]$	Missing
	$T_{S=0} = (3) - (1)$	Missing	$T_{S=1} + S_{T=1} = (4) - (1)$

One additional quantity of interest is relevant to studies of cue-taking. The contagion rate ( $\alpha$ ) can be estimated as the ratio of secondary to direct treatment effects:  $\frac{S_{T=0}}{T_{S=0}}$  (Nickerson 2008). The contagion rate describes the percentage of direct treatment that diffuses between individuals. For studies of cue-taking, it is the percentage change in the position of a cue-taker that results from changing the position of her cue-giver.

With these estimands defined, we can pose several hypotheses regarding cue-  


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 $\mathbb{E}[Y_i | d_i = 0, d_{-i} = 1]$  implicitly assumes  $S_{T=1} = S_{T=0}$ .

taking:

**$H_1$ : Cue-taking affects position-taking.  $S_{T=0} > 0, \alpha > 0$**

Legislators are thought to take cues from one another (Kingdon 1989). As a result, secondary treatment effects, and thus the contagion rate, should be positive. But how prevalent and influential is this cue-taking? Is it an occasional behavior or one that guides the majority of decisions legislators make? The larger the contagion rate, the greater the percentage of legislators' positions that depend on cues from peers.

**$H_2$ : Exposing a legislator to a cue will exert no additional effect if the legislator has already received a direct briefing.  $T_{S=1} + S_{T=1} = T_{S=0}$ .**

**$H_3$ : Briefing a legislator who has been exposed to a cue will exert no additional effect.  $T_{S=1} + S_{T=1} = S_{T=0}$ .**

$H_2$  and  $H_3$  describe a world in which briefings and cues are perfect substitutes. They also describe treatments that lead legislators to their fully-informed positions. It is possible one of the treatments, but not the other, leads to fully-informed positions (which is why the hypotheses are stated separately). A more modest proposition is that cues and briefings are imperfect substitutes. In that case, the combined effects of direct and secondary treatments would be smaller than the sum of their separate effects ( $T_{S=1} + S_{T=1} < T_{S=0} + S_{T=0}$ ).

**$H_4$ : The combined effects of briefings and cues will be greater than the sum of their separate effects.  $T_{S=1} + S_{T=1} > T_{S=0} + S_{T=0}$ .**

$H_4$  describes briefings and cues as complements. Repeated messaging may magnify the effectiveness of individual messages. Experimental evidence among voters

finds that repeated campaign messaging leads to larger behavioral effects than one-off communications (Green and Zelizer 2017; Zelizer 2018). This finding would suggest that even after receiving a briefing or a cue, legislators retain some uncertainty about legislation that is diminished by further information-sharing.

### **3.2 An experiment on bill briefings and cue-taking**

To estimate the causal effect of information on legislators' position-taking, two experiments were conducted over a two-year assembly in the same state legislature as the previous chapter.

#### **Treatment**

A staffer for the Veterans Caucus conducted one-on-one policy briefings with legislators to discuss randomly-selected veterans legislation. In-person briefings ensure that treatment is only administered to the assigned legislator. Briefings included both an oral discussion and a printed research report that contained bill-specific policy analyses (see Table 2.1 in the prior chapter for an illustrative research report used in a bill briefing). Analyses included the problem addressed, fiscal considerations, and statutory changes the bills would effect (Bimber 1991). Information came from bill sponsors, the caucus chair, the committee for fiscal review, the state code, and independent research reports from federal agencies and academics.

## Units

The unit of random assignment was the legislator-bill dyad. The first study included 76 legislators and 16 bills, and the second included 81 legislators and 16 bills, for a total of 2,512 observations. Over 75% of the legislature’s membership was included in the studies. The 32 bills represented nearly all veterans legislation sponsored over the two years.

## Treatment assignment

In each study, legislators were briefed on four of the sixteen eligible bills. Assignment procedures differed slightly across the two studies. The first study selected bills for treatment using block random assignment within legislator.<sup>3</sup> Treatment assignment was independent across legislators, which allowed multiple legislators in each office suite to be assigned to direct treatment for the same bill.

The second study features three complications to its assignment procedure. First, it included an additional treatment arm. Briefings were delivered either by the caucus staffer or by an advocate for a veterans’ interest group. To maintain parallelism between the studies (and because the advocate treatment appears to have been minimally effective; see Appendix B.2), advocate treatment effects are not displayed in the analyses below.<sup>4</sup>

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<sup>3</sup>As in any block randomized design, the fact that some blocks (in this case, legislators) may be more prone to a particular outcome or more susceptible to treatment does not bias estimates or require standard errors to be clustered at the block level (Gerber and Green 2012).

<sup>4</sup>All estimators for staffer treatment effects take the advocate treatment into account. Hereafter the “staffer” term is dropped, as treatment refers only to the activities of the staffer.

Second, legislators were prohibited from receiving briefings from both the caucus staffer and the advocate. Legislators were first assigned to either the caucus staffer, advocate, or no treatment condition, and then assigned to be briefed on four of the sixteen eligible bills. This multi-level assignment requires standard errors to be clustered.<sup>5</sup>

Third, restricted randomization ensured that no two legislators in the same office were assigned to direct treatment for the same bill. With two treatment arms, interactions between direct and secondary treatment for the staffer and advocate treatments would have yielded an unmanageable number of potential outcomes. The restriction maximizes the number of observations exposed only to cues.

## **Compliance**

Both interventions featured high rates of compliance. In Study 1, 74 of the 76 legislators (97%) were briefed in-person. In Study 2, 25 of the 29 legislators (86%) were briefed.<sup>6</sup> Due to noncompliance, analyses below estimate intent-to-treat effects (ITT). ITTs represent the average change in cosponsorship of assigning a unit to treatment and do not account for whether units actually received the treatment.

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<sup>5</sup>It is not exactly clear what level should be used for the clustering. Although assignment was not performed at the level of the individual legislator-bill dyad, it also was not performed at the level of the legislator. Standard errors could be clustered at the level of the legislator out of conservatism, or standard errors can be obtained through randomization inference. I adopt the latter approach.

<sup>6</sup>Noncompliance resulted from several causes, all seemingly independent of treatment assignment.

## Outcomes

Cosponsorship is an important form of position-taking. It signals to agenda setters the breadth of support for legislators' proposals (Kessler and Krehbiel 1996; Harbridge 2015). Unlike roll call voting, cosponsorship is largely unaffected by parliamentary circumstances or party whipping (Lugar 2017). Spatial models of position-taking predict that uncertainty will diminish legislators' willingness to cosponsor legislation, so briefings and cues will relax these constraints and promote cosponsorship (Gilligan and Krehbiel 1987; Peress 2013).

## Spillover model

Contagion is modeled between legislators who share office suites. Due to space constraints in the capitol building, 68 of the 76 legislators in Study 1 and 62 of the 81 legislators in Study 2 shared office suites. Assignment to offices is not random, and legislators frequently maneuver to occupy suites with friends. Legislators in one-person offices are dropped, as they cannot be assigned to secondary treatment. This leaves 2,080 legislator-bill observations.<sup>7</sup>

There are two- and three-person office suites in the capital. This raises two complications. First, probabilities of assignment to secondary treatment vary across office sizes. Inverse probability weights account for these differences (Gerber and Green 2012). Second, legislators in three-person suites could be exposed to spillover through both suitemates. Due to the small number of such observations, they are

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<sup>7</sup>See Table B.4 in Appendix B.2 for estimated effects in one-person suites.



not displayed.<sup>8</sup>

## Results

Table 3.2 displays weighted average cosponsorship rates by direct and secondary treatment assignment, with the number of observations in each condition in parentheses. The rows represent a legislator’s own briefing assignment ( $D_i$ ) and the columns the assignment of her officemate ( $D_{-i}$ ). The untreated copponsorship rate is 10.4%. Cosponsorship rates are higher among all three treatment conditions at 19.1 – 20.8%.

*Table 3.2: Summary of cosponsorship by briefing and cue-taking assignment.*

		Cue-taking	
		$D_{-i} = 0$	$D_{-i} = 1$
Briefing	$D_i = 0$	10.4 (1,127)	19.1 (372)
	$D_i = 1$	20.8 (257)	19.7 (88)

Observations assigned to the advocate (200) or multiple caucus spillover treatments (36) are omitted.

To account for imbalance in the profile of bills assigned to treatment,<sup>9</sup> treatment effects are estimated with weighted least squares regression with bill- and legislator-specific fixed effects:<sup>10</sup>

<sup>8</sup>Nevertheless, all estimators take into account the possibility of multiple cue-taking assignments.

<sup>9</sup>Table B.1 in Appendix B.2 shows that controlling for bill- and legislator-specific fixed effects diminishes the magnitude of the staffer treatment effect in the second study. Bills with high baseline probabilities of cosponsorship were, by chance, disproportionately assigned to the caucus treatment. To prevent conflating treatment effects with the effects of the skewed actualization, estimation must account for the imbalance in baseline cosponsorship probabilities across bills.

<sup>10</sup>Appendix B.2 displays robustness checks. Table B.2 presents results without legislator-specific dummy variables, since their large number diminishes degrees of freedom. Table B.3 presents results for only the first study to show that the skewed actualization in the second study does not drive results.

$$\begin{aligned}
Y_{ib} = & a + b_1 d_{ib}^{10} + b_2 d_{ib}^{01} + b_3 d_{ib}^{11} + \\
& g_1 \text{Bill } 1_b + g_2 \text{Bill } 2_b + \dots + g_{B-1} \text{Bill B-1}_{b-1} + \\
& h_1 \text{Leg } 1_i + h_2 \text{Leg } 2_i + \dots + h_{I-1} \text{Leg I-1}_{i-1} + u_{ib}
\end{aligned} \tag{3.1}$$

where  $Y_{ib}$  indicates cosponsorship by legislator  $i$  on bill  $b$ ; the three indicator variables  $d_{ib}^{i-i}$  indicate ego and alter treatment assignment for legislator  $i$  and bill  $b$ , and  $u_{ib}$  represents unmeasured determinants of cosponsorship. Bill and legislator-specific indicator variables account for varying baseline levels of cosponsorship.  $d_{ib}^{10}$  represents legislators assigned only to briefings;  $d_{ib}^{01}$  legislators assigned only to cue-taking; and  $d_{ib}^{11}$  legislators assigned to the combined treatment. The key parameters of interest are  $b_1$ ,  $b_2$ , and  $b_3$ , the average intent-to-treat effects of briefings, cues, and combined treatments. Standard errors and p-values are obtained through randomization inference. 10,000 simulated treatment assignments yield the sampling distribution of treatment effects under the sharp null hypothesis.

Table 3.3 presents results. Estimated effects of briefings and cues are positive, substantial in magnitude, and unlikely to have occurred by chance. Estimated briefing effects are 4.5 percentage points ( $p < 0.05$  one-sided), and estimated cue-taking effects 3.5 percentage points ( $p < 0.05$  one-sided). These estimates are large relative to the 10% baseline rate of cosponsorship. Briefing effects allowing for inter-office contagion (4.5) are similar in magnitude to briefing effects in the previous chapter assuming strict non-interference (5.0 - 5.4).<sup>11</sup>

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<sup>11</sup>This chapter includes a larger subject population. For an apples-to-apples comparison, see Appendix B.2.

Table 3.3: Estimated briefing and cue-taking effects.

	Briefing $T_{S=0}$	Cue-taking $S_{T=0}$	Combined $T_{S=1} + S_{T=1}$
$\widehat{\text{ITT}}$	4.5	3.5	11.8
$(\widehat{\text{SE}})$	(1.9)	(1.6)	(2.9)
$N^{(a)}$	2,080	2,080	2,080

Standard errors and p-values obtained using randomization inference and 10,000 simulated assignments.

Observations assigned to advocate direct or secondary treatment (200) or multiple staffer secondary treatments (36) are not displayed.

How do briefings and cue-taking treatments relate to one another? The estimated contagion rate  $\alpha$  is 79%. This rate is toward the high end of the range of estimates from the observational literature. For the bills in this study, for every 10 cosponsorships encouraged through briefings, 8 additional cosponsorships followed through cue-taking.

The combined effects of briefings and cue-taking are 11.8 percentage points ( $p < 0.001$  one-sided). To determine whether combined effects are significantly larger than the sum of standalone briefing and cue-taking effects, 10,000 simulations were conducted under the hypothesis that the combined effect equaled the sum of the standalone effects.<sup>12</sup> Only 4.9% of simulations yielded estimates larger than 11.8 percentage points.

These results are instructive in several respects. First, information is highly influential whether it reaches legislators directly or second-hand. Estimated effects

<sup>12</sup>The full schedule of potential outcomes was created under the assumption  $T_{S=0} = 4.5\text{pp}$  and  $S_{T=0} = 3.5\text{pp}$ .

are large in magnitude compared to baseline cosponsorship rates, suggesting that information constraints are binding on many decisions. Second, position-taking is highly contagious. Cosponsorship contagion rates within legislative offices are even larger than voting contagion rates within households (Nickerson 2008; Foos and de Rooij 2017). Third, briefings and cues are complementary. Legislators are responsive to repeated information treatments.

### **Extensions**

In his canonical study of cue-taking, Kingdon (1989) describes legislators waiting until the last minute, often as the roll is called, to ask peers for guidance. This implies a purposive view of cue-taking. Legislators consciously seek out instruction from one another to overcome their informational constraints. It stands in contrast to a more incidental form of cue-taking in which information is shared through casual interactions. If legislators engage in purposive cue-taking, they will seek information only when they are required to evaluate legislation. For most legislators, this would mean a bill must reach the floor before they focus on it. As a result, secondary effects will occur only for bills that reach the floor.

Bills reached the floor after treatments were delivered, so bill progress is a post-treatment covariate. If treatments affected which bills reached the floor, estimates of heterogeneous briefing and cue effects on cosponsorship by bill progress would be biased. However, it is unlikely that these particular treatments affected bill progress. Treatment was assigned at the individual, not bill, level, so all bills were assigned to treatment for some legislators. Further, most observations were assigned to the

control condition. Three-fourths of observations in Study 1 and over four-fifths in Study 2 were assigned to the control group. So despite large individual effects of treatment on cosponsorship, aggregate effects are modest.

Table 3.4 presents results from Equation 3.1 fit separately for the 17 bills that failed in committee and the 15 bills that reached the floor. Regression yields conditional average treatment effects (CATEs) based on bill progress. CATEs are estimated treatment effects among subsets of the population; they are not causal estimates of bill progress on cosponsorship.<sup>13</sup>

Briefings generated large positive direct and combined effects regardless of whether bills reached a vote. In fact, estimated briefing effects are larger for bills that failed in committee (5.2 percentage points;  $p < 0.05$ ) than bills that reached the floor (3.4 percentage points). This pattern is consistent with floor debate diminishing the informational disadvantage of untreated legislators relative to treated legislators, although the difference falls short of conventional levels of statistical significance.

Cue-taking appears influential only for bills that reached the floor. Estimated cue-taking effects for bills that failed in committee are 0.3 percentage points compared to 8.6 percentage points for bills that reached the floor. Legislators appear to engage in cue-taking only when bills reach a vote.

The focus of this paper is cue-taking within offices. Indeed, the second experiment assigned briefings in order to maximize power for the estimation of within-office cue-taking. However, legislators may engage in cue-taking in other ways. Legislators

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<sup>13</sup>For a similar reason, one would not cluster standard errors by bill. CATEs compare cosponsorship between observations that were and were not assigned to treatment for two disjoint subsets of the sample. They do not estimate the effect of the bill-level covariate on outcomes.

Table 3.4: **Estimated briefing and cue-taking effects by bill progress.**

	Briefing $T_{S=0}$	Cue-taking $S_{T=0}$	Combined $T_{S=1} + S_{T=1}$
<b>Bills that Failed in Committee</b>			
$\widehat{\text{ITT}}$	5.2	0.3	9.5
$(\widehat{\text{SE}})$	(2.2)	(1.9)	(3.6)
N	1,114	1,114	1,114
<b>Bills that Reached the Floor</b>			
$\widehat{\text{ITT}}$	3.4	8.6	13.9
$(\widehat{\text{SE}})$	(3.3)	(2.9)	(5.0)
N	966	966	966

Standard errors and p-values obtained using randomization inference and 10,000 simulated assignments.

Observations assigned to advocate direct or secondary treatment (200) or multiple staffer secondary treatments (36) are not displayed.

may share information with their deskmates on the chamber floor (Masket 2008), peers from similar districts, and ideologically-similar peers (Coppock 2014). Perhaps one of these relationships is more relevant to cue-taking than legislators' offices.

Table 3.5 displays estimated briefing and cue-taking effects for each of these alternative spillover models.<sup>14</sup> Cue-taking is estimated separately for each type of relationship using Equation 3.1. Regressions use the same subset of data in which each subject was eligible for spillover from an officemate, deskmate, neighboring representative, or ideological peer.<sup>15</sup> This leaves 1,760 observations, compared to the

<sup>14</sup>Appendix B.1 describes the spillover networks for each alternative cue-taking relationship.

<sup>15</sup>Not all legislators shared desks with another experimental subject. As a result, each contagion model could use different subsets of the data.

2,080 observations in the main analysis of office contagion.

**Table 3.5: Estimated briefing and cue-taking effects under alternate spillover models.**

	Offices	Desks	Districts	Ideology
Briefing ( $\widehat{T}_{S=0}$ )	4.7	2.9	5.8	4.0
(SE)	(2.0)	(4.8)	(5.0)	(4.9)
(N)	(210)	(218)	(219)	(228)
Cue-taking ( $\widehat{S}_{T=0}$ )	3.7	2.3	3.6	2.3
	(1.8)	(4.8)	(5.1)	(4.9)
	(328)	(230)	(220)	(252)
Combined ( $\widehat{T}_{S=1} + \widehat{S}_{T=1}$ )	10.3	16.7	8.4	14.3
	(3.1)	(6.5)	(6.8)	(6.5)
	(80)	(73)	(68)	(57)

Standard errors and p-values obtained using randomization inference and 1,000 simulated assignments.

Interest group treatment and multiple indirect treatment conditions omitted.

Weights equal to inverse of probability of treatment assignment.

Covariates include bill and legislator dummy variables.

Estimated direct effects are positive in each model, ranging from 2.9 to 5.8 percentage points. We would not expect changing the contagion model to significantly affect estimates of direct treatment. Estimated secondary effects are also positive in each model, but estimated effects are largest in the office model (3.7 percentage points), followed by district proximity (3.6), ideology (2.3) and desk (2.3) models. Effects are estimated imprecisely, so we cannot differentiate estimates from the alternative models. Estimated standard errors for secondary treatment under the office model (1.8 percentage points) are significantly smaller than standard errors under the alternative models (4.8 to 5.1 percentage points) due to the restricted randomization.

The restricted randomization was employed with respect to intraoffice spillover, not spillover in the alternative models. The restriction prevented observations from being assigned to a mix of advocate and staffer treatments or, in the second study, multiple staffer spillover treatments. This serves two purposes. The staffer by advocate interaction conditions are excluded from the main analysis, so the more units that are assigned to them, the fewer units that are available for estimating the treatment effects of interest. Treatment by treatment interaction conditions are also low probability events. Despite being unlikely for any given observation, the units that are assigned to these conditions receive extreme inverse probability weights, which harms precision of the estimates.

### **3.3 Discussion of cue-taking's effectiveness**

Many quantitative analyses and formal theories treat legislators as atomistic actors with immutable policy positions. The results in this study show that strong non-interference assumptions about legislative behavior, explicit in many experiments but also implicit in many observational studies, are unwarranted in at least some contexts.

The strength of this research design is the identification of causal effects, but it suffers from numerous external validity concerns. First, the intervention provided a specific type of information that may not be representative of legislators' day-to-day discussions. Technical policy information may differ from political intelligence such as polling or the preferences of other politicians. Further, briefings were conducted by a staffer for a bipartisan caucus, which may be more trusted and influential than



other information sources. The experiments included bills in an issue area with broad, bipartisan support.

Second, the contagion network in this legislature may not represent other networks. The network itself was not randomly assigned, so effects cannot be attributed solely to sharing an office. Observed effects may conflate office sharing with a different underlying causal pathway, such as legislators' friendships. Further, the degree of interoffice communication likely varies across offices and over time in ways that may affect the magnitude of cue-taking in other contexts.

Third, while this paper relaxed the non-interference assumption with respect to legislators, it still assumes strict non-interference across bills. It took this approach because of the prominence of studies of cue-taking between legislators, but the assumption that legislators evaluate each bill independently of other legislation on the agenda is supported by no empirical evidence. Non-interference across bills could be violated if briefings addressed similar proposals or if subjects interpreted bills being omitted from briefings as an indication of their quality.<sup>16</sup>

Fourth, cosponsorship is an important position-taking behavior, but its relative low cost may make it particularly well-suited to observing contagion. Roll call voting may be immune to interference because it is more directly consequential to policy outcomes or more closely monitored by party leaders. At the same time, the fact that party leaders actively whip roll call votes suggests it too is subject to interpersonal influence.

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<sup>16</sup>The experiments were designed to avoid such inferences by informing subjects that time constraints necessitated picking a subset of bills, that the briefings were an effort to spread information, and that briefings did not represent a valence judgment by the caucus.

Fifth, two studies were conducted, but in the same legislature. Although this legislature shares many characteristics with other state legislatures, it may not represent other legislative contexts. Legislatures with more staff support may exhibit less diffusion if legislators outsource information-gathering to aides. On the other hand, diffusion may be stronger if aides collaborate across offices.

Each of these external validity concerns is an empirical question and an opportunity for further research (Nickerson 2008). A key contribution of this paper is to illustrate that the large-scale experiments needed for studying contagion are possible in legislative studies. Further, incorporating a model of contagion into the experimental design itself improves precision and facilitates the estimation of secondary effects.

If contagion occurs in other legislative experiments, then legislators may be even more responsive to informational interventions than is currently thought. Contagion might cause particularly large attenuation in estimated effects in block-randomized designs, since blocks contain like-minded legislators who might share information (Butler and Nickerson 2011; Kalla and Broockman 2015). That said, contagion may attenuate or exaggerate treatment effects on a case-by-case basis.

This design does not identify the mechanism underlying cue-taking. Legislators may discuss the nuts and bolts of policy with one another or they may seek out a word or two of guidance as they are voting. Cue-taking may occur through public displays of bill support like cosponsorship such that a cue-giver is unaware that she is actually influencing others. Mediation analyses face daunting obstacles, but there is potentially much to be learned from creative designs that set out to measure or

manipulate mediators (Bullock, Green, and Ha 2010).

Finally, cue-taking raises questions about the effects of homophily on legislative outcomes. Legislative networks are characterized by like-minded individuals associating with one another. For example, no legislator in this study shared an office with a member of the opposing party. If contagion occurs across homophilic networks, improving information may exacerbate rather than ameliorate divisions. Reforms to reduce polarization should encourage information-sharing between dissimilar peers, not strengthen cue-taking between like-minded legislators.

# Chapter 4

## Deliberation

“Few in their right mind will argue that [Congress] suffers from too much deliberation, analysis, or thought.” - Senator Howell Heflin (via Loomis 2000, 9)

A defining feature of representative lawmaking, deliberation has drawn attention from statesmen and scholars for centuries. Deliberative processes are thought to legitimize the aims and methods of democratic government and improve its policy outputs (Bessette 1997; Habermas 1997; Gilligan and Krehbiel 1987). Despite its importance to democratic theory, there is little evidence that legislative deliberation actually affects policymaking. This study examines whether effective deliberation is possible in today’s legislatures. To what extent does deliberation change minds or shape legislators’ policy positions? This micro-level question speaks to broad debates about partisanship and effective lawmaking in contemporary legislatures.

Any study of deliberation as a consequential policymaking activity must confront

the widely-held belief that legislators have fixed policy positions (Smith 1989; Connor and Oppenheimer 1993; Bessette 1994; Landa and Meirowitz 2009). Legislators either pursue their own strongly-held personal preferences or represent the wishes of constituents. Either way, they have made up their minds before they even enter the legislature. In this case, open-minded deliberation does not occur. Legislators may use deliberative processes to appeal to voters, but deliberation is political theater with no immediate impact on policymaking.

For deliberation to change legislators' minds, policy positions must be susceptible to influence. Parliamentarians and legislators were among the first to note that legislators' policy positions often change, sometimes in response to diligent investigation and research, at other times in response to baseless rumors or "anonymous whispers" (Treadway 1938, 114; see also Chadwick and Gilbert 1887; Ilbert 1901; Luce 1924; Mason 1938). Formal theorists have developed several reasons why information broadly conceived may influence policy positions. Information clarifies the relationship between policy instruments and outcomes (Gilligan and Krehbiel 1987; Austen-Smith and Feddersen 2006; Meirowitz 2006). Deliberation may help legislators discover which of many conflicting decision making considerations is most important for a given bill (Maass 1983; Hafer and Landa 2007; Dickson, Hafer, and Landa 2008). Legislators' positions may be based on a biased view of the world in the first place, so information may correct or exacerbate these pre-existing biases (Lodge and Taber 2005, 2013). For any of these reasons, deliberation may change positions.

The literature can hold two such contradictory attitudes regarding the stability

of individual positions due to the lack of empirical evidence. Existing empirical work on deliberation only partly addresses its influence on individual or collective choice. Many case studies trace the formation of policy proposals through public discourse, but they do not claim discourse causally influences policy outcomes (Landy, Roberts, and Thomas 1990; Granstaff 1999; Derthick and Quirk 2001; Mucciaroni and Quirk 2006; Wirls 2007). Studies have measured the quantity or quality of speech, perhaps to show how deliberation varies with institutions, but they cannot identify the effect of particular institutions on discourse (Smith 1989; Sinclair 1989; Connor and Oppenheimer 1993; Steenbergen et al 2003; Bara, Weale, and Biquelet 2007; Bächtiger et al. 2008; Taylor 2012). These approaches address parts of the causal process — whether quality deliberation occurs and whether policy changes during deliberation — but they do not connect deliberation to policy outcomes using a causal identification strategy.

To estimate whether deliberation affects legislators' individual positions, I conducted a field experiment in a state legislature. Certain bills were randomly selected for supplemental deliberation among a bipartisan group of legislators. This study breaks ground in several ways: 1) by estimating the causal effect of deliberation on individual position-taking; 2) by assessing the impact of deliberation on bill-level outcomes; 3) by examining deliberation effects within and across party lines; and 4) by exploring the limits of deliberation.

## 4.1 Deliberation in partisan legislatures

Given the severe polarization of contemporary politics, it may not be possible for legislators to engage in open-minded deliberation. Partisan bickering and grandstanding have overwhelmed debate in committees and on the floor. A “partisan steamroller” (Sinclair 2014, 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 partisan policymaking in 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). This does not sound conducive to effective deliberation.

Effective deliberation is difficult between dissimilar legislators for several reasons. In principal-agent models of information-sharing, even if an agent has private information that will improve a policy, she may not share it with a principal who has divergent interests. It is not hard to imagine that a partisan legislator may refuse to share her expertise with members of the opposing party. Further, deliberation helps legislators decide which decision making consideration should guide their choice. Often it seems Republicans and Democrats talk past one another, with each refusing to acknowledge the considerations advanced by the other. Finally, the limited experimental research on legislators’ decision making finds they are subject to a range of cognitive biases (Sheffer et al 2017). Most troubling is that partisan lawmakers seem to reach different conclusions from the same factual information (Baekgaard et

al 2017). Bipartisan deliberation, or deliberation between any groups of dissimilar legislators, seems unlikely.

I argue that bipartisan deliberation can be effective if there is asymmetric information across party lines. The idea is based on canonical information signaling models. In these models, legislators do not take positions due to uncertainty about policy proposals (Gilligan and Krehbiel 1989). The more uncertain they are, the less likely they are to take a position. As a result, providing information, perhaps through deliberation, is particularly effective when information is most scarce. Although the models predict that truthful communication is easier between like-minded legislators, there may be more to communicate between dissimilar legislators.<sup>1</sup> In some situations, legislators' skepticism of certain peers will be outweighed by the novelty and import of the information. In a sense, the best hope for effective bipartisan deliberation is that legislators interact so little across party lines in the first place.

There are several reasons to think legislators are particularly uninformed about proposals from out-partisans. Policy information is often transmitted through partisan channels (Smith 2007). Party leaders monopolize information to control their caucuses (Curry 2015). Legislators take cues from like-minded peers who are more likely to be in-partisans (Kingdon 1989). In-partisans also likely represent more similar districts and have more similar personal ideologies than out-partisans. For all of these reasons, it is plausible that asymmetric information across parties causes a "partisan penalty" in bill support. Legislators' uncertainty about bills from out-

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<sup>1</sup>This idea of information asymmetries underlies weak-tie models of sociological networks (Granovetter 1973; see also Ringe, Victor and Carman 2013). See Appendix A.1 for a more formal discussion of the effect of uncertainty on position-taking.



partisans decreases their support independent of the bills' content.

This theory, derived from signaling models and ethnographic studies of information in legislatures, suggests the following primary hypothesis about the effect of deliberation on individual behavior:

***H*<sub>1</sub>: Deliberation will increase aggregate bill support among legislators including out-partisans.**

Deliberation will increase overall bill support if three conditions are met: 1) uncertainty constrains legislators' support for legislation; 2) information reduces uncertainty; and 3) informative communication is possible. This last condition may be more difficult to satisfy between dissimilar legislators. However, if information asymmetries are severe enough, deliberation may prove even more effective across parties than within them.<sup>2</sup>

Secondary hypotheses address the effects of deliberation on the disposition of legislation:

***H*<sub>2</sub>: Deliberation will increase bargaining over bill content.**

Legislators are reluctant to amend legislation if they are uncertain about its content. In fact, if information constraints are severe enough, legislative principals will restrict their ability to amend legislation to improve informational efficiency (Gilligan and Krehbiel 1989). Since deliberation improves information, it should make legislators other than the sponsor more likely to amend proposals. I examine both the number of amendments filed and the number successfully attached to legislation.

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<sup>2</sup>Deliberation could also correct legislators' prior expectations about the bills' content. In this case, deliberation would likely increase support among some legislators and decrease it among others. Results from each experiment are less consistent with this mechanism than with uncertainty reduction.

**$H_3$ : Deliberation will increase the probability of bill passage.**

Deliberation can help bring bills to the floor if large cosponsorship coalitions signal to agenda setters that a bill is broadly popular and merits plenary time (Kessler and Krehbiel 1996). Cosponsorship can also signal to agenda setters that a bill will not roll the majority party (Cox and McCubbins 1993). Deliberation may also convince skeptical legislators to vote for the proposal.

## **4.2 An experiment on deliberation**

An experimental approach identifies the causal effect of bipartisan deliberation on legislators' policy positions. The design is similar in spirit to deliberative polls conducted among voters (Luskin, Fishkin, and Jowell 2002; Farrar et al. 2009). There is a notable difference in that legislative deliberation is potentially more consequential to policy outcomes.

### **Subjects**

The intervention was 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 Glassman 2014; Ringe, Victor and Carman 2013). Lacking formal gatekeeping or proposal powers, they are thought to affect policymaking as clearinghouses of information. 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 1989).

The BFC is unique because its members are all first-term legislators.<sup>3</sup> First-term legislators are thought to be more partisan than their more senior colleagues because they lack relationships with peers across the aisle (Francis 1962; Price and Bell 1970; Caldeira, Clark, and Patterson 1993; Sarbaugh-Thomas et al 2006). To the extent novice legislators are particularly uninformed about policy or one another, they may be particularly responsive to deliberation.

### **Recruitment and assignment procedure**

A caucus staffer conferred with BFC members (or with their staff) to select at least two bills per member that the member would be willing to present to the caucus. The 16 members of the BFC proposed 45 bills in total, ranging from two to five per member. One bill was selected at random by the caucus staff for each member to present at a BFC meeting. Because each member was allowed to present only once, the probability of treatment assignment varies across members. Inverse probability weights are used in all analyses below for this reason.

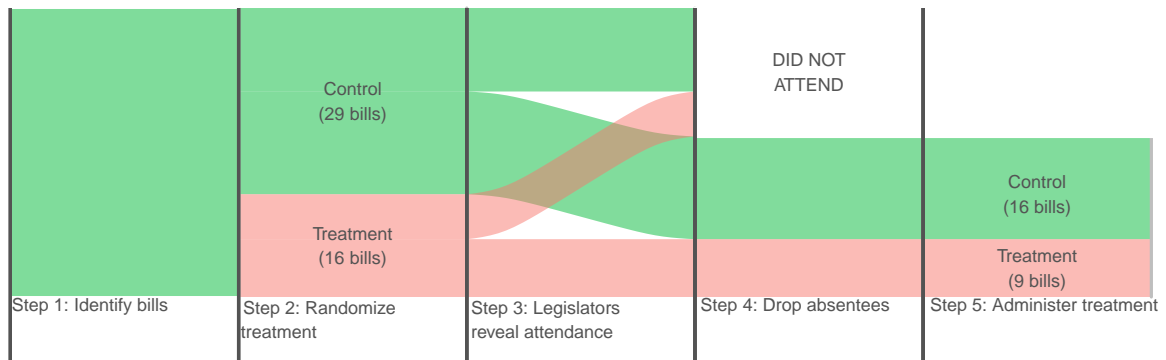
Legislatures are busy places, and legislators do not always attend meetings. Caucus meetings are no exception. Only nine BFC members, responsible for 25 bills, attended caucus meetings. Bills sponsored by absentee members are dropped from the analysis. Bills can be dropped without introducing bias because legislators were

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<sup>3</sup>Many legislatures feature first-term caucuses, as new members face unique challenges adjusting to the legislature. 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.

informed of treatment assignment only after they revealed their attendance (see below for an extended discussion on this topic). Figure 4.1 displays the procedure for selecting bills, assigning them to treatment conditions, dropping bills, and administering treatment.

*Figure 4.1: Deliberation experiment procedure*



## Treatment

Treatment is the opportunity to present a bill at a caucus meeting. This design gave sponsors the opportunity to discuss their bills but did not dictate the content of the discussion. 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 particular persuasive or informational message. Nevertheless, presentations generally contained both technical policy information and persuasive appeals.

Sponsors often began with a detailed discussion of technical, substantive policy information. One bill allowed public buses to drive on highway shoulders to ease traffic congestion. The sponsor described which highways would be eligible for the program and the program's flawless safety record in another state. Legislators usually took questions about how bills related to current law. Members asked whether a bill to fine car drivers for blocking bike lanes could be addressed under existing statutes. Members discussed what proposals would cost if enacted.

Legislators also made more persuasive appeals. 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. Legislators flaunted support from important interest groups or executive branch officials. The public transit bill was supported by the state Department of Transportation, while a bill to mandate the use of child safety seats was endorsed by numerous children's hospitals. Overall, sponsors focused on the practical effects of their legislation or their sincere reasons for sponsoring it. They largely avoided discussing ideological or partisan considerations, and none explicitly invoked electoral motivations.

### **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 2000; Woon 2008). Harbridge (2015) argues that cosponsorship is particularly relevant to studies of bipartisanship. Bipartisanship occurs early in

the process, in deciding which bills reach the legislative agenda. For this reason, bill sponsors build diverse coalitions of cosponsors to signal to agenda setters the breadth of support for their proposals (Kessler and Krehbiel 1996). Bipartisanship will go unnoticed if scholars focus on roll call voting alone.

Whereas cosponsorship is voluntary and holds no formal role in the policymaking process, roll call voting directly determines whether bills become law. 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, the large observed rates of 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>4</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 whether bills are amended and whether they 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 we would want to examine separately if not limited by the data.

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<sup>4</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.

## Compliance

The intervention faced unique and challenging forms of noncompliance. Most challenging was that some legislators did not attend caucus meetings. Absenteeism could affect the experiment in several ways. The absence of a single legislator not only makes it impossible for that legislator to receive treatment, but it also prevents that legislator from administering treatment for the bill she sponsored. Absenteeism also affects the definition of the subject population and the treatment itself. We would like to estimate the impact of deliberation on legislators who attended the meetings and on those who did not. The two groups receive very different treatments, but there are reasons to expect deliberation might affect both. If attendance was revealed post-treatment, we could not estimate heterogeneous effects by attendance.

The solution to this problem is surprisingly simple: legislators were informed which bills had been selected for treatment only after meetings began. Legislators were called upon, in random order, and asked to present on the (randomly selected) bill. By construction, then, bill sponsors' attendance could not be affected by treatment assignment. As a result, the 20 bills proposed by the 7 absentee members can be dropped from the study, and the estimands redefined with respect to the remaining 25 bills sponsored by 9 attendees.<sup>5</sup> Withholding notification about treatment assignment also converts attendance from a post-treatment to a pre-treatment covariate. Treatment effects can be estimated separately for legislators who did and

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<sup>5</sup>This design resembles a matched rollout protocol (Nickerson 2005). As long as treatment was not attempted for any units in a block, in this case for any bills by a given sponsor, the block can be dropped and the estimand redefined with respect to the remaining blocks. Rollout protocols preserve large application rates, the percentage of units assigned to treatment that receive it, and increase power compared to designs that retain blocks for which treatment was not attempted.

did not attend the meetings.<sup>6</sup>

Dropping experimental bills diminishes power. The loss of 20 of the 45 bills is particularly damaging for the estimation of the bill-level outcomes. The study remains well-powered for estimating individual-level outcomes.

The risk of informing legislators of treatment assignment at the last minute is that sponsors may not speak about the selected bill. This is the second form of non-compliance. Table 4.1 displays treatment delivery by treatment assignment among the final set of experimental bills. Seven of nine bills assigned to treatment were discussed during the caucus meetings. Only one of sixteen bills assigned to control was discussed.

*Table 4.1: Compliance with deliberation assignment.*

		Treatment Delivered?	
		No	Yes
Treatment Assigned?	No	15	1
	Yes	2	7

Bills receiving treatment that were not included in the study are omitted from this table.

The effect of assigning a bill to treatment on the probability that treatment was delivered, the ITTd, is estimated by regressing treatment delivery on treatment assignment at the bill level (Gerber and Green 2012).  $\widehat{\text{ITTd}} = 0.71$  ( $\widehat{\text{SE}} = 0.16$ ). Attendees largely complied with treatment assignment. Because  $\widehat{\text{ITTd}}$  is relatively

<sup>6</sup>Contagion of information is possible, and even expected, between attendees and absentees, which is why the latter group is relevant.



large, all analyses below report intent-to-treat effects on the outcomes of interest.<sup>7</sup> To avoid the syntactic complications caused by noncompliance, the rest of the chapter will refer to “treated bills” wherever the technically correct but more cumbersome language of “bills assigned to treatment” is appropriate, and it will refer to “bills assigned to control” as “untreated” or “control” bills.

## Balance

Table 4.2 presents tests of covariate balance for the 25 experimental bills. Balance is evaluated across the following bill-level covariates: fiscal cost; whether a fiscal review was conducted of the bill; pre-treatment cosponsorship; and whether the bill’s senate sponsor was a member of the same party as the house sponsor.<sup>8</sup> Standard errors and p-values are obtained through randomization inference.

Bills assigned to treatment have a higher fiscal cost, are more likely to have been granted a fiscal score, and are less likely to have house and senate sponsors from opposing parties. However, none of these differences reaches conventional levels of statistical significance. An omnibus test examining whether the covariates jointly predict treatment assignment generates an F-statistic of 124, which is still smaller than 19% of statistics from simulated random assignments.

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<sup>7</sup>Under the standard assumptions, estimated complier average causal effects are 42% larger than the intent-to-treat effects reported ( $\widehat{CACE} = \frac{1}{\widehat{ITTd}}$ ).

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

Table 4.2: Balance of deliberation assignment.

	Treatment Assignment		Difference
	Control	Treatment	
Fiscal Cost (\$ in mm) (SE)	0.2 (4.7)	32.9 (15.5)	32.6 (19.9)
Fiscal Review	0.437 (0.086)	0.720 (0.160)	0.283 (0.238)
Pre-treatment cosponsorship	0.035 (0.006)	0.027 (0.007)	-0.008 (0.012)
Bipartisan sponsor	0.187 (0.027)	0.080 (0.080)	-0.107 (0.107)
F-statistic		124	
N	16	9	

Significance indicated at  $p < 0.10$  (\*) and  $p < 0.05$  (\*\*) two-sided. Standard errors and p-values obtained from randomization inference with 10,000 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 of the broader policy agenda. Legislators selected bills that they thought were appropriate for bipartisan discussion. They 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, and effective policymaking organizations.

## Results

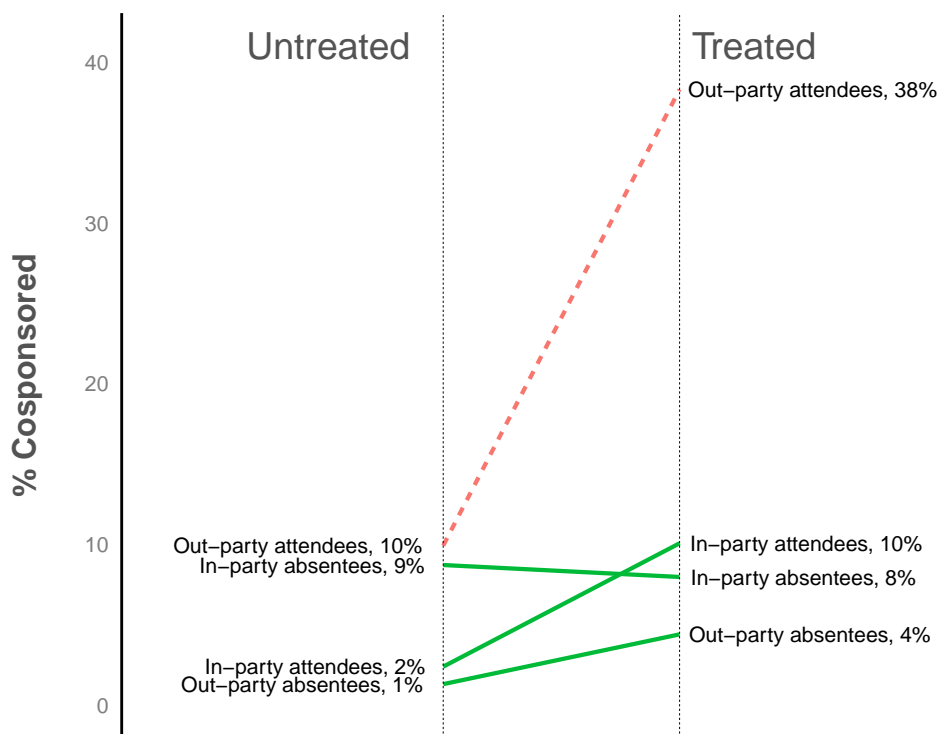
Figure 4.2 and Figure 4.3 display legislators' cosponsorship and roll call voting support for treated and control bills. The weighted average cosponsorship (roll call voting) rate is displayed for in- and out-partisans, broken down by whether the individual attended the caucus meeting. As is commonly the case, baseline cosponsorship rates are low. Fewer than 10% of legislators cosponsored any given untreated bill. Treated bills received more support than untreated bills, particularly among legislators who attended the meetings. Out-party attendees were 28 percentage points more likely to cosponsor treated bills than untreated bills, compared to an 8 percentage point difference for in-party attendees. Absentees were not substantially more likely to cosponsor treated bills.<sup>9</sup>

Figure 4.3 displays estimated treatment effects on roll call voting. The dependent variable is the weighted percentage of legislators who voted "Yes" on treated and

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<sup>9</sup>See Appendix C.2 for estimated ITTs by group. Estimated ITTs for out-party attendees are substantively large and unlikely to result from sampling variability ( $p < 0.10$  one-sided with standard errors clustered by bill). However, estimated effects for this group are not statistically distinguishable from effects for the other groups.

Figure 4.2: Cosponsorship by deliberation assignment.

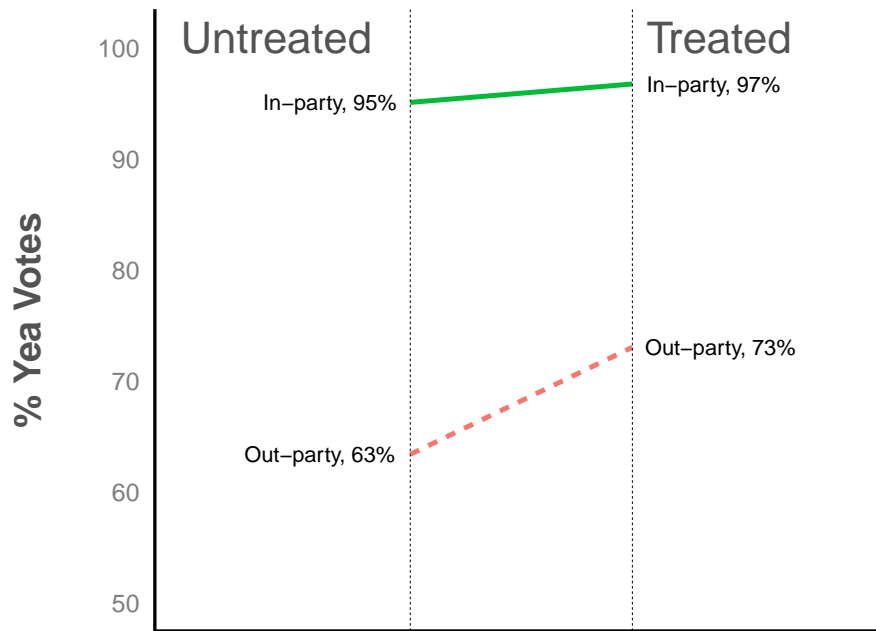


untreated legislation.<sup>10</sup> In-partisans voted in favor of over 95% of both untreated and treated bills. Out-partisans voted for 63% of untreated bills and 73% of treated bills.

Estimated treatment effects and accompanying standard errors are obtained using weighted least squares regression. A model that compares cosponsorship (or voting) on treated bills to untreated bills, conditional on the estimated ideology of the subjects, is the following:

<sup>10</sup>Attendees and absentees are lumped in together, as fewer bills reached a vote.

Figure 4.3: Roll call voting by deliberation assignment.



$$Y_{ij} = a + b_1 d_j + b_2 \text{Ideology}_{ij} + u_{ij} \quad (4.1)$$

where  $Y_{ij}$  indicates cosponsorship (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 clustered by bill.<sup>11</sup>

<sup>11</sup>While there are few clusters, twenty-five for the cosponsorship analysis and eight for the roll call voting analysis, intra-cluster correlations are relatively low, avoiding some of the pathologies of cluster-robust variance estimates (Bertrand, Duflo, and Mullainathan 2004). Estimated standard errors and p-values are verified by randomization inference.

In order to improve the precision with which the parameter  $b_1$  is estimated, Equation 4.1 controls for a pre-treatment covariate that predicts the dependent variable. Cosponsorship and roll call voting are strongly predicted by legislators' ideological predispositions (Poole 2005; Peress 2013). The variable  $\text{Ideology}_{ij}$  measures the ideological distance between the subject  $i$  and the sponsor of bill  $j$ . Ideology is estimated by applying the DW-NOMINATE scaling algorithm to prior session roll call votes.<sup>12</sup>

Estimated ITTs for in-partisans are small. Although only 9.6% of in-partisans cosponsored the average untreated bill, deliberation increased cosponsorship by only 0.03 percentage points. The positive effect among in-party attendees is balanced out by the negative effect among in-party absentees. Deliberation increased supportive roll call voting by only 2 percentage points, although the high baseline rate of voting support provides a ceiling for deliberation's effects on this subgroup.

Estimated effects for out-partisans are much larger in magnitude. The  $\widehat{\text{ITT}}$  of treatment on cosponsorship is 4.4 percentage points ( $p < 0.05$  one-sided). This estimate more than doubles the 3.9% baseline cosponsorship rate among out-partisans and brings out-partisans' cosponsorship nearly in line with in-partisans'.  $\widehat{\text{ITT}}$  on voting is 35.2 percentage points ( $p < 0.01$  one-sided). This covariate-adjusted estimate is much larger than the 10 percentage point unadjusted difference-in-means estimate displayed in Figure 4.3, but it falls in line with previous experimental studies of roll call voting (Bergan 2009; Butler and Nickerson 2011).<sup>13</sup>

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<sup>12</sup>Three of the 99 legislators did not serve in the prior session. Their observations are assigned an ideological distance of 999, and a second predictive covariate, "Invalid Ideological Distance" is assigned a value of 1 for these legislators and 0 for all others. Table C.2 in Appendix C.2 shows that ideological distance is strongly predictive of cosponsorship and roll call voting in this sample.

<sup>13</sup>Bergan (2009) estimates that legislators assigned to receive emails from a grassroots lobbying campaign in favor of a smoke-free workplace bill were 8 to 14 percentage points more likely to vote

Table 4.3: Estimated deliberation effects.

	Cosponsorship		Roll Call Voting	
	In-partisans	Out-partisans	In-partisans	Out-partisans
$\bar{Y}_{\text{Control}}$ (%)	9.6	3.9	97.8	67.7
$\widehat{\text{ITT}}$ (pp)	0.0	4.4**	2.0	35.2**
$(\widehat{\text{SE}})$	(5.0)	(2.5)	(4.5)	(12.6)
N	1,543	932	411	316
Clusters	25	25	8	8
ESS	165	189	83	44

Weights equal to inverse of bill's probability of assignment to realized condition.

Significance indicated at  $p < 0.10$  (\*) and  $p < 0.05$  (\*\*) one-sided. SEs and p-values are clustered at bill-level

Deliberation effected large gains in support from out-partisans, but only marginal increases in support among in-partisans. As a result, the intervention substantially reduced polarization in policy coalitions (see Appendix C.1).

### Treatment effect heterogeneity

Why did deliberation increase bill support so much among out-partisans? Did sponsors talk down the ideological components of legislation and amplify the valence components? Or did deliberation reduce informational asymmetries across the parties? Examining heterogeneous treatment effects by legislators' ideology speaks to

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in favor of the bill than untreated legislators. Butler and Nickerson (2011) estimate some legislators were up to 50 percentage points more likely to vote against a tax cut when informed that their constituents opposed it.

these questions.

Figures 4.4 and 4.5 display cosponsorship and voting by legislators' ideological distance to bill sponsors.<sup>14</sup> The dashed, green lines reflect subjects who are in the same party as bill sponsors; the solid, red lines subjects in opposing parties. The left panels indicate observations assigned to control, the right panels treatment. Although ideological distance is, on average, larger between out-partisans, some out-partisans are more ideologically-similar than some in-partisans.

Among out-partisans, bill support declines as distance increases. The relationship between position-taking and ideology is modest for cosponsorship, but stronger for roll call voting. The negative slopes indicate that ideology predicts position-taking. The relationship is much weaker among in-partisans.

Beyond the negative slopes, the most striking feature from the figures is the large intercept shifts between in-partisans and out-partisans. Among untreated observations for both cosponsorship and roll call voting, in-partisans are more likely than out-partisans to support bills. This is true at any given ideological distance. Legislators apply a "partisan penalty" in position-taking. Asymmetric information is one of several possible explanations for this gap.

Deliberation reduced the partisan penalty by increasing average support among out-partisans. There is little evidence that deliberation effects varied by ideology.<sup>15</sup> These results are more consistent with deliberation reducing informational asymme-

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<sup>14</sup>Although the ideal metric would measure the distance between legislators and bill proposals, precisely estimating the ideological content of specific proposals is difficult (cf. Peress 2013).

<sup>15</sup>Appendix C.2 presents a regression analysis of heterogeneous effects by ideological distance. There is limited evidence that deliberation increased the relationship between ideology and cosponsorship.



Figure 4.4: Cosponsorship by deliberation assignment and ideology.

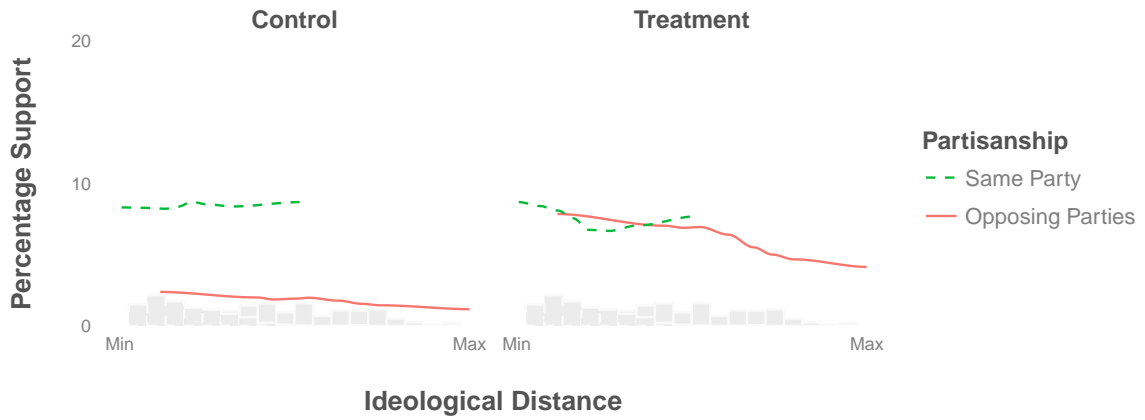
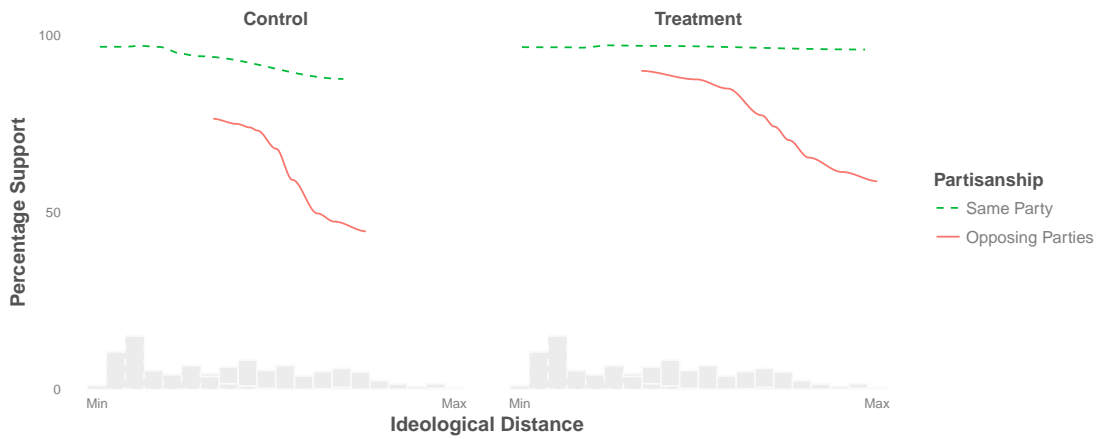


Figure 4.5: Roll call voting by deliberation assignment and ideology.



tries between parties than with persuading legislators to use non-ideological decision making considerations.

## Policy consequences of deliberation

What were the consequences of deliberation on the disposition of proposals? Table 4.4 presents estimated treatment effects on bill amendment and passage.  $\widehat{ITT}$ s and  $\widehat{SE}$ s are obtained through weighted least squares regression and verified through randomization inference. Table 4.4 also displays the weighted average amendment and passage rates among control bills.

*Table 4.4: Estimated deliberation effects on bill-level outcomes.*

	<b>Amendments Filed</b>	<b>Amendments Attached</b>	<b>Passed</b>
$\bar{Y}_{\text{Control}}$	0.19	0.19	0.29
$\widehat{ITT}$ ( $\widehat{SE}$ )	-0.03 (0.18)	-0.11 (0.17)	-0.05 (0.18)
N	25	25	25

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

Weights equal to inverse of bill's probability of assignment to realized condition.

There is little evidence that treatment affects bill amendment or passage. In fact, ITT estimates for both outcomes are in the opposite direction of predictions. Deliberation reduced the number of amendments filed per bill from 0.19 to 0.16 and the number of amendments adopted from 0.19 to 0.08.<sup>16</sup> Deliberation reduced the probability of bill passage by 5 percentage points. Low power and large standard errors limit the usefulness of these estimates. Nevertheless, they do not suggest that treatment significantly affected whether bills received a roll call vote.

<sup>16</sup>Three of the four bills received 1 filed amendment; the fourth received 2. Results do not change substantially if we define amendment as whether a bill received any amendment.

### 4.3 The limits of deliberation

Deliberation fails if it does not lead policymakers toward more-informed positions (Landy, Roberts, and Thomas 1990; Lascher Jr. 1996; Mucciaroni and Quirk 2006). Because it is difficult to obtain legislators' positions under different information environments, previous studies indirectly infer deliberative failures. For example, previous works have judged the 1981 Reagan tax cuts as a failure of deliberation because they did not pay for themselves, nor did the 1992 deregulation of the cable industry lead to lower cable fees for consumers as promised. The large investment in the Yucca mountain national nuclear waste repository must be a deliberative failure as it may never be used (Jacob 1990; Quirk 2005). The experimental approach in this paper compares legislators' positions with and without deliberation. It provides clear causal evidence that standard deliberative processes fail, at least some of the time.

Did the supplementary caucus deliberation guide legislators to their fully-informed positions? A case study of one bill suggests that it did not. An unexpected pressure campaign from unhappy constituents made it clear that legislators were unaware of an important political calculation. For this bill, the failure of deliberation incurred costs in terms of legislators' popularity and lost plenary time.

A first-term Democratic legislator in the minority 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 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 and imposition of potentially costly requirements. 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. Caucus deliberation had failed to raise the important consideration that Tea Party activists might oppose a bill that created new regulations on parenting. It also failed the legislature as a whole by squandering scarce plenary time.

What can we learn from this case study? Unsurprisingly, caucus deliberation does not cure all of the pathologies of ineffective committee and floor debate. The case also shows that bill sponsors may have trouble anticipating relevant political considerations for out-partisans. Nevertheless, deliberation was, initially, effective across parties. Republicans agreed with the sponsor's position. Only after it became apparent they had ignored the interests of a vocal constituency did Republicans change their positions. They also punished the sponsor. They withdrew cosponsorships of his other bills; re-referred them to committee; and challenged any legislation he attempted to pass on the consent calendar. In the repeated game of legislating, communication is possible between dissimilar legislators, but mistakes or misleading communication is punished.

## 4.4 Discussion of deliberation's effectiveness

This study makes several contributions to the field of legislative deliberation. First, legislators do change their minds as a result of talking with peers. Second, informative communication can occur between dissimilar legislators. Third, a legislative caucus can foster information-sharing despite lacking formal parliamentary powers such as gatekeeping power. Finally, legislators' bipartisan efforts are limited by pressure from vocal partisan extremists.

Deliberation is still possible in today's highly partisan legislatures, but we may have been looking in the wrong places. Committees and floor debates, historically the focus of scholars' attention, have become arenas for partisan bickering and point-scoring. Bipartisan caucuses, on the other hand, typically operate without much public attention or oversight from party leaders. The freedom to discuss policy outside the formal policymaking process may be central to caucuses' effectiveness.

This study provides more evidence that legislators' positions are contextual. Position-taking is not only a function of the ideology that legislators bring into the legislature. Activities within the legislature also shape legislators' positions. While activities inside the legislature have been overlooked too often in legislative studies (Lee 2009), this paper suggests one approach that is well-suited to studying them: conducting randomized control trials, under the guidance of legislative leaders, to evaluate the effectiveness of legislative institutions and behaviors.

## Chapter 5

# On Legislative Evaluation

“You can grumble all you want about those idiots in the Congress. But if you’re not helping to educate the idiots, you’re not doing your job.”

— Dr. Vernon Ehlers, physicist, professor, and U.S. Representative (1993 – 2011) (via Slotnik 2017).

Contemporary American legislatures are broken. In addition to gridlock and polarization, legislators face a steady deterioration of the institutions, behaviors, and norms that support informed lawmaking. Legislatures have fewer policy staff, less committee expertise, and more leader-driven policymaking than they did fifty years ago.<sup>1</sup> As a result, American legislatures today resemble less the textbook Congress of

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<sup>1</sup>The decline of the seniority system has reduced incentives for legislators to invest in specialized expertise and has centralized policymaking in the majority party leadership (Sinclair 1989). Members of Congress have shifted many of their staff members to district offices, and what staff remains in Washington deals more with communications than policy (Sunlight Foundation 2010; Lee 2016). Members of Congress spend less time in Washington than they used to, and in some states legislators are barred from long-term service through term limits (Rosenthal 1998).

the post-WWII era than American legislatures in the Progressive and New Deal eras. Unfortunately, these earlier legislatures were widely viewed as incompetent (Rocca 1921).<sup>2</sup> Contemporary legislatures receive similarly dismal reviews. Legislators vote for bills that contain substantial “errors and ambiguities” (Tankersley and Rappeport 2018), and, in some cases, they confuse which bill they are voting on in the first place (Seipel 2018). Legislators themselves complain that the policymaking process is broken (Tester 2017; McCain 2017).

What should legislative scholars do about the current state of policymaking? In the past, when legislatures struggled, political scientists advocated for reforms. From the Progressive Era to the 1970s, scholars recommended changing legislative procedures and institutions. They advocated for more staff, better salaries, stronger committees, and new legislative research bureaus. The goal was to ensure legislation was based on an expert understanding of public policy. Together, these reforms laid the foundation for evidence-based policymaking.<sup>3</sup> These reforms were, by and large, successful.<sup>4</sup> But the reforms, and the legislative competence they effected, did not last.

It is tempting to blame partisans for the decline in legislative competence over the past few decades, but scholars also bear responsibility. Political scientists advocated for a more rigorous and empirical policymaking process, but we did not conduct

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<sup>2</sup>Lawmakers and parliamentarians themselves noted that public policy was riddled with errors, reflected a dearth of policy expertise, and was often made in a rush at the end of legislative sessions (Ilbert 1901; O’Neal 1914; Luce 1924; Mason 1938; Massachusetts (State) 1943; New York (State) 1946; Galloway 1951).

<sup>3</sup>This phrase was popularized in the 1990s but the concept is much older.

<sup>4</sup>In 1980, Alan Rosenthal wrote that “No longer a relic of the past, the legislature has built up capacity and become heavily involved in the governance of the state” (Rosenthal 1981, 340).

rigorous and empirical evaluations of the reforms we recommended. This was a great opportunity missed. As a result, there is little clear, causal evidence linking many legislative processes to outcomes that matter to legislators and voters.

I believe that scholars should work with stakeholders to evaluate legislative procedures. Evaluation differs from the vast existing empirical literature on legislatures in an important way. Causal identification is paramount. Cross-sectional and time series studies that lack clear identification strategies require strong assumptions to determine the impact of legislative features. Instead, evaluation focuses on cases where legislative operations change, in particular when they change independently of other confounding factors. Evaluation recommends experimentation.

This research program requires a proactive engagement with legislative affairs. The researcher is not in a position to change legislative operations herself<sup>5</sup> (although she may suggest topics to be studied), but she will be working alongside legislators and staff who are changing the way the legislature operates. The researcher's role is to design evaluation programs so any reforms can be studied. This participatory role may make some legislative scholars uncomfortable. However, a look at the history of our discipline shows that scholars have often intervened to shape legislatures.

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<sup>5</sup>Unlike studies of representation in which the researcher can randomly provide information about constituents to legislators, such as voters' policy preferences or appeals for government services, the researcher of policymaking processes cannot, without legislators' support, change institutions or behaviors inside the legislature.



## 5.1 Political scientists have shaped modern legislatures

The development of modern legislatures has been marked at each stage by an activist and influential community of legislative scholars. The Constitution laid out the powers of Congress but made no explicit provision<sup>6</sup> to ensure they were wielded effectively. The founders thought the informational challenges of lawmaking would subside as the new federal government matured.<sup>7</sup> Across the Atlantic, Jeremy Bentham also thought that concerns about legislators' "intellectual aptitude [and] active talent" (Bentham 1817, 254) would resolve themselves naturally. Aptitude would be revealed through parliamentary debates, and the public, in its wisdom, would recognize and vote out incompetent lawmakers. Other than elections, no legislative institution was adopted in the Constitution to ensure informed policymaking.

The growing complexity of government during the Second Industrial Revolution<sup>8</sup> belied optimism about legislative competence. John Stuart Mill characterized members of Parliament as "inexperience sitting in judgment on experience, ignorance on knowledge" (Mill 1865, 93). Lord Bryce described the "keen, though limited, in-

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<sup>6</sup>Deliberations in the Constitutional Convention suggest age requirements for legislators seemed largely to follow from the British custom. In Federalist 62, Madison does discuss a limited concern for competence, arguing that the Senate's deliberative nature required a "greater extent of information and stability of character" (Madison 1788) among its members than those in the House.

<sup>7</sup>"No man can be a competent legislator who does not add to an upright intention and a sound judgment a certain degree of knowledge of the subjects on which he is to legislate.... It is true that all these difficulties will, by degrees, be very much diminished. The most laborious task will be the proper inauguration of the government and the primeval formation of a federal code. Improvements on the first draughts will every year become both easier and fewer." (Federalist 53).

<sup>8</sup>Why informational challenges became so severe during this period, the Gilded Age in America, is an interesting question, but beyond the scope of this essay. I would conjecture that the growth of newspapers, the proliferation of the telegraph, and the consolidation of railroads led to an explosion in the exchange of ideas, of trade, and of the demands placed on lawmakers.

telligence” (1906, 65) of American legislators. Courtenay Ilbert noted that modern legislating “usually requires more expert knowledge than a private member of Parliament can command” and that the many technical questions “beset and baffle the private member in his attempts at legislation” (Ilbert 1901, 216-217). To remedy this ignorance, Mill advocated a reform to the policymaking process. Elected representatives should be allowed to sanction policy proposals drafted by expert commissions but not draft their own bills.<sup>9</sup> Although this astonishing suggestion was not strictly adopted, the concept of delegation would become central to theories of effective legislative policymaking.

Progressive Era political scientists took a far more active and democratically-minded approach to improving the scientific foundation of legislation. They addressed the process by which legislation was drafted and its content determined. One early reform was the adoption of bill-drafting bureaus (Jones 1952).<sup>10</sup> Legislators and their clerks had been responsible for writing their own legislation, with frequently unfortunate results.<sup>11</sup> New bill drafting bureaus were staffed with lawyers whose sole responsibility was to write legislation. In Colorado and Washington, these bureaus were even set up within the state universities (Cleland 1914). Lawyers were hired to review legislation before it was signed by the Governor and to revise existing statutes to resolve inconsistencies. These changes sought well-crafted legislation that was consistent with current law and free of drafting errors.

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<sup>9</sup>Mill probably had in mind commissions like those used to reform the Poor Laws and the London Sewer system. In an odd twist of history, both commissions included Edwin Chadwick, once Bentham’s personal secretary, as a driving intellectual influence.

<sup>10</sup>The first regular bill-drafting bureau was adopted in Parliament in 1869.

<sup>11</sup>For one example, Charles McCarthy refers to “jokers” in legislation prior to the adoption of rules for professionalized bill drafting (McCarthy 1912, 197).

Progressives also thought the substance of legislation should rest on expert knowledge. The first modern legislative reference bureau, created in Wisconsin by Charles McCarthy,<sup>12</sup> ensured that a legislator could request expert legal or financial advice on any topic of legislation.<sup>13</sup> Policy research was to be impartial and scientific. The purpose of the reference bureau was not to endorse policy proposals, but to make sure any proposal could be drafted and evaluated with the guidance of experts. Bill drafting bureaus and legislative reference libraries spread throughout American legislatures.<sup>14</sup>

The New Deal Era raised new fears about executive dominance, which led political scientists to devise new legislative institutions. One such institution was the legislative council, a group of legislators who would engage in long-term policy planning even when the legislature was not in session (Rhodes 1946; Davey 1953). Political scientists recommended other reforms such as removing constitutional limits on legislative pay to make public service a more attractive career (APSA 1945; Lederle 1947). They also argued for a rationalization of the committee system in Congress

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<sup>12</sup>McCarthy, a PhD in Political Science from the University of Wisconsin, explained the informational problems legislators face and the role of the researcher in helping: “The legislator is a busy man; he has no time to read. His work is new to him; he is beset with routine.... If he does not investigate for himself, he often is deceived by those who are seeking the accomplishment of their own selfish ends. Therefore, we can be of the greatest service to him, if we index, digest and make as clear as possible all kinds of information.” (McCarthy 1912, 215).

<sup>13</sup>McCarthy thought it was the responsibility of experts to support legislators: “We have heard a great deal of condemnation of the legislature. It is easy and popular too, to sneer, censure, and criticize — but we have heard very few suggestions as to a remedy.... If it is difficult to get information because of the great variety of subjects now coming before our legislators, the only sensible thing to do is to have experts gather this material.” (McCarthy 1912, 223).

<sup>14</sup>During the 1910s, the House and Senate in Congress each had one lawyer to draft legislation (Luce 1922). By the 1940s, the Office of the Legislative Counsel had expanded, although still only to a total of 14 staffers (APSA 1945). The Library of Congress established its Legislative Reference Service in 1914, but Congress only made it a free-standing support organization with the Legislative Reorganization Act of 1946.

and state legislatures (APSA 1945; New York (State) 1946). Again, these reforms were widely adopted.

Academics were recommending fundamental changes to how legislatures operated. This activism was broadly sanctioned and supported by the discipline's professional organizations. The American Academy of Political and Social Science was founded in 1889 to bring social science to bear on public policy issues.<sup>15</sup> The AAPSS, through book publishing and its in-house journal, frequently recommended legislative reforms (Mason 1938; Treadway 1938).<sup>16</sup> The AAPSS continues to publish, and new organizations have been established to facilitate political scientists' influence on public policy.<sup>17</sup>

The leading professional association of political scientists has joined the fray. In 1941, the American Political Science Association convened a special committee on the organization of Congress. Working closely with the La Follette-Monroney committee on legislative reorganization, the APSA committee recommended increased staffing, a simplified committee system, and registration requirements for lobbyists. These reforms were adopted in the landmark Legislative Reorganization Act of 1946. Political scientists also guided the reorganization of state legislatures. Massachusetts' legislative reorganization committee included Daniel Marsh, President of

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<sup>15</sup>Its mission was to “synthesize and advance research that addressed social challenges that might be redressed with more effective policy” (AAPSS 2018).

<sup>16</sup>The Academy published books identifying problems with state legislatures and city councils (with titles including “Decay of State and Local Government” (1890), “Our Failures in Municipal Government” (1893), and “A Problem of Primaries” (1906)), and proposing solutions (see “Reform of Our State Governments” (1894) and “Modernizing our State legislatures” (1936) (AAPSS 1908; Buck 1936)).

<sup>17</sup>The Scholars Strategy Network aims to “improve public policy and strengthen democracy by connecting scholars and their research to policymakers, citizen associations, and the media” (SSN 2018).

Boston University, and A. Chester Hanford, Professor of Government and Dean of Harvard College (Massachusetts (State) 1943).

The 1970s saw the apex of state legislatures' lawmaking capacity, a development widely credited to Alan Rosenthal. Over his career, Rosenthal consulted with at least 35 state legislatures on their organization. His 1968 study of the Connecticut legislature illustrates the scope of his impact. He recommended switching from biennial to annual sessions, creating nonpartisan offices of legislative research and fiscal analysis, establishing offices for party caucus staff, and tripling legislators' salaries.<sup>18</sup> All of these recommendations, and more, were adopted. Rosenthal was also central to the founding of the National Conference of State Legislatures, and his seminars for legislators and staff shaped a generation of legislative leaders.

Anywhere we look in a contemporary legislature — at staff, committees, reference bureaus, bill drafting bureaus, legislative councils, party caucuses, and lobbying rules — we see the influence of political scientists. The informational resources available to contemporary legislators emerged from legislative studies and were adopted because of academics' advocacy. It is hard to imagine contemporary legislatures if political scientists who perceived a problem with legislative performance had not suggested solutions.

## **5.2 Improving legislatures is the academics' responsibility**

The provision of policy expertise is subject to a collective action problem. Only groups with specialized interests or means of overcoming these problems will incur

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<sup>18</sup>This is presumably only one reason Dr. Rosenthal was so well-liked by legislators.

the costs of informing policymakers. To the progressives, this informational advantage was a source of moneyed interests' influence over policy.<sup>19</sup> Progressives looked for nonpartisan experts in public policy to balance the scales. They found academics. Not only were academics nonpartisan experts with a commitment to the public interest, but many were already being compensated with public funds!<sup>20</sup>

Academia has long claimed a responsibility to engage with problems of public affairs.<sup>21</sup> This responsibility continues in the mission of American universities even today. In 2017, Columbia University President Lee Bollinger announced a new initiative, Columbia World Projects, to bring academic research to bear on matters of global importance. He identified one such problem as “the capacity of liberal democratic institutions to identify and deal with significant policy concerns” (Bollinger 2017). What institution is more central to democracies than legislatures?

There has been criticism of activist scholars. The central objection is that academics should not engage in politics. In the past, professors who offered their expertise to lawmakers were “censured as endangering the life of the university — accused of throwing it into politics” (McCarthy 1912, 137). Many progressive scholars went beyond studying legislative operations to endorsing specific policies,<sup>22</sup> which did in-

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<sup>19</sup>“Is it better to allow such irresponsible parties [as the trusts] to have the power of fixing rates and prices rather than the state? Is it better to permit them to make laws than the state?” (McCarthy 1912, 17).

<sup>20</sup>This led McCarthy (1912, 13) to ask “Why should the public not avail itself of their services?”

<sup>21</sup>This idea was expressed in the Progressive Era as The Wisconsin Idea, the notion that faculty experts should work with legislators on ground-breaking legislation (McCarthy 1912; Turner 1921). The Wisconsin Idea lives on, though it is threatened in, of all places, Wisconsin, where Governor Scott Walker sought to remove the idea from the University of Wisconsin's mission statement in 2015 before backing down.

<sup>22</sup>These positions included breaking up the trusts, regulating railroads, enacting labor regulations, and reducing the power of party bosses.

deed court partisan conflict. Today, we are accustomed to academics being criticized for their support of partisan policy positions. Again, there is good reason. I have been present in committee hearings where legislative supporters of a policy invite their expert academics, opponents invite their expert academics, and the only concrete result is that academia looks partisan and uncertain.

Informed policymaking, though, has been a nonpartisan issue of procedure, rather than a partisan issue of policy outcomes, for at least a hundred years. Henry Emery, a professor of political economy at Yale when he was named chairman of the National Tariff Commission in 1909, believed that expertise could be nonpartisan even on highly partisan issues.<sup>23</sup> The academic's role was to provide expertise regardless of which direction policy was moving. Even a tariff opponent would prefer tariffs to be efficiently and fairly imposed. Alan Rosenthal was widely respected by Republicans and Democrats alike because they understood his advice related to the operations of the legislature and not its outputs. Many legislative reformers have been parliamentarians who, by their position, eschew partiality but protect the legislature's reputation for competence (Ilbert 1901; Mason 1938; Galloway 1951).<sup>24</sup>

This nonpartisan concern for legislative efficiency has been acknowledged by legislators themselves. Members of the New York Joint Committee on Legislative Methods declared there "is a nonpartisan interest in efficient policy deliberation" (New York (State) 1946, 17). Members of the Massachusetts Special Commission on Leg-

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<sup>23</sup>"It is a common belief that in a matter of such political significance as the tariff, non-partisanship is impossible. In my opinion this belief is unduly cynical and pessimistic." (Emery 1912, 25).

<sup>24</sup>"As a member of the staff of the Library of Congress, I view the legislative scene with as much nonaxiological detachment as an anthropologist would describe the customs and mores of primitive tribes on some tropical island." (Galloway 1951, 41).

islative System and Procedure asserted that the public is entitled to demand their representatives have “capacity to enact accurate and effective legislation based on reliable research” (Massachusetts (State) 1943, 11). Recognizing their common problems, legislators have welcomed help from impartial political scientists.

Despite this impartial focus on legislative procedures, there remain legitimate questions about whether academics should impact public policies. Even if they are not doing so directly, academics may influence policy by improving procedure. Efficient procedures may help legislators pass bills, the thinking goes, but they may help legislators pass the wrong bills.<sup>25</sup> Pathologies and biases in representation are real, and the extent to which policy outputs reflect the interests of different social groups is important.

However, concerns about policy outcomes do not outweigh concerns about legislative procedures. Informational efficiency and policy representation are distinct concerns,<sup>26</sup> and we should not ignore the former out of fear of the latter. Poor representation and bad policymaking may well occur, but withholding expertise seems a poor response. Starving legislatures of information and efficient institutions will make all legislating harder, especially for bills in the public interest. If we grant that procedural problems are legitimately costly — and there are ample such accounts by journalists, academics, and legislators — we cannot accept them out of an absolutist aversion to influencing policy outcomes.

Normative questions notwithstanding, political scientists, economists, and public

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<sup>25</sup>However one chooses to define “wrong” legislation, academics’ influence only becomes problematic if it leads to undesirable outcomes.

<sup>26</sup>See Federalist 62 (Madison 1788) and Mill (1862) for two takes on this dichotomy of problems facing the representative legislature.



health experts have long claimed a role in public policy. Advocacy for specific health or economics policies is broadly supported, perhaps because the problems are so visible. Public health epidemics and unemployment hit close to home. The problems facing legislatures are more distant, but they are no less real. The legislative scholar who identifies a cost of ineffective procedure is just as responsible for addressing it as is an economist, public health expert, or criminologist who discovers a problem in their field.

The more robust criticism, it seems to me, is not that previous efforts to improve legislative operations changed policy outcomes in a negative way; it is that we have little evidence that they changed any outcomes at all. The irony of the movement for evidence-based policymaking is that political scientists failed to collect evidence about the effectiveness of evidence-based policymaking.<sup>27</sup> It is unclear how well institutional reforms solved the legislature's problems or if they caused new problems of their own.

Earlier reorganizations were based on the best guidance political scientists had to offer, but reformers were strikingly confident. Reference bureaus, staff changes, and committee reforms were not rolled out gradually. They were implemented all at once, universally, on a permanent basis.<sup>28</sup> In a way, they were implemented in the same way as most public policies: with a sharp discontinuity and no plan to judge whether the new regime actually worked better than the old.

The flaws of this stark approach led to the current popularity of evidence-based

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<sup>27</sup>Contrast the vast empirical literature on term limits, a legislative reform generally opposed by political scientists, with the lack of studies on earlier informational reforms.

<sup>28</sup>This does not mean that reforms were binding on the legislature in perpetuity, just that they were not intended to expire.

policymaking. Part of its logic is that policymaking is an ongoing enterprise, and there should be continued efforts to evaluate and improve policy. Where are the ongoing efforts to evaluate and improve legislative institutions?

### **5.3 A research program of legislative evaluation**

The purpose of legislative evaluation is to identify the causal effect of specific legislative institutions, rules, and activities on outcomes of interest. Prior legislative evaluations have utilized a variety of causal identification strategies. Naturally-occurring experiments provide leverage for studying the effects of term length on legislative entrepreneurship (Titunik 2016) and of office location on position-taking (Rogowski and Sinclair 2012).<sup>29</sup> Non-randomized observational designs can identify causal effects subject to the validity of their assumptions. Berry and Fowler (2016) use a difference-in-difference design that assumes parallel trends across legislators to estimate the effect of committee chairmanships on pork spending. Phillips and Kirkland (2017) use a regression discontinuity design to estimate the causal effect of divided government on the passage of state budgets. These works share a careful consideration of the measurement and identification challenges that make estimating causal effects so difficult.

Naturally-occurring experiments and well-identified observational designs face clear limits. Academics are searching for any historical randomization that applies

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<sup>29</sup>These two studies engage with long-standing questions about legislative organization. The Federalist Papers considered many arguments about the effects of term length on legislative behavior, and effects of the architectural design of parliaments on political outcomes have been studied since at least Ilbert.

to politics, but at some point we will run out. Further, not every institution was adopted in a way that facilitates the identification of causal effects. These designs only work for rules or institutions that already exist. If a legislative leader wants to enact a rule that has not been widely adopted, how does the legislative scholar predict its effectiveness?

The obvious answer, in my view, is to work with the leader to design a study to evaluate the proposed rule change. Often this will recommend experimentation. Experiments have been widely adopted in political science. Even politicians have welcomed experiments to evaluate their campaign strategies and the effectiveness of public policies. Experiments have also been used to study interactions between legislators and the public. The next step is to use experiments to study operations inside the legislature.

Experimental evaluations must be conducted alongside legislators and their staff for practical and ethical reasons. Any experimental study faces a common set of ethical concerns about respect for persons, beneficence, and justice. Working under legislators' guidance ensures legislators are informed about the research activity, can object to any potentially harmful intervention, and will benefit from the findings. I have found legislators are quick to understand the logic of experimentation, and they are deeply curious about the effectiveness of legislative procedures. They welcome experimental projects that are responsibly, impartially, and carefully conducted.

Legislative evaluation does not just benefit legislative stakeholders and our public policy. It offers a way forward for students of legislatures who feel, as I do, that empiricists have been unable to address many essential questions posed by theories

of legislative organization. How effectively do committees solve commitment, coordination, and collective action problems? Do legislative institutions cause partisan polarization? Are there procedural solutions to gridlock? Across these foundational topics, theory has outpaced empirics. The only way we can convincingly answer them is to better understand the causal relationships between legislative processes and outcomes.

Evaluation is better suited to some research questions than others. Information is particularly conducive to experimental study. Information is central to theories of legislatures, and it happens to be particularly difficult to measure observationally. As a result, there are many unanswered questions of practical and theoretical significance. Evaluation studies of information can examine the impact of committee research reports, party caucuses, floor debate, and fiscal estimates on individual position-taking and policy outcomes.

Beyond information, coordination and collective action problems could be studied without much difficulty. Coordination could be examined through Dear Colleague letters and collective action problems through the selection of parliamentary leaders. Legislatures feature a long list of relatively low-rank leadership positions, including regional whips, delegation leaders, class representatives, and caucus chairs. Do these positions encourage legislating in the collective interest? Issues surrounding partisanship might raise problems for an impartial researcher, but some topics are still suitable for study. For example, do party caucus meetings affect polarization? Do legislators support interventions to facilitate bipartisanship?

Some topics are also easier to evaluate than others from a research design perspec-

tive. Activities like lobbying and deliberation can be experimentally manipulated for each legislator across a range of bills. Many rules are assigned at the bill level, while committee appointments are made at the legislator level. Institutions generally apply to all legislators and bills for a given session. As these clusters of experimental units grow larger and larger, researchers will have to develop more creative strategies for well-powered evaluations. Designs may have to include multiple legislatures or collect highly prognostic pre-treatment covariates.

This research program will be effective if it generates sustained interest in improving legislative procedures. Writing after World War II, congressional historian George Galloway laid out the stakes for the failure of American legislatures:

“Representative government has broken down or disappeared in other countries. Here in the United States it remains on trial. Its survival may well depend upon its ability to cope quickly and adequately with the difficult problems of a dangerous world. Congress is the central citadel of American democracy and our chief defense against dictatorship. Hence the importance of congressional reorganization and of further steps toward strengthening our national legislature.” (Galloway 1951, 68).

Legislative reform movements in the past have been intermittent and their successes impermanent. As a result, American legislatures have, from time to time, lapsed into dysfunction. They are too important for this to happen. In 2018, the challenges facing our legislatures might be different, but they once again call out for remedies. Evaluation may prove an effective tool for improving our public policy and our understanding of legislatures.

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# Appendix A: Briefings

## A.1 Why information influences position-taking

Why does information affect legislators' policy positions, and why might information's effects vary across legislators? This section describes a simple model of decision making under uncertainty in which legislators' prior uncertainty about the connection between policy instruments and policy outcomes constrains position-taking.

Assume legislators are risk averse and policy oriented. The utility legislator  $i$  receives from policy  $x_p$  can be given by the following utility function:

$$u_i(x) = -(x_p - x_i)^2$$

where  $x_i$  is the legislator's ideal policy outcome;  $x_p$ , the policy's ideological content, may not be known with certainty. Suppose legislators' prior beliefs are that  $x_p$  is uniformly distributed in  $[0,1]$  (with mean  $\bar{x}_p$ ) and that the prior distribution of  $x_p$  is fully contained within the support for the distribution of legislator ideal points.

Legislators' prior, uninformed expected utility from a bill, given by integrating over their utility function, is the following:

$$\mathbb{E}[u_i(x_p)] = -(\bar{x}_p - x_i)^2 - Var(x_p)$$

Utility is decreasing in ideological distance between the legislator and their expectation about the policy's content.  $Var(x_p)$  represents the costs of uncertainty.

Suppose legislators support a bill if their utility exceeds a critical threshold,  $u^*$  (Peress 2013).<sup>1</sup> Support could mean voting for the bill or choosing to cosponsor it. The legislator's probability of supporting the bill can be given by a random utility choice model that allows bill support to be increasing in utility with a particularly large increase when utility approaches the threshold:

$$Pr(\text{Support} = 1) = \frac{1}{1 + e^{-u^* + \beta \mathbb{E}[u(x_p)]}}$$

In this framework, information can influence support via utility in two ways. It can reduce uncertainty ( $Var(x_p)$ ) or correct a prior expectation ( $\bar{x}_p$ ).

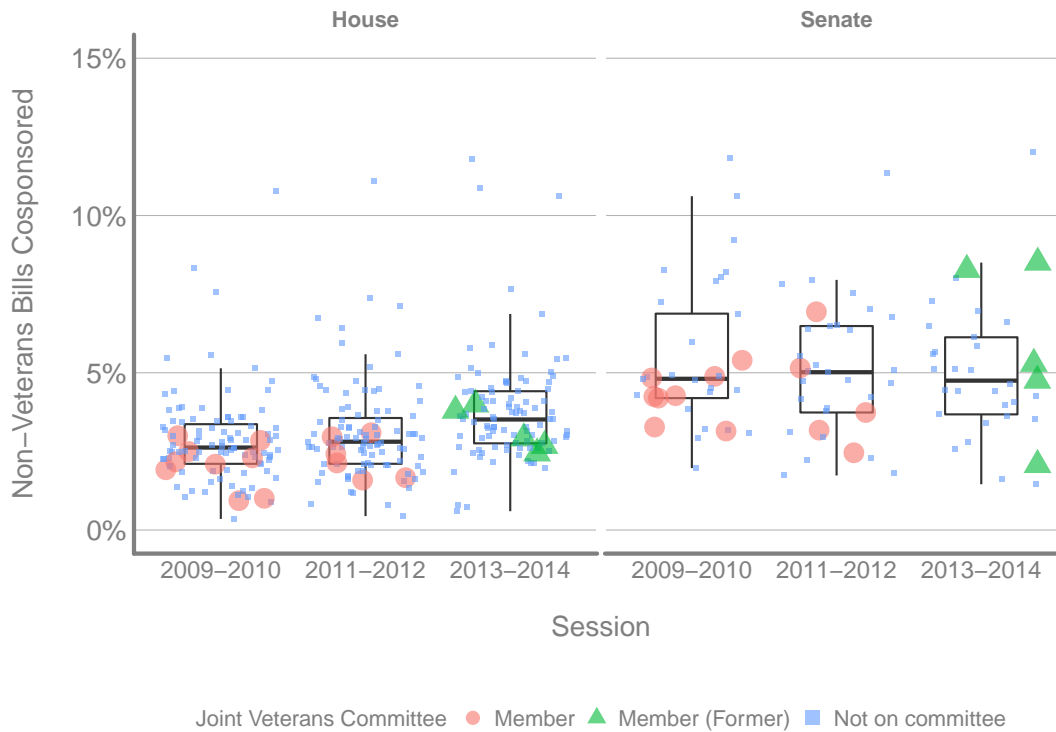
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<sup>1</sup>This threshold could also be the utility from a status quo policy.



## A.2 Placebo tests for non-experimental analyses

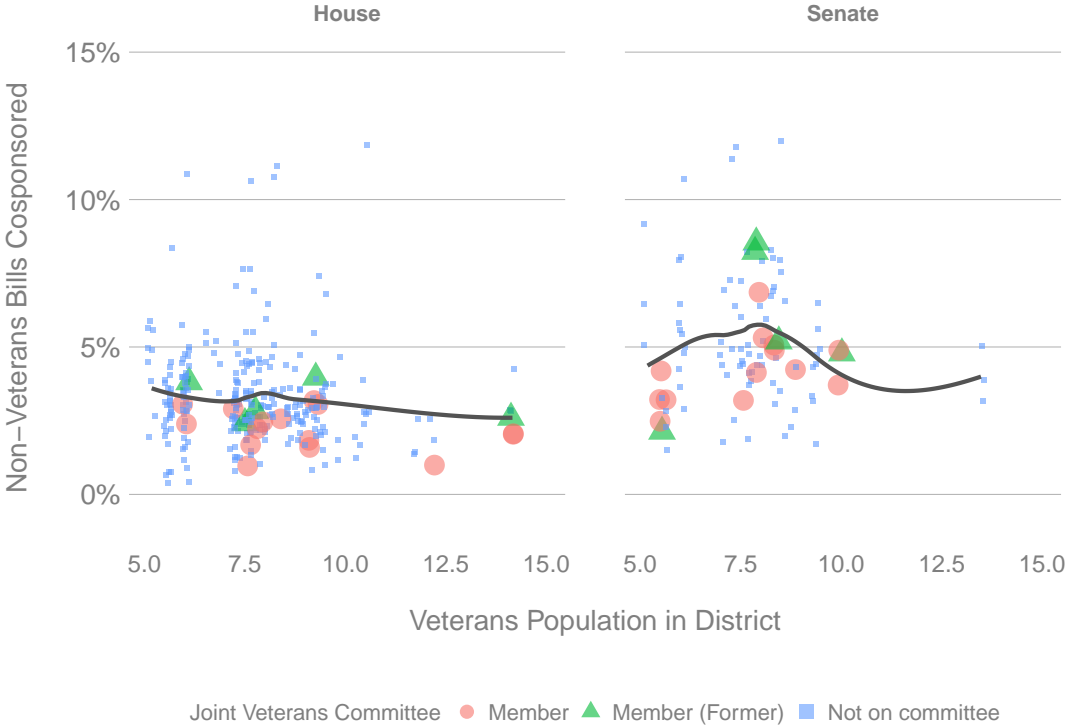
Figure 2.1 shows that cosponsorship of veterans bills declined substantially following the closure of the joint veterans committee. The stark changes on veterans legislation are not observed on other issues. Figure A.1 shows cosponsorship of all bills excluding veterans legislation.



*Figure A.1: Non-veterans bills cosponsored per legislator.*

Figure A.2 shows that legislators from districts with a large percentage of veterans are uniquely engaged with veterans issues. They do not cosponsor non-veterans

legislation at higher rates.



*Figure A.2: Non-veterans bills cosponsored per legislator, by district veterans population.*

### A.3 Supplemental results

To ease interpretation,  $\widehat{ATE}$  estimates from logistic regression are presented as the difference in the predicted probabilities of cosponsorship due to treatment. For the same reason, logistic standard errors are converted to predicted probabilities by taking the difference in predicted probability of cosponsorship of a one standard error change in the estimated average treatment effect, centered at the estimated value.

*Table A.1: Estimated briefing effects (logistic regression).*

DV: Cosponsorship	
$\widehat{ATE}$	.054**
$\widehat{SE}$	(.019)
95% C.I.	(0.016,0.092)
Regression Model	Logistic
Fixed Effects <sup>(a)</sup>	Yes
N	1,216

(a) Bill and legislator fixed effects.

Logistic regression estimates converted to predicted probabilities.

Robust standard errors and p-values presented.

One-tailed p-values indicated at  $p < .05$  (\*),  $p < .01$  (\*\*).

Figure A.3 displays legislator-specific difference-in-means  $\widehat{ATE}$  and statistical significance (p-values from Fisher's exact test and verified with randomization inference). There are no negative, statistically significant  $\widehat{ATE}$  for individual legislators. Due to the small number of bills per legislator,  $\widehat{ATE}$  must be quite large (c. 50

pp) to attain conventional levels of statistical significance. Observations are jittered slightly in horizontal direction to increase visibility.

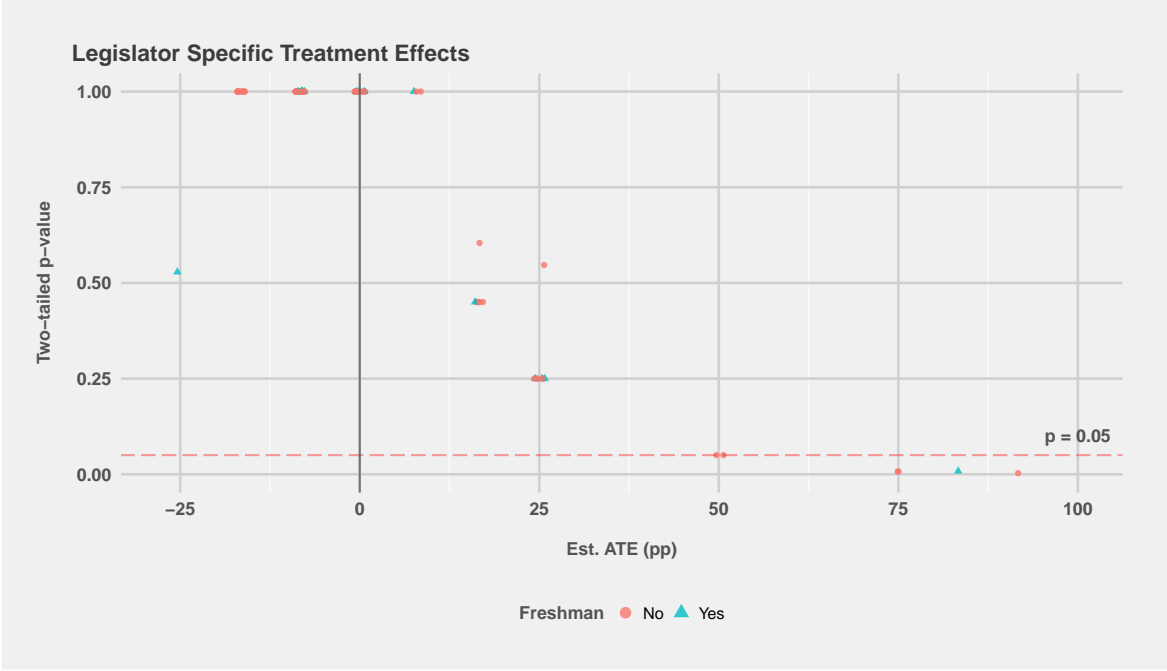


Figure A.3: Estimated statistical significance of legislator-specific briefing effects.

# Appendix B: Cue-taking

## B.1 Construction of alternative cue-taking models

Video of floor proceedings was used to create a seating plan for all 99 legislators in the lower chamber. Of the 157 subjects in the two experiments (with subjects defined as a legislator in a given study, since seating plans change), 132 shared a desk with a legislator who was also included in the study. A legislator is defined as exposed to secondary treatment if her deskmate was assigned to the bill briefing.

Each subject was matched to another subject in a neighboring district to create pairs of geographically proximate legislators. Subjects were grouped into pairs and not larger groups to maintain parallelism with other diffusion models and to prevent the possibility of subjects being exposed to multiple spillover treatments. Distance is calculated by the latitude and longitude of districts' municipal seats. Pairs were created through an algorithm that minimized the aggregate distance within pairs.

DW-NOMINATE ideology scores were constructed based on legislators' roll call voting during the first session.<sup>1</sup> Legislators were paired based on their first and

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<sup>1</sup>Using roll call voting from the session during which Study 1 was implemented maximizes the

second dimension ideology scores, again through an algorithm that minimized the aggregate distance within pairs.<sup>2</sup>

The spillover models are not aggregated into one complex model and estimated jointly due to the large number of treatment conditions that would result.

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number of legislators with valid ideology scores. This covariate is post-treatment for Study 1 and pre-treatment for Study 2. There is little reason to think treatment on the limited number of bills in these studies, many of which did not receive a vote, influenced legislators' DW-NOMINATE scores.

<sup>2</sup>Bowers, Frederickson, and Panagopoulos (2012) and Coppock (2014) utilize more complex models of treatment spillover across ideology networks. This simple two-person model is used to maintain parallelism with the other models.

## B.2 Supplemental results

Table B.1: Estimated briefing and cue-taking effects (study 2 only).

	(1)	(2)
Staff		
Briefing $\widehat{ITT}$	16.7	4.4
(SE)	(4.8)	(3.7)
Cue-taking $\widehat{ITT}$	18.0	6.4
	(4.4)	(3.0)
Advocate		
Briefing $\widehat{ITT}$	1.4	0.7
	(4.9)	(3.6)
Cue-Taking $\widehat{ITT}$	5.8	4.4
	(4.5)	(3.0)
N	992	992
Covariates	None	Bills Legislators

Standard errors and p-values obtained using randomization inference and 10,000 simulated assignments.

**Table B.2: Estimated briefing and cue-taking effects excluding legislator fixed effects.**

	Briefing $T_{S=0}$	Cue-taking $S_{T=0}$	Combined $T_{S=1} + S_{T=1}$
$\widehat{\text{ITT}}$	5.3	3.6	12.1
$(\widehat{\text{SE}})$	(1.9)	(1.7)	(2.9)
$\widehat{p}$ (one-tailed)	0.004	0.013	0.000
N	2,080	2,080	2,080

Standard errors and p-values obtained using randomization inference and 10,000 simulated assignments.

Observations assigned to advocate direct or secondary treatment (200) or multiple staffer secondary treatments (36) are not displayed.

Estimated probability of combined effects smaller or equal to sum of direct and indirect effects is 4.1%.

**Table B.3: Estimated briefing and cue-taking effects excluding legislator fixed effects (study 1 only).**

	Briefing $T_{S=0}$	Cue-taking $S_{T=0}$	Combined $T_{S=1} + S_{T=1}$
$\widehat{\text{ITT}}$	4.3	0.0	10.5
$(\widehat{\text{SE}})$	(2.2)	(1.9)	(2.9)
$\widehat{p}$ (one-tailed)	0.024	0.487	0.000
N	1,088	1,088	1,088

Standard errors and p-values obtained using randomization inference and 10,000 simulated assignments.

Observations assigned to multiple staffer secondary treatments (36) are not displayed.

Estimated probability of combined effects smaller or equal to sum of direct and indirect effects is 13.2%.



*Table B.4: Estimated briefing effects in one-person offices.*

	Study 1	Study 2	Combined
Briefing $\widehat{ITT}$	-5.3	5.7	1.1
( $\widehat{SE}$ )	(5.2)	(6.4)	(4.7)
$\widehat{p}$ (one-tailed)	0.840	0.179	0.397
N	128	304	432

Standard errors and p-values obtained using randomization inference and 10,000 simulated assignments.

# Appendix C: Deliberation

## C.1 Deliberation and polarization of position-taking

Deliberation substantially reduced polarization in policy coalitions. Figure C.1 plots the predicted probability of cosponsorship (y-axis) against the ideology of legislators (x-axis) for bills sponsored by Democrats (solid, blue lines) and Republicans (dotted, red lines).<sup>1</sup> In the left-hand panel, which includes untreated observations, there is clear partisan polarization. Democratic bills are cosponsored by legislators with left-of-center ideologies and Republican bills by legislators right-of-center. In-party cosponsorship is on the order of ten times as large as out-party cosponsorship. There is minimal cross party cosponsorship. In the right-hand panel, bills assigned to deliberation demonstrate far less polarization in support. Liberals cosponsor Republican bills at nearly the same rate as conservatives and vice versa. The curves are bimodal, not unimodal. Similar, but muted, patterns are also evident in roll call voting in Figure C.2.

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<sup>1</sup>The three legislators who did not serve in the prior session of the given assembly are omitted from the display.

Figure C.1: Deliberation and cosponsorship polarization.

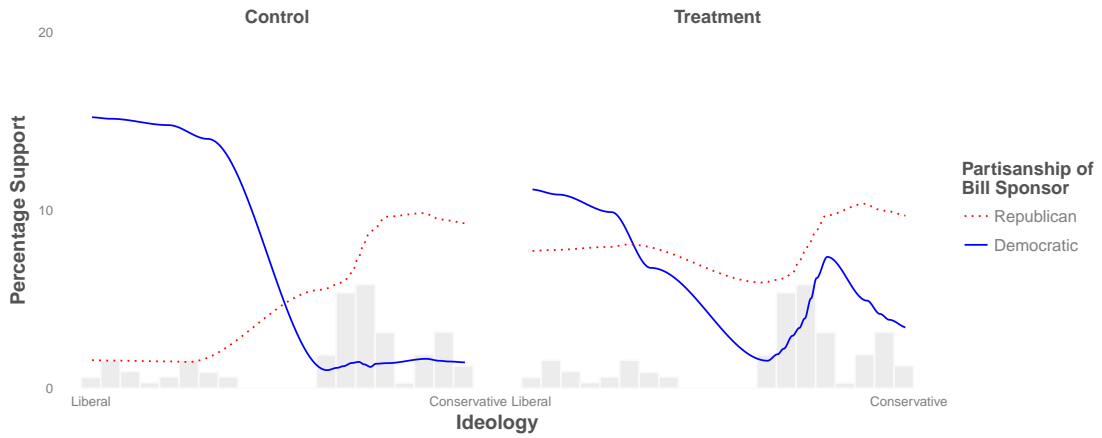
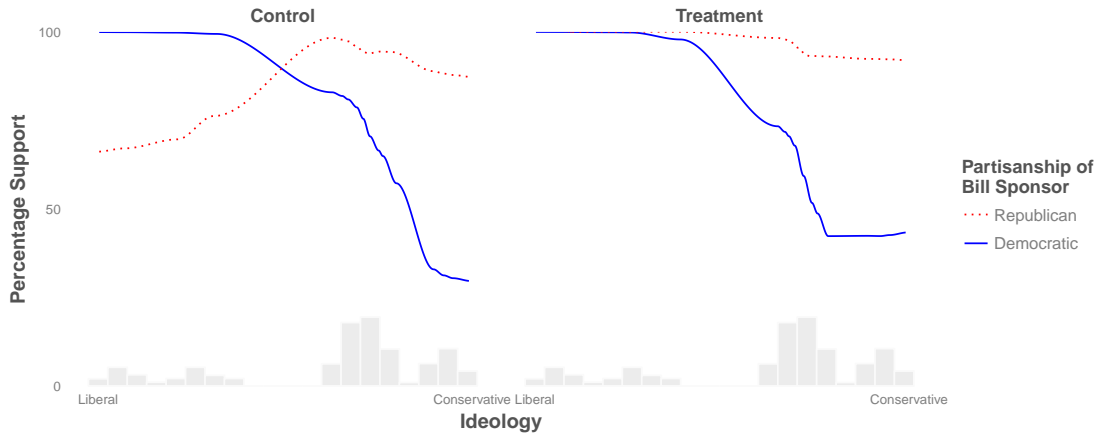


Figure C.2: Deliberation and roll call voting polarization.



## C.2 Supplemental results

### Estimated deliberation effects by attendance and partisanship

Table C.1: Estimated deliberation effects by attendance and partisanship.

	Attendees		Absentees	
	In-Party	Out-Party	In-Party	Out-Party
$\widehat{\text{ITT}}$	0.08	0.28*	-0.01	0.03
(SE)	(0.06)	(0.17)	(0.05)	(0.03)
N	139	47	1,379	885

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

Weights equal to inverse of bill's probability of assignment to treatment.

Standard errors and p-values, clustered at bill level.

### Estimated heterogeneous deliberation effects by ideology

I estimate heterogeneous effects of treatment by ideological distance with the following regression:

$$Y_{ij} = a + b_1 d_j + b_2 \text{Ideology}_{ij} + b_3 (d_j^* \text{Ideology}_{ij}) + u_{ij} \quad (\text{C.1})$$

where  $Y_{ij}$  indicates support by legislator  $i$  for bill  $j$ ;  $d_j$  is an indicator variable for whether the bill was assigned to treatment;  $\text{Ideology}_{ij}$  is the ideological distance between legislator  $i$  and the sponsor of bill  $j$ ; and  $u_{ij}$  represents unmeasured determinants of turnout. Weights are again utilized to account for differential probabilities of treatment assignment across bills.

Table C.2 displays results among out-partisans only. Columns (1) and (3) report results excluding the interaction between treatment and ideological distance, the same results presented in Table 4.3. Columns (2) and (4) include the interaction term. To evaluate whether the interaction improves model fit, Table C.2 reports an F-test comparing the fit of the model with the interaction to the model that includes only the base terms.

The estimands of interest are  $b_1$ , the average intent-to-treat effect of assigning an observation to treatment on support, and  $b_3$ , the average intent-to-treat effect interacted with ideological distance.  $b_3$  indicates whether treatment increased support more among ideologically-similar or dissimilar legislators.  $b_3 < 0$  indicates treatment increased support more among ideologically-proximate legislators than among dissimilar legislators, as predicted by signaling models. I again report standard errors and associated p-values clustered at the bill level, which are verified with randomization inference. Table C.2 reports the number of subjects, bill clusters, and the effective sample size.

There is little evidence that treatment is more effective for ideologically-proximate legislators than dissimilar legislators. The interaction term is negative for cosponsorship but positive for roll call voting. Neither estimate achieves conventional levels of statistical significance. F-statistics indicate that there is no significant improvement in model fit by including the interaction between the treatment assignment indicator and ideological distance.<sup>2</sup>

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<sup>2</sup>Models (2) and (4) also interact the treatment indicator with an indicator for whether the subject has a valid ideological distance, which is why reporting the F-statistic is not redundant.

Table C.2: Estimated heterogeneous deliberation effects by ideology.

	Cosponsorship		Roll Call Voting	
	(1)	(2)	(3)	(4)
$\hat{b}_1$ (ITT)	4.4**	10.4*	35.2*	13.8
( $\widehat{SE}$ )	(2.5)	(8.1)	(12.6)	(36.3)
$\hat{b}_2$ (Distance)	-5.7**	-3.0	-76.1**	-89.6**
	(2.7)	(2.4)	(17.0)	(37.2)
$\hat{b}_3$ (Interaction)		-5.4		20.2
		(5.4)		(40.7)
F-statistic		0.897		1.308
N	932	932	316	316
Clusters	25	25	8	8
ESS	189	189	44	44

Weights equal to inverse of bill's probability of assignment to realized condition. Significance indicated at  $p < 0.10$  (\*) and  $p < 0.05$  (\*\*) one-sided. Cluster-robust SEs and p-values reported.