How Responsive Are Legislators to Policy Information?
Evidence from a Field Experiment in a State Legislature

Adam Zelizer*

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Abstract

Theories of legislative committees, lobbying, and cue-taking assume information affects legislators’ support for policy alternatives. However, there is little direct, empirical evidence to support this foundational assumption about legislative behavior. This paper reports results from a field experiment in which state legislators were randomly assigned to receive policy research about pending proposals. Results show that policy information increased aggregate cosponsorship by 60% above baseline rates. For one bill covered critically, information diminished cosponsorship and roll call voting support. Results are broadly consistent with information signaling models’ predictions about the importance of information to position-taking.

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*PhD Candidate, Department of Political Science, Columbia University, New York. apz2002@columbia.edu. The author wishes to express his appreciation to Gregory Wawro, Don Green, Shigeo Hirano, Carlo Prato, John Marshall, Sharyn O’Halloran, Winston Lin, Patricia Kirkland, Alex Coppock, and Michael Schwam-Baird for discussing the research design and providing comments on the paper. The author would also like to thank the legislators and staff members who welcomed and supported the project, including Rep. John R., Rep. Karen C., Scott G., Tyler L., and Davis P.
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; Potters 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 influences individual positions or collective policy outcomes. Few empirical studies examine how information varies across legislators, whether information affects individual behavior, and to what extent institutions 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 instead of directly examining the question of interest: does providing legislators with policy research change their support for legislation? 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 information.

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1For example, tests of informational models of legislative organization examine the ideological composition of committees (Krehbiel 1991), and studies of lobbying examine which legislators meet with lobbyists (Hojnacki and Kimball 1998).
This paper contributes to the study of decision making under imperfect information in several ways. First and foremost, it shows that policy research affects position-taking. On average, legislators were 60% more likely to support bills selected for briefings. While most briefings painted bills in a positive light, one bill was covered critically due to a flaw in its drafting. Legislators briefed on this bill were less likely to cosponsor and vote for it.

Second, the empirical design directly engages 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.

**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 1889; 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 across time as predicted by theory. In particular, institutions, electoral incentives, and legislators’ own backgrounds 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, patterns of position-taking suggest that legislators are indeed constrained by imperfect information. Both sponsorship and cosponsorship vary across legislators, and across 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 where the experiment was conducted resembles many other state legislatures in the United States. It is a low professionalism legislature, ranked in the bottom half of Squire’s (2007) index because of its part-time legislators, low staff support, short annual sessions, and poor 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 organized interests engaged in some lobbying, veterans groups did not. Of the 579 lobbyists representing 737 separate groups registered with the state ethics commission, none represented veterans. In addition, the legislature also lacked committee coverage of veterans issues. Although thirty-six states convened a full standing committee on Veterans or Military

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2We 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.

3The state is not named in order to preserve ongoing research projects in the legislature.
Affairs in their lower chambers,⁴ the state where the intervention occurred 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 discontinuation of the joint committee offers an opportunity to examine its relationship to position-taking. Figure 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 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 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 session of its operation, 12.5% in the second, and 4.7% in the session after its closing. Median Senate cosponsorship declined from 17.6% to 15.9% to 10.5% over the three assemblies.⁵ Results are not driven by committee members, and placebo tests show that declines in bill cosponsorship were unique to veterans bills (see Appendix D).

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

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⁴Includes the Government, Military, and Veterans Affairs Committee in the Nebraska Unicameral Legislature. Data from state legislative websites. Data collected for 2015 to 2016 sessions.

⁵The decline in average cosponsorship from the second to third assembly was 6.9 percentage points (p < 0.001 two-sided from t-test) and in the Senate 5.1 percentage points (p < 0.05).
and more reluctant to take positions.\footnote{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.}

**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 information from other factors that might affect position-taking (like ideology), but we can at least examine whether veterans are more engaged with 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 if all legislators were equally likely to file veterans bills.\footnote{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).} 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. Veterans make up 5.2% – 14.2% of district populations, with a statewide average of 7.7%. Districts with the largest veterans’ presence feature a large military base.

Figure 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 with the median number cosponsored 14.0%. In the Senate, the relationship is even stronger. These results are unique to veterans issues. Placebo tests in Appendix D show

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8 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.

9 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. These districts do not drive the results reported below.
that legislators representing more veterans were no more likely to cosponsor non-veterans legislation.

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

**Experimental overview**

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 1 displays an illustrative research report, scrubbed of information that would readily identify the state. The goal was for legislators to come away from briefings 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’ only effort was 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.¹⁰

**Experimental units**

To increase power, multiple bills and legislators were included in the study. Sixteen veterans bills, representing nearly all veterans legislation proposed during the session, were selected for inclusion. Seventy-six legislators were included: first-term representatives and committee chairs, members of both parties, and members and non-members of the veterans caucus.¹¹ Party leaders, the caucus chair, and the caucus chair’s officemate were excluded from the study due to their familiarity with the purpose and scope of the study. Nevertheless, 75% of the chamber’s membership was included.

Treatment assignment occurred at the legislator-bill dyad level.¹² Four bills were selected for treatment for each legislator through block random assignment. With 76 legislators and 16 bills, there are a total of 1,216 legislator-bill dyads.

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 rea-

¹⁰Because treatment was not a generic blandishment to support legislation, there is no reason to expect a priori that information’s effect would be to shift all legislators, on all bills, in the same direction, namely toward supporting proposals. The Appendix deals with this issue in more detail, laying out a model of individual choice in which briefings, by reducing uncertainty, tend to increase policy support. The model is a direct extension of the signaling models that underlie informational theories of committees.

¹¹Eighteen legislators were caucus members and 58 were not. The caucus did not discuss the legislation during its meetings during that session.

¹²Because 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).
son. The downsides of using multiple bills include an additional non-interference assumption: treatment is assumed not to diffuse across bills. Fortunately, there is little evidence that it does.\textsuperscript{13} It is also possible that briefings increase support for some bills but reduce it for others, leading the overall average treatment effect to understate the total change in cosponsorship. This turns out not to be a major concern, as briefings turned out to be almost universally supportive (the exception is discussed at length below).

**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.\textsuperscript{14} 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, so it is important legislators cosponsor the “right” bills (Campbell 1982; Bernhard and Sulkin 2013). To some, cosponsorship is a better indicator of individual priorities than roll call voting. Former Senator Richard Lugar explains:

“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

\textsuperscript{13}All legislative experiments with multiple legislators assume treatment does not spill over between legislators. This assumption seems strong, so it is addressed in a standalone paper that estimates treatment contagion across legislators. There is evidence of contagion, but allowing for it does not change the results reported in this paper. There is no evidence of contagion across bills.

\textsuperscript{14}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.
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 not because they were particularly unpopular or seriously flawed.\textsuperscript{15} They just were not budgeted. For example, the bill described in Table 1 was probably not enacted due to its $200,000 projected cost, not because legislators found it politically useful 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 bills 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 in the study.\textsuperscript{16} 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. Rather than estimating treatment effects for all bills, 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, and the point of including it is only to show that briefings affect more than just cosponsorship, observing effects on one bill is sufficient to show that policy research influences many position-taking activities.

\textbf{Results}

Table 2 displays bill cosponsorship by treatment assignment.\textsuperscript{17} 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

\textsuperscript{15}Ironically, the one bill with a clear drafting flaw reached the floor and became law (though it was fixed along the way). See below.

\textsuperscript{16}Average treatment effects for all bills are clearly off the table due to attrition, or survival, of observations.

\textsuperscript{17}Bill sponsorship is included in cosponsorship.
(ATE) is 5.4 percentage points. \( \hat{ATE} \) is also estimated through linear regression with bill and legislator specific fixed effects:\(^{18}\)

\[
Y_{ij} = \alpha + \gamma_1 \text{Legislator}_1 + \gamma_2 \text{Legislator}_2 + \ldots + \gamma_{75} \text{Legislator}_{75} + \\
\delta_1 \text{Bill}_1 + \delta_2 \text{Bill}_2 + \ldots + \delta_{15} \text{Bill}_{15} + \\
\beta d_{ij} + \epsilon_{ij}
\]  

(1)

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;\(^{19}\) and \( d_{ij} \) is a treatment indicator. \( \beta \) is the ATE. ATE estimates obtained from logistic regression are available in the appendix.\(^{20}\) Robust standard errors and one-tailed p-values are presented for all estimators.\(^{21}\)

The information treatment increased cosponsorship by 5.0 to 5.4 percentage points on average across all bills and legislators (\( p < 0.01 \) 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 aggregate 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, and it was also the only bill whose briefing was substantively different, and more critical, than other bills. A miscommunication between the sponsor and the bill drafting bureau resulted in the bill being written with what was widely perceived to be a flaw (and which the sponsor mentioned to the author was an unintended error in the draft bill). The

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\(^{18}\)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.

\(^{19}\)One legislator and one bill serve as the baseline for comparison.

\(^{20}\)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.

\(^{21}\)Standard errors and significance tests were verified with randomization inference, which yielded smaller standard errors and p-values in all cases.
specific nature of the error is beyond the scope of this paper, but it became a focal point for briefings on Bill 16. The flaw was repairable, which was communicated in briefings, and it was eventually fixed — but not until the bill had reached the chamber floor, after briefings had occurred. Although it was not intended, this bill allows us to examine the effects of critical briefings and, more broadly, whether briefing content matters.

Table 4 shows the effect of treatment on cosponsorship and roll call voting for 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, and 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 \( (p < 0.10 \text{ one-sided from randomization inference}) \). Treated legislators also voted for the the bill at a 17 percentage point lower rate \( (p < 0.05 \text{ one-sided}) \).\(^{22}\) Despite the sponsors’ appeals, treated legislators hesitated when asked to support the bill.

We should be cautious interpreting results for Bill 16. Examining treatment effects on a single bill, chosen ex post, raises valid “garden of forking path” criticisms. 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.\(^{23}\)

\(^{22}\)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.

\(^{23}\)On the other hand, if briefings indeed communicated a negative perspective on the bill because of its flaw, legislators should have learned during floor debate that the flaw was fixed. It must have been the case that they did not learn of the fix, or their opposition to the bill was a response to another poorly received aspect of the bill.
Now let us turn to a more direct discussion of heterogeneous treatment effects, in particular to heterogeneous effects predicted by 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 ideology from the receiver, she can truthfully communicate helpful information.\textsuperscript{24} However, there are two distinct reasons to expect research effects to vary across legislators, both of which suggest briefings should be more influential among like-minded than dissimilar legislators. 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 non-partisan caucus staffer, legislators still may have interpreted information in light of the bill sponsor. Thus briefings on bills from like-minded sponsors 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 how utility translates into cosponsorship. Prior work argues legislators cosponsor bills only if their expected utility surpasses a threshold (Peress 2013). Since baseline cosponsorship rates are low, the legislators closest to the threshold will be those predisposed to support proposals. Thus equal shifts in utility will cause only predisposed legislators to cross over the threshold.\textsuperscript{25}

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

\textsuperscript{24}“Not too far” depends on the amount of uncertainty in the mapping between policy proposal and policy outcome.

\textsuperscript{25}For more information on the choice model underlying cosponsorship, see Appendix C.
bill sponsor. For this study, cosponsorship similarity is more practical, and predictive of the behavior of interest, than other measures of ideological similarity based on roll call voting or campaign donations. Results below show that cosponsorship similarity is highly predictive of cosponsorship of experimental bills.

Cosponsorship similarity is constructed as follows:

\[
\text{Cosponsorship Similarity}_{ij} = \frac{\sum_{b=1}^{B} \text{cosponsor}_{ib} \times \text{cosponsor}_{jb}}{\sum_{b=1}^{B} \text{cosponsor}_{jb}}
\]

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 \(j\). 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. Results are presented for both sets of cosponsorship similarity scores, 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 1 to include an interaction between treatment and cosponsorship similarity:

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\(^{26}\) Campaign finance-based ideology scores are not available for many of the legislators, and the legislature of interest featured relatively few contested roll call votes with which to estimate ideology using DW-NOMINATE or another scaling algorithm. Neither campaign donation- or roll call voting-based measures are predictive of cosponsorship in this sample.

\(^{27}\) Results are robust to scaling by the number of bills cosponsored by \(i\).
\[ Y_{ij} = \alpha + \gamma_1 \text{Legislator}_1 + \gamma_2 \text{Legislator}_2 + ... + \gamma_{75} \text{Legislator}_{75} + \]
\[ \delta_1 \text{Bill}_1 + \delta_2 \text{Bill}_2 + ... + \delta_{15} \text{Bill}_{15} + \]
\[ \beta_1 \text{Cosponsorship Similarity}_{ij} + \beta_2 d_{ij} + \beta_3 d_{ij} \times \text{Cosponsorship Similarity}_{ij} + \epsilon_{ij} \]  

Equation (2) is estimated using both measures of cosponsorship similarity, with and without legislator and bill-specific fixed effects.\(^{28}\)

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

Table 5 displays regression results. First, cosponsorship similarity is highly predictive of observed cosponsorship. A one standard deviation increase in cosponsorship similarity is associated with a 4 to 8 percentage point increase in cosponsorship. Given low baseline cosponsorship rates, this is strong validation of the cosponsorship similarity measure. It is also an additional benchmark for the large magnitude of average briefing effects.

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 effects

\(^{28}\)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 \( \hat{\beta}_1 \) and \( \hat{\beta}_3 \) 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.

\(^{29}\)The standard deviation of cosponsorship similarity is 10.7 percentage points.
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. Cosponsorship depends not only on ideology, but also on legislators’ personal relationships and information. Thus it is safer to say that “like-minded” legislators are more influenced by results 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.

**Alternative explanations**

The analyses in this paper 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 5 suggests that they did not. It displays estimated individual treatment effects against legislators’ cosponsorship of veterans bills in the assembly before the intervention. It also differentiates members of the Veterans

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30It 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 is determined in the first place.

31Individual treatment effects are estimated with substantial uncertainty. Figure A1 plots the magnitude
Caucus (displayed as circles) from non-members (triangles).

Treatment effects do not appear to vary substantially by engagement with veterans issues. Estimated effects initially increase with prior veterans cosponsorship, but then decrease. Estimated effects by caucus membership are displayed in Table 6. Estimates are slightly higher among the eighteen caucus members (6.3 percentage points) than among non-members (4.4), but the difference is modest. There is considerable variation in individual treatment effects, but information was not exclusively influential for the most pro-veteran legislators.

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, such as roll call voting or bill sponsorship, raising awareness would not. After all, when legislators vote on bills, they are, one hopes, aware they exist.

We can leverage bills’ differential progress 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 only observe briefing effects due to raising awareness 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 falls well short of conventional levels of statistical significance. 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.

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of individual effects against their probability of occurring under the sharp null hypothesis. Individual effects approach conventional levels of statistical significance only when they approach 50 percentage points. For this reason we examine aggregate trends, not specific legislators.
Discussion

The experiment described in this paper is unusual in at least four respects. First, an information treatment 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. Finally, it includes multiple bills.

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’ attitudes on healthcare policy or other highly salient, high-conflict issues. 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. While these are legitimate concerns, ultimately the only way to address them is through more research. One of the benefits of this paper is that it provides a benchmark 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 fully-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 close legislators come to their fully-informed positions.

While field experimentation is widely used in studies of public opinion and voting behavior, it is a new but growing part of legislative studies. This paper shows that experimentation, though novel, is well-suited to studying important, long-standing questions in legislative studies, while at the same time appealing to stakeholders inside the legislature. Party leaders, caucus leaders, and bill sponsors all approved the intervention. With backgrounds in medicine, business consulting, and their own campaigns, legislators were no strangers to
experimental evaluation techniques. If done responsibly, with the approval and oversight of government officials, experimentation can help evaluate the performance of political institutions and activities while advancing social science research. Rigorous, transparent, and nonpartisan evaluation is one way political scientists can help legislatures become effective policymaking bodies and thereby stand up to empowered executives and restore the public’s frayed trust in legislative institutions.
References


Table 1: Illustrative research report

<table>
<thead>
<tr>
<th>Bill</th>
<th>Removing Limits on ROTC Courses for Scholarship Students</th>
</tr>
</thead>
<tbody>
<tr>
<td>Sponsors</td>
<td>House Sponsor / Senate Sponsor</td>
</tr>
<tr>
<td>Overview</td>
<td>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.</td>
</tr>
<tr>
<td>Current Law</td>
<td>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.</td>
</tr>
<tr>
<td>Problem</td>
<td>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. Cap can cost ROTC students 2 semesters of eligibility for the public scholarship.</td>
</tr>
<tr>
<td>Solution</td>
<td>Exempt ROTC courses from the cap.</td>
</tr>
<tr>
<td>Cost</td>
<td>Increase of $200,000+ per year in state education funding.</td>
</tr>
</tbody>
</table>

Information that could identify the state of interest is removed.
Table 2: Cosponsorship, by treatment assignment

<table>
<thead>
<tr>
<th>Cosponsored?</th>
<th>Control</th>
<th>Treatment</th>
</tr>
</thead>
<tbody>
<tr>
<td>No</td>
<td>838</td>
<td>263</td>
</tr>
<tr>
<td>Yes</td>
<td>74</td>
<td>41</td>
</tr>
<tr>
<td>Cosponsorship Rate</td>
<td>8.1%</td>
<td>13.5%</td>
</tr>
</tbody>
</table>

Table 3: Estimated effect of informational briefing on cosponsorship

<table>
<thead>
<tr>
<th>DV: Cosponsorship</th>
<th>(1)</th>
<th>(2)</th>
</tr>
</thead>
<tbody>
<tr>
<td>( \hat{\text{ATE}} )</td>
<td>.054**</td>
<td>.050**</td>
</tr>
<tr>
<td>( \hat{\text{SE}} )</td>
<td>(.022)</td>
<td>(.017)</td>
</tr>
<tr>
<td>95% C.I.</td>
<td>(0.011,0.096)</td>
<td>(0.016,0.084)</td>
</tr>
</tbody>
</table>

Regression Model
<table>
<thead>
<tr>
<th>Simple</th>
<th>Multiple</th>
</tr>
</thead>
<tbody>
<tr>
<td>No</td>
<td>Yes</td>
</tr>
</tbody>
</table>

N 1,216 1,216

(a) Bill and legislator fixed effects.
Robust standard errors and p-values presented.
One-tailed p-values indicated at \( p < .05 \) (*), \( p < .01 \) (**).

Table 4: Estimated effect of informational briefing on voting

<table>
<thead>
<tr>
<th>Cosponsorship</th>
<th>Roll Call</th>
</tr>
</thead>
<tbody>
<tr>
<td>( \hat{\text{ATE}} )</td>
<td>-.163</td>
</tr>
<tr>
<td>( \hat{\text{SE}} )</td>
<td>(.119)</td>
</tr>
<tr>
<td>( \bar{Y}_{\text{Control}} )</td>
<td>.782</td>
</tr>
<tr>
<td>Fixed Effects(a)</td>
<td>No</td>
</tr>
<tr>
<td>N</td>
<td>76</td>
</tr>
</tbody>
</table>

(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.
Table 5: Heterogeneous treatment effects, by Cosponsorship Similarity.

<table>
<thead>
<tr>
<th>Cosponsorship Similarity</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>0.332**</td>
<td>0.678**</td>
<td>0.620**</td>
<td>0.760**</td>
</tr>
<tr>
<td>SE</td>
<td>(0.121)</td>
<td>(0.114)</td>
<td>(0.170)</td>
<td>(0.163)</td>
</tr>
<tr>
<td>$d$</td>
<td>-0.025</td>
<td>-0.029</td>
<td>-0.005</td>
<td>-0.028</td>
</tr>
<tr>
<td></td>
<td>(0.029)</td>
<td>(0.031)</td>
<td>(0.050)</td>
<td>(0.060)</td>
</tr>
<tr>
<td>$d^* \text{Cosponsorship Similarity}$</td>
<td>0.470*</td>
<td>0.515*</td>
<td>0.250</td>
<td>0.369</td>
</tr>
<tr>
<td></td>
<td>(0.198)</td>
<td>(0.223)</td>
<td>(0.288)</td>
<td>(0.366)</td>
</tr>
</tbody>
</table>

| Post-treatment covariate | Yes | Yes | No | No |
| Fixed effects (a) | 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.

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Table 6: Estimated effect of informational briefing on cosponsorship, by caucus membership

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
</tr>
</thead>
<tbody>
<tr>
<td>$\hat{\text{ATE}}$</td>
<td>.044**</td>
<td>.064</td>
</tr>
<tr>
<td>SE</td>
<td>(.019)</td>
<td>(.039)</td>
</tr>
<tr>
<td>$Y_{\text{control}}$</td>
<td>0.079</td>
<td>0.088</td>
</tr>
<tr>
<td>Caucuse Members?</td>
<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td>N</td>
<td>988</td>
<td>288</td>
</tr>
</tbody>
</table>

Regressions include bill and legislator fixed effects.
Robust standard errors and p-values presented.

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

*Figure 1: Percentage of veterans bills cosponsored per legislator, by assembly. Data: state legislative website.*
Figure 2: Percentage of veterans bills cosponsored per legislator, by district veterans population. Data: state legislative website, state Department of Veterans Affairs.

Figure 3: Hypothesized effects of treatment on cosponsorship, by cosponsorship Similarity. Solid, dark blue line indicates prior, or uninformed, probability of cosponsorship. Dashed, orange reflects probability of cosponsorship after receiving information for alternative types of informational influence.
Figure 4: Observed heterogeneous effects of treatment on cosponsorship, by Cosponsorship Similarity. Solid, dark blue indicates binned cosponsorship probabilities for untreated observations. Dashed, orange reflects binned cosponsorship probabilities for treated observations. Both lines reflect fourth-degree polynomials fit to the unweighted data. The dashed straight line indicates a 45 degree line.
Figure 5: Estimated treatment effects for each legislator, by prior cosponsorship of veterans bills. Effects for caucus members are indicated by circles and for non-members by triangles. The white line reflects a polynomial regression fit to the data, with the shaded region indicating the uncertainty of the regression’s predicted values. Uncertainty is estimated by simulating 1,000 random assignments under the hypothesis that true individual effects equal those estimated.
Online Appendix A: Supplemental figures. Legislator specific estimated treatment effects.

Figure A1: Legislator specific difference-in-means $\hat{\text{ATE}}$ and statistical significance (p-values from Fisher’s exact test and verified with randomization inference). There are no negative, statistically significant $\hat{\text{ATE}}$ for individual legislators. Due to the small number of bills per legislator, $\hat{\text{ATE}}$ must be quite large (c. 50 pp) to attain conventional levels of statistical significance. Observations jittered slightly in horizontal direction to increase visibility.
Online Appendix B: Identification of legislator specific treatment effects

The research design includes 76 legislators and 16 bills, with random assignment occurring on the 1,216 legislator-bill observations.

Let $Y_{ij}$ represent cosponsorship by legislator $i$ on bill $j$. Using a potential outcomes framework, let $Y_{ij}(d_{ij})$ represent the potential cosponsorship outcome for legislator $i$ on bill $j$, where $d_{ij} = 1$ if the legislator is treated for the bill and $d_{ij} = 0$ if the legislator is not treated.$^{32}$ The parameter $\tau_{ij} \equiv Y_{ij}(1) - Y_{ij}(0)$ is the effect of treatment on cosponsorship for each legislator-bill observation. Because $Y_{ij}(1)$ and $Y_{ij}(0)$ cannot both be observed, $\tau_{ij}$ is unobserved.

We can, however, estimate average treatment effects across all legislators and bills. Further, we can estimate legislator and bill specific treatment effects because each legislator and bill is randomly assigned to both treatment and control for some observations.

Let $\text{ATE}_i$ be the average treatment effect for legislator $i$ across all bills in the study. The legislator specific average treatment effect across all bills is given by the following equation (Gerber and Green 2012):

$$\text{ATE}_i = E[Y_j(1)] - E[Y_j(0)]$$

Note we drop the $i$ subscript from the potential outcome notation since all outcomes are those for legislator $i$. $E[Y_j(1)]$ is the expected value of cosponsorship by legislator $i$ when treated on a randomly selected bill $j$ from the set of all bills in the study.

We cannot observe both $Y_j(1)$ and $Y_j(0)$ for all bills; instead, we observe $Y_j(1|D_j = 1)$ and $Y_j(0|D_j = 0)$ where $D_j$ is the actual treatment status as implemented in the study. However, because of random assignment to treatment, we can state that the expected value of all bills if treated equals the expected value of bills that are treated under a specific random assignment:

$$E[Y_j(1)] = E[Y_j(1|D_j = 1)]$$

The analogous condition holds for bills assigned to control.

$E[Y_j(1|D_j = 1)]$ and $E[Y_j(0|D_j = 0)]$ are estimated using the sample means in the treatment and control group for legislator $i$, $\hat{\mu}_1$ and $\hat{\mu}_0$. An estimate of the legislator specific ATE is thus given by the following equation:

$$\widehat{\text{ATE}}_i = \hat{\mu}_1 - \hat{\mu}_0$$

Bill specific treatment effects and overall average treatment effects across all bills and legislators are identified using the same concepts.

$^{32}$We conflate treatment assignment with treatment delivery due to the lack of noncompliance.
Online Appendix C: How are legislators influenced by information?

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:

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

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). 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:

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

In this framework, information can influence support via utility in two ways. It can reduce uncertainty ($Var(x_p)$) or correct a prior expectation ($\bar{x}_p$). This model yields several predictions about the effect of information on the likelihood that legislators support a given policy. For the purposes of this paper, it is worth noting that legislators who are below, but close to the threshold will benefit most from an informative message.

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33 This threshold could also be the utility from a status quo policy.
Online Appendix D: Placebo tests of information and position-taking

Figure 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 D1 shows cosponsorship of all bills excluding veterans legislation.

Figure D1: Percentage of non-veterans bills cosponsored per legislator, by assembly. Data: state legislative website.

Figure D2 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 D2: Percentage of non-veterans bills cosponsored per legislator, by district veterans population. Data: state legislative website, state Department of Veterans Affairs.
Online Appendix E: Estimated effects obtained from logistic regression

To ease interpretation, \( \hat{ATE} \) 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 E1: Estimated effect of informational briefing on cosponsorship

<table>
<thead>
<tr>
<th>DV: Cosponsorship</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
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<td>.054**</td>
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<tr>
<td>( \hat{SE} )</td>
<td>(.019)</td>
</tr>
<tr>
<td>95% C.I.</td>
<td>(0.016,0.092)</td>
</tr>
</tbody>
</table>

Regression Model: Logistic
Fixed Effects (a): 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 (**).